

Exploring ideas or exploiting theories? – An essay on irrelevant research and two modes for posing research questions and making research contributions.

(WORK IN PROGRESS)

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Introduction

When has someone made a research contribution? Is a contribution made automatically when an author claims that a particular piece of work has made a specific contribution? To make such claims is a common practice in introductions and conclusions in contemporary academic writing. It is, however, also a common belief that a contribution automatically is made when a text has been published in a journal or a book, although some may argue that this only is the case when it has been cited in another piece of work. A related assumption is that the higher the defined impact factor of the journal, or the more prestigious the publishing house of the book, the more significant the contribution is supposed to be. Related to this understanding is also that some authors, assume that it is necessary to cite texts published in journals with high impact factor if their own text should have a chance to be seen as have given any considerable contribution. This belief is also shared by scholars who mainly read this type of publications. Journal editors also have an interest in citations when encouraging authors to cite earlier articles published in the journal, partly in order to keep a debate going in the journal but also since impact factor algorithms are based on the number of citations of articles published in the journal. Another widespread belief is also that pieces of work that scores high in citation indexes such as google scholar or web of science gives more, or at least better contributions than work that not score so high in these measurements?

However, there is also a practical dimension attached to research contributions, manifested in expectations of that research results should be possible to use as point of departure in decision making, action or innovation processes in organizations? It is also likely that funders of research have expectations on research projects, and that the output of research should be satisfactory from their point of views. Yet another practical related situation is the form of research contributions the business school or the university where the researcher is employed emphasize. They tend to promote contributions that can be transformed into measurable outputs that make the school or university score well according to international standards used for international quality ranking (Wedlin, 2006). The most common measurement of research quality is the number of articles published in high rated journals by the scholars of the school or university? Evaluators from accreditation or certification bodies also emphasize such

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forms of publications, and seats of learning that score high in this regard may have good chances to be given his or her blessing in the form of an accreditation or a certification that is believed to represent top quality of the business school or university?

Still, although somewhat old fashioned these days, it might also happen that a more scholarly related assumption of contribution-making rules in some contexts, such as simply when a piece of work has been picked up on by other scholars irrespective of where and in what form it is published, and it not matter where this second piece of work is published?

The role of research in society

These different remarks on contribution making in research mark that there is not a general consensus in the world about what we mean by research contributions and when it can be argued that a contribution actually has been made. This creates ambiguity in society of the role of research, which sometimes leads to problems in establishing constructive dialogues between research funders, politicians, business school and university managers, evaluators of higher education, publishing houses, researchers and decision-makers in organizations. Moreover, when it comes to knowledge provision in society there are alternatives for those who demand knowledge. If they do not like the kind of contributions coming from academic knowledge production they can go to consultants, executive training, think tanks and media.

In the last decades, in the waves of the new public management movement in the Western world, academia has been under high pressure. Reforms in order to replace the Humboldtian model where the role of the university was to embrace free thinking and where decisions were taken by scholars in order to serve research and teaching have been successful in many contexts and disciplines (Ginsberg, 2011), and management and organization studies are no exceptions. Still, although the governance of higher education has, to a considerable extent, been taken over by administrators from scholarly governance, the alternatives to knowledge supply in society through the higher education system have been on the rise globally. The number of consultancies have exploded world-wide (Furusten & Werr, 2017) as has the number of think tanks of various kinds (Garsten & Sörbom, 2018). Thus, it is not an understatement that decision makers in society to a large extent are highly inspired by consultants, think tanks and media today. There is a possibility that turning to academia is considered to be a final desperate solution, although it is necessary to conduct research inquires to confirm whether this is likely to be the case or not. This suggests that if we want to discuss what a research contribution is, we also need to discuss the role of research in society, and the role of research compared to the role of alternative knowledge production and distribution.

Consequently, depending on what role research is considered to play in particular social contexts, it is also likely that people have different views of what they see as a research contribution. This can be discussed in different ways, but I will here concentrate on three general but also mutually exclusive angles of the role of research in society:

- (1) Is research the oil in the social machinery?
- (2) Is it the gravel in the machinery that makes it stop every now and then and create time for reflection and innovation?
- (3) Is the gravel in the machinery actually the oil that keeps the social machinery moving more slowly but also become innovative and sustainable at the same time?

