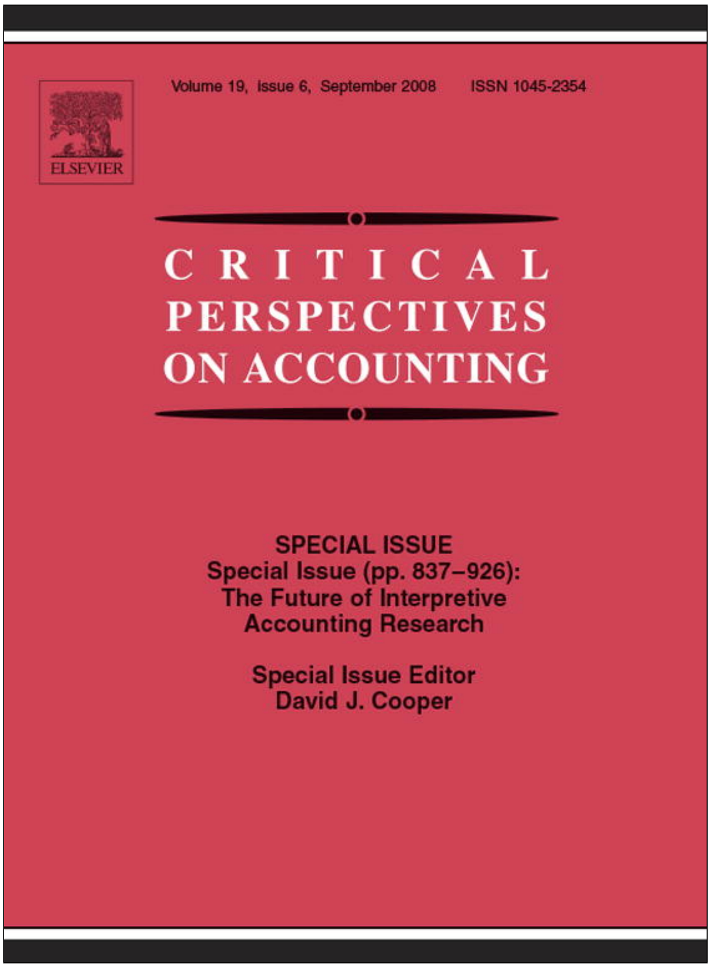
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Why interdisciplinary accounting research tends not to impact most North American academic accountants

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Abstract

Interdisciplinary accounting research (IAR) has not impacted the North American academic accounting research community to any material extent. This commentary is intended to provide a North American perspective on why this situation exists and what might be done to change it. The commentary explains that in the eyes of so-called "mainstream" researchers IAR research appears to suffer from one or more of three main problems—lack of relevance, questionable research contributions, and/or poor communication of findings. It is difficult to discern the contributions of even the best IAR research. The commentary concludes by providing suggestions for making the contributions of IAR research more easily recognizable by mainstream researchers.

1. Purpose of this commentary

Currently, in the United States, accounting research using economics-based paradigms, theories, and jargon and using either analytical research methods or analysis of large samples of archival ("objective") data rules the roost. Researchers who do not follow those mainstream rules find it very difficult to get their work published in the top-ranked U.S. journals. And U.S. business/management schools, particularly the more highly regarded ones, are increasingly interested both in having their faculty publish mostly, or even exclu-

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sively, in the top-ranked journals and in generating SSCI citations. Little else counts. Many of the contributors to the IAR debate have observed this reality, and they are concerned. They seem as instrumental as any U.S. mainstream researcher!

Increasingly I conduct research that can be described as "mainstream," and I don't apologize for doing so. But I retain sympathy and appreciation for IAR research. I too want to learn how accounting practices function in the field; I believe that non economics-based concepts, theories, and paradigms can contribute substantively to our understanding; I value the learning that can be gained from small sample field studies and qualitative research; and while I am interested in average relationships, such as represented by regression coefficients, I am sometimes even more interested in learning much more about the empirical "outliers." I am currently engaged in two research projects that most would classify as "interventionist research," which violates a mainstream research principle requiring researcher independence and objectivity. And like some IAR researchers, I have gone into the field to study one topic only to find something much more interesting, which causes me to totally change research directions. I also feel some of the same frustration that IAR researchers express when they say that their work is not being recognized and appreciated by business school deans, journal reviewers, and many U.S. colleagues.

