

# **Integrating Practice into Accounting Research**

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**Comments welcome**

## **Abstract:**

I argue that academic research in accounting has strayed from producing work that is useful to either practitioners or policy makers. I use three criteria to arrive at that assessment: (i) how many products and processes has accounting research produced in the last 50 years? (ii) how much overlap do we observe between issues that CFOs and CEOs worry about and our published research; and (iii) is the science or the knowhow in academe in a particular area ahead of that in practice? I conjecture that tuition-funded research drives this problem. I review several initiatives that have been tried at Columbia and elsewhere (i) to better integrate academic research and practice; and (ii) to disseminate our findings to practitioners. I suggest that Management Science set up a forum to encourage submissions of papers that use rigorous methods to address pressing applied problems.

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## **Integrating Practice into Accounting Research**

*“Your abstract is too abstract for me!”*

### **1. Introduction**

This is the reaction of an ex-CEO of a very successful Fortune 50 firm and a potential donor to Columbia Business School when he skimmed through some of my work on corporate culture. And I prided myself on working on topics that were of relevance to policy and practice! In my role as the vice dean of research at Columbia, I have met several alumni who are senior executives at large corporations and at least a dozen influential regulators. To my dismay, I have sensed a general reluctance on the part of alumni donors to directly fund academic business research. These alumni were more than happy to write checks for vast sums of money to support teaching activities, a new building for the school and student scholarships. Why the hesitation to fund academic research? Why are people in the field who could potentially benefit from our research not too keen to fund it? Do we have a natural market for most of our research output outside of fellow professors and PhD students?

To answer some of these questions, I often ask some of our alums to reflect on their long and illustrious careers and come up with a couple of difficult intractable questions they wish they had answers to and, by extension, they would fund research into. Inevitably these questions would seek practical knowledge related to “how to” or techniques and processes to run their businesses more efficiently.

For instance, the ex-CEO referred to in the opening quote was interested in research into the micro-behavior, measures and processes that managers can implement to repair a damaged corporate culture. Alumni who were investors would typically have questions on measurement and valuation: How should we isolate the impact of foreign currency on a company’s sales, costs and income? How do I estimate the labor costs of a U.S. company? How can I parse the cost structure of a company into fixed and variable costs? What is the best measure of operating performance? How do I deal with terminal value in a valuation model? How do I measure cost of capital? Where do price-

earnings (P/E) multiples used by the market and analysts come from? How should I measure credit risk for a company? How do I measure corporate culture? How do I assess the ESG (Environmental, Social and Governance) footprint of a company? How should I value data? How much should one pay for a digital acquisition such as say Google's acquisition of Waze?

Alumni who were managers would have questions on how to allocate business resources more efficiently: How do I allocate capital between capex/stock buybacks? How should we pick projects that divisional managers pitch to us? How much capital should we hold? A few requests were specialized: Can someone help me understand the differences between statutory and GAAP accounting in the insurance industry? How do I measure the contribution of my bank to systemic risk? What is the impact of extreme weather events on the prospects of insurance/cruise ship industries? How do I quantify the impact of cyber risk on a firm?

I would try to find answers for them in published accounting (and other) research but would end up concluding that these questions were unaddressed in general or when addressed, the answers were incomplete at best. Our literature has analyzed applied questions such as measurement of cost of capital (summarized in Easton 2009), ways to mitigate the impact of terminal value via residual income valuation (Frankel and Lee 1998, Penman and Sougiannis 1998, Ohlson 1995) and sources of P/E multiples (Easton and Zmijewski 1989, Collins and Kothari 1989, Liu, Nissim and Thomas 2002 and Bradshaw 2004) but I found little that directly answers most of the other questions that practitioners raised. Capital allocation has attracted a lot of scholarly attention but as Busenbark et al. (2017) argue, very few studies have actually opened the black box behind how managers actually allocate capital rather than test models based on assumptions about how managers allocate capital.

Published work in our top journals seems to focus more on abstract empirical regularities rather than on generating knowledge to solve specific applied problems that executives in practice and regulators in policy face. Some work tends to seek out datasets of measures of ESG or governance or culture or credit risk generated by commercial information intermediaries to document correlations with capital or labor market outcomes such as stock returns or CEO turnover. Little

fundamental work in academe has gone into understanding how one can actually measure labor costs, or Environmental, Social and Governance (ESG), or culture or cyber risk. Deep specialization in a topic area (e.g., insurance or foreign currency accounting) is highly valued in practice but rarely found in accounting academe perhaps because specialized academic work (i) does not attract a large number of citations; and/or (ii) does not lend itself to ready classes that faculty can teach.

Excited about discovering several new research streams from these applied questions, I attempted to recruit collaborators to work on applied relatively unexplored areas such as (i) what is the scientific basis, if any, for the P/E or EBITDA (earnings before interest, tax, depreciation and amortization) multiples that analysts use in valuation?; (ii) why do analysts appear to routinely ignore footnote data in financial statements?; (iii) can we come up with a process to quantify labor costs and hence fixed costs for U.S. companies?; (iv) how much capital should a firm hold and how should it allocate capital, given its strategic objectives?; and (v) can we measure systemic risk for banks and insurance companies using our knowledge of how financial statements work?

Junior scholars would often tell me, “Ah, but that question is not publishable because referees would say the paper is normative or purely descriptive.” Others would tell me, “you need a structural model or an exogenous shock to test that.” But I would counter “how do you know the structure for the model unless you ask people in the field?” How do we know whether these structural models actually capture what managers in practice do in terms of allocating resources or managing risk, let alone whether they are able to be implemented? And, trying to establish causality using obscure shocks that are tangentially related at best to the phenomena at hand is not very helpful in addressing some of these first order questions.<sup>2</sup> A few senior collaborators would say, “these questions are not academic enough.”

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<sup>2</sup> We study slow moving institutions in accounting research such as the actions of the FASB or the SEC. Accounting standards are usually crafted over several years. Hence, it is hard for accounting researchers to identify relevant shocks to identify causal associations.

To make sure that my experience was not idiosyncratic, I conferred with senior scholars at other top institutions. One of them said, “anything practically oriented or policy oriented is strongly discounted by many of my colleagues. They prefer to work on models that have no practical use. I saw one of my colleagues challenged in a research seminar to name one practical application of his theory – and he could not. Seems like you need tenure first before you do anything practical – even then...”

How did we get to a state where applied questions of interest to practice or policy in our field are perceived to be unscholarly enough for our academic journals? I attempt to answer that question in this paper. I argue that academic research in accounting has strayed from addressing practical problems of importance to practice and policy. I hypothesize that tuition-funded research is the prime driver of this problem. Next, I outline a few initiatives that have been tried at Columbia and elsewhere to better integrate academic research with policy and practice.<sup>3</sup> However, I caution that most of my perspectives on the literature originate from accounting work, especially from the sub-fields of financial reporting, corporate governance and executive compensation, because that is the literature I know best.<sup>4</sup>

The remainder of the paper is organized as follows. In section 2, I suggest that academic research in accounting is increasingly aimed towards questions that referees and editors might find interesting, as opposed to problems that practice and policy makers are looking to solve. In section

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<sup>3</sup> The management area has worried about the relevance of management research to practice (Rynes, Bartunek, and Daft, 2001; Bennis and O’Toole 2005; Khurana 2007; Rousseau and McCarthy, 2007; Pfeffer, 2007; Alvesson and Sandberg 2013). The accounting area is less represented in this debate (e.g., Kaplan 2011, 2018; Basu 2012, Dechow et al. 2018, Schrand 2018). Other critiques of accounting research include Demski et al. (1991) and Zeff (2019). My contribution to that literature is three fold: (i) I hypothesize that the likely root cause of this conflict is tuition-funded research, an issue that has not been covered by this literature, as far as I can tell; (ii) my focus is on issues relevant to the accounting area based on my personal experience as an academic accounting researcher, thesis advisor, journal editor and referee over the last 20 years; and (iii) I enumerate concrete measures to try and address the divide between theory and practice at my home institution and at other universities.

<sup>4</sup> In particular, I do not extensively discuss whether accounting research in audit and taxation has impacted policy or practice. Perhaps scholars in those sub-areas can compile a list of products and solutions in these sub-areas of accounting research.

3, I discuss the factors potentially responsible for this concern. Section 4 is devoted to a potential research agenda in financial reporting that highlights a few questions relevant to practice. Section 5 discusses specific mechanisms that can alleviate the research-practice gap. Section 6 suggests how INFORMS and Management Science can contribute. Section 7 concludes.

## **2.0 The problem: Accounting research has strayed**

### *2.1 The downside to tuition-funded research*

I often ask junior faculty “how do you decide what to work on?” Most of the answers I get center around the potential publishability of the question in a top journal. Many say that they pick questions based on what their colleagues and editors at top journals want to see and respect. Very few say, “I am trying to solve a practical problem or work on something policy or practice can use.” Why is that?

Assistant professors are required to amass a publication record and impress top scholars in the field so that assistants can receive recommendation letters praising their scholarly work. Unlike engineering and medicine, tenure decisions at business schools rarely consider the importance of raising soft money for research by way of grants from industry or government organizations. In fact, I have seen letter writers dismiss papers that might be useful to practice as “too applied.”

Sponsored research in fields like medicine and engineering does suffer from its own incentive problems such as the researcher’s desire, on the margin, to make design and data choices to please the sponsor. However, the first order dominant need to go out and convince even one stakeholder to commit hard dollars to a research project forces the scholar to make the research relevant to at least one paying customer. Most of the top 50 business schools are funded by tuition for their undergraduate, MBA and Executive MBA programs. Unfortunately, the ability of tuition dollars to discipline researchers into pursuing research relevant for even the classroom, let alone for policy or practice, is limited.<sup>5</sup> Put another way, the average accounting publication costs a top 50

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<sup>5</sup> Tuition funded research arguably also encourages the need to cater to students to ensure that teaching ratings are high, regardless of the actual content of the class.

business school somewhere in the neighborhood of \$200,000.<sup>6</sup> Others have estimated the cost to be as high as \$400,000.<sup>7</sup> Except for schools with massive endowments, much of this investment is funded by tuition. Sometimes, donations from alumni are used to finance research that is not at all relevant to the donors. How should we think about a reasonable return on such a big investment? Is tuition-funded research sustainable in the long run? Would it not be prudent for schools to encourage faculty to finance part of their research endeavor via industry, trade association or government grants?

My colleagues in Engineering are quick to point out that they are tired of chasing grants and would much prefer to fall back on the luxury of tuition funded research. Some argue that the pressure of seeking grants and the desire to keep up with ever increasing competition in scholarly papers in engineering pushes researchers to pursue incremental improvements in algorithms, on the margin. These scholars would much rather focus on long term problems without the pressing need to convince another body for the next set of grants.

Ideally, tuition-based research should lead to “curiosity” based research that is relevant to either practice, policy or even to students. My worry is that the absence of accountability, financial or otherwise, to anyone other than referees, editors and letter writers, hinders the production of long-term work that is useful to the key stakeholders of the business school.

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<sup>6</sup> The average entry-level assistant professor in accounting at a top 50 business school is paid somewhere in the range of \$180,000 to \$220,000 for nine months. Most assistant professors are promised summer support for two months at least for the first three years of their employment. Assuming 30% employer borne overheads such as that for healthcare and contribution to pension plans, the total cost of an assistant professor to a school is \$286,000 to \$350,000. Assuming that the average professor spends 60% of his/her time on research and publishes one top tier accounting paper a year, the cost of a top tier publication to the school is \$171,000 to \$210,000. The imputed costs of a top tier publication are higher when the paper is co-authored with faculty in the same school or if certain faculty in the school are not very research-active, assuming research-inactive faculty are not expected to teach more. An alternate way to estimate the cost of a publication is as follows: assume faculty produce just teaching and research (ignoring service for now). Assume a research-oriented assistant professor requires \$300,000 in resources to teach three courses and produce one top-tier article, while an adjunct at the same school requires \$150,000 (\$75,000) to teach six (three) courses and produces no articles. Thus, the incremental cost of a top tier publication is approximately \$225,000 ignoring all of the intangible benefits (if any) of having a tenure track faculty member.