Some research funders see research as the oil in the machinery and expect results that should be possible to implement immediately in practice. It is likely that these researchers work in an applied or collaborative research mode where the idea is to come up with research results that are experienced as practically relevant by the organizations the researcher collaborate with (REF Collaborative Research). It is not unlikely that the collaborative partner also is funding this particular research, which means that it is not to be surprised that they have practically related expectations of the research projects they fund. Still, there are also other types of research funders with similar expectations and similar views of the role of research results. Examples are industry organizations, public agencies or the like that may define research projects they see a need for and give research assignments to researchers to conduct the research.

However, there are also research funders who see research as a slowly moving knowledge system that gradually makes us wiser where higher wisdom is a goal in itself and something that is a core ingredient of a civilized democratic society? Such beliefs are related to assumptions of that if decisions in society are taken by wise and educated people, it is likely that disasters caused by ignorant decision makers can be reduced and that there are hope for a more sustainable future for the planet and for mankind. Examples of this type of funders are some national public research councils (not all) and private research foundations (such as the Wallenberg and Wallander/Browaldh foundations in Sweden and the Rockefeller foundation in the USA). This view of the role of research builds on the Humboldtian model where decisions about research and its course of development best are taken by the scholarly community. It is not necessarily, however, so that representatives of these types of research funders not see research as the oil in the social machinery, but they see as their role to have a long-term perspective and to support research accumulation where it not necessarily is so that every single project can come up with results possible to measure on short term basis. They are more inclined to embrace the idea of accumulation, where bits and pieces of research gradually build a higher wisdom, and that this is a guarantee for that society sometime in the future will be served with knowledge that leads to different types of innovation. Therefore, compared to the logic ascribed to the first category of research funders, this type is better represented by the second and third modes of what role research is likely to play in society, thus either as the gravel that makes processes to stop and thereby have a chance to take other directions, or that the gravel actually is likely to see as being the oil in the social machinery.

A more ambiguous position to what role research play in society is the one higher education organizations has today. The current system of how the output of higher education organizations is measured means a pressure on them to produce measurable results. If the results are satisfactory according to international quality measurement standards, it may mean that contemporary managers of higher education organizations

see performance measurements that match the standards as evidence of that the university or business school performs with high quality. For managers of higher education organizations, a high productivity in terms of what is measured, hopefully manifested in desired certificates and accreditation as well as top positions on prestigious rating lists for universities and business schools, is seen as that the internal processes are of high quality in terms of research contribution making. For managers of higher education organizations, considerations of this kind are central, which means that whether or not the research produced in their organizations actually give any considerable contribution of some sort is likely to be subordinated on their agendas.

Research contributions seen form an exploratory or exploitive mode

Following from the above discussion to clarify what a research contribution means is complex and can be discussed from several angles. Nevertheless, just because it is complex does not mean that it not is important to discuss. In this paper, I chose to discuss two contradictory modes for assessing research contributions, exploratory research at one side and what here is called exploitive research at the other. A starting point is that we see a number of signs of that the exploitive kind of research is winning ground in several social science disciplines. Is this a good or a bad sign? Is it a sign of progress in research where we now have come that far that we have reached a point where basic knowledge of all important social processes has reached a level of satisfaction? Where nothing new needs to be discovered and new path breaking theories not is needed any more, the only kind of research needed is the one where already achieved knowledge and theories are confirmed? This is not the policy governments or research funding organizations propose or are up to, nor is it a strategy higher education organizations are basing out. Nevertheless, if we believe in Ginsberg (2011) this scenario is the one we are on our way to realize today, where universities are run by administrators and where the role of scholars more is transformed towards working systematically in an exploitive mode in order to produce outputs that enable managers to tick boxes based on numbers of publications in prestigious international journals. Scholars who are productive in producing measurable outputs are for these reasons attractive to hire for tenure track positions and to promote. This is a development observed by many scholars (e.g. Wedlin & Pallas (eds), 2017). If this is the situation for the role of research in contemporary society it intimates that there is a well-established belief that the status of knowledge in society now is that we have reached a state where there in the world today only exist one idea of what role research and science should play and only one mode of making research that generate such desired research contributions? Still, in practice all researchers do not walk this lane, and there are differences between academic disciplines and research traditions, and there are researchers that still think there is more to discover in the world and that there are more phenomena to explore.

The exploratory and the exploitive research modes are here seen as two contradictory ends of a spectrum to assess research contributions. By discussing research contributions from these ends, the aim of this paper is to bring in nuanced perspectives in the understanding of the role of research in society and the pros and cons of research contributions made in each mode of research for future knowledge creation in society.