Still, while I am not totally in the U.S. mainstream, perhaps I can sympathetically but critically comment on the state of IAR, and in particular, its lack of impact in the United States and those countries that tend to follow U.S. norms. I am willing to read anything that I can learn from. And I am interested in many of the IAR areas of focus, which I understand to be, particularly, "accounting in action" and social, political, cultural, and ethical understandings of accounting. Thus, if IAR, or certain aspects of it, does not appeal to me, it is highly unlikely that it will appeal to the far-right zealots in the U.S. accounting research mainstream.

Like the typical American accounting researcher, though, I am not a sophisticated consumer of IAR writings. With maybe only a handful of exceptions, leading U.S. doctoral programs include no study of IAR. They do not require or even encourage students to read anything by, for example, Habermas, Foucault, Giddens, Latour, or (heaven forbid) Marx, and they provide no training in interpretive research methods. Education even in the related area of "field research methods," which occasionally appeals to mainstream management researchers, is quite rare in U.S. accounting doctoral programs. While I am not even sure what the term IAR encompasses, I do occasionally look at papers that perhaps most everyone would label as being examples of IAR. It is on the basis of my reading of these papers and conversations with IAR-oriented colleagues that provides the basis for this commentary on the polyphonic debate.

2. Is there a problem?

While I felt some of the pain being expressed in the IAR commentaries, and I agreed with some of the arguments and approaches being suggested, much in the commentaries struck me as excessively introspective, almost defensive whining. This conclusion is shaped by my observation that I personally have not learned much from the IAR literature. I know there must be something in there. I know many IAR authors personally, and I know them to be smart, learned people and serious academics. But something is missing for me. Why is that? And what might be done about this problem, if it is indeed a problem? Answering those questions is the main purpose of this commentary.

My first reaction is that *from my perspective* I don't see a problem. It is not important to me that IAR be seen as important. I have nothing at stake. I don't engage in such research, and I don't keep up with developments in that field. There are lots of literatures that I do not keep up with. Even though I am a management control and performance measurement researcher, I don't keep up with the cybernetic control literature or the social psychology literature, to name two examples. With scarce time all of us must filter and focus. IAR writings rarely make it through my filtering screen. Too bad for me if there is something important in this literature, but I have never been persuaded that I might be missing something.

I accept some of the blame. My training is not broad enough, and I have not taken any steps to try to become more well-read in this direction. To a large extent, I have invested the time I have available to expand my horizons to become more conversant with the mainstream accounting literature. Understanding the mainstream literature (e.g., agency theory) has not only improved my research, it has also increased my chances of having my research accepted by the mainstream, which my university suggests to me is in my own personal best interest.

But IAR researchers must also accept some of the blame for what is, indeed, a problem from their perspective. To have their contributions recognized, they have to be able to make their case that their research is important to potentially interested people in related fields, like me, and they have not done so.

3. Causes of the problem

I have identified three factors that contribute to the lack of IAR research impact on outsiders like me. First, some of the IAR research seems to be lacking in relevance, at least how I define relevance. Second, some of the IAR research, even when relevant, seems to make little or no contribution. Third, the findings of some of the IAR research, even when the research is well done, are not well communicated. It seems particularly inaccessible to outsiders. Let me explain.

3.1. Lack of relevance

My definition of relevance seems to be different from that of at least some of the IAR researchers in the debate. Some IAR researchers conclude that their studies are obviously relevant because they are studying actual practice. But studying actual practice does not necessarily mean a study is relevant to me.

I am a professor in a business school, a professional school which, by my definition, is highly applied. As an individual, I am curious about a lot of things; I am not just career oriented. I'd like to know where the end of the universe is and what's beyond it. I'd like to know what causes autism, and what we might do to prevent the problem. And so on. But when I am in my business school professor role, and particularly when time is short and it's time to prepare to teach my next class or to design my next research study, my scope of curiosity narrows. I want something that I can use. That is relevance to me.

Thus most of my curiosity is focused on things that have applicability. I want to learn how the accounting-related world works, what causes it to work in the ways it does and, importantly, how it can be made better. I teach theory and practice, and I define theory to include all "currently useful generalizations." My personal taste is for knowledge that I can use in the short-term. I want to read books and papers that give me something that I can use to improve my teaching in tomorrow's and next week's classes and my next couple of research projects. Clearly some academics, even those in professional schools, can and should take a longer view and do research that might pay off decades in the future, but I do not read much of that research.

Fortunately, knowledge that is useful to me comes in many forms. I value new ways of looking at the world (e.g., frameworks), theories, surveys of practice, and examples that can be used to illustrate important principles. Good IAR research should be able to provide these kinds of relevant findings, but not all IAR research does.

