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Wicked problems and innovation research: A conceptual review

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ABSTRACT

Purpose of the study: The catastrophic consequences of the COVID-19 pandemic seem incalculable. The scientific discovery system seems to have failed to anticipate or adequately address the consequences of the outbreak- a form of research failure. The purpose of this paper is to argue that this research failure reflects a wicked problem of a lack of innovativeness, one not only of natural science, but also of social science, given that the natural science system is itself nested within human systems of behaviour.

Design/methodology/approach: This paper takes the form of a conceptual review paper.

Findings: Knowledge of the coronavirus-related consequences of research failure was absent prior to the outbreak. In hindsight, potential externalities, or benefits to society of improvements in the discovery system, and its management, are now evident. Rittel and Webber and Nelson describe certain characteristics of wicked problems, which are taken here to provide useful insights into how to address the wicked problem of research failure.

Recommendations/value: A novel proposed research agenda suggests how improvements might be achieved in the productivity of the research process itself.

Managerial implications: The novel research agenda suggested here suggests that improvements to research effectiveness may require the further development and formalisation of a sub-field of management, namely 'research management'.

Keywords

COVID-19; Coronavirus; Innovation failure; Management research; Research failure; Research management

JEL Classification: O32

1. INTRODUCTION

For millennia, humankind has been under threat from various cataclysmic events. In the past several decades humanity has contributed directly to life-threatening events, requiring urgent global attention. In 2004, a tsunami cost the lives of almost a quarter of a million people, one of the deadliest catastrophic disasters in recorded history. In 2020, the catastrophic loss of human life due to the rapidly spreading coronavirus has been described as a tsunami (Allemandou & Dodman, 2020), as have the economic consequences of long shutdowns during the outbreak (Cameron, 2020). Having predicted the current outbreak, a growing body of literature is concerned with similar and other existential threats to human existence.

Such threats are numerous and take many forms. They include the proliferation of knowledge of advanced biotechnology, nanotechnology, machine intelligence, and other dangerous technological applications, as well as the catastrophic consequences of global pandemics, climate change, and human geopolitical conflict (for a review of these scenarios, see Callaghan, 2018). According to Bostrom (2013:15):

> "Existential risks are those that threaten the entire future of humanity. Many theories of value imply that even relatively small reductions in net existential risk have enormous expected value. Despite their importance, issues surrounding human-extinction risks and related hazards remain poorly understood…A moral case can be made that existential risk reduction is strictly more important than any other global public good…Perhaps the most cost-effective way to reduce existential threats today is to fund analysis of a wide range of existential risk and potential mitigation strategies, with a long-term perspective."

The costs of a failure to invest in research to mitigate such threats are usually only visible in hindsight, as the consequences of the coronavirus outbreak illustrate. According to Callaghan (2018), exponentially increasing stocks of technological information and knowledge are not being matched by a similar increase in wisdom or knowledge of how to manage these phenomena. The problem of catastrophic threats is therefore a problem of management, requiring management theory development, the focus of a subfield of management- 'research management.' Research and R&D may however currently be facing a crisis, which may have contributed to a catastrophically inadequate response to the coronavirus epidemic.

An argument that research is facing a crisis in its productivity is supported by economic models that predict that it is becoming harder to produce discoveries or develop new ideas (Kortum, 1997; Segerstrom, 1997). These models, in turn, find support in evidence that for decades now, returns to (increasing) investments in the employment of researchers and in research resources have been declining- a burden of growth effect (Jones, 2009). Similarly, Gordon (2016) points to a decrease in total factor productivity that has occurred since the 1970s in the form of a global productivity slowdown (for a recent overview of the problem of slowing global productivity growth from a science, technology and innovation perspective, see Soete, 2019).

If the consequences of the coronavirus pandemic provide an appropriate example of the costs of research failure, then they also highlight the absence of a dedicated stream of literature in the management domain focused on the management of research or the research process itself. Lacking from the literature is a stream of literature that is specifically and explicitly concerned with addressing research failure. The development of such a sub-field of the broader management field may be of importance at this time.

Applying a conceptual review methodology, this paper reviews relevant theoretical literature in its attempts to derive insights into how to address research failure. The objective of the paper is, therefore to derive implications for the current practice of management and management research by identifying certain failures of the current paradigm of management research to solve existential threats. We pose the research question, Why has current practice of management and management research failed to solve existential threats to business itself, the same threats that threaten humankind itself? Our approach contributes to existing management scholarship, in that we link literature from innovation to that on wicked problems and provoke novel thinking about solving problems that are understudied. We suggest that without such novel and provocative thinking, the consequences for businesses and its stakeholders might ultimately be existential. Another primary contribution of this work is the notion that the ‘management’ of the scientific research process itself and the development of appropriate scholarship in this area may help to avert catastrophic threats. Yet, seemingly absent from the science and innovation policy literature is sufficient literature suggesting that management as a field may be uniquely placed to fill this void.