Who should provide contemporary and future decision-makers with relevant knowledge to tackle contemporary and future societal challenges?

“Towards the future with yesterday’s knowledge”

If research is believed to be a type of activity for exploring the unknown, this means that researchers should be engaged in crossing borders and questioning taboos. They should search for stones to turn that others for some reasons have missed to see, or left behind, to see if there is something interesting there. This means that they should not avoid questioning understandings that have become taken for granted, and they should criticise earlier explorers and their theories, and challenge already established truths as well as develop earlier theoretical achievements further and make them more reliable. If we also believe that research is supposed to contribute with nuances to institutionalised beliefs, alternatives to established norms, and important input to decision-makers, researchers should also be encouraged to go in such directions. They should be given signs and resources in order to get there. All in the meaning to come up with better explanations to why things are happening in the way they do, so that we all are better prepared to take wiser decisions for the future!

The question is, how can we get there, and what are the differences between attempts made by others than researchers in this regard, like politicians, journalists, think tank people and consultants? McCloskey (1986) once said that there is some form of methodology behind all sorts of argumentation. Her point was that there is not only one methodology that is scientific and all others are not. This means that you can work in different modes and still claim to make contributions to on-going discussions. The main difference between researchers and other potential contributors is that the methods used by researchers are supposed to be more robust so that the arguments become mobile, stable and combinable (Latour, 1987; Furusten, 1995;1999).

So, how does the way towards making research contributions look like? Are we on a collegial hunt for better explanations (explorers), or are we forced into individualistic hunting for ticking boxes of measurable individual merits (exploiters)?

Exploring or exploiting?

Exploring has a more positive connotation than exploiting, but it is hard to think of any example of exploration where no dimension of exploiting has taken place. We always wear our earlier experiences with us since everything that is taking place today do so because of actions taken before. Moreover, all actions are to some extent based on consciously or unconsciously enacted theories prominent in the social settings where the actions are taken, but also in other social settings in which these are embedded. Thus, what we do and how we do it is socially constructed over long periods of time. If

we put this meaning into exploitation, all actions are to some extent likely to be seen as exploitations of theories.

The main difference between exploring and exploiting in the meaning used here is to separate between the purpose and ambition behind particular actions. One extreme is the classical adventurer who travels to far-away places where no civilized man has been before. This is the image of the classical discoverer who investigates white spots on the map and draws new maps or complement already existing ones. The ambition is to explore what no one else has seen, to discover new parts of the world in order to make the map of the world more complete.

The exploiter, on the other hand, is a researcher who uses theories developed by others with no ambition to explore something new or to find new explanations to particular phenomenon. What primarily drives them is to focus on publishing in a system where quality in research is assessed in terms of number of publications in high ranked journals.

Next I will discuss these two modes of making research contributions and argue that they represent two distinctively different ways of understanding the role of research in society.

Exploring ideas

The meaning of exploring is to study, examine, analyse or investigate something not already known. It is a way to become familiar with something not known, by testing it or experimenting with it and improvising in order to bend the limits for what is reasonable and relevant. Consequently, exploring is not only a practice restricted for researchers. It is something all sorts of people do while learning, but exploring in research means examining something, a thing or an idea systematically, i.e. not randomly looking for anything. This means that the explorer does not rely on that discoveries come by chance, meaning that we know what we are searching for but we do not know what we will find. This means that exploring in research means that we are on a hunt for explanations, and we try to use reliable methods to search for what we are looking for. This can be manifested in the roads we take to go to new places, combined with how we characterise what we see on our way and what is going on there. Exploring means to open our minds and curiosity and search for structures, beliefs, norms, mechanisms and patterns of behaviour in a new place. A place in this regard does not necessarily have to be a place in a physical geographical meaning, it can be an organization, a social context, a web chat and so on and so forth. Thus, it can be a physical or virtual place, a discourse or action where something is going on, where we suspect that something actually is going on but we are not sure of what and in what way and why it turns out in the way it does. It can also be a search for what was going on in a context in the past. This means that when consciously employing certain methods we do not just let things come to us, we do not rely on serendipity.

