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October 5, 2019

# Making Research Relevant

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Available at: <https://works.bepress.com/shyam-sunder/491/>

## **Making Our Research Relevant<sup>1</sup>**

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### An Overview

- Social Systems: Design vs. Evolution
- Who is the Client?
- Fundamental Science vs. Problem Solving
- What Can We Do? Give up research rewards, and break the grip of legacy journals

### Social Systems: Design vs. Evolution

We can look at social processes as a result of engineering design through deliberate analysis. We can also look at social processes, such as accounting, as a result of evolution or emergence from complex interactions among innumerable elements and actors in society. These two processes can be reconciled. Even if we take an evolutionary view of social systems, we can think about our design or engineering interventions as mutations in the evolutionary process. The label of mutations implies a certain degree of humility about our imperfect understanding, and the tentativeness of our proposals. In this evolutionary world, perhaps research can help identify the mutations that might have a better chance of yielding more desirable outcomes.

However, complexity theory suggests that given the imperfections of our understanding, we cannot really know, until long after we have implemented a change in a social system, what consequences will occur. Many social scientists say that social systems are simply too complex for us to be able to forecast the consequences with any significant degree of precision.

One can take it as a pessimistic assessment, but it is better taken as a reality check. Professors Lev and Rajgopal mentioned the paper about the 20 years of FASB which appears to have had little effect. Indeed, 117 years of rule-writing does not seem to have yielded much documented improvement.

Our ability to engineer desirable changes in social systems — any social system, because accounting is only one example of it — is highly circumspect. We need to approach this problem with greater humility than boards, professions, and academics

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<sup>1</sup> Presented at American Accounting Association Conference on Impacting the Future in New York on October 5, 2019. I am grateful to Elizabeth Viloudaki for copy editing assistance.

have exhibited so far. People, including those in positions of power, simply do not possess enough wisdom.

### Who is the Client?

Who does the research serve? There seems to be a fair amount of consensus that under the current set up, we ourselves are the primary clients of our own research. There is nothing wrong with that if we do research to satisfy some fundamental curiosity about the world, or if we do research to make the world better in some definable fashion. But I fear accounting research has come to serve little purpose other than seeking rewards and rents for us, the researchers. Once the rewards are realized through publication, hiring, promotion, tenure and raises etc., the research seems to reach the end of its purpose. It has been documented that accounting research is little read by management scholars outside accounting; unfortunately, evidence from citations analyses suggests that it is not read much by accounting scholars either.

Business schools turned to doing social science research after the Ford Foundation Report in the 1950s to start using basic disciplines of economics and psychology and applying quantitative methods to address problems of management as an addition to the heretofore exclusivity of the case-based approach. At that time, it was the right thing to do. However, we have interpreted that redirection too literally to the exclusion of education and practice, and stayed with that interpretation for far too long, with insufficient active reflection and debate.

Presently, one can argue that the client of our research is the university, management, other participants in the organization, or as Prof. Rajgopal suggested the paying party — the deans, the university, and ultimately the students and their families. Perhaps direct solicitation of funding from other parties (beyond tuition, as Prof. Rajgopal suggests) may refocus accounting research on the goals of paying clients.

### Fundamental Science vs. Problem Solving

In accounting, there is a widespread illusion that somehow, we do fundamental science like mathematics or physics or biology, and being such pure researchers, we should not have to “appease” outside funding parties. I am not going to argue that there is no aspect of accounting which calls for fundamental attention. Having said that, the vast body of what is published in accounting journals does not fit this bill. Most of the research is simply counting leaves on one tree or another in the forest, and writing papers about it, with no purpose or end use in sight.

On the flip side, a large amount of fundamental research in the above-mentioned “pure science” discipline is funded through the “appeasing” process that many of us in accounting think is below our dignity. An applied management discipline exists mostly to teach the young, and when possible, solve the problems of the world. That is why people are willing to support the universities that employ us. They do not mind allowing us some freedom to do mental calisthenics in attempt to keep our minds fit and sharp

for the classroom. But it is a stretch to take that as license to spend most of our time on leaf-counting excursions, growling on any suggestions that we spend more time teaching and addressing any real problems of organizations that we might be capable of solving. Instead, under the pretense of doing fundamental work, we have kept ourselves busy gathering more-or-less random facts, coming up with manufactured hypotheses that few people believe in, or feel need to be “tested”, and subjecting them to statistical testing, without careful consideration of which facts are worth gathering, which hypotheses are worth testing, and which theorems are worth proving. Since the number of facts waiting to be gathered, hypotheses waiting to be tested, and theorems waiting to be proved is unbounded, I liken this to a football field with no boundaries or goal posts; we are kicking the ball around the field and keeping score in the number of kicks.