Based on the analysis, this paper also contributes to the literature by making the following arguments. *First*, it argues that the processes of natural science are nested, or embedded, within networks of human activity. These networks suffer from problems similar to those of certain other human systems in that the problem of research failure might itself be characterized as a wicked problem. In other words, our scientific research system- the discovery system- itself suffers from a wicked problem- of research failure. This crisis might broadly be described as the ‘burden of knowledge’ effect (Jones, 2009), in that it seems to be becoming more difficult for an individual to reach or keep up with an expanding frontier of knowledge. According to Jones, this effect is reflected in the rise of collaborative research teams, longer periods of doctoral study, and the increasing ages of Nobel Prize winners.

Second, the paper argues that social science research that obscures, rather than enlightens, can be dangerous, as it abdicates its responsibility to light the way to safe solutions to the wicked problems of our time. If the proliferation of technological knowledge increases the vulnerability of human populations to the dangers of this same proliferation, what is then not clear are the potential effects of this proliferation in exacerbating dangers associated with wicked problems. Rittel and Webber (1973:168) define wicked problems as problems that rely on “elusive political judgement for resolution” but not “solution” as social “problems are never solved”- at “best they are only re-solved- over and over again.” Wicked problems are, therefore, “those that are complex, intractable, open-ended, unpredictable” (Alford & Head, 2017:397). As indicated, an important argument of this paper is that the lack of innovativeness of our scientific discovery system itself is caused by the complexities of human behaviour that are studied by social science (and management), but that this lack of innovativeness is a wicked problem in itself.

Third, this paper argues for a *revolution in social science thinking, in that if we do not solve certain longstanding wicked problems we will soon face existential threats*. It is also argued that the current coronavirus pandemic is an example of such an existential threat. In seminal work, Nelson (1977; 2011) highlights the consequences of these types of problems in his ‘the moon and the ghetto’ metaphor. Nelson (1977) asks why it is possible to visit the moon but not to solve problems such as ensuring adequate education for the poor (those in the ghetto), or to mitigate rising healthcare costs, reduce pollution, or eliminate drug abuse. These problems, together with others associated with immigration, global warming, amongst others, still defy attempts to solve them (Nelson, 2011), hence they are described here as wicked, following Rittel and Webber (1973). Despite their seeming insolubility, these problems remain knowledge problems and the subject of research efforts to solve them.

Other literature, however, seems to be more optimistic with regard to our eventual ability to solve knowledge problems. For example, a stream of literature suggests that there should be scale effects in knowledge creation, and that our capability to learn and manage these problems of technological proliferation should outstrip the rate of increase of these problems. In particular, Romer's (1990) endogenous growth theory suggests that knowledge of how to better combine resources can take the form of 'recipes' of knowledge that can be shared.

In this way, knowledge recipes created anywhere in the world can proliferate, benefitting everyone. Of course, not all such knowledge can be appropriated, or used by others. Nevertheless, Weitzman's (1998) arguments that relate to recombinant growth suggest that there should not be a shortage of productivity growth, as there is essentially no limit to how many times otherwise scarce resources can be combined and re-combined. Indeed, according to the principles of probabilistic innovation (Callaghan, 2017), the problem-solving technologies of the fourth industrial revolution might be enhanced by radical increases in research productivity enabled by the same technological advances. For example, crowdsourcing has been shown to reduce time and costs involved in complex research fields such as biomedicine- crowdsourced R&D being the application of crowdsourcing to scientific or formal research.

As discussed, less optimistic scenarios are summarized by literature suggesting that we are currently under threat from a host of existential threats (Bostrom, 2013; Callaghan, 2018) and that our investments in research might be yielding declining returns (Kortum, 1996; Segerstrom, 1996; Jones, 2009). If this is so, then the rate at which we can create knowledge of how to manage this proliferation may be outstripped by an exponential proliferation of information and technological capabilities (Callaghan, 2019). For example, there are currently debates about whether it is now already too late to address increasingly catastrophic consequences of climate change (Farbotko, 2020; Hulme, 2020; Jewell & Cherp, 2020; Moser, 2020; Shultz, Sands, Kossin, & Galea, 2020), and it is clearly too late to halt the consequences of the coronavirus pandemic, which are still currently escalating.

The proliferation of these existential threats may be inexorably related to the way technology will change over time, and what these changes will be in the near future. According to Mokyr (2010:14):

> "Technology moves at a certain speed and in certain directions, and the study of innovation helps us understand these laws of motion. Moreover, to come to grips with why technology

> changes the way it does, we need to be clearer about the way in which prescriptive knowledge (technology) and propositional knowledge (science and general knowledge about nature) affect one another. Knowledge about the physical environment creates an epistemic base for techniques in use. Technology, in turn, sets the agenda for scientists, creating a feedback mechanism…Technological change, like all evolutionary processes, was often wasteful, inefficient, and frequently wrong-headed. It was inevitably so, because by definition the outcome of the project was unknown, and so mistakes were made, duplicatory efforts took place, blind alleys were entered. Moreover, a great deal of what seems to use successful innovation was not adopted, for reasons that ex post seems hard to fathom and at times frivolous. But the degree of inefficiency of the innovative process was not constant over time".

