Scientific research, and particularly management research, is in dire straits, accused of lack of relevance and impact and an unhealthy preoccupation with theoretical and methodological rigor. Marco Busi suggests some solutions.

Research that matters

Thoughts on reinventing scientific (management) research
One of the most illustrious management thinkers of all times, Peter Drucker himself, was both a great defender of the importance of scholarly management research and a bemused observer of its remoteness from the very environment it is supposed to study and develop.

“Sure, we want and need research. But consider the modern medical school...The emphasis in medical school is not on publication but on the ability to treat patients and make a difference in their lives.” he said.

As many readers will be well aware, though, that may be easier said (mostly by those outside the realm of academe) than done (mostly by those inside).

The cure to this virus is, in fact, not yet at all clear. The good news is that it is now a top priority for many scholars and practitioners from different disciplines, who are coming up with several ideas and proposals.

In an article published by Global Focus back in 2008, Oxford’s Professor Andrew Pettigrew put forward a number of hypotheses about how this situation could be improved.

As Chair of the EFMD R&D Steering Committee and a then EFMD vice-president, Professor Pettigrew suggested that we should look at scientific rigor and practical impact as two sides of the same coin; we should build a quality network of relationships and work through “knowledge brokers”; we should focus on pursuing the right themes at the right time; and we should create environments for the co-production of research.

The original, noble purpose of universities was to conduct research that would contribute to advancing societal understanding and well-being. And being a scholar automatically implied doing research that matters, that influences the way people live, behave and work.

But a nasty virus is now threatening science’s fundamental role in explaining life’s events and bringing about the innovation we need to progress further, in business as well as society.

The enabling conditions

Stakeholders inside and outside the scientific community have identified and understand well the enabling conditions that facilitate the spread of this virus: competitiveness of science, rewards and career progression systems based on bogus measures of quality, and the “publish or perish” mentality to mention but a few.

In fact, the very policies of a vast part of the academic and scientific publishing worlds – absurdly – feed the virus. As Ben Schiller neatly summed up in a 2011 Financial Times article: “[...] promotion is based on articles few managers read; and [...] accreditation bodies and rankings providers count journal entries, and citations, to assess worthiness”.

The symptoms

The symptoms of the virus are also clear to all: lack of relevance and lack of impact.

A prominent voice on this topic, the Journal of Management Inquiry, supports the argument that “a growing preoccupation with theoretical and methodological rigor [may] underpin the increasing generation of theory and research that is irrelevant to managers”.

This is a view firmly held by practitioners as well. Schiller’s FT article quotes Dan LeClair, senior vice-president at AACSB, stating that: “[...] major donors are asking tough questions like ‘you have all these faculty members who you are very proud of, but can you tell me how this research has made a difference?’”

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London Business School’s Professor Costas Markides has a different, perhaps more drastic suggestion, saying that “the first step to generate more innovation in academic research is to change our purpose or paradigm, which says that our goal is to publish or perish. It is not! Our goal is to generate ideas that help change the world, that create value. And if you don’t develop a new paradigm or use new mind-sets you are never going to have progress in academia”.

After studying how exemplary scholars in the management field as well as the hard sciences stay true to the “noble purpose”, in Doing Research That Matters, Shaping the Future of Management, I present my own hypotheses about how we could follow Professor Markides’ suggestion to develop a new mind-set. Here, I would like to present three:

First, I propose that we must use a system thinking approach to understanding what we need to have in place in order to do research that matters; I then, second, propose an “individual thinking” approach to doing what needs to get done; and, third, I propose that we need a revolutionary move “from incrementalism to impressionism”.

Use system thinking to understand

System thinking postulates that the whole is greater than the sum of its parts. Similarly, the main hypothesis behind the “research that matters model” is that doing research that matters requires the convergence of the vision and values of three separate elements of the system of scholarly research: the “Insight Generator” – the person doing the research; the “Insight Incubator” – the university (or company) hosting the researcher; and the “Insight Distributor” – the entity distributing the researcher’s work.

Our collective objective, therefore, should be to ensure such convergence, where researchers, universities/companies and publishers/accreditation bodies all share the same view of research that matters and the same desire to generate insight that will change the way we view and act on certain problems.

Use individual thinking to start

However, acting collectively is difficult: who should start embracing this new mind-set? Clearly, this is really a chicken and egg problem. My second hypothesis is that we should not be asking ourselves that question but instead adopt an “individual thinking” mentality and focus on what we can do as individuals to improve the situation.

Regardless of whether we are researchers, university leaders or publishers/reviewers/auditors if we are honest in our attempt to shape the future of management we must embrace our ethical responsibility and moral duty to work both introspectively – to learn how we can do better- and extrospectively – to do what we can to positively influence the other two elements.

Isaac Newton humbly noted how he was able to see further because he stood on the shoulders of giants. Similarly, to understand what we can do as individuals we may look for inspiration and guidance from those who did it before us.
I refer to people committed to the goal of doing research that matters as “Futureers”, from future and engineers. These are the people who commit their lifetime to engineering the future, be that through advancing medicine, technology, management or whatever other field. They are neither the “pure academic” nor the “pure practitioner” type. They are just innovators.

As such, Drucker’s advice seems perfect: “Because innovation is both conceptual and perceptual, would-be innovators must also go out and look, ask and listen. Successful innovators use both the right and left sides of their brains. They work out analytically what the innovation has to be to satisfy an opportunity. Then they go out and look at potential users to study their expectations, their values, their needs”.

Studying the Futureers’ behaviour and approach to research may help us understand what the “right mind-set” looks like and what we can do to develop it and apply it. Doing so certainly enabled me to identify certain traits common to the “ideal” Insight Generator, the “ideal” Insight Incubator and the “ideal” Insight Distributor.

Join the revolution
My third hypothesis is that we need to start a real revolution in order to reinvent science, especially management science.

We need a few brave individuals with the courage to move from today’s prevalent incrementalism to a completely new way of teaching and doing management research. A very good way to start this revolution is to adopt MacDonald and Karn’s Tinker Bell solution, which aptly suggests we should laugh at, rather than admire, research – and, more importantly, scholars – deemed ‘good’ by traditional measures of quality and impact.

But I believe we need more than that. We must first admit that the virus has spread much deeper into the system than we want to admit. And then we must start changing management research the same radical way that the Impressionists changed painting and music in the late 19th and early 20th century.

Where the Impressionists embraced the principles of freedom of technique, we must embrace freedom of methodology; where the Impressionists changed the way people measured aesthetic beauty, we must change the way we measure research quality; where the Impressionists embraced a personal, rather than an “academic”, interpretation of the subject, we must have the courage to propose ideas that are anti-dogmatic; where the Impressionists sought the truthful reproduction of nature, we must seek to observe first-hand the phenomena we are studying.

Despite the ridicule and humiliation that the Impressionists endured, their perseverance led them, against all odds, to accomplish a revolution in the history of art, providing a technical starting point for future generations of artists. If my parallel holds then we as management researchers and innovators can expect to suffer much ridicule and face many hurdles but, if we persevere, we can also expect an everlasting legacy.

I fully agree with The Economist that ‘science still commands enormous…respect’. But that is not enough to turn a blind eye on problems that are clear to all of us. We should all work hard to cure the system of the virus.

Did I find a cure? No, I did not. But fostering the debate will undoubtedly lead to it.

ABOUT THE AUTHORS