Exploration	Serendipity
• Purposive	• Informal experimentation
• Systematic	• Accidental discovery
• Prearranged	• Spontaneous invention
• Exclusive	• Highly democratic
• Elites	• Ideas produced by chance
• Routinely produced ideas	• Social conversation
• Professionalism	• Play (non-professionalism)
• Collegial enterprise	• Democratic – open for everyone

The table shows that things can be discovered both through exploration or serendipity, but the latter is not the result of intended actions, it is more accidental. It is true that important discoveries can happen by serendipity, that the hunt was to find explanations to something else, but while walking this road something unexpected showed up, something that probably not would have been seen if this particular path had not been trodden. Still, in its ideal form discovering through serendipity is open for anybody since it means that things just happen by chance, meaning that true serendipity is a democratic form of discovery open for anyone, no one is excluded. In practice, however, not all will find their way in to hidden places where particular new discoveries can be made through serendipity. To find such places takes certain skills and experiences, and to do research in an explorative mode is about taking off somewhere purposive, systematic, and prearranged. Such exploration is only possible to be open for professional elites. This is for example the case in certain fields of professional work, such as artists, scientists, and entertainers. They routinely explore, whereas, the people at play (both children and adults), and the seekers of sensory stimulation never do this, an observation that holds equally well for many nonprofessional kinds of work.

”... social science researchers ...should not rely on accidental serendipity but instead try to discover new ideas by systematically exploring social groups, processes, and activities. To accomplish this, however, they must intentionally put themselves in a position to make discoveries, rather than carrying out their daily research agenda by passively awaiting the moment when they are struck, as it were, with serendipity.” (Stebbins, 2001)

Thus, as researchers working in an explorative mode we cannot wait for data to come to us. We have to go to places where the phenomenon we are exploring is likely to take place and try to observe it when it is happening or talk to people that have been involved when it was happening, or read evaluations and other reports about what did happen. In explorative research, it is important to share observations with other scholars and practitioners. It is only by sharing we can make contributions and it is also important for qualifying the data and analyses. Thus, sharing results and publishing is central, but if the standard is that the only type of publication that counts is the ones that can be published in top journals, this important publishing of science in the making is in danger. Who dares to take time and energy to engage in this when it does not give any credits in the individual merits portfolio? In explorative research, all forms of publications are important and play a role to keep the scientific accumulation machinery going.

In sum, explorative research means research that aims to generate new ideas and weave them together to form grounded theory, or theory that emerges directly from data (Glaser & Strauss 1967). It does not necessarily have to be grounded theory in its pure forms, but it represents research carried out in ambitions to find better explanations to what is going on in society. It means that the mode of explorative research is the opposite to confirmatory research where the main activity is to test theoretically generated hypotheses, and the goodness of the test hinges on the quality of the research design. It may look as exploratory research is glorified here, that is, however, not the main point in this paper. The point is rather to characterise different types of research practices and for that purpose develop a dichotomy of two extreme positions. As defined by Stebbins (2001) social science exploration is a broad-ranging, purposive, systematic, prearranged undertaking designed to maximize the discovery of generalizations leading to description and understanding of an area of social or psychological life. Such exploration is, he means, depending on the standpoint taken, a distinctive way of conducting science—a scientific process—a special methodological approach (as contrasted with confirmation), and a pervasive personal orientation of the explorer. Defining exploratory research in this way, as a hunt for discovery, also marks a critical standpoint against too much focus on exploitive research. This is very clearly expressed by Kirk and Miller, 1986, p. 15 in Stebbins, 2001) who say:

Most of the technology of ‘confirmatory’ non-qualitative research in both the social and natural sciences is aimed at preventing discovery.

Exploiting theories

Exploration requires flexibility and open-mindedness differing from exploitation and its reliance on control of variables and prediction of outcomes using hypotheses. Exploiting also has a negative connotation. It intimates actions taken in a meaning to treat someone unfair and benefit from their work. Confirmatory work is a form of what here is meant by doing research in an exploitive mode, where the starting point is the work of others, from which researchers construct hypotheses and then test them in order to confirm or deny them.

This means that exploitive research as the concept is used here not is the same as what we all do when we discover things. We always use and rely on the work of others. No one ever starts from a blank sheet. Researchers read the work of others, consult colleagues, present our work at seminars and conferences and take notes and listen to comments and suggestions. Researchers also benefit from comments and suggestions in reviews in peer-review processes. This is how the collegial quality assurance system is supposed to work in the science community. Researchers are usually keen to use all input they get in these ways. However, compared to the negative connotation of exploitation, these inputs are intentionally shared between colleagues, and they are given by individuals to individuals. This means that although scholars benefit from the work of others, this is a shared act, meaning that using what is intentionally shared not is something that is on the cost of others. This is not what is meant by exploiting theories

here, it is rather an example of how the professional peer-review system in science is supposed to work.