The innovative process is not necessarily efficient, its outcomes may be uncertain, and path dependent technological change might not necessarily result in optimal outcomes (Arthur, 2013). Whether it is too late to arrest the onset of existential threats may depend on how quickly we are able to innovate or create knowledge of how to effectively manage these threats. More specifically, whether it is too late to address these threats may depend on the impact of the proliferation of information and novel technological potentialities on the dangerous, or potentially catastrophic wicked problems we are currently facing. Even technologies such as social media can shape sociopolitical forces, and collective behaviour can have powerful outcomes (Callaghan, 2016).

Given these debates, as discussed previously, the objective of this paper is to provide a conceptual critical review of literature relating to wicked problems, and to make the argument that we do not have a choice but to radically improve the effectiveness of our research processes to solve wicked problems facing humanity, and particularly the wicked problem of a lack of innovativeness in our scientific discovery system- termed here ‘research failure’. It is also argued that management as a discipline should rise to this challenge. This challenge is captured by Nelson’s (1977) metaphor of the moon and ghetto, a useful analogy for the issues introduced here. Finally, an argument is presented, that our ability to avert impending climate and other catastrophic disasters hinges on whether we either fail or succeed in applying novel technological knowledge and innovations to the knowledge-creation process itself.

The paper proceeds as follows. First, in Section 2, the methodological approach of the study is discussed. In Section 3, wicked problems are discussed as a particular type of problem- the type we urgently need to be able to solve in order to avert existential threats. Current thinking is shown to now consider wicked problems in light of a sense that time is running out to solve them. Next, in Section 4, literature is reviewed that suggests that technological outcomes do not necessarily evolve optimally, and policymaking cannot assume that technological path dependencies will (on their own) solve problems that are becoming existential threats. Disequilibrium is discussed as a natural paradigm of problem-solving thinking that may be appropriate for solving wicked problems. Section 5 considers Nelson’s work on problem-solving to derive insights into the wicked problem of a lack of innovativeness in our discovery system. Solutions to the problem of research failure are then discussed. Section 5 considers limitations and recommendations for further research. Section 6 discusses limitations and makes recommendations for further research. Section 7 concludes.

2. METHODOLOGY

The paper follows precedent of other conceptual papers to provide a narrative conceptual review of the conceptual terrain relating to wicked problems and innovation research that has sought to understand how to solve them. On the basis of analysis, certain themes are identified, and propositions are derived. These propositions form the basis for what we suggest is a useful agenda for further research. Our approach is appropriate in that research questions we raise are answered by a synthesis and integration of diverse and multiple theoretical ideas.

3. WICKED PROBLEMS

If it is becoming increasingly dangerous to fail to solve wicked problems, it is important to understand specifically why they are so difficult to solve. According to Rittel and Webber (1973:135):

> “A great many barriers keep us from perfecting [an idealized] planning/governing system: theory is inadequate for decent forecasting; our intelligence is insufficient to our tasks; plurality of objectives held by pluralities of politics makes it impossible to pursue unitary aims; and so on...the classical paradigm of science and engineering- the paradigm that has underlain modern professionalism- is not applicable to the problems of open societal systems. One reason the publics have been attacking the social professions, we believe,

> is that the cognitive and occupational styles of the professions- mimicking the cognitive style of science and the occupational style of engineering- have just not worked on a wide array of social problems…We shall want to suggest that the social professions were misled somewhere along the line into assuming they could be applied scientists- that they could solve problems in the ways scientists can solve their sorts of problems. The error has been a serious one."

From Rittel and Webber's perspective, inappropriate problem-solving methodologies have caused wicked problems to proliferate. Given their complex nature, any perspective that defines wicked problems also implies their solution, according to the same definition- wicked problems, therefore, need to be defined more precisely, using more finely grained approaches (Alford & Head, 2017).

For Rittel and Weber (1973), wicked problems essentially exhibit ten characteristics. (1) They cannot be definitively formulated. (2) They have no stopping rule- no criteria to indicate that or when a solution has been found. (3) Their solutions are not objectively true or false, but subjectively good or bad. (4) They have no objective tests of their solutions, as the consequences of actions continue forward in time. 5. There is little opportunity to learn by trial-and-error, as every solution can be consequential. (6) They do not have an exhaustive set of solution or actions that can be described. (7) They are essentially unique. (8) Wicked problems can often be considered to be symptoms of other problems- the level of a problem cannot be determined on logical grounds. (9) How a wicked problem is explained will define its solution, and there are multiple explanations. (10) People are liable for the consequences of their attempts to address wicked problems.