<sup>7</sup> See <https://poetsandquants.com/2014/07/16/the-shockingly-high-cost-of-an-academic-article-400k/>

## *2.2 Is it accreditation or tenure?*

A few colleagues have suggested that AACSB (The Association to Advance Collegiate Schools of Business) accreditation criteria or the practice of granting tenure at the business school is the root cause behind the disconnect between academic research and practice. Accreditation dictates the minimum number of PhDs the school needs to have on its faculty. It is worth pondering what the optimal faculty composition (research-heavy versus adjuncts) and performance expectations (in terms of research and teaching) would look like without these constraints.

Others have suggested that tenure is also a potential contributor because tenured faculty are the only voting members in the profession with respect to publications, promotions, and in setting the direction of the school. The modal motive of tenured faculty is arguably to prolong what they do and to accept people to the “tenured club” only if they choose to comply with the tacit rules of publishing papers in the top journals. Arguably, the process of seeking letters from senior faculty at top schools to promote assistant professors contributes to this problem. The promotions and tenure committee (P&T) or other tenured faculty expect to see a letter from each of the top schools before promoting a junior professor. If a senior letter writer says, “no one does that type of research here,” the P&T committee responds “then, it must not be important.” Hence, a few schools can single handedly set the norms associated with “what is good research,” unless we are willing to accept letters from top scholars in an area no matter where they are, within reason.

Having said that, medicine and engineering also deal with accreditation and tenure. Unlike the business school, they differ in how research is financed. Perhaps the financing mechanism also changes the social norms related to the respect accorded to practice-relevant research. Hence, I am not sure accreditation and tenure are the sole contributors to the gulf between practice and academic work.

## *2.3 Metrics to assess impact on practice*

How do I know that our academic work has not had much influence on practice and policy? I rely on three potential markers: (i) how many “products” has academic research created? (ii) to



what extent do concerns that keep up CFOs/CEOs at night get reflected in academic work either in the near or long term? and (iii) is academe ahead of industry in terms of science or knowhow in a particular domain? As a caveat, I do not attempt to measure the impact of research on policy making.<sup>8</sup> Admittedly, my markers are not necessarily the best or the only ways to measure the impact of academic accounting research on practice. But anonymous conversations with a large number of faculty from other schools over the years suggests dissatisfaction with the current state of affairs.

### *2.31 How many products have we created?*

One marker of impact of research in practice is to ask, “how many usable products or business processes has accounting research created since the days of Ball and Brown (1968)?” I can think of perhaps four sets of such products: (i) Bob Kaplan’s work on activity-based costing (ABC) and the balanced scorecard model (Kaplan and Cooper 1988, Kaplan and Norton 1992); (ii) Bell-Ohlson framework that formalized residual income valuation models and spinoffs such as EVA (economic value added) (see Ohlson 1995); (iii) accruals and other accounting signals based trading strategies identified by Sloan (1996), Beneish (1997) and Piotroski (2000) that quant hedge funds use and ETFs (exchange traded funds) now rely on; and (iv) continuous auditing systems, which were first developed at AT&T Bell Labs by Rutgers professor Miklos Vasarhelyi (see Vasarhelyi and Halper 1991).

I am certain my list is incomplete and the American Accounting Association (AAA) might want to compile a comprehensive inventory to address the concerns of skeptics of the relevance of academic accounting research. However, this short list of products surely contrasts with the long list of marketable innovations created by engineering and medicine groups in the university. Other disciplines in the business school also appear to do better than us on this dimension. Academic research in finance can claim some credit for coming up with the capital asset pricing model (CAPM), the Altman Z-score and the Ohlson O-score model to predict bankruptcy, the pricing of derivative

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<sup>8</sup> Several authors have expressed reservations about the impact of accounting research on policy making (e.g., Ronen 2012, Leuz 2019).

instruments via the Black Scholes model and the current trend in smart beta investing. Why is that the case?<sup>9</sup>

### 2.32 Reality check

An alternate way to measure the relevance of accounting research is to compare topics on the agenda of Chief Financial Officers with the content of papers published in the field (McGrath 2007). Deloitte's CFO Insights magazine produces a yearly round up of issues that keeps CFOs up at night.<sup>10</sup> The 2018 version highlights the following issues: (<https://www2.deloitte.com/us/en/pages/finance/articles/whats-keeping-cfos-up-at-night-in-2018.html>) (i) trade wars sparked by the Trump administration; (ii) the impact of rising interest rates on 2019's global growth; (iii) capital allocation between investment, stock buybacks and dividends; (iv) execution risks stemming from insufficient budgets, demoralizing workloads, a deficient inventory of skill, spotty stakeholder support, ineffective governance, and an embedded culture of resistance; (v) talent squeeze as employees need to shift beyond accounting, reporting, and compliance to a staff that is more adept in the analysis, prediction, and decision support; (vi) cyber risk; (vii) communications with activist shareholders on how the windfall from the new tax cut legislation will be distributed; (viii) data science will lighten the transactional burden of the CFO and hence open up time for input into strategic decision making; (ix) leaving a legacy as a talent developer, ethical leader and an effective business partner.

Several of these questions fall within the domain of accounting research (e.g., capital allocation between investment, stock buybacks and dividends, execution risks mentioned above, talent squeeze, cyber risk and data science to lighten the CFO's administrative burden). Admittedly,

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<sup>9</sup> Penman (2008) argues that modern finance came to the fore when it focused on practical products such as investment performance benchmarks, risk metrics, pricing models and so on. He suggests that a focus on products can bond researchers to collaborate and coalesce their research efforts.

<sup>10</sup> This line of argument would suggest that media mentions could be a plausible measure of the impact of scholarship on research. Burton et al. (2017) document that the average natural science publication receives 118 times more attention than the average accounting article, which is the lowest attention receiving discipline studied by those papers.

our comparative advantage is not the “here and now” of business. To evaluate whether academic research catches up with issues of concern to practice with a lag, I repeated the exercise for the year 2014 (<https://www2.deloitte.com/us/en/pages/finance/articles/cfo-insights-concerns-company-performance-pressure.html>). The list of issues then were: (i) worldwide economic health related to concerns about the global economic malaise, uncertainty about Central Bank moves, and a slowdown in China; (ii) policy/regulation uncertainty related to taxes and health care in particular; (iii) cyber security breaches; (iv) talent availability and costs related to identifying people not only with the necessary skill sets, but also intangibles such as curiosity and the ability to team; (v) competition in industry due to prolonged low-interest-rate cycle, pricing, overcapacity and disruptive technology; (vi) organizations’ ability to execute business plans with an eye on a multitude of foreign accounting, tax, legal, compliance, and fraud risks; (vii) how to leverage the unprecedented quantities of data companies collect from their own operations, supply chains, production processes, and customer interactions; (viii) the delicate balancing act between investing for growth and maintaining aggressive cost agendas; (ix) worry about black and grey swan events, geopolitical and otherwise, that might take them by surprise; and (x) time management.

I am not sure accounting (or even finance) research has addressed many of these concerns between the years 2014-2018. A casual glance at the top three journals in accounting (The Accounting Review, Journal of Accounting Research and Journal of Accounting and Economics) will confirm that academic research barely resembles what practice is concerned about. Practitioners and academics generally appear to live in parallel universes. How can we bridge this yawning gap between academic work and practice?

### *2.33 Is academe ahead of industry in terms of knowhow?*

Another marker of the problem is to consider areas where the knowledge generated in academic groups in the business school are ahead of practice. If that were the case, we would expect academic departments to experience significant difficulty in retaining talent. There are rumors that Google and Uber have hired out vast swathes of the entire computer science departments of Carnegie

Mellon University (<https://techcrunch.com/2016/04/26/it-isnt-just-uber-carnegie-mellons-computer-science-dean-on-its-poaching-problem/>). When did we observe such a war for talent in accounting academe?<sup>11</sup>

In many cases, cutting-edge research in finance and accounting is being conducted by the top hedge funds such as Two Sigma and investment banks such as Evidence Lab set up by UBS or fintech start-ups, or by technology firms such as Google, Amazon or Microsoft. Private sector companies have massive R&D budgets. More important, the threat of obsolescence, a takeover or even eventual bankruptcy is far more pressing than at that top business schools. Having said that, I do believe that we, as accounting academics, have a lot to add to research conducted in industry. In my experience, hedge funds, investment banks and fintech companies are overflowing with high quality finance talent or engineers who are adept at data science and even artificial intelligence. But few of these finance and data science professionals fully understand the power, availability and the limitations of the accounting data they use to formulate or back test trading strategies that invariably rely on some transformation of accounting numbers reported in financial statements.<sup>12</sup>

### **3.0 Why have we strayed?**

I list nine potential reasons, summarized in Table 1, and developed in the following paragraphs.

#### *3.1 Human capital entering the profession*

In my experience, entering PhD students in accounting programs at top schools are well trained in economics and finance but are not particularly curious about how accounting institutions

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<sup>11</sup> A few senior accounting faculty members have been hired by hedge funds. However, computer science has experienced what one colleague refers to as the “lost generation” of PhD advisers as a swathe of potential faculty advisers for PhD students have been lured away by the private sector.

<sup>12</sup> How much of our work has impacted policy? I am not sure how to measure the impact of accounting research on standard setting and accounting policy but off-the-record interviews with a couple of standard setters suggest that published research was of limited use in their day to day decision making. However, economists appear to have much more of an impact on public policy relative to the impact that accounting academic research has had on standard setting.

and financial statements work. In fact, the job market papers by the top accounting candidates in the recent past barely address accounting questions. More disturbing, many of the entering PhD students have little practical experience in practice or policy and hence have little to say about practice. I have heard from colleagues at other schools that assistant professors are well versed in the academic literature in accounting but can barely read and make sense of a company's 10-K or internal accounting systems.<sup>13</sup>

On top of that, the published papers in accounting journals are becoming increasingly indistinguishable from those that might appear in a corporate finance journal. The profession needs to have an urgent conversation about the costs and benefits of producing research that is so close to what finance journals publish. Cross-pollination from related areas is a development worth celebrating and I am guilty of my fair share of joint work with finance colleagues. Accounting is inherently a multi-disciplinary area in that virtually every area of business leaves footprints on internal and external financial statements. Cross-disciplinary work also gives us a chance to chip away at the silos that are endemic to academe. Business problems are rarely accounting or finance or marketing problems. Inter-disciplinary awareness and cooperation is one of the most powerful ways of making an impact on practice.

However, it may be time to wonder about our comparative advantage as an academic discipline. What would stop deans of business schools or presidents of universities from asking us to merge with finance and economics groups at our schools if our research output is not statistically different from what finance and economics are interested in? It is not obvious why business schools

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<sup>13</sup> A potential explanation for this trend is to consider a world where the top 50 schools want to hire the best talent. If that talent now comes from high-powered research-oriented PhD programs who select students based on GMAT (especially the quantitative component of the GMAT) rather than, say, their institutional background, we will end up with assistant professors who are not as comfortable with practical problems that afflict financial reporting. One relatively 'easy' fix would be to stop recruiting PhD students who know little about accounting. Instead, if schools emphasized attracting students with prior education and/or experience in accounting, these PhD students would naturally gravitate toward more practice-oriented research questions. Bob Bowen suggests an empirical test of this proposition: consider a sample of ADS (Accounting Doctoral Scholars program) students who came from auditing and compare their paper topics to a sample of PhD students with little institutional knowledge, proxied by a degree outside of business.

need assistant professors, working on corporate finance type topics, to teach accounting to undergraduate and MBA classes. As a prominent researcher confided in me, “I fear that we might go back to the days when CPAs from practice used to be hired by business schools to teach accounting.” On top of that, a vast amount of human capital in accounting academe under-invests in understanding questions of fundamental interest to accountants and accounting. The profession needs to find the optimal balance between its core competence and the need to build bridges with other areas in the business school.