Incidentally, a unique contribution of this accounting literature to our statistical epistemology is the dominant attitude that learning arises from failure to reject the null hypotheses, as opposed to the rejection of a null hypothesis. As a stamp of distinction, accounting journals have relabeled the “failure to reject” with a positive spin as “consistent with”.

When a large part of research work is compared to leaf-counting, it is easy to see that there is a limitless supply to keep thousands of us busy for our lives counting and reporting various factoids. Without a larger purpose, and without even curiosity driven by our life experiences, it makes little sense to just continue this work to serve our own personal interests and rent seeking.

### What Can We Do?

Let me share two thoughts. An effective way of improving the state of affairs in accounting research and to redirect it towards impact and relevance may be to stop rewarding it, at least in the management schools and accounting.

To my knowledge, it takes a generation or more in the social sciences to know which reforms (even after they are implemented) have what effects. The consequences of the Bolshevik revolution, social security, Medicare, and even accounting standards work themselves out over multiple decades, while millions of people adjust their behavior to their altered circumstances. In the case of unimplemented social science research, that period is even longer. There is little chance that the accomplishments and consequences of social science and management research can be known within the lifetime of the researcher (except in a self-referential way where a research paper or a book itself is seen and rewarded as an accomplishment — the current practice).

We at the universities and schools of management have exhibited little ability to assess research accomplishments in any meaningful way during the past sixty years. Rewarding research (measured by written or published papers and books) has accumulated a mountain of words at an accelerating pace. One has to dig long and hard to find kernels that might be of value to anyone other than the researcher.

Instead, since we are unable to distinguish quality in a timely basis — faculty committees, outside letters, citation counts included — we could stop rewarding research but let the faculty who wish to do so continue to use a university's specified resources to conduct research without any expectation of rewards. Shocking as it may sound to some, I speculate on some potential consequences:

- Faculty will be hired, evaluated, promoted and retained based on teaching excellence primarily assessed by their colleagues (supplemented by student assessments). This will help attract different type of people — who may have somewhat greater interest in pursuing a life of the mind, and place higher value on intrinsic fulfillment that lies beyond the usual concerns about jobs and careers — to university faculty.
- Faculty will have more time to prepare and teach, and to advise students. They can teach more courses and help cut the cost of university education.
- Faculty will direct more of their attention to innovation in teaching, engaging with — even inspiring — students.
- The volume of research output will decline sharply because less research motivated only by extrinsic rewards from publications will be conducted.
- The quality of published work will improve because the choice of spending one's own time on research will become an incentive-compatible activity.
- Many academic journals will disappear, and the faculty time devoted to refereeing and editing them will be saved.
- Management faculty will have more time and reason to engage with the management community to learn about their problems, analyze them, and take them to their classes for discussion. In effect, all parties except the journal publishers will benefit.
- Finally, due to great imperfections in university mechanisms to evaluate research in our field of management and accounting, publication-based rewards have created a great many of problems. The legacy journals which have come to dominate our discipline will either reform themselves to change their focus or will become defunct.

Funding in social sciences is not as trouble-free as in natural sciences. The moral rationale for university research and its funding, of for example the zika virus, is on a relatively firm foundation because few people can argue that this effort is inappropriate for university faculty. Indeed, such examples have been widely used to defend, promote, and support funding of university research.

But let us consider management school research which might help an investment bank devise a complicated derivative instrument, or help a marketer design a selling campaign, or a CFO manipulate financial reports to help them make more money. Since there is often another party on the other side whose legitimate interests cannot be ignored, moral foundations and defensibility of such work is not as firm; this problem is more generic than we may think.

Like Professor Lev, I also ask my Ph.D. students to read business papers and magazines, Matt Levine's finance column in Bloomberg, and other sources. They have some of the most interesting problems for us to analyze, research and address.

In conclusion, the focus of management schools should be on learning and teaching, with research as a useful byproduct. Not on churning out meaningless publications to earn rewards or rents. As long as we reward publications, many of us will read a published paper in our academic journals, make some minor changes and write another paper in the hope of getting published on the tails of their predecessor.

So-called research has now become the focus of management education, instead of the reverse. The relationship has reversed to the detriment of management students, faculty, corporations, and I would argue, society. Let's examine this trend and act to change it.