The term 'wicked problem' has, however been used in different ways. According to Alford and Head (2017: 410/411):

> "The term 'wicked problems' has been bandied about indiscriminately by some public officials and scholars. Some of them use it to describe situations which they effectively over-estimate: problems which might be somewhat complicated in conventional terms, but not in fact wicked, as exemplified by large engineering projects like the Channel Tunnel. Others use it to describe problems that are in fact wicked, but from varying perspectives about policy implications. Of these, the more pessimistic overestimate the difficulty, seeing these problems as insuperable and therefore in the 'far-too-hard' basket. Others

> acknowledge the intractabilities, but nevertheless err on the side of optimism, assuming these problems are susceptible to the usual mix of rational understandings, organisational routines, coordination systems and databases."

What is novel in the approach we take here is our categorisation *of the human scientific research system itself and its failure to innovate sufficiently to address serious societal problems as a wicked problem*. This academic failure has important implications for its study that we feel are novel and that require a different research focus and agenda than many current approaches. Some have extended discussions of wicked problems to a consideration of 'super wicked problems,' in that consequences arise if they are not solved.

Some have used the term 'super wicked problem' to describe problems with four characteristics- namely (1) that "time is running out; (2) that those who cause the problem also seek to provide a solution; (3) that the central authority needed to address it is weak or non-existent; and, partly as a result, (4) policy responses discount the future irrationally" (Levin *et al*., 2012:123). These four features together contribute to a policy-making tragedy in that traditional analytical techniques are poorly suited to solving such problems.

To mitigate the potential for policy tragedy associated with super wicked problems, path-dependent policy interventions are needed that "constrain our collective selves" by triggering sticky interventions that entrench support while expanding the group of people that they cover (Levin *et al*., 2012). Thus, "nurturing countervailing polices that might trigger path dependent" outcomes that are societally beneficial might be helpful, in that "generating path dependencies might foster desired policy outcomes in the future" (Levin *et al*., 2012:124). Management research should focus on how policy can mitigate research failure by framing it as a wicked knowledge problem, in a similar way to other wicked problems such as climate change- the difference being that investments in solving research failure might pay off in research solutions to problems like climate change.

According to this approach, it might be important to identify the interaction of certain policies with their potential to trigger positive path-dependent processes, even if they may initially be fragile, or be relevant only to a small group. This approach might be necessary to mitigate a tendency of political institutions to give more weight to short-term interests at the expense of long-term interests (Levin *et al*., 2012). Levin *et al*.'s classification is important in its focus on the fact that *time seems to be running out* (Bostrom, 2013; Callaghan, 2018) to solve certain wicked problems. According to Levin *et al*. (2012:127):

> "The notion that time is running out separates many environmental concerns from social challenges. In the latter case, much of which is considered appropriate policy is mediated by the political system. Stakeholders with various interests interact and attempt to influence each other's policy preferences. The political system then responds, or fails to respond, with some kind of policy intervention. Losing coalitions tend to regroup, build more support for their ideas, and then attempt once again to influence the policy agenda…Those wishing to address super wicked problems such as climate change, however, do not have the luxury of 'coming back' to the political system for a retry, exacerbating the 'one shot' problem noted by Rittel and Weber. The time dimension will, at some point, be too acute, have too much impact, or be too late to stop or reverse…Climate change is arguably the most illustrative case of time running out. Significant impacts will occur; with each passing year, they become more acute; and if we do not act soon, the risk of harm to human communities and ecosystems, as well as non-linear change and catastrophic events, increases."

The notion that time is running out is therefore a key theme in this literature, even if policy responses to these threats have to date been ineffective. There seems to date to have been insufficient policy responses to these threats. What might account for this lack of policy response? Is it possible that many of the world's policy decision makers believe that technological progress will solve the most pressing of these problems on its own? Or that markets will solve them on their own?

4. THE TECHNOLOGICAL FALLACY

With reference to Kay's (2013) discussions of the contemporary typewriter standard, according to Arthur (2013:1186):

> "…QWERTY, as a standard—or better as an example of what the market has served us up in the long evolution of one particular technology- has become in economics a focal point, a rallying point for a larger issue: whether the market can lock us into an inferior standard. And this itself is part of a still larger issue: whether the free markets of capitalist economies can drive us into inferior outcomes."

Arthur (2013) stresses that technologies compete- as do firms and products- but that the best technology does not always become an accepted standard. Lock-in can occur, and the market can accept lock-in as an outcome. It is therefore incorrect to assume that equilibrium outcomes produce unique solutions that are always optimal. This incorrect assumption has, however, led to a further assumption- that the paths followed to reach optimal outcomes are irrelevant, and that 'history does not matter'.