### *3.2. The decline of empirical research in managerial accounting*

Another perspective is that most business problems are managerial accounting questions – not necessarily financial reporting problems. Economists talk of marginal costs and marginal revenue or fixed and variable costs but the rubber meets the road with measurement. Managerial accounting provides empirical meaning to these concepts in businesses. Capital budgeting (project selection) and operational excellence (how to best run the business) are likely the most important tasks facing a manager and can be characterized primarily as “managerial accounting” problems. Yet, financial reporting papers dominate the top journals.

Financial accounting research is easier to conduct due to access to more and better data, more rigorous methods and higher probability of generalizability, which in turn, make it less risky for the researcher to invest her energies in such research. The greater demand for financial accounting research topics among PhD students and junior faculty leads to a circular situation whereby fewer top schools produce managerial research (in accounting at least) and those faculty who try are more likely to fail to meet tenure standards and thus end up at ‘lesser’ schools. Because of inadequate managerial faculty, the school cannot staff all of the MBA managerial accounting classes and eventually several of such classes get cut from the curriculum. Management Science, as a journal, might want to invest in stemming this negative cycle and encourage new directions in empirical management accounting research.

### *3.3 Preoccupation with cross-sectional regression results*

Our literature is preoccupied with what happens to the average firm in the cross-section. The stated reason is the need for generalizability. But there are other research perspectives worth considering. Work in strategy and management, including those related to academic cases, tend to focus on outliers. Extreme winners and extreme losers are studied in depth for lessons on what managers should do or potentially avoid. Of course, outliers suffer from selection issues but cross-sectional results do not necessarily provide managers or investors with decision-relevant information either. Practitioners care a lot about whether their situation is similar to the one the researcher has studied. During the course of my survey work, I have often heard CFOs say, “I don’t care about the average firm. My firm is not average!” It is very difficult for the practitioner to judge whether her firm is close to the average firm in the sample.<sup>14</sup> Hence, they are more likely to turn to case studies or the vast array of best-selling books in the world of investing, finance and management for advice on how to run their business.

#### *3.4 An explosion of “fill the hole” type work*

The top five specialist journals in accounting (The Accounting Review, Journal of Accounting Research, Journal of Accounting and Economics, Contemporary Accounting Research, Review of Accounting Studies), collectively publish at least 150 articles a year. It is unclear how many of these papers are read by practitioners and policy makers. The acceptance rate of these journals is as rigorous as ever, perhaps less than 10%. Given such output and selectivity, one would expect the literature to generate high impact insights that would influence practice and policy. But that has not happened because, in my assessment, far too much of scholarly energy is devoted to finding gaps in the literature and developing contributions to fill these gaps. This approach has also been referred to as “matrix approach” where researchers draw up a matrix of the usual dependent

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<sup>14</sup> A colleague suggests a related problem while teaching academic studies in his/her MBA class: “one thing that constantly comes up is that studies “control for industry effects,” whereas students want specific details on how the industries are actually different from each other!” Along similar lines, another colleague points out that “there is very little appreciation of the industry specificities related to anything corporations do in virtually all studies. In fact, we try to remove any potential industry confounds and come up with behaviors, patterns and regularities that are applicable to all firms. This is not so in management where studying fashion, tourism, retail is perfectly legitimate on its own.”

and independent variables, look for cells that are not filled in and then write papers addressing those empty cells. It would be interesting to review the “contribution to the literature” section of these papers to assess how many claim to address a question of importance to practice.<sup>15</sup>

Incrementalism in empirical accounting research rarely challenges the status quo or asks whether there is anything fundamentally wrong with the existing literature or whether the usual data sources even capture the underlying phenomena studied. One of my favorite questions in accounting seminars is to ask, “have you picked a sample of 20 random firm years in your sample to satisfy yourself that (i) you have really measured what you claim to measure; (ii) whether the causal chain you propose actually holds for most of these 20 firm years?” Remarkably, most presenters at these seminars struggle to give me three or four examples or case studies of the effect they claim to document.<sup>16</sup> It would be particularly useful to set a norm where papers give examples of the phenomenon they discuss for use in a classroom.

### *3.5 Lack of professional incentives*

Currently, professional status, raises and job offers from top institutions rarely follow if a scholar were to dedicate her time to working on problems that practice cared about. In fact, a few

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<sup>15</sup> Typically, the dependent and independent variables on the y-axis and x-axis of the “research matrix” come from one or more of the following constructs: (i) stock price response to an accounting report; (ii) earnings management, accruals based or one based on real actions or some measure of financial reporting quality; (iii) voluntary disclosure, usually measured as the frequency and quality of management earnings forecasts; (iv) some proxy for audit quality; (v) some proxy for tax avoidance or management of tax expense; (vi) some measure of cost of capital, either equity or debt; (vii) analyst forecasts of earnings or debt ratings; (viii) some measure of information asymmetry between the manager and investor such as bid ask spreads; (ix) comparability of financial statements; (x) some measure of accounting conservatism; (xi) some measure of misreporting such as restatements, the SEC’s Accounting and Auditing Enforcement Releases (AAERs) or security class action lawsuits; (xii) specific debt covenants; (xiii) CEO compensation or CEO turnover; (xiv) analyst coverage; (xv) institutional ownership; (xvi) shareholder litigation; or (xvii) some proxy for the quality of corporate governance such as board composition or the composition of board committees. Given the current emphasis on causality in both accounting and finance research, the “fill in the gap” type work has morphed into examining two of the above constructs around exogenous shocks.

<sup>16</sup> In a related issue, Harvey (2017) warns against the practice of HARKing (or Hypothesizing After the Results are Known). For instance, an author starts out with a hypothesis that might be highly relevant to practitioners that X1 influences Y. The author runs the regression Y on X1 and includes 19 control variables X2-X20 but finds no correlation between Y and X1. However, there is a strong correlation between Y and X7. The author changes his story to focus on Y and X7 and X1, X2,..X6, X8,..X20 are the new control variables. It is perhaps no surprise that the new story is not useful to practitioners.



colleagues who have invested in solving problems encountered in practice feel that their efforts have actually been penalized by the academic community. Some see investing in solving applied problems as an effective way to lose mobility in the top tier academic job market. Their academic colleagues look down on their work as too applied, lacking theory and hence not “pure” or scholarly enough.<sup>17</sup> What can we do to remedy this situation? Will deans start, by means of baby steps, to condition rewards and pay raises or just celebrate a faculty member’s ability to demonstrate impact of their research on practice or policy?<sup>18</sup>

A professor from a state school asks, “my own Dean’s office values me only because I am a “producer” of top tier publications and I guess they would be very unhappy if I stopped producing those publications.<sup>19</sup> Every top tier publication I produce helps the school make one small incremental leap in next year’s FT50 rankings, which seems to be all that they care about, because a good FT50 ranking drives enrollment and tuition revenues to our degree programs.” My sincere hope is that the divide between FT50 publications and applied work relevant to policy and practice will diminish in the long run. If not, we will eventually lose our legitimacy to grant these degrees among the stakeholders who support us (students, parents, the Government via subsidized student loans, donors and alumni and employers).

A related comment I often hear in defense of the current system is that faculty who publish in the top tier journals are likely to be leading thinkers in that area and will be best placed to train

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<sup>17</sup> Such dismissal of applied work is compounded by questions about whether accounting is sufficiently scholarly or scientific which, in turn, potentially leads to over compensating in favor of efforts to appear even more rigorous than finance or economics research. The inherently applied nature of accounting is a potential strength of our research. However, in some circles, that feature has become a liability.

<sup>18</sup> The comment raises the legitimate question of what do deans talk about when they showcase faculty research? In my experience, books written by faculty members seem to feature prominently in such conversations.

<sup>19</sup> Harvey (2017) posits another version of this problem. In general, journals like to publish positive results because papers that support the hypothesis get more citations and journals compete on impact factors. Authors figure that out and engage in data mining and p-hacking to find the positive result. This positive result may have little or no economic or practical implications – it is just statistically significant and the paper get published. However, subsequent examination reveals that the positive result turns out to be purely a statistical overfit and hence does not stand the test of time. There is barely any downside for the academic because many schools simply count the number of FT50 publications.

young minds. Some often claim that the refereeing process keeps us mentally sharp, intellectually curious and open to new ideas. I have no quarrel with any of these arguments. Would these positive spillovers from research diminish if the profession, as a whole, were to focus on applied problems?

### *3.6 Hesitation in conducting normative research unlike economists*

We are often told by senior scholars and PhD advisors to stay away from normative work on how policy should be formulated partly because it is impossible for a researcher to balance the costs and benefits of all the stakeholders affected by the regulation. While these warnings are wise in their own right, such cautionary statements have resulted in under-investment in evidence-based debate on the costs and benefits of standard setting. As highlighted in Khan et al. (2017), researchers in other areas, such as law and economics, intensely question the efficacy of mandatory disclosure of information related to health care, nutrition, workplace hazards, and environmental emissions (Oates et al. 1989; Winston 2006; Fung, Graham, and Weil 2007). We rarely see empirical work that considers the value added by years of standard setting or the SEC's regulatory agenda. We seldom observe work in top journals on what good accounting for a particular transaction might look like.

Van Aken (2004) distinguishes between the explanatory paradigm and the design paradigm. The explanatory paradigm, as predominantly followed in business schools, is concerned with understanding what is, while design science (as found in medicine and engineering) is concerned with what should be. Design science is concerned with providing solutions to problems. Van Aken (2004) argues that successful scholarship requires a partnership between the explanatory sciences and the design sciences.

### *3.7 Unease with descriptive work*

Given that lot of what accounting research focuses on is explanatory in nature, I do not understand the profession's unease with descriptive work. Several of the rejection letters my colleagues get from editors and referees have stated that their papers are descriptive. I am not sure why descriptive work is un-interesting. On the contrary, I believe accounting research is preoccupied with testing a few hypotheses in a generic form over and over again (for instance, (i) accounting

choices and earnings management are affected by managerial compensation and taxes; (ii) the stock market reacts or mis-reacts to accounting information; or (iii) taxes matter to accounting and real decisions). The world of business is ever-changing, rich and interesting and to me, there is considerable value in accumulating systematic knowledge about what actually happens in the field, and which process innovations have actually worked or not worked in a particular context and why. Helfat (2007) advocates research that is motivated primarily by the desire to understand phenomena rather than by building or testing theories before adequate data have been collected.

A regulator told me, “I used to enjoy reading accounting research till around five to ten years back when you guys would just document stylized facts. Such descriptive work helped us focus our regulatory and enforcement efforts in particular areas.”<sup>20</sup> Policy makers are looking for rigorous research on the causes and consequences of regulatory experiments in other settings and other contexts. A good descriptive study on how a particular policy initiative (say relaxation of quarterly reporting of financial statements) has worked in the U.K or the European Union is useful to a U.S. regulator mulling over the same initiative in the U.S. Of course, the social and business context in these countries are different, the results may not be causal and may not generalize *in toto* to the U.S. context. But, some relevant evidence is better than (i) none at all; or (ii) evidence from an exogenous shock that is even less generalizable to the current context.

### *3.8 Reluctance to work on applied problems as research teams*

Many of the questions relevant to practice are usually tackled by large teams of managers and employees in their organizations. However, we are pre-occupied with papers written by a team of two or three scholars. Most of my colleagues would hesitate to get involved in a research project with four or more colleagues as they fear dilution of credit. Performance evaluation systems in dean’s offices and letter writers constantly try to ascertain the “lead” author or the intellectual thought

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<sup>20</sup> Along similar lines, Sam Peltzman (2015) has stated, “the most significant offense to the Coasean tradition ... is the downgrading of honest descriptive work... A statistical or historical investigation that does not pretend to identify a causal relationship but does illuminate facts that need further exploration is what I would regard as something that is vintage Coasean tradition.”

leader on a piece. While performance evaluation has its place, such a preoccupation with the parsing of credit creates disincentives for teams of researchers to tackle big questions.