Exploiting theories in the meaning referred to here is instead a research practice that is something different than departing from the work of others in discovering new things. Basically, exploitation as used here means a walk on paths already trodden and constructed by others without intentions of discovering something new. It means looking out on the world through key wholes others have designed without ambitions of seeing something new. The ambition is instead mainly to use the work of others as means for getting published as quick and as much as possible.

Researchers are today in general and to a larger extent than some years back rewarded based on how much they publish and where, and the technique for posing research questions have turned more into gap-spotting and gap-filling than curiosity and interesting problematizing (Alvesson & Sandberg, 2011). To some extent this general development of research can be seen as normal when research fields are maturing. More and more narrow research lanes are established that all tend to be dominated by particular schools of thoughts. One way to define this development is to characterize management and organization science as to a larger extent have been sorted in an increasing number of knowledge silos where the main interest is to maintain and safeguard particular schools of thoughts. This means that either you are in or you are out of these schools, and if you want to get published you have, to a larger extent today, be aware of what kind of work that is likely to get published in particular knowledge silos. Thus, walking the narrow lanes defined by others, pick up on the research gaps they have defined and profit from this by starting to fill them without problematizing the need and meaning for it, means that you become a user of others' arguments, not an explorer that discovers new things with the help from studies made by others. This is what is meant by exploiting theories in this paper.

Based on my own observations during the last decade at international conferences and academic seminars, there is a trend that the focus in the academic discourse has changed from being mainly explorative, and focused on how the research could be developed into the next step towards mainly be about how single papers can be improved in order to get published. Young scholars without tenure positions tend today, for good reasons if they strive for a career in academia I must admit, to a larger extent be driven by a pressure to publish a fixed number of articles in a limited number of particular top ranked international journals. The place for doing this becomes more crowded all the time since more and more scholars are targeting the same journals. The interest in explorative, critical and boundary spanning, taboo breaking discussions is becoming more and more rare. Courses and discussions of how to get published replaces courses in cross-disciplinarity, multi-methodology and perspectives of theories. It can therefore be argued that the international research community is in a more instrumental turn of producing publications rather than producing research and new discoveries and better explanations to societal phenomena. This means that today we see a greater number of theoretical exploiters in the international research community that are:

- Spotting theoretical gaps defined by others
- Following arguments made by others

- Are strategically narrow minded in order to publish at the right places
- Technically skilled in how to set up publishable articles
- Specialized disciples of silo-ified theoretical schools
- Publish focused rather than knowledge generation focused

An exploitive turn – so what?

The dilemma of posing research questions

That it is fair to state that we are in an exploitive turn of research is supported by observations by a number of scholars (e.g. Wedlin, 2006; Ginsberg, 2011; Alvesson and Sandberg, 2011; Pallas and Wedlin, 2017; Mingers & Willmott, 2013). This is also a mark of a growing criticism directed towards the relevance of the kind of management research that is dominating the business school research. Alvesson & Sandberg (2011) address, for example that there is too much focus today on filling research gaps and too little on posing interesting research questions. There is an underlying criticism amongst many scholars at many universities and business schools, at least in Europe, about the managementization of higher education and research where constantly more focus is directed to the production of measurable results and productivity. The focus is on organizations within the system and their productivity, and based on what is measured and how, the university is ranked and compared to other universities in the world. This means that universities, and departments and schools within them, compete with each other in terms of ranking, and the focus from management is to increase the production of measurable output, thus the kind of output that the ranking institute has decided should be measured (Wedlin, 2006). This is a consequence of what Brunsson and Sahlin-Andersson (2002) describe as an increase in the number of organizations in society that all are supposed to have hierarchy, rationality, members and objectives. More focus on organizational performance is therefore not a surprise. Still, it may have consequences for the joint endeavour of the development of higher education in general and research in particular.

Universities that play this game seriously also develop systems for recruitment and promotion where it is likely that they favour a research output that fits to the global ranking criteria. This means that there is a risk that scholars interested in positions at contemporary universities or in getting research grants, experience a pressure of that the most important output from research is that it is publishable in high ranked journals since a large production of such research means that the university is likely to strengthen its position in the international ranking game.