If economic markets can lock into inferior outcomes (and do not necessarily lock into the best outcomes), then this can have important implications for technological change, and what influences it. What then of optimality in the formation of standards? According to Arthur (2013: 1187);

> "…and once again, the debate very obviously is not about keyboards. It is about an ideology, in this case the libertarian one that markets steer us to the correct outcome…Markets do lock in, and frequently. Optimality is a much trickier issue...Most often, a standard taken up at the outset is optimal for the people choosing. Later the criterion shifts and the now difficult-to-budge outcome looks non-optimal…Saying an outcome depends on historical accident is also tricky. What appears to be accidental at a course level may turn out to look inevitable at a more detailed level, then again to be largely accidental at a still more detailed level."

Market forces cannot therefore be relied on to produce the most innovative standards, and explicit human choices might also be problematic in attempts to derive optimal standards. What is extremely important here is the suggestion that *technological progress can in some instances be arbitrary*. And, if steered by state actors, then the problem of *information asymmetry is clearly an issue- how were we to know of the substantial positive externalities associated with pandemic research* that have been made painfully apparent by the consequences of the pandemic.

Different stakeholders may have different perspectives on optimality. If there is no *a priori* logic to technological progress, or the preferences of markets, then it may be necessary to look to other solutions to wicked threats. What can we then expect of changes in technological trajectories? One of the most respected theorists of technological change is Schumpeter. According to Schumpeter (1937:158):

> "I felt very strongly that…there was a source of energy within the economic system which would of itself disrupt any equilibrium that might be attained. If this is so, then there must

be a purely economic theory of economic change which does not merely rely on external factors propelling the economic system from one equilibrium to another. It is such a theory that I have tried to build."

Rosenberg and Hall (2010) highlight this quote and Schumpeter's (1937) criticism of the Walrasian stationary processes associated with neoclassical equilibrium analysis. According to Rosenberg and Hall (2010), Schumpeter is suggesting "that the essence of capitalism lies not in equilibrating forces but in the inevitable tendency of the system to depart from equilibrium- in a word, to disequilibrate." The fact that the innovative process itself has "has dynamic and hysteresis-like properties means that the static economic modeling will be of limited use for analysis" (Rosenberg and Hall, 2010:5). If disequilibrium is the defining characteristic of technological progress, then it is by definition uncertain.

Even if it is fundamentally uncertain, technological progress can however also be a positive force for good. This is especially so if its use for good could be incentivised. If technology as a form of knowledge is non-rivalrous in production and can be used by others at little or no cost, then the social marginal cost of sharing it is zero (Mokyr, 2010). Although the social marginal product is positive, under the theoretical conditions of an optimal static solution in which it is freely available to all there would however be little incentive to undertake costly R&D (Mokyr, 2010). This dilemma has given rise to debates about how to incentivize optimal innovative activity.

The rise of open source knowledge production typically associated with reputational incentives seems to be a contemporary development, but according to Mokyr (2010:13) much innovation in the past operated in the same way, and the "dichotomy according to which science operated according to open-source systems whereas technology was subject to private property constraints is seriously exaggerated." Although open source research methods are already extensively used in medicine and biomedicine (Callaghan, 2015), researchers have been found to be slow in applying technological innovations to their research (Rubin and Callaghan, 2019), with the current scientific discovery described as slow, gridlocked and dangerous, in that it seems unable to anticipate or dealing with pandemics, the rise of antibiotic resistance, or other looming societal threats (Callaghan, Callaghan and Jogee, 2019). Such literature predicted the consequences of the coming pandemic, and the inadequacy of the scientific response to its consequences.

Knowledge of the underlying processes of innovation can reduce the wastefulness of innovation (Mokyr, 2010). According to Mokyr (2010:15), what “has assured the decline in access costs [the ability of those who can make the best use of knowledge to access it] is that the technology of access itself has been improving through discrete leaps.” These ‘leaps’ include inventions such as the printing press and the internet, and many more have resulted from institutional and technological advances. A particularly important advance is “the creation of open science and the placement of useful knowledge in the public realm and its codification in languages that can be understood or translated easily” (p.15). Certain proposed solutions to the wicked problem of scientific non-innovativeness and failure to adequately respond to societal threats (see Callaghan, 2015) essentially correspond to Mokyr’s suggestions here- that there is substantial wastefulness in innovation processes, that can be reduced by the application of technology to the innovation process itself. The discussion now turns to Nelson’s work in this area for further insights into how to solve wicked threats when we have almost run out of time to solve them.

5. NELSON’S MOON AND GHETTO LOGICS

According to Nelson (2011), technologies- the systematized professional know-how related to the performance of tasks- have developed unevenly. These differences cannot be explained by differences in effective demand. Some have suggested that the strength of the underlying sciences that underlie a field of practice might explain certain of this inequality. This relates to the question of how technological progress proceeds. According to Nelson (2011:683):

> “So, what do we know about how technological progress proceeds? Over the past half-century, a large interdisciplinary body of scholarship has developed concerned with this question. For the most part that research has focused on the advance of product designs and production processes in fields like electronics, aircraft, agriculture, chemical products. Recently there has been a surge of study of advances in various areas of medical practice. In contrast, there has been much less research and writing on the evolution of teaching practices, or modes of business management, or knowledge of the effectiveness of different forms of economic organisation.”