Consider the scientific team assembled by Intergovernmental Panel on Climate Change (IPCC, see <https://www.ipcc.ch/>). The IPCC is organized as three working groups. The first group examines the physical science underpinning climate change. The second and the third working groups deal with the impact and mitigation of climate change. A remarkable 831 experts were originally selected as lead authors and editors from 3,598 nominations across the three working groups to work on the scientific panel ([https://www.ipcc.ch/site/assets/uploads/2018/02/FS\\_select\\_authors.pdf](https://www.ipcc.ch/site/assets/uploads/2018/02/FS_select_authors.pdf)).

How would something like this work in accounting research? In my experience, real progress in evaluating the costs, benefits and consequences of accounting regulation is stymied by a lack of consensus among researchers on some of these basic questions: (i) how to measure earnings management? (ii) how to distinguish earnings management that is explained by benign economic changes in the business versus opportunistic manipulation? (iii) how to measure the frequency and quality of disclosure of non-mandatory financial information by managers? (iv) how to measure comparability of financial statements? and (v) how to measure a company's cost of capital? Would the American Accounting Association (AAA) be willing to set up a scholarly body similar to the IPCC to work on these questions? Such a one-time collective effort might jump start the productivity of future researchers in the field.

### *3.9 This is what consultants do*

The main objection I often hear to my proposal for practice-driven research is that a top university is not a trade school. I am often told that we should concentrate on trying to understand how the world works at a fundamental level and not necessarily respond to the ever-changing concerns of CEOs and CFOs. I am not sure my proposal is inconsistent with this objection. It is useful to refer back to the work of Stokes (1997) to frame this discussion:

Figure 1: Stoke's quadrants

		<b>Relevance:</b>	
		<i>Considerations of use</i>	
		<b>No</b>	<b>Yes</b>
<b>Rigor:</b> <i>Quest for fundamental understanding</i>	<b>Yes</b>	<i>Bohr's quadrant</i> Basic disciplinary research	<i>Pasteur's quadrant</i> Professional schools, Business schools
	<b>No</b>		<i>Edison's quadrant</i> Consulting firms

Figure 1 above, adapted from Stokes (1997), graphically depicts the quadrants his book describes along the two axes of (i) rigor with an intent to fundamentally understand a phenomenon; and (ii) relevance, defined as the desire to use the findings for an applied purpose. According to Stokes (1997) and as summarized in Tushman and O'Reilly (2007), some research is aimed at improving our understanding of a phenomenon, with no thought of specific use (e.g., Neils Bohr and the discovery of atomic structure). Other research is done simply to develop applied uses (e.g., Thomas Edison and the invention of the phonograph) and still other research emerges from both a quest for fundamental understanding and a desire to apply the findings (e.g., Pasteur and the development of microbiology). Research by consultants falls in Edison's quadrant. The aspirational goal is for accounting academic research to fall in Pasteur's quadrant where scholars seek a fundamental understanding of phenomena with a desire to tackle problems relevant to practice. Unfortunately, a vast majority of accounting research falls in the Bohr quadrant with no near-term plan to move to Pasteur's quadrant.

Consultants routinely produce surveys and studies that are timely and very relevant to practice, usually in the very short run. For instance, all major accounting firms have issued detailed implementation guidance on what the new accounting standard for leasing, ASC 842, implies for companies. Many of the surveys that consultants conduct will not stand up to academic scrutiny. In fact, in many surveys, consultants rarely reveal the number of observations used or how they might have addressed non-response bias. McKinsey's (2017) study on identifying companies with long horizons has been influential despite questions about the rigor of the measures used to identify such horizons (Summers 2017, Rajgopal 2017). Top tier academic research, on the other hand, is highly rigorous. Good researchers take great pains to ensure that the underlying data is reliable and the research design is the best it can be, given the limitations of the context. The challenge is to make them more relevant and timely for practice.

### *3.10 More on applied research v/s consulting*

Critics often suggest that the distinction between practice-influenced research that I advocate and consulting is not sufficiently sharp. As stated in section 3.9, consultants and academics have their own comparative advantages. Moreover, engineering and medicine appear to manage any apparent tension between consulting and practice-influenced research quite well. I asked an engineering colleague and the chief technology officer (CTO) of a large technology company what business schools could learn from how engineering schools and companies finance academic research as opposed to consulting projects.

These interviews suggested several interesting differences between our two schools. First, a key question relates to the boundary demarcating university R&D and corporate R&D groups. In engineering, when the answer to a problem is discovered at a university, corporate R&D takes over to address refinements to the basic solution and to facilitate mass production. That is, universities discover the science behind the prototype and corporations mass produce and refine the prototype. Corporate R&D groups hire engineering academics as consultants to address tactical problems which require timely access to expertise that such professors can provide to hasten refinement and mass

production. It is not obvious where a similar boundary lies between business school research and (i) a large market for corporate advice dominated by firms like McKinsey; and (ii) in-house investments in management practices at companies. Is it inconceivable to think of a future where a few business schools offer inter disciplinary faculty teams for a fee to address applied problems that businesses face?

Second, well-funded technology companies such as Google or Microsoft are happy to give grants to engineering faculty in top schools because they want to build relationships between the company and the researcher. These relationships nudge the researcher to work on problems that Google and Microsoft care about. Such work also creates a pipeline of PhD and masters' students that these companies can hire from. We do not observe such incentives at play in business schools partly because academics are less likely to work on applied problems.

Third, at least in the asset management business, the quest for alpha or abnormal return from trading is a zero sum game. That is, asset management companies are more likely to hire academics as consultants so that the knowhow on how to generate alpha is captured by the company that funds the research. In contrast, the giants in the technology industry are more likely to possess the resources or the network effects to exploit advances created by basic university funded research even if such research is published in engineering academic journals. Moreover, patents make it easier to enforce property rights to research in engineering relative to business. Perhaps the business school could consider investing in process patents for business processes and metrics that faculty generate.

Fourth, government entities such as the National Science Foundation (NSF), in general, hesitate to fund enquiries into business problems perhaps because (i) business schools are seen as wealthy; and (ii) of the perception that taxpayer funds are better deployed in addressing larger scientific challenges of national interest than applied business problems. One strategy is for the American Accounting Association (AAA) to lobby Congress or NSF for taxpayer funding on accounting issues that affect society as a whole. For instance, early detection of corporate fraud can potentially save state sponsored pension funds several millions of dollars. Better sovereign

accounting practices can identify inefficient resource allocation of public finances and potentially save the taxpayer sizeable sums.

Finally, both the engineering professor and the CTO asked me to consider a way to recreate something akin to the DARPA model in business research (<https://www.darpa.mil/about-us/about-darpa>). DARPA or the Defense Advanced Research Projects Agency was set up in 1957 to make pivotal investments in breakthrough technologies for national security. Its current budget is around \$3.3 billion financed by Congress via taxpayer funds. Engineering faculty compete for DARPA funds. Along similar lines, the MacArthur Foundation's: "100&Change" is a competition for a \$100 million grant to fund a single proposal that promises real and measurable progress in solving a critical problem for our time (<https://www.macfound.org/press/press-releases/new-macarthur-competition-award-100-million-help-solve-critical-social-problem/>). DARPA routinely creates programs around "DARPA Hard" aimed at addressing high risk, high reward problems ([https://www.researchgate.net/publication/275674141\\_Point\\_of\\_View\\_Changing\\_Culture\\_Through\\_Visionary\\_Thinking\\_Applying\\_the\\_DARPA\\_Hard\\_Test\\_for\\_Innovation](https://www.researchgate.net/publication/275674141_Point_of_View_Changing_Culture_Through_Visionary_Thinking_Applying_the_DARPA_Hard_Test_for_Innovation)). The National Science Foundation (NSF) recently announced the 2026 Idea Machine, challenging the community to come up with "Big Ideas." ([https://www.nsf.gov/news/special\\_reports/nsf2026ideamachine/index.jsp](https://www.nsf.gov/news/special_reports/nsf2026ideamachine/index.jsp)). Can we convince either Congress or a private foundation or a collation of companies, in collaboration with business schools, to contribute seed capital to support research into applied business problems aimed at transformational, as opposed to incremental, change? Such an initiative is more likely to succeed if the research is directed towards solving applied problems that plague industries or society as a whole rather than on issues specific to a particular company.<sup>21</sup>

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<sup>21</sup> Amadeo Pugliese points out that a related institution is the Australian Research Council Linkage Funding Scheme. The scheme supports collaborative research and development projects between higher education organizations and partner organizations which generate new knowledge and involve risk or innovation. It does not support projects that are essentially consulting arrangements or where there is little or no public benefit. A partner organization must provide a cash and in-kind contribution for the project that at least matches the value of the grant being requested from the ARC. Prof. Pugliese reports that the program eventually led researchers to venture out of their offices into the field to look for interesting questions.



#### 4.0 A potential research agenda

When I ask junior faculty why they prefer to work on corporate finance, as opposed to accounting, questions, they often tell me, “all the interesting questions in accounting have been answered.” Sections 4.1 and 4.2 provide examples of two research agendas that the field could pursue to converge academic work with practice in financial accounting. I encourage other colleagues to contribute similar ideas in managerial accounting, auditing and taxation.

##### *4.1 A research agenda rooted in financial statements*

Economics 101 tells us that value is added by some combination of materials, labor, capacity and managerial talent. However, the standard reporting model for U.S. companies, shown below, classifies expenses by function and is not very useful to an analyst curious about these value drivers.

Revenue		X
Less Cost of goods sold (COGS)		X
Selling, General and Admin expenses (SGA)		X
Research and Development (R&D)		X
Depreciation and amortization	X	
Interest expense	X	
Provision of taxes		X
<hr/>		
Net income		X

In general, it is difficult for an analyst to estimate how much material the firm consumed as that number is hidden in COGS. COGS includes manufacturing labor costs and overhead and most companies do not separately disclose those numbers. Labor costs are hidden in every functional line item on the income statement where labor is employed. For instance, compensation paid to scientists and engineers is in the R&D number. However, very few U.S. firms actually disclose total labor costs, disaggregated by function. Even if firms follow IFRS and disclose labor costs, there is very little information on how many employees work in specific functions.

Capacity costs are poorly handled by the current model because we typically straight-line the historical cost of property, plant and machinery without asking how much capacity costs the firm needs to incur to ensure it retains its market share (also referred to as “maintenance capex.”). Such maintenance capex should be expensed in the income statement, as opposed to growth capex (capacity costs incurred to acquire new customers and new markets), which should ideally be capitalized. Financial statements tell us very little about the quality of managerial talent and corporate culture. One of the CFOs interviewed for the Dichev et al. (2013) told us, “we spend a lot of time conducting a fundamental analysis of the numbers. Very few conduct a fundamental analysis of the people running the company.”

On top of that, there is little information in the current reporting model to parse out the fixed and variable components of the firm’s cost structure. Absent detailed data on costs, it is difficult to answer a question that confronts analysts every quarter: if sales go up by 5%, how much would net income go up by? Answering that question requires information about operating leverage and these data are not easily available from the current income statement.

In the class I co-teach at Columbia with Trevor Harris, we ask the students to recast the income statement into the following format (where price refers to price per unit of the product or service sold, quantity refers to the volume of the product or service sold, FX refers to the component of revenue or costs attributable to changes in foreign currency rates and c refers to variable cost per unit sold):

Revenue (price or $p \cdot \text{quantity}$ or $q \cdot \text{currency impact}$ or FX)	X
Less: Cost of materials ( $p \cdot q \cdot \text{FX}$ )	X
Labor costs:	
Manufacturing labor costs	X
Fixed cost ( $\text{time} \cdot \text{FX}$ ) v/s variable cost ( $c \cdot q$ )	
Research labor costs (mostly fixed)	X
Selling labor costs	X

Fixed v/s variable		
G&A labor costs (mostly fixed)	X	X
Maintenance capex		X
Interest expense		X
Provision of taxes		X
<hr/>		
Net income		X

In other words, we ask students to use supplementary data outside of the financial statements to think hard about the actual drivers of value, decomposed into volume, price and fluctuations in foreign currency (FX). The objective is to integrate insights from standard costing in managerial accounting (volume variance, price variance and other variance from other line items such as foreign currency) to understand the sustainability of a firm’s earnings. The intuition is that revenue and income increases driven by price hikes or currency changes, as opposed to quantity changes of the product sold, are less likely to sustainable in the long run.