If the focus of producing research is on publication instead of exploration of societal phenomena and to develop theories for how to handle future social challenges, then the criticism against an increased irrelevance of current research is easy to understand. This has consequences for how research questions are posed. Questions that are critical and challenging to dominating theories, or that suggest cross -“silo” or multi-disciplinary

design may be seen as too risky to support. Instead, it is likely that constantly more efforts are devoted to less risky research that follows established theories and methods, and more are repetition of arguments already made than provoking thoughts that challenge them.

Posing research question and the irrelevance of research

With the discussion above of the explorative and exploitive modes of the practice of research and the introductory discussion of the role of research in society in mind, it is also important to pose the question of how we pose research questions, and how we motivate why a particular type of research is needed. Thus, when should research be done? When is it motivated to do research? The layman answer is probably – “when there are white spots that have not been explored before”. This trigger, however, a follow up question. If we do not know about it, how can we know that it is a white spot? Moreover, there might be reasons for that this spot has not been explored, and it is also worth considering if all white spots should be explored? Maybe we know enough without exploring a particular spot? In history sciences, it is not unusual to find scholars that are really excited when they find a historical sequence that has not been told before, like when the history of an old company has not been written, the biography of a famous business man has not been told, or when the story of rural life in a small village in mid Sweden in the late 18th century not is told. Seen from this perspective there is an enormous number of white spots in society, but will we be wiser if all of them are explored? Must everything unknown be revealed just because it is not?

Another layman answer to when research in social science is motivated is when there are societal challenges that we need to know more about how to tackle. A topical issue today is, for example, what organizations have to do in order to contribute to a sustainable future of the planet. How should they both be run with financial efficiency and take responsibility for society and the future? How can these two dimensions be combined in innovative and effective ways that also generate profit to the owners? This is seen by many decision-makers in society as a huge white spot and they put lots of resources into exploring it. Traditionally most resources put into research about sustainability have been directed to natural science, in order to develop more sustainable transportation, production and energy techniques, but also in how to scrutinize the condition of the planet. Today, however, the interest has spilled over more to governance, organization and management issues. Still, even though sustainability is topical, should anything be explored? Is everything important to study, and how can we know when we know enough? When is the white spot explored to a satisfying extent?

Exploring white spots, the unknown, hunting for explanations to societal phenomena and developing theories for how great societal challenges can be tackled are examples of empirically based research questions. If we believe Alvesson & Sandberg (2011) this is, however, not the most common starting point for posing research questions today. They argue that gap-spotting in existing literature is what dominates. This means that scholars, to a larger extent today than before, tend to motivate their research in terms of that theoretical gaps have been identified in earlier studies, and because of this there is a need for research that fill them. This is, in a way, the same logic as in history science

when someone finds a story not told before. The question is, however, what more will we learn about what if that story is told. Transferred to gap-filling what more will we learn if the theoretical gap specified by someone is filled? Is it necessary to do it because it has not been done before?

Posing research questions on the logic that someone else has pointed out that there is a white spot or a theoretical gap is one way to do it. How should we define this behaviour? Is it exploring ideas or exploiting theories? If we see research as a collective journey this is a reasonable strategy. Someone else takes on and add where someone else ends, and if it is suggested by someone that one limitation in one's own work is that more studies of a particular kind is needed, and when someone picks up on this call and follow the suggestion, it is not exploitation in its literal meaning here. However, if the intention not is to participate in the collective explorative journey, and contribute to a collective struggle to develop better and more comprehensive theories for the understanding of why things are happening in the way they do, then it can still be exploitive. It is a matter of research ethics and sharing.

Whether or not exploration or exploitation is the main driver for the researcher to pose research questions in terms of white spots in need of exploration or theoretical gaps that need to be filled, the question of what contribution the one or the other type of research question will bring needs to be considered as well. Should all spots be explored and all gaps be filled, or are there other ways to pose research questions? There is today a growing criticism against the relevance of contemporary research efforts. A constant narrowing of research focus, reduction of risk taking and criticism, increased tendency to follow the leaders and repeating of arguments as well as exploiting the research by others, the greater the risk of producing irrelevant research!

Mind the gaps!

It is, of course, reasonable to argue that there is a need for more research because there is a white spot or a theoretical gap, but is it the only way, and in all cases the most relevant ones? These questions are important to rise today since there is, no doubt, a turn in management and organization science for normalization. This suggests that management and organization science is about to be established as a normal science. To become normal is, of course, not completely problematic, but along with it comes standardization. Not even standardization is necessarily problematic, but if the standards are based on what is considered as normal science, then it is important to reflect on where this is taking us.