If wicked problems can to some extent persist due to a lack of research relative to other fields, then this is another dimension of the knowledge asymmetry problem. Could a lop-sided approach to studying the consequences of a lack of innovation in our scientific discovery system have contributed to the consequences of the outbreak? Again, hindsight offers useful insights into this question. According to probabilistic innovation theory, there should be a balance between the investments in research to solve important societal problems and their costs. In other words, the costs of the outbreak are very large. The research effort was not extensive enough.

In hindsight, the externalities are clear. The societal benefit ratio relating to research into societal threat response should be balanced- now we have a better idea of the radical improvements needed. Will we heed this lesson, however? Immediate and sufficiently substantial investment in the area of research management would be needed match the consequences of the pandemic and could go some way to protect us from the next one. Or allow us to better manage the long-term consequences of this one.

If we consider other potential threats, it is clear that societal benefit ratios relating to these threats remain very small. How much research investment is being made in climate change research? It is surely substantial, but what should guide decisions about this investment is the question how much investment would be needed to effectively avert the threat? If we had asked this question with full knowledge of the coming outbreak, what would the societal benefit ratio numerator of pandemic response research have looked like? The question here is whether we are learning from the current experience, and if we are going to adjust our societal benefit ratios adequately, in the absence of information about the next coming threats. Using probabilistic approaches that fully apply emerging technological capabilities to the research process itself is important, as stressed by Callaghan's body of work in this area- probabilistic research methodologies need to be developed in order to match the probabilistic nature of the opportunities and threats that face us at this time. The coronavirus outbreak is an example of a probabilistic threat- it can increase exponentially, whereas our scientific learning capacity is linear. To learn faster than a microorganism, we might need to develop probabilistic research methodologies, such as crowdsourced R&D (Callaghan, 2015).

What other insights can Nelson's analysis offer us regarding the wicked problem of scientific non-innovativeness? Technological progress has typically been considered to be evolutionary, with winners and losers in a competitive process largely determined through an ex-post selection process (Nelson, 2011). This evolutionary process acts in conjunction with human purpose and know-how. A field can progress on the basis of its ability to control its core operational tasks- its ability to identify, control and replicate its effective practices, its ability to obtain timely feedback on its competing practices, its ability to conduct deliberate experimentation, and its ability to learn from off-line R&D (Nelson, 2011). A strong body of scientific understanding is however a necessary condition to be able to take advantage of these abilities.

As discussed, it is argued here that social science research is key to be able to take advantage of the new opportunities available to human systems offered by novel technologies- but that this requires a constant process of innovating human research systems themselves (and tackling the wicked problem associated with systems of human innovativeness). Whereas certain technologies with strong cores are typically consistent in how they are used, other bodies of practice, or other technologies, can have a weaker core, according to Nelson (2011). An example of the latter are the bodies of practice associated with teaching children to read or to do algebra, which have an idiosyncratic element. The effectiveness of activities with weak cores are disproportionately vulnerable to the skills and motivations of those performing the work, which tends to be labour intensive. Nelson (2011:685) explains this logic further:

> "I propose that teaching is a canonical example of human goal-oriented activities where we have not yet identified or achieved a controllable replicable set of procedures- a core, to use the language I established earlier- that is broadly effective across the range of relevant contexts. Performance in meeting the needs these activities aim to address tends to be highly uneven. There can be some instances of great effectiveness, but also many (as contrasted with unusual) instances of frustrating failure. I note that the activities tend to be labor-intensive, reflecting both as cause and effect the lack of standardization."

We might need to acknowledge that the human aspects of the research system itself may have a weak core. These aspects may represent the Achilles's heel of our research disaster response. According to Nelson (2011:685), virtually "all modern societies are frustrated with the performance of their systems of primary and secondary education," with rising costs per student and almost static productivity, unlike the high productivity growth rates of certain other economic sectors. There is also "growing dissatisfaction with the effectiveness of the system in providing children with a strong education, with particular concerns that a large fraction of children in disadvantaged economic conditions are not taking to what is taught" (Nelson, 2011:685). Educational research has to date seemingly failed to raise educational productivity, questioning its comparability with the medical model of research.

It is argued here, however, that although the medical model of research described by Nelson is seemingly more effective than social scientific research, absent from Nelson's analysis is a discussion of the aspects of medical research that overlap with those of what he describes here as 'educational research'. A lack of adequate response to the pandemic seems to have highlighted the need to consider these human systems of natural science research that have components that are not dissimilar to those with a 'weak core' that he describes here. What is useful in Nelson's analysis is the way he describes certain mechanisms that constrain progress in research- roadblocks to innovativeness. According to Nelson (2011:685):

> "The principal reason why it has been so difficult to make cumulative significant advances in educational practice, I would argue, is that it has proved very difficult to discover or develop a body of educational practice that can be controlled tightly, and replicated easily, that at the same time is effective in the variety of contexts where education must proceed. Practices that are effective seem to depend on the backgrounds, knowledge, and motivations of the student body, and also on the skills and personality of the teacher…to date no artifact has been developed for educational purposes that has the power of an antibiotic for dealing with infection. Related to that, it has proved nearly impossible to identify the key elements that lie behind effective practice when that occurs, other than very broadly."