These questions are even more difficult for technology companies that rely on intangible assets for revenue generation. How much maintenance capex and R&D should Netflix invest to ensure it can retain its market dominance in the streaming market? How should we deal with technology companies which are network businesses because networks increase in value with use, as opposed to tangible assets which generally tend to lose value with greater usage?<sup>22</sup> How should digital companies price their product such that they can recover the large fixed costs they incur to create their products? Research into these questions could constitute a fundamental contribution to both the academic literature and to analysts and investors.

#### *4.2 Research into pervasive use of heuristics*

The real world is complex and executives are perpetually pressured for time. Fire-fighting and a reactive, as opposed to a proactive, use of executive time is now more the norm than the

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<sup>22</sup> Baruch Lev (2001) laid the foundation for a framework to begin answering some of these questions.

exception. Such constraints often lead executives to seek “silver bullet” solutions to difficult problems. Many of these simplistic answers manifest themselves as heuristics. The world of accounting and valuation is replete with heuristics. For instance, Moody’s relies on an ad-hoc 6X or 8X multiple of operating lease rents to capitalize operating leases on a firm’s balance sheet. This is despite footnote availability of a long time series of data on future cash flow obligations on operating leases. Moreover, Moody’s assumes that the capitalized lease asset and the liability are set to the same number although we know that’s unlikely to be true, except at the beginning and the end of the lease term. This is because the leased asset is usually depreciated at a rate faster than the rate relative to the rate at which the principal component of the lease liability is paid off. Do these heuristics provide a reasonable approximation of a more sophisticated model? Why do these heuristics persist when a simple spreadsheet model can provide better data?

Every year, I interview three analysts, drawn from the buy side and the sell side, before I teach my elective on fundamental statement analysis. I usually ask them questions that we address in the class such as (i) how do you forecast revenue? (ii) how do you estimate the fixed and the variable component of the company’s cost structure? and so on. Most analysts forecast costs as a proportion of sales. For instance, when asked about next year’s R&D costs, they would forecast R&D for the next year as X% of forecasted sales where X is the usually the historical average of R&D/Sales. But forecasting a specific line item as a function of sales is conceptually inaccurate because the underlying assumption is that the specific cost is variable in nature. R&D spending does not necessarily rise or fall in proportion to sales. Lab scientists and software engineers are not necessarily hired and fired as annual sales rise or fall. When confronted with this argument, the analyst would often say, “forecasting is very hard and assuming X% of sales is a reasonable approximation.” Is X% of sales indeed a reasonable approximation?

For another example, consider the rampant use of EBITDA multiples. EBITDA or earnings before interest, tax, depreciation and amortization, is supposed to proxy for operating cash flows although they obviously exclude working capital accruals that should be subtracted out to derive

operating cash flows. More damaging, reliance on EBITDA assumes that the company does not have pay for interest or taxes or set aside resources for maintenance capex. I have asked several analysts (i) how they decide to value a company at 10X earnings or 10X EBITDA; and (ii) are they not aware that EBITDA is an inaccurate and potentially misleading measure? Where does 10X come from? The answer I often get that, “we need some kind of valuation shorthand to discuss the stock with our clients and EBITDA multiples serve that role.” When I asked the analyst about the number of classes devoted to teaching MBA students discounted cash flow calculations (DCF) and the residual income model, the analyst said, “I estimated a DCF perhaps once last year. I rely mostly on P/E and EBITDA multiples and I stopped using residual income models for technology companies as they report negative earnings. Eventually, I stopped using residual income models for all companies.” Are these statements a “market test” of whether better technology is cost-effective? Or do these statements suggest that most of these analysts did not fully understand the better technologies in their MBA (or finance) programs?

In general, it might be worth understanding (i) how did these heuristics become so prevalent in the real world?; and (ii) what are the costs and benefits of using such heuristics? Documenting clear long-term costs to reliance on heuristics might convince analysts to take the academic tools more seriously. Alternatively, we might discover that heuristics used in practice are “good enough.” Such a finding would raise questions about the value of over-refining existing tools used in academe for fundamental analysis and valuation.

## **5. Mechanisms to address the research-practice divide**

Barring a short-term change in how faculty are rewarded or evaluated, one way to change things is to get faculty out in the field and closer to trenches. The academic medical doctor at a university hospital spends almost half of her time interacting with patients. That activity earns the university much-needed revenue. More important, it provides the academic doctor hundreds of data points every day on permutations of diseases and how potential cures interact with how those diseases evolve. In an ideal world, the academic accounting professor needs (i) a similar feedback loop from

the real world on emerging problems; and (ii) data on whether suggested interventions remedied that problem -- or failed because the proposed solution did not, ex-ante, recognize the boundary condition under which the fix did not work.

I survey four sets of initiatives, summarized in Table 2. The first set discussed in section 5.1 is intended to bring the academic closer to the field. The second set of ideas in section 5.2 is designed to disseminate our ideas better. Sections 5.3 and 5.4 are related to actions one can take to modify the publication process and to use the classroom as a stage to integrate research and practice. We have tried a few of these ideas with some success at Columbia. Individual schools might want to consider such prototypes depending on that school's appetite for change, political will to alter the status quo, and their cultural milieu.<sup>23</sup>

## ***5.1 Mechanisms to get academics closer to the trenches***

### ***5.1.1 Industry sabbaticals or leave***

The proposal is for the faculty member to spend time in a company related to her field of expertise for the sabbatical year. The objective is for the faculty to get a better understanding of how real-world decisions are made and the bottlenecks affecting the firm's progress towards its goals. When the faculty member returns to academe, perhaps the fresh perspective or the proprietary data or the social connections acquired during the sabbatical year will influence the research agenda and teaching in the classroom. At a minimum, MBA students usually view faculty who have spent time in the real world with greater credibility.

There are a couple of logistical issues that need to be worked through. First, several schools may not want the faculty member to be paid during the sabbatical year. However, the corporation would likely prefer to pay the faculty member for her time to avoid being sued for intellectual

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<sup>23</sup> Not everyone on the Business School faculty has to fit the same mold. Surely there is room for all types of rigorous researchers as long as the work is well motivated, well-done and is insightful. Ultimately, the tension, if any, seems to be between relevance and rigor. My hope is that all scholarly journals will eventually evaluate relevance with reference to practical problems and will assess with a 10/20/30-year horizon, whether the "basic" research published in the past yielded practical knowledge later.

property (IP) conflicts later. Second, the university has to work out who owns the IP created by the faculty member at the company during the sabbatical. Third, identifying a worthwhile company to visit requires building trust on both sides. The individual has to be interested in the company's activities and more important, the company has to believe that it can benefit from letting the faculty member hang around for six months to a year. Having said this, such industry sabbaticals appear to be quite routine in Engineering, especially in Computer Science. Business schools could do well to build off of those precedents.

### *5.1.2 Problem exchange seminars*

The idea here is to bring in distinguished alumni or other experts in specific areas and ask them to suggest a couple of questions that they have had persistent trouble addressing in their careers. The responses tend to be bi-modal. A few executives are brilliant at reflecting on this question and hence come up with very thoughtful questions. Others have a harder time stepping back from the day to day noise of executive life to articulate somewhat higher-level conceptual questions that they had to grapple with in their careers. Hence, one has to either vet the speakers ahead of time or judge the success of the program at the portfolio level, rather than on a talk-by-talk basis.

The exchange could go the other way as well. At Columbia, we are in the middle of organizing talks where faculty present ideas from their work that they believe have impacted practice or policy. This forum, directed at alumni and other colleagues, can become a platform to celebrate and disseminate applied research conducted by faculty.

A success story in this regard is a newly formed initiative at Columbia Business School to understand sovereign risk from financial statements. One of the alums of the school was a successful investor in sovereign bonds of some of the troubled European economies (Italy, Greece and Portugal). Remarkably, the trade that made the investor buy such bonds at depressed prices was triggered by a detailed review of the financial statements of these countries. New Zealand is the only country in the world that follows accrual accounting. Sovereign accounting is primarily cash based and most notably ignores the billions and sometimes trillions of dollars of off-balance sheet

obligations such as pensions and healthcare obligations to citizens. Moreover, the debt assumed by state-owned enterprises is often not consolidated on the country's balance sheet. On the other hand, even long-term debt is often booked on the balance sheet at the maturity amount, not the present value. After sorting through these disclosures, the investor concluded that concerns about default risk of debt of Greece, Italy and Portugal were overstated and the market price of their sovereign debt was lower than what fundamentals would dictate.

This investor was interested in encouraging research and teaching of sovereign financial statements at Columbia Business School. Very few business schools offer classes in or conduct research on the fundamental analysis of sovereign financial statements. Hence, an interesting idea from the field has potentially opened up a whole new area of inquiry and scholarly research.

### *5.1.3 Venture accelerator*

Taking the idea of problem exchange seminars mentioned above to its logical extreme, the school could consider establishing a start-up accelerator that encourages faculty to set up businesses to translate their academic ideas to products and processes. Let's say a professor comes up with new ways of measuring the Environmental, Social and Governance (ESG) rankings of a company, an area where existing commercial products can use some improvement (see <https://www.breakingviews.com/features/guest-view-esg-ratings-arent-reliable-enough/>).

Maintaining that dataset on a monthly or quarterly basis becomes a bottleneck to successfully monetizing the idea. Some scholars try to find the resources to conduct such maintenance via a center (e.g., John Graham's marginal tax rate data). More often than not, such data is given away to institutional investors and brokerage houses for a small fraction of its true market value. Commercial vendors, with a fraction of the expertise or quality, command higher market prices for their data. This is one kind of social transfer from academe to practice that often goes unrecognized. On top of that, it is somewhat hard to patent such a new process.

However, a start-up set up exclusively to produce and market such rankings nurtured by the Business School with patient capital and advice from alumni or friendly venture capitalists is more



likely to succeed at the endeavor. Apart from benefiting from their founder stakes, faculty can learn a lot about how businesses are created and nurtured from such an exercise. The business school, at least on a portfolio basis, can reap dividends from its stake in such faculty-initiated ventures.<sup>24</sup>

#### *5.1.4 Professor in residence programs*

Many business schools have an executive-in-residence program. These are usually populated by retired executives in the C-suite, members of the board of directors or venture capitalists who donate a day or two of their time every week or a month to the school. In my experience, students recognize the value of this resource much more than faculty. These executives could serve as a sounding-board for ideas on how the school can integrate academic work with practice, given its unique culture and constraints.

However, I propose that we invert the model. What if we place interested professors in companies for a day or two per month? For instance, private equity firms typically meet on Friday mornings to discuss potential deals. What if we have a professor, interested in mergers and acquisitions or private equity or entrepreneurship, were to observe these meetings for a year? The professor gets closer to the actual decision-making process of the private equity firm. The business, assuming we have taken care of confidentiality concerns, gets a smart and initially unpaid outsider in the room and might potentially benefit from research or theoretical insights the faculty can bring to the conversation. Exposure to repeated interactions and transactional frictions in the deal room might change the nature of research questions addressed by the faculty member and potentially get them to access proprietary data that would further their inquiries of fundamental importance to their area.

#### *5.1.5 Faculty lightning talks for expert practitioners*

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<sup>24</sup> An anonymous colleague from a state school suggests that the knowledge we produce as academics should be made available for the benefit of society especially in state schools where taxpayer dollars contribute much of the operating budget. Could revenue from such accelerators actually relieve state schools from the financial pressure they face from their legislatures?