The standard normal science can be defined as consisting of a set of qualifying mechanisms, such as established theories and methods, peer reviewing by anonymous reviewers, fixed criteria for quality quantified in number of publications in top journals, fixed criteria for positions and promotion. This suggests that there is one way, and one way only for making an academic career. The signal is walk this road or forget about it! The narrower you reach, and the truer you are to dominating arguments made by academic "stars", or topics, theories and methods favoured in particular journals, the more likely it is that you will become published.

Normal science means that a point is reached where the borders of a discipline are set and the most important discoveries within it are made and that it now is time for polishing the theories by filling the gaps. This is then supposed to lead to a state where we have solid theories that can be used in order to govern society into a desired order, and not only an ideologically desired order, but a scientifically proven and evidence based order. This suggests that management and organization science should be on the move to reach the same status as economics and maybe soon have a Nobel prize of our own. If we see ourselves as we are on this journey, that we are part of this grandiose enterprise, then it makes sense of hunting gaps, be on tours for gap-spotting. It makes sense to fill collectively agreed gaps, and collective agreements are only supposed to have taken place when such things are pointed out in articles published in top ranked journals.

The ranking system is supposed to help us sort out good from bad science, and if you really want to make theoretical contributions, it is only work published in the top ranked journals that counts, and the idea of normal science also suggests that research means accumulation of knowledge, which on its turn is supposed to mean that the latest publications always are the best! This gives us a system where research questions become more and more narrow, where the purpose of doing research more and more is about to stabilize already made discoveries, where we are more concerned about confirming what others have said before us. As a consequence, we see more research designs where hypotheses are tested.

This can, of course, be the road we all should go, that we should construct knowledge silos with clear barriers, strict contents and standardized methods, that we should not search for cross-fertilization between knowledge silos, that we should not have open minds and go multidisciplinary, and that we thereby should leave the handling of great societal challenges to consultants, think tankers, politicians, lobbyists, journalists, activists and managers of large international organizations! Or, maybe we should mind the gaps, think carefully about if they really are there, if they are, if it is worthwhile filling them or if they are there because a particular theoretical lane is a dead end. Going further in that direction might not be the way forward. Maybe the dominating argument in an established knowledge silo needs to be confronted with arguments cultivated in other knowledge silos! Maybe a specified theoretical gap not really is a gap, maybe it is a signal of that a critical juncture might have been identified, and where it now is relevant to mobilize new energy, bring in other thoughts, theories and methods to be able to improve the explanations to why things tend to turn in the way they do. Maybe it is not true that everything already worth discovering have been explored, and maybe paying too much energy on exploiting theories is a mechanism that drives research to provide less relevant contributions.

The dilemma of making contributions

As briefly discussed in the introduction, defining when a contribution has been made is debatable. From a normal science perspective, it is however easy. A contribution has

been made when an article is accepted for publication in a peer-reviewed journal. The higher ranked the journal is, the more significant and important contribution. From this perspective, not much contributions are to be found in monographs, or even worse book chapters in edited volumes or articles in low ranked journals. Instead, from a normal science point of view, scholars who successfully publish a fixed number of articles in top ranked journals automatically become top scholars. Universities with ambitions to score high in global university ranking want to hire these people, or promote them as quick as possible if they already are employed. It is like in international football, where the rules in science have become as clear as in football. In football the team that scores most goals wins the game, and the team that wins most games becomes the champion. There are clear rules for when a goal has been scored, and having top scorers on the squad increases the team's chances to score more goals than other teams. So far, the metaphor works. Scholars with great record of nice publications can be seen as top scorers, and having top scorers on board at universities increases the chances for the university to score high in rankings. However, football teams cannot only have top scorers in the squad, there need to be players with other qualities and ambitions on board as well. There need to be players who are skilled in winning balls from the other team, players with qualities in preventing other teams from passing the ball, in marking their top scorers, in passing the ball, in running fast and in chasing and stressing players in the other team and so on and so forth. In order to make the team work, and in order to facilitate for the top scorer to be able to score, the team need players with different qualities. The normal science perspective, however, suggests that universities should only have scholars with one quality on board, to get published in top ranked journals. The only purpose with research has then become to produce publications. This system leaves the exploration to the past, or to a few that have taken the lead in the global knowledge silos, leaving the rest of us to becoming exploiting followers!

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