It might be asked- is Nelson's description here of the challenges in solving problems in educational practice all that different from solving those related to a different domain of educational practice- that of research productivity- in pursuit of societally important innovations? At the heart of this problem, for Nelson, is the way that teaching practice has deterred strong standardization- whereas education might be the most important of the "areas where society has been experiencing difficulties in advancing know-how," other areas, such as criminality, recidivism and crime prevention also share the same problem. Find or develop a core to the practice seems to be key to our ability to solve these types of wicked problems. Interestingly, Nelson references the power of an antibiotic in dealing with infection as a counterfactual argument to innovation failure.

The development of antibiotics is the outcome of a research process that might itself be a good example of the wicked problem of scientific non-innovativeness. There is a large literature that suggests that antibiotic resistance may be spreading much faster than we can develop new drugs against microbes (Harbarth *et al*., 2015; Huttner *et al*., 2013). The resistance of bacteria to all antibiotics termed by some the coming antibiotic apocalypse (MacKenzie, 2015) this threat includes highly resistant strains of tuberculosis, threatening to become totally drug resistant (Klopper *et al*., 2013).

How do we tackle wicked problems like scientific innovation failure- research failure? According to Nelson (2011), broad practices that are effective should be identified, and these should be used together with support for those that are skilled and motivated, and with high investments of resources. Addressing these types of problems can be particularly difficult, as labour-based services such as education are also subject to cost inflation resulting from productivity growth in other sectors- Baumol's law. According to Nelson (2011:687):

> "Solving problems by developing better ways of doing things generally is to be preferred to dealing with problems by trying to do the best one can with the practices in hand and allocating more resources to the effort. However, unfortunately, where there is no effective core, and there is no clear vision regarding where one might be found or developed, a solution through innovation is not likely in the cards. This is not to argue that research in the field should be abandoned. Important findings and inventions can occur unexpectedly. But a research-support program should be understood as a search for useful knowledge, in an arena where the light is dim. In such a context, one should not expect too much, or bet too much. And if this is the case, it is important to recognize it."

Given that externalities associated with the failure of research to adequately anticipate or address the consequences of coronavirus pandemic are now visible in hindsight, it is perhaps unacceptable to simply 'not expect too much' or to simply recognize and accept that the complexity around the phenomenon of innovation failure makes it impossible to adequately address such problems.

Policy thinking related to advancing technological progress used to largely focused on increasing or reallocating R&D resources, but this has changed with the development of the concept of an innovation system, which identifies the activities, actors, investments, and actors and the patterns of interactions between them, as well as their organization and governance (Nelson, 2011). An advantage of this approach is that government support of R&D is not limited to cases of market failure, and that it expands policy focus to a wider range of public policies that can improve R&D and facilitate technological change in an area (Nelson, 2011).

Policies that are politically feasible or that are effective can differ according to sectors, and policy analysis needs to be aware of these differences- this can make innovation projects on the scale of the Manhattan or Apollo Projects less effective as they ignore unique characteristics of sectoral innovation systems (Nelson, 2011). Private actors may also not necessarily reveal their operational needs, and might not have the necessary incentives to adopt new technologies. According to Nelson (2011:689):

> "There is good reason to believe that, without major breakthroughs in the technologies employed to produce and use energy, the costs of dealing effectively with the global-warming problem will prove to be so high that there will be political stalemate. And, unlike the case of education, or homelessness, where it is hard to see efforts to create and develop significantly superior know-how as having a high change of success, there is a real promise in this area."

The political stalemate of the forces that have led to the current state of the scientific innovation and discovery system has now been broken by the outbreak. The costs of inadequate investments in appropriate research are now clear- they are globally catastrophic. The time has come for a new way of thinking about science that is informed by our new knowledge of its externalities. Based on the discussions above, the following two propositions are derived.

Proposition 1. Investments in research need to be adjusted immediately, given new information as to the externalities of appropriate research (and the consequences of research failure)

Proposition 2. Research interventions to address societal threats should be commensurate with the scale of the threat (research efforts should match the potential consequences of the threats)

Implications of these related propositions suggest a question: What types and scale of research investments might have been sufficient to avert the catastrophe associated with the outbreak? Indeed, what percentage of school leavers even end up with a career in science? The societal benefit ratio related to this one example clearly illustrates a problem- whereby societal threats affect us all, we have seemingly made too few investments in science to protect us. Threats potentially affecting billions of people should have investments made to solve them that is at the same scale as these threats. Gordon (2016) points out that most of the advances made in medicine date back to the innovative century- from 1870 to 1970- and that since then science has failed to advance at the same rate.