Most schools usually have alumni at important positions in companies, venture capital firms, investment banks or even at non-profits. More often than not, the dean's office in the school has a ready list of such individuals who are potential high net-worth donors to the school. I propose using these individuals as an intellectual asset, as opposed to treating them merely as financial assets. The idea is to find a common research theme that the faculty is interested in, say mergers and acquisitions. Accounting faculty might be interested in how M&A transactions are used to create provisions or to book cookie jar reserves. Finance faculty may care about how firms decide to acquire targets and how to finance such investments. Management faculty may want to explore cultural barriers to integration of the target company with the acquirer. Imagine setting up a panel of three or four top alumni who have participated in or financed many M&A transactions. Interested faculty deliver a 15-minute lightning talk on their research project to such a panel. The panel can assist with several questions: (i) is the research topic a matter of first order concern in their experience to the field of M&As? (ii) can the panel find specialists in their firms that can help the faculty either with advice or data to refine these research questions; and (iii) best of all, would they be willing to fund the research project?

A wise man once told me, "if you want money from someone, ask for advice." Panels such as these are likely to increase alumni's intellectual engagement with the school and improve the chances that one of these alums would actually give the school a big financial gift down the line.

#### *5.1.6 Alumni mentors for junior faculty*

Along similar lines, we have tried to find alumni mentors for junior faculty. Increasingly, junior faculty in many areas of the business school are well versed with the academic literature in their field but are not necessarily fully aware of emerging trends in practice. One of the unique aspects of New York is that a lot of smart people are working on the same transaction or in the same area, except some happen to be in academe and the others happen to be practice. The idea is to connect the faculty member to an alumnus who works in the same domain area. The mentoring can range from a coffee every semester to a deeper engagement where the alumni can share tacit

knowledge related to that domain area with the faculty member. I acknowledge that this idea might be difficult to implement in a campus town where the university is perhaps the largest employer. But, even a regular Skype meeting between the junior faculty member and an alumnus who shares the same professional interest could be valuable.

#### *5.1.7 Pin factory trips*

Columbia Business School Dean, Glenn Hubbard coined the term, “pin factory trips,” drawing on how Adam Smith’s visit to a pin factory inspired his insights into specialization and division of labor in the first chapter of his book, “The Wealth of Nations.” Every semester, we take interested faculty on a half-a-day visit to an interesting company based in New York city. Recent venues of such pin factory visits include hedge funds, investment banks, media and fashion companies. Senior executives, usually alumni, spend half a day with faculty presenting information about the opportunities and challenges presented by a function (say hiring and retention) or a particular area of the company (how ETF trading is challenging the jobs of sell side analysts) or the application of new technology such as data mining or artificial intelligence or block chain. Half a day is a more realistic time commitment one can expect of both busy faculty and senior executives at large corporations. These visits give faculty a first-hand glimpse into the frictions and difficulties that businesses grapple with. Some faculty might become curious enough to write a case about the company. Others invite these executives back to campus as guest speakers or judges in competitions at the school. Some faculty schedule follow-up visits with individual executives. Although half a day is arguably not a lot of time, even a curated peek into a slice of the business helps with getting more faculty more engaged with problems confronting practice.

#### *5.1.8 Regulators can publish research questions*

Academics working at regulatory institutions such as the SEC or the FASB often tell me that they come across at least one unanswered question a day, if not more, as they deal with difficult emerging questions brought to them by practice for regulatory guidance. On the other hand,

graduating PhD students and assistant professors are constantly looking for interesting research questions to work on.

The logical course here seems to be to make the market in research questions. Regulatory agencies might want to publish questions for which they need answers or evidence.<sup>25</sup> They could go further and provide access to senior regulators for interviews or even proprietary data to budding scholars. Every year, the regulator could hold a conference to invite the researchers working on these quasi-commissioned projects. I know that the SEC does organize academic research seminars but my guess is that they cannot directly influence the topics covered by authors in these seminars. I advocate a model where the SEC or another regulator actively commissions research studies after vetting applications received from academic scholars interested in a research question the SEC or the regulator cares about.

#### *5.1.9 Marketing Science Institute*

The Marketing Science Institute (<http://www.msi.org/about-msi/>) was set up with the intention of bringing practitioners and academics together. Corporate partners pay a fee for participation. The Institute uses these fees to fund research projects on questions identified by corporate partners. Editors of leading journals are inducted as members of the Institute presumably with a view to both educate them on issues that practice cares about and to potentially consider papers that come out of this process at their journals for eventual publication. Periodic conferences provide funded academics a chance to present their work to corporate members. The Institute also provides a forum to enable one on one interactions between scholars and executives.

More intriguing, the Institute actually publishes a short list of research priorities for the next three years (<http://www.msi.org/research/2018-2020-research-priorities/>). The process actually

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<sup>25</sup> The Government Accounting Standards Board (GASB) publishes an annual call for research into priority areas for the GASB ( see [https://gasb.org/cs/BlobServer?blobkey=id&blobnocache=true&blobwhere=1175834663598&blobheader=application%2Fpdf&blobheadername2=Content-Length&blobheadername1=Content-Disposition&blobheadervalue2=161346&blobheadervalue1=filename%3DCrain\\_2017\\_RFR.pdf&blobcol=urldata&blobtable=MungoBlobs](https://gasb.org/cs/BlobServer?blobkey=id&blobnocache=true&blobwhere=1175834663598&blobheader=application%2Fpdf&blobheadername2=Content-Length&blobheadername1=Content-Disposition&blobheadervalue2=161346&blobheadervalue1=filename%3DCrain_2017_RFR.pdf&blobcol=urldata&blobtable=MungoBlobs)). I thank Divya Anantharaman for bringing this to my attention.

followed to come up with these priorities is interesting. The Institute begins with an open-ended survey with the trustees of the Marketing Institute, which includes corporate members and faculty, to identify a few research priorities. These are followed by in-depth interviews with other members and a broader outreach among current and past academic trustees, executive directors and young scholars to identify pressing marketing challenges and critical issues for the future. The trustees then go back and prioritize the relative importance of these broad topics. I believe such a detailed process to discover a list of pressing challenges facing the field, agreed upon by academe and industry, is unique and worth emulating. Moreover, the Marketing Science Institute awards a prize for the paper that has made the most significant contribution to marketing practice and thought. Perhaps the AAA would consider creating an institution similar to MSI for accounting research.<sup>26</sup>

#### *5.2.0 Wharton customer analytics initiative and InnoCentive.com*

I recently came across an interesting model to engage with practice from the marketing group at the Wharton school (<http://wcai.wharton.upenn.edu/for-researchers/research-opportunities/>). The Wharton Customer Analytics Initiative (WCAI) attempts to connect datasets and fundamental problems in practice contributed by industry partners with academics who might be interested in studying and proposing solutions to these problems with the explicit understanding that such solutions can be packaged as a peer-reviewed article for academic journals. The initiative appears to have its beginnings in the marketing arena but has since expanded to cover questions of interest to other disciplines as well. For instance, the topics covered include: (i) understanding the interplay between boards, executives and company performance based on a dataset provided by Equilar, an executive compensation consulting firm; (ii) an anonymous Fortune 500 software company that wants an in-depth analysis of its customers' habits; (iii) an anonymous beauty retailer was interested in understanding how people fall in love with products; and (iv) the Lincoln Financial Group was interested in predicting and exploring the lifetime value of financial advisors and agents.

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<sup>26</sup> The Journal of Marketing has institutionalized the publication of academic work that integrates theory and practice (see <http://theorypractice.org/about/>).

The Wharton model is similar to a commercial version of the “crowd sourced” R&D model pioneered by InnoCentive.com (<https://www.innocentive.com/>). InnoCentive was set up by a group of pharmaceutical researchers working for Eli Lilly in the year 2001. Companies or entities post RFPs (request for proposals) seeking solutions to complex problems post them on the InnoCentive’s website. The exact question and the expected deliverable are clarified in the RFP. The deliverable is usually a theoretical answer to the problem and is sometimes followed by a desire to hire the identified collaborator for testing the solution in a lab or for creating a prototype for the product. Expectations related to who owns the intellectual property (IP) and the compensation paid to the winning participant are discussed in the RFP. The sponsoring company usually retains the right to decide what constitutes a winning entry.

In 2006, Prize4Life partnered with InnoCentive to launch the \$1 million ALS (amyotrophic lateral sclerosis) Biomarker Prize designed to find a biomarker to measure the progression of ALS. In February 2011, the \$1 million prize was awarded to Dr. Seward Rutkove for his creation and validation of a clinically viable biomarker. Business schools might want to think about setting up their version of InnoCentive.com to attract applied projects from alumni that teams of faculty can work on.

### *5.2.1 Coalition of the willing for field studies*

Randomized controlled trials (RCTs) are often seen as the gold standard in research designs. However, getting individual companies to sponsor RCTs is very difficult. I have worked with a company for a year and half pitching proposals, tweaking them, ensuring that both the University and the company’s lawyers do not object to the proposed intervention only to have the company pull the plug on the project the week before the intervention was to go live. I could afford this costly distraction because I have tenure. A couple of such failures would be devastating to the career prospects of a junior faculty member interested in conducting field studies given the loud tick of the tenure clock in the background.

As Patty Dechow suggests, lawyers tend to constrain interaction between business firms and researchers for two reasons. They are uncomfortable with statistics and large sample studies as they rely more often on cases to give precedents. Lawyers in corporations or asset management firms are only accountable for the downside. Hence, there is rarely a benefit to their careers for the slightest remote chance of a dispute or of information leakage. How do we change these incentives?

I propose that schools invest in assembling a set of companies (“a coalition of the willing”) who would be open to running RCTs in their businesses. Lawyers and senior executives from both sides could potentially come up with a list of standard approvals and protocols once for say a period of three to five years so that faculty can be spared the red tape and get to testing hypotheses in conjunction with managers. The potential benefit to both parties could be immense. The management literature has advocated the idea of “evidence-based management” as a promising way for researchers to have impact on practice (Rousseau and McCarthy, 2007). Managers in the field often have interesting conjectures on how specific interventions such as a change in workflow or an incentive scheme would affect productivity or the quantity or quality of the output. Some of them would hire a consulting firm to test those conjectures for them. An academic team could potentially test these conjectures in a more rigorous manner in exchange for access to real world data and a potential publication in a top journal.

## ***5.2 Mechanisms to better disseminate our findings***

Another concern is that practitioners cannot find scholarly work and even if they do, the arcane language and convoluted structure of a typical journal article puts off a practitioner from even skimming the introduction. The obvious recommendation is that we consider presenting our academic work in a short reader friendly articles, similar to the practice followed by *Science* and *Nature*.

### ***5.2.1 Topic du jour talks for students and alumni***

Students have often told me that they have no idea what academic faculty do in the 35 plus hours of the week that they are not in a classroom lecturing or facilitating case discussions. Students

and even alumni only have a vague sense that the professor is involved in some kind of academic research. The only reliable access that students have to research findings are the newsletters that schools publish to translate recent publications of a few faculty. But these tend to cover stories on research questions that the faculty, not necessarily the students and alumni, care about.

A proposed model to inform students about academic research would be to organize a series of “topic du jour” talks for them. The objective is to find a burning topic that is making the headlines in the business press and get faculty to suggest nuances and ideas on the topic that go beyond the usual commentary seen in the financial press. To make this concrete, consider the substantial fall in the stock price of Apple partly because it stopped releasing information about the number of iPhones sold (<https://www.nytimes.com/2018/11/02/business/dealbook/apple-iphones.html>). The school assembles a panel of academics from different disciplines to comment on the story. The accounting professor could potentially argue that a revenue surprise, driven more by quantity of products sold rather than price, could signal more sustainable future earnings. Hence, Apple’s refusal to release quantity information might suggest lower earnings in the future quarters. The marketing/strategy professor could counter-argue that Apple intends to make most of its future revenue via services from the installed base of iPhones and the move need not necessarily be negative. The ultimate purpose is to showcase the application of the faculty’s expertise, gained from many years of research, to current problems and in the process extend students’ understanding of how the world of business works.

#### *5.22 Faculty presentations on select research issues to alumni*

Treating alumni and potential donors as intellectual assets, as opposed to mere financial assets, is likely to fetch higher dividends in the long run. In other words, the inherent emotional connection with the school that alumni have can be only be strengthened if they were to relate to and appreciate the research conducted by faculty on topics they care about. Moreover, the cost to individual faculty of discovering alumni who are interested in and potentially have a real-world use for their work is high. The school can fruitfully play the role of a market maker by connecting these



two parties. Hence, the idea is to have a set of faculty members present their research to a list of curated alums and related faculty who might be interested in the topic followed by drinks/dinner and conversation. These meetings need to be small and closed door in nature to ensure that interested alumni and faculty get to connect on topics of mutual interest.

### *5.23 Publicizing research findings*

We also need to work on disseminating our findings better to the practitioner audience. To cite a personal example, one of my papers, Graham, Harvey and Rajgopal (2005) was perhaps the first to show that a meaningful proportion of CFOs surveyed would sacrifice shareholder value increasing positive net present value projects to meet a quarterly earnings consensus estimate expected by Wall Street. However, McKinsey (2017)'s version of our survey question is cited far more often in the popular press and by influential commentators such as former Vice President Al Gore speaking at the Davos World Economic Forum. Unfortunately, academics can never match the marketing muscle of a McKinsey or a consulting firm. Even more regrettable, academics are often at the bottom of the totem pole after consultants, think tanks, trade associations and popular management books, when practice looks for advice! Is it because we are trained to write in a stilted or a formal way in academic papers lest we end up sounding like journalists?<sup>27</sup>

### *5.24 Versioning*

Another way to address the dissemination issue is to consider versioning the same findings for a different audience. Drawing from personal experience, Graham, Harvey and Rajgopal (2005), a paper that tries to understand why CFOs manage earnings or make voluntary disclosures, is published in the *Journal of Accounting and Economics (JAE)*. We wrote a practitioner version of that article for the *Financial Analyst Journal* in 2006. The academic version links findings back to the scholarly literature in the field on managers' motivations and relies on statistical tests to convince

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<sup>27</sup> The AAA has launched a pilot partnership with a company called Kudos to disseminate our work (see <http://aaahq.org/Research/Information-For-Scholars>). Authors write a plain-English summary and Kudos disseminates it through social media. I have been told that very few people have taken advantage of this service thus far.

the academic reader about the basis for inferences. The practitioner version on the other hand, relies on pictures, graphs and interview quotes of CFOs to enliven specific findings without emphasizing the statistical significance of those results. We successfully followed a similar strategy with our second survey paper, Dichev et al. (2013). The practitioner version of that paper is also published in the *Financial Analyst Journal* (2016). We have been told that the readership of the *Financial Analyst Journal* consists predominantly of buy and sell side analysts, hedge fund managers, investment bankers and Chief Financial Officers. The objective is to reach an audience that we usually would not as it is unclear whether such practitioners subscribe to scholarly accounting journals or have the inclination to digest their content, even if they do subscribe.<sup>28</sup>

#### *5.25 Need for skilled translators*

Many academics hope that their scholarly work will be translated for use in policy and practice. One of the best translators in the private sector is Michael Mauboussin, an adjunct professor at the Columbia Business School and the ex-Head of Global Financial Strategies at Credit Suisse. Under Michael's leadership, Credit Suisse has published a series of books for clients that synthesize how academic research in finance and accounting can be used to make better investment decisions.<sup>29</sup> How do we create a pool of such skilled translators? Can the AAA set up a journal devoted to synthesis and translation of academic work?<sup>30</sup>

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<sup>28</sup> The "FAME Jagazine" at UCLA publishes accessible versions of academic articles (see <http://www.fame-jagazine.com/readers/index.html>).

<sup>29</sup> Here are a few of titles directed by Michael Mauboussin: (i) *Capital Allocation Outside the U.S.: Evidence, Analytical Methods, and Assessment Guidance*; (ii) *Disbursing Cash to Shareholders: Frequently Asked Questions about Buybacks and Dividends*; (iii) *Measuring the Moat: Assessing the Magnitude and Sustainability of Value Creation*; (iv) *Sharpening Your Forecasting Skills: Foresight Is a Measurable Skill That You Can Cultivate*; (v) *What Does a Price-Earnings Multiple Mean? An Analytical Bridge between P/Es and Solid Economics*; (vi) *What Makes for a Useful Statistic? Not All Numbers Are Created Equal*; (vii) *Total Addressable Market: Methods to Estimate a Company's Potential Sales*; and (viii) *Thirty Years: Reflections on the Ten Attributes of Great Investors*.

<sup>30</sup> *Accounting Horizons*, in my view, does not appear to play this role as of now. The *Accounting Review* could require two versions of each paper – the full blow academic tome and a Reader's Digest version aimed at practitioners. In the accounting area of Management Science, Suraj Srinivasan and I ask authors to voluntarily summarize the implications of their accepted papers for policy or practice at the end of the paper.

As a potential role model in the classroom context, consider the “Science to Practice” initiative launched by the Marketing Science journal. That initiative creates translations of articles to classroom slides (<https://pubsonline.informs.org/page/mksc/science-to-practice>). The slides are accompanied by a short teaching note that covers: (i) the types of courses/classes where the topic is relevant; (ii) the basic idea being conveyed to students; and (iii) a teaching strategy or technique that will transmit or explain the idea to an audience. Marketing Science has also launched an Ambassador program, where scholarly practitioners interview authors of articles and write about them on social media (<https://twitter.com/MarketngScience>).

### ***5.3 Modify the publication process***

In this sub-section, I advocate greater involvement of practitioners in the academic publication process.<sup>31</sup>

#### *5.31 Practitioner referees*

During my time as the editor of the accounting track of Management Science, I have used a practitioner referee twice. Their involvement added a lot of value to me, the authors and the paper. I asked the academic referee to specifically focus on the research design and the contribution to the academic literature. I asked the practitioner referee to comment on (i) whether the problem addressed by the authors was one of first order importance in the real world; (ii) whether the data used in the paper was reliable? and (iii) whether the findings were believable? I was able to access practitioner referees partly because New York City has a large talent pool of PhDs who work in the private sector in virtually every area of finance. I realize that editors may have more difficulty in finding practitioner referees. I advocate their use on select questions and not on all papers. Moreover, journals could consider setting up a separate editorial board of senior practitioners who are willing to engage with academic research and are willing to commit to say three reviews a year.

#### *5.32 Practitioner coauthors*

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<sup>31</sup> Mohan Venkatachalam points out that Rex Bates, a partner at Stein Roe and Farnham, Chicago, discussed Beaver (1968) in the Journal of Accounting Research. Somehow that tradition has dissipated in recent times.

I have collaborated with practitioner co-authors on writing op-eds for business journals and one research project. Practitioners have their ears to the ground and are quick to appreciate and surface emerging problems in the world of business or regulation. Practitioners are also skilled at obtaining funding for a project from professional organizations and trade associations such as the Financial Executives Institute (FEI). They can also convince businesses to share proprietary data for us to analyze. Academics can add value by (i) structuring the broad question into a testable hypothesis; (ii) by designing rigorous tests; and (iii) by bringing scientific skepticism to claims on both sides of a policy or a practical debate.

However, practitioners are interested in quick resolution to the debate and do not want to wait for three or four years for the academic piece to be published.<sup>32</sup> One compromise to engage practitioners is to co-author an applied version of the scholarly working paper for a practitioner outlet. But academic journals and editors frown on such a practice claiming that the authors are “double dipping” or are trying to publish the same content twice, once in the academic journal and the other in the practitioner journal. Accounting journals might want to revisit their policies to at least allow practitioner-academic teams to publish the key conclusions from a research project, without a detailed discussion of the underlying data, research methods and the robustness tests, in a practitioner outlet.

#### ***5.4 In the Classroom***

Integrating theory and practice merely in research would be inadequate. If the current drift continues, I fear for the ability and the legitimacy of our faculty to train future managers in undergraduate and MBA programs. I list a few classroom related initiatives to apply theoretical insights to real world problems in a classroom: (i) Hacking for X series; (ii) mandatory case writing; (iii) Stanford’s co-teaching with practitioner model; and (iv) the role of executive education.

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<sup>32</sup> Brain Bushee, the co-editor of the accounting track of Management Science suggests that the “Fast Track” process of Management Science could potentially alleviate this problem. Two of the criteria for Fast Track submissions are: (i) “the paper will have immediate applicability to some managerial or societal problems”; and (ii) “the paper will be appealing to a broad audience such as The Economist, Wall Street Journal or the Financial Times.”

#### *5.41 Hacking for X series*

Hacking for Defense (H4D) is an academic course at Columbia that tasks students with real problems that have been extracted from the Department of Defense, intelligence agencies, and the U.S. government at large (<https://entrepreneurship.columbia.edu/resources/h4d/>). The semester before the class the offered, the instructor contacts the DoD for a specific defined problem the DoD needs a solution to. The DoD commits a senior executive as their sponsor or the point of contact. The sponsor commits to spending considerable time for several weeks with the student team. Next, the instructor publishes the list of problems and asks students interested in taking the class to identify the problems they want to work on. The instructor interviews students before letting them register for the class to ensure that that he/she is serious about the course and has the ability and willingness to work on these problems. Selected students are grouped into teams based on the projects they are interested in. Once the semester starts, student teams meet with their sponsor weekly, research their problems and present their journey towards a solution to the class and instructor every week. A project report is due at the end of the semester.

Several projects have successfully morphed into entrepreneurial ventures. Many students have been hired by the sponsor. Unlike a business school case, where the question is known but the answers are not, students learn to ask the right questions associated with solving the problem posed by the sponsor during the course of the semester. The same model can be applied to most areas in business, including accounting (“Hacking for X”). The instructor could potentially contact a few analysts at investment banks or hedge funds to pose problems related to fundamental analysis or valuation that curated student teams can work on.

#### *5.42 Mandatory case writing*

The business school could consider mandating a rule that requires every faculty member to write at least one case. Apart from the enhanced credibility that the instructor enjoys from teaching her own case in the class, cases and the associated field work that goes into such an exercise forces every faculty member to engage with real world problems. While some may never write another

case, my conjecture is that a sizeable minority will find real world problems stimulating enough to write more cases and perhaps even change the research questions they ask.

#### *5.43 Stanford's co-teaching with a practitioner model*

50% of the electives at Stanford Business School are co-taught with practitioners (<https://poetsandquants.com/2018/12/10/our-interview-with-stanford-gsb-dean-jonathan-levin/?pq-category=business-school-news>). Dean Levin of Stanford Business School states, "It (co-teaching) integrates the academic mission and the practical application...you see the interplay between someone who can bring in academic frameworks and thinking and someone who just knows what the reality is. David Kreps, one of our faculty members, used to describe that model by saying it generates friction, but it is friction that generates light."

Co-teaching models are expensive for most schools who do not have Stanford's resources. For instance, should the faculty member co-teaching with a practitioner get full credit for the class? If not, why would that faculty member volunteer to teach the class? But assuming the model is financially viable or if deans can convince skilled practitioners to co-teach for a nominal honorarium, the benefits are obvious. Most faculty would benefit from an education from the practitioner about the latest technological transformations such as Artificial Intelligence (AI), machine learning and robotics that are poised to change every area of business. The added benefit of a constant challenge to academic frameworks over a semester by a star practitioner is bound to stimulate faculty interest in applied problems.

#### *5.44 Executive education as a role model*

Executive education can serve as a vehicle to focus faculty minds on applied problems. I know of a few schools who encourage faculty to write a book on an applied problem before putting them in front of an executive. Faculty at those schools have confided in me that an applied book, as opposed to several articles in top journals, was more likely to buy them credibility with the executives in their classrooms. Otherwise, academics who have published many scholarly articles are seen as removed or too distant from the action in the trenches. Executives are a demanding audience and

unlike daytime MBA students, are likely to quickly challenge material that is not descriptive or reflective of real-world problems that they grapple with on a day to day basis. Executive Education does not have to satisfy AACSB accreditation criteria based on discipline-oriented tracks. Moreover, the clear financial incentives associated with executive education make it easier for deans and administrators to assemble faculty teams drawn from different functional areas. Getting these functional teams to co-author a research paper, on the other hand, is a non-trivial task because scholarly journals are excessively specialized (except perhaps for Management Science).