A Manhattan Project-type intervention is needed. Not one focused on external outcomes, perhaps, but one focused inwards, on improving the effectiveness of the research process itself. Many different methodologies currently being applied, such as those used by InnoCentive (innocentive.com) or FoldIt (fold.it.) have yet to be developed into fully fledged scientific methodologies. If focused inwards, such a project will have economies of scale in that innovations in the research processes of different fields will carry the benefits of these into the world. But a field is needed to deliver these innovations. A subfield of Management might be uniquely suited to this task- Research Management.

Having presented the key arguments of the paper, and having derived propositions for further research, we now contextualise the most salient points of our arguments for current practice and scholarship of management and management research, to make management recommendations.

First, in practical terms, our analysis suggests that management theory and practice may be the key to managing existential threats, but that the research and knowledge creation process itself, should be the primary focus of practical attempts at improvement. Practical applications of management theory and practice achieve extraordinary feats, such as safely landing and taking off thousands of tons of aircraft at airports on schedule on a daily basis. Similarly, the complexity of global information and telecommunication networks is managed effectively in real time. We suggest that for some reason the same scrutiny and research efforts have not been turned inwards on the practical and real-world aspects of the scientific research system itself. Managers and management scholars should engage reflexively with their research tasks to seek radical improvements in research processes. In many instances, the tools and technologies already exist to improve research effectiveness but are often simply not used (Rubin & Callaghan, 2019).

Second, managers and management scholars should seek to expand their activities as important stakeholders to have a greater say in how the research process itself is managed and innovated. As discussed, novel technologies now exist to radically improve the speed and quality of research across different fields. Indeed, literature demonstrates how research processes can be radically scaled up, as evidenced by large-scale successes like Wikipedia, the Polymath Project (solving mathematical problems online), Galaxy Zoo (classifying hundreds of thousands of galaxies), GenBank (global collection and categorization of genetic material), and others (Nielsen, 2012). Nielsen shows how practical solutions can be found to problems of scalability, but according to analysis here achieving change in the way things are done in research practice seems to be a wicked problem in itself.

Finally, we suggest that it may be management as a field that might be tasked with effecting real world change in the research 'industry' itself, and in the identification of the characteristics of this industry. This will require a pragmatism that is perhaps unique to management as a field- to derive practical implications from a body of theory related to innovation studies or economics of innovation (Jones, 1995; Jones, 1999; Jones, 2009, 2010; Aghion *et al*., 2017; Bloom *et al.,* 2017; Galvao *et al*., 2019; Figueiredo & Fernandes, 2020). This body of literature seems to suggest that applying the latest technologies to radically improve the research process itself might be key to achieving scalability in the research process itself. Such novel thinking may contribute to efforts to tackle mounting existential threats.

6. LIMITATIONS AND RECOMMENDATIONS FOR FURTHER RESEARCH

This work suffers from limitations germane to all conceptual review research of this nature. Although we point to certain causal linkages and extend ideas relating to wicked problems and how to solve them, we cannot ascribe causality, or make causal claims. Nevertheless, we derive propositions that extend previous literature, and provide directions for future research. For example, our first proposition implies that externalities or consequences of underinvestment in research reform and process improvement largely remain unknown. An example of this is the coronavirus pandemic, in which the costs of appropriate preventative research could have been much lower than the human and societal costs of the pandemic itself. Similarly, our second proposition implies that research and innovation investments to address problems (and existential threats in particular) will need to match the scale of the threat itself. Marginal costs of investments in research should match marginal societal benefits. Further research should seek to identify and quantify the scale of these threats and the shortfall of investments in research to address them. Further management research should seek to understand how management theory and practice can ensure these objectives are achieved in real world contexts.

7. CONCLUDING REMARKS

The objective of this paper was to provoke novel thinking around the possibility that a fundamental threshold limitation to knowledge creation is responsible for the failure of our scientific discovery system to anticipate or effectively mitigate the consequences of the coronavirus outbreak. It was suggested that this fundamental threshold limitation derives from the uniquely human characteristics of the discovery system. And that the discovery system suffers from a wicked problem in that it has failed to prioritise innovativeness, to the extent necessary to protect human populations from catastrophic threats. Although these conclusions are based on hindsight in the wake of the outbreak, and of (revealed) knowledge of the costs of its consequences, it was also argued that there exists a longstanding literature on wicked problems, and that the wicked problem of non-innovativeness in natural and social science is not new. Given that the externalities of research failure are now clear in hindsight it seems clear what we have to do. A radically increased scale and scope of research investment may be necessary in order to radically improve the responsiveness of the research system to the needs of human populations. Key to this may be an unflinching commitment to tackle the wicked problems that underlie research failure, in order to address other wicked problems that are inextricably related to our failure to respond threats, be they climate change, global conflict, or the next coming threat from microbial organisms.

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