## **6.0 How can INFORMS and Management Science contribute?**

### *6.1 The big tent approach adopted by INFORMS*

Colleagues have suggested that Industrial Engineering (IE) and Operations Research (OR) are fields that really walk the talk of integrating practice with research. First, OR journals publish more applied work (Manufacturing and Service Operations Management, Production and Operations Management). Simultaneously, several OR journals publish basic theoretical work (e.g., Operations Research). Second, engineering and business school IE/OR colleagues suggest that applied work in OR is not looked down upon or considered “less than” equal, unlike many parts of the business school. Third, the diversity of objectives in the various prizes INFORMS gives out reflects the importance given to all kinds of research, be it basic or applied (<https://www.informs.org/ORMS-Today/Public-Articles/April-Volume-45-Number-2/INFORMS-awards-prizes>).

It is heartening to read the following introduction to INFORMS prizes: “what is great operations research, management science and analytics (OR/MS/A)? Is it resolution of a longstanding mathematical question? Is it a MOOC (massive open online course) that attracts thousands of students? Is it an algorithm that saves a company millions of dollars? Is it a series of practical contributions that address important social problems? Great OR/MS/A can be any of these ... and many other things besides. In order to recognize achievements like these, INFORMS has created 20 professional awards.”

Consider a few: (i) “The Impact Prize was established to honor contributions in research or practice that have had broad impact on the profession, either through widespread adoption of original research or widespread dissemination in practice through methods such as consulting or software development.”; (ii) “The Franz Edelman Award is named in honor of Franz Edelman, vice president for Business Systems and Analysis at RCA, who established the Operations Research Group there as one of the earliest industrial OR/MS/A groups in North America. The award recognizes outstanding implementations of OR/MS/A, and it is widely viewed as the premiere award for the practice of OR/MS/A;” (iii) “The INFORMS prize recognizes effective integration of OR/MS/A into organizational decision-making. The principles of OR/MS/A should be applied in novel, varied and lasting ways that makes a significant contribution to the success of an organization. The prize was most recently awarded in 2017 to both the U.S. Air Force and to the Walt Disney Company;” (iv) “Daniel H. Wagner Prize - Daniel H. Wagner established himself as a leading consultant in mathematical approaches to naval tactics. The Wagner Prize recognizes the author or authors of a paper that exemplifies successful, real-world application of innovative OR/MS/A methodology and clarity in exposition;” (v) “President’s Award recognizes important contributions to the welfare of society at the global, national or local level by members of the OR/MS/A profession. The prize was most recently awarded in 2017 to David Hunt (Oliver Wyman) for his influential work implementing operations research solutions in the transportation industry and for his work to create the new Pro Bono Analytics service in INFORMS.” Strikingly, there is only one award for theoretical contributions, the John von Neumann Prize for making “fundamental, sustained contributions to theory.”

## 6.2. A “theory meets practice” section in Management Science

Building on the rich and diverse intellectual foundation laid by INFORMS, I propose that Management Science create a “theory meets practice” forum within the regular journal with the objective of publishing rigorous research that deals exclusively with applied policy or practical issues. The forum could create an editorial board comprised of top scholars, practitioners and policy



makers. Every three years, the forum could publish a list of research priorities in a field. Submissions to the journal would be reviewed by (i) a practitioner referee, who judges the paper for relevance and importance to an applied area or policy problem; and (ii) by an academic, who reviews the paper for rigor related to data, methods, design or the validity of the modelling approach to the problem. Similar to the structure of the journal, “Health Affairs,” the forum could encourage articles in three categories: (i) standard journal articles, that addresses practical or policy issues that rely on quantitative, qualitative or case-based work; (ii) a descriptive data section that highlights important new data on practical and policy matters; and (iii) commentaries that address important, current challenges in practice or policy by providing constructive ways to tackle long standing problems.

Management Science (MS) is seen as an “A” journal in most top schools. Hence, MS has the legitimacy to attract young scholars with an inclination towards applied problems to contribute to such a forum. A few initial submissions could even be invited from an author team consisting of practitioners and academics.

## **7.0 Conclusions**

Can we ever reduce the gap between accounting research and practice? I hold a deep-rooted belief that applying rigorous academic methods to solve applied problems in policy and practice is possible and mutually beneficial for the academy and for practitioners. However, careful thought needs to be given to the mechanisms that might enable better integration between the two worlds. A long run response is to encourage faculty to raise soft money via grants from the private sector or the government to solve applied problems faced by sponsoring institutions much like the design sciences such as engineering and medicine. Such a move will reduce the extent of the cross-subsidization of research by tuition. In the short run, the profession might want to seriously investigate ways and means to (i) publish and celebrate research of use to policy and practice; and (ii) to get faculty closer to the trenches in the real world where business decisions are taken. Management Science is perhaps the only inter-disciplinary top tier journal that spans most functional disciplines in a business school.

MS can begin to lead us to the ultimate objective of creating a porous intellectual boundary between theory and practice.

Can we direct our intellectual rigor towards questions relevant to the applied world? Otherwise, accounting, as a discipline, risks its very existence as an autonomous area in the business school. Ultimately, the business school draws legitimacy from published academic research to grant degrees to students. Failing to address the widening gap between practice and our journals will steadily damage such legitimacy. Stakeholders will eventually ask why they cannot get an MBA from a McKinsey or an Apple University.

When will we reach the tipping point where tuition based funding of research dries up? I am not sure. Perhaps it is already here in some schools. A colleague tells me that the economics department of a top tier state school, which is not affiliated with the business school, was asked to post a bond of \$5 million with the university to cover the annual cost of the newly added faculty member. The rest of us would be better equipped to deal with such an eventuality if we acted sooner rather than later.

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**Table 1: Why has Accounting Research Strayed from Practice and Policy?**

Section #	Issue	Summary
3.1	Human capital entering the profession	<ul style="list-style-type: none"> <li>• New PhDs and faculty are more interested in methods and corporate finance/economics questions</li> </ul>
3.2	Decline in managerial accounting research	<ul style="list-style-type: none"> <li>• Managerial accounting research is arguably more relevant to practice but is harder to publish in top journals</li> </ul>
3.3	Pre-occupation with cross sectional regressions	<ul style="list-style-type: none"> <li>• Practitioners don't care about the "average" firm as much as researchers</li> </ul>
3.4	"Fill the hole" type work	<ul style="list-style-type: none"> <li>• Top journals publish approx. 150 articles a year but very little of that work has practical or policy impact</li> </ul>
3.5	Lack of professional incentives	<ul style="list-style-type: none"> <li>• Focus on applied work is seen as not scholarly enough by the profession and hence penalized</li> </ul>
3.6	Normative research	<ul style="list-style-type: none"> <li>• Accounting hesitates to work on normative questions and hence has less impact on policy</li> </ul>
3.7	Descriptive work	<ul style="list-style-type: none"> <li>• Descriptive work in the Coasean tradition is undervalued in accounting</li> </ul>
3.8	Working in research teams	<ul style="list-style-type: none"> <li>• Reluctance to work in large teams, unlike the sciences, to tackle big questions</li> </ul>
3.9	This is what consultants do	<ul style="list-style-type: none"> <li>• Dismissing applied work as consulting mischaracterizes research at a business school</li> </ul>
3.10	More on applied research v/s consulting	<ul style="list-style-type: none"> <li>• Engineering hands off the prototype created by university R&amp;D to corporate R&amp;D for production. Corporate R&amp;D might then hire engineering academics as consultants to hasten production. How does business school R&amp;D interact with corporate consulting and in house discovery of management processes in firms?</li> </ul>

**Table 2: Proposed Mechanisms to Address the Practice-Research Divide**

Section #	Mechanism	Proposal
		<ul style="list-style-type: none"> <li>• <i>Mechanisms to get research faculty closer to the trenches</i></li> </ul>
5.1.1	Industry sabbaticals/ leave	<ul style="list-style-type: none"> <li>• To get the professor to spend the sabbatical year with a company in an area convergent with the professor’s research interests</li> </ul>
5.1.2	Problem exchange seminars	<ul style="list-style-type: none"> <li>• Bring alumni from practice to share a couple of questions they had trouble addressing during the course of their careers</li> </ul>
5.1.3	Venture accelerators	<ul style="list-style-type: none"> <li>• The B school invests in faculty ventures that sell products to industry</li> </ul>
5.1.4	Professor-in-residence programs	<ul style="list-style-type: none"> <li>• To place the professor for a day a week into a company that is convergent with the professor’s research interests</li> </ul>
5.1.5	Faculty lightning talks for expert practitioners	<ul style="list-style-type: none"> <li>• To assess the real-world relevance of the question and obtain data or other support from alums</li> </ul>
5.1.6	Alumni mentors for junior faculty	<ul style="list-style-type: none"> <li>• Link an assistant professor with a senior alumni mentor from industry</li> </ul>
5.1.7	Pin factory trips	<ul style="list-style-type: none"> <li>• Half a day immersion trips for faculty to local companies</li> </ul>
5.1.8	Regulators can publish research questions	<ul style="list-style-type: none"> <li>• To connect regulators with unresolved policy questions with faculty looking for interesting questions to work on</li> </ul>
5.1.9	Marketing Science Institute model	<ul style="list-style-type: none"> <li>• Get industry and academe to agree on a research agenda and provide funds and support to execute that agenda</li> </ul>
5.2.0	InnoCentive.com	<ul style="list-style-type: none"> <li>• To set up a platform to connect faculty with practical problems posted by businesses</li> </ul>
5.2.1	Coalition of the willing for field studies	<ul style="list-style-type: none"> <li>• The School identifies a list of companies willing to support randomized control trials (RCTs) by faculty</li> </ul>
		<ul style="list-style-type: none"> <li>• <i>Mechanisms to better disseminate our findings</i></li> </ul>
5.21	Topic du jour talks for students and alumni	<ul style="list-style-type: none"> <li>• Faculty with relevant research and expertise discuss a burning topic in the business press</li> </ul>
5.22	Faculty presentations to alumni	<ul style="list-style-type: none"> <li>• Connect faculty with donors to converse over drinks and dinner on specific themes</li> </ul>
5.23	Publicizing research findings	<ul style="list-style-type: none"> <li>• Via op-eds or press articles</li> </ul>
5.24	Versioning	<ul style="list-style-type: none"> <li>• Writing a jargon-free version of the paper for practitioner journals</li> </ul>
5.25	Need for skilled translators	<ul style="list-style-type: none"> <li>• The AAA potentially gets articles translated to classroom slides or practitioner notes</li> </ul>



**Table 2: Proposed Mechanisms to Address the Practice-Research Divide (cont'd)**

Section #	Mechanism	Proposal
		<ul style="list-style-type: none"> <li>• <i>Modify the publication process</i></li> </ul>
5.31	Practitioner referees	<ul style="list-style-type: none"> <li>• To assess whether the topic is a first-order concern in the real world and whether the results are believable</li> </ul>
5.32	Practitioner coauthors	<ul style="list-style-type: none"> <li>• To discover emerging problems in practice</li> </ul>
		<ul style="list-style-type: none"> <li>• <i>In the classroom</i></li> </ul>
5.41	Hacking for X classes	<ul style="list-style-type: none"> <li>• Students solve applied problems brought by businesses</li> </ul>
5.42	Mandatory case writing	<ul style="list-style-type: none"> <li>• To force faculty to engage with an applied problem in a company at least once a year</li> </ul>
5.43	Co-teaching with practitioners	<ul style="list-style-type: none"> <li>• To get academe and practice to meet in the classroom</li> </ul>
5.44	Executive education	<ul style="list-style-type: none"> <li>• To create incentives for faculty to solve applied problems of use to executives</li> </ul>