3.2. Little or no research contributions

My impression is that many IAR papers, even ones that are focused on seemingly relevant topics, make little or no clear contribution. At the very least, I cannot see it. For me to conclude that a study has made a contribution, I want to know how the study has advanced our understanding. To help me understand the contribution, I want the papers' authors to explain what we knew before the study was conducted and what we know after. The difference, if there is one, is the contribution of that study.

Only in rare cases do I believe a statement in a paper's introduction to the effect that "Little is known about . . ." Even emerging practices, for example, seemingly a favorite topic of IAR researchers, emerged from somewhere, and their emergence is probably not very different from other practices that emerged somewhere at an earlier time, and were studied. Every researcher-asked question and every observed behavior reflects some expectation that the researchers had in their heads. I want to know if the researchers found what they expected. Without a connection to prior knowledge, a mass of new details does not provide a contribution. Papers need an anchor in order for the authors to be able to claim a finding as surprising.

IAR researchers need to be willing to make generalizations. I accept that one strength of IAR is that it "fits the very situation of the organization." That's good. But we won't advance much if we conclude that every situation is unique. The tangible output of exploration is new frameworks and theories. We also need some replication and refinement. For the frameworks and theories to be accepted, they must be tested in the same, as well as in different, settings. As the findings probably do not apply universally, their "boundary conditions" and situational contingencies must be explained.

Too high a proportion of the IAR papers that I have seen are labeled as exploratory. Then there is too much "reinvention of the wheel" and not enough generalization.

Too high a proportion of the IAR papers that I have seen seem just to provide merely one more example of theory that is already known. They conclude merely that Habermas or Foucault (or whomever) was correct.

Too high a proportion of the IAR papers that I have seen claim that one of their contributions is in providing a “thorough” investigation of the field. Nothing is thorough. All researchers must choose scope limitations and foci. Those should be made clear.

Too high a proportion of the IAR papers that I have seen claim that one of their contributions is in providing rich, detailed description. Only in rare cases have I found that to be a strength of the papers. Most of the time I find it tedious to read rich description, even in areas that are central to my research and teaching interests. In the budgetary sphere, for example, I know that notions of “top-down versus bottom-up budgeting,” or high or low budgetary participation, are simplistic. But as general descriptors of budget formulation processes, I have not found much better. In short, I don’t want to read just rich description. Authors need to boil all that complexity down into simpler statements of theory that have some general applicability. I need parsimony, not more complexity. If a better understanding of budgeting could come by seeing it through the lens of a “justice” or “structuration” model, just to pick two possibilities, then I would be interested. But I need to have that understanding explained to me in terms that I can comprehend.

Accounting may, indeed, serve thousands of purposes other than those of decision facilitation and influencing. By now that is a well established and agreed upon “fact.” I teach cases to my MBA students showing, for example, that managers are using purposely distorted cost accounting allocations to manipulate sales peoples’ behaviors (e.g., to ensure that they will not give away profit margins) or overly complex systems to maintain a power base (e.g., by being the only person who really understands what is going on). But these are generally only interesting sidelights, noise in the greater milieu, so to speak. I teach cases describing these atypical situations to expand my students’ horizons, to teach them how to think through the complexity of real situations and how to deal with unusual situations that they may encounter at some point in their careers. But the focus of my teaching, and research is on what my many years of observing practice suggest to me are the most common, core purposes and uses of accounting. Perhaps IAR researchers are focusing mostly on the exceptions. How many of the potentially thousands of uses of accounting are important? Where should be spending our limited research resources?

3.3. Poor communication

Many IAR papers seem to fail to communicate to audiences that are not IAR insiders. I think there are three root causes of the communication failures: unusual paper organization, a celebration of ambiguity, and an excessive use of jargon.

To mainstream researchers, the organization of many IAR papers hinders communication. With but rare exceptions, all mainstream accounting research papers use the same outline. They start with a motivation for the study. They then include sections describing theory development/literature review, research methods, findings, and conclusions. That consistent method of organization makes it easy to read the papers. The sections fit together tightly, and readers know exactly where to find what they are looking for.

Most IAR papers seem to be organized differently, and their organization is inconsistent. I’ve seen the explanations from IAR researchers, such as to the effect that standard modes of paper organization are suitable only for those using “unreflexive theories.” But I am not convinced by that argument. Remember that I am a field research practitioner, even one who has engaged in "exploratory" field research. I still believe that the best way for me to communicate my field research contributions is to use a traditional paper outline. I explain up front why my topic is important to study. Then I explain what I expected to find when I went into the field and what I found. If I found only what I expected, I have only a replication, which is not very interesting. But, fortunately, I usually find something novel, and maybe even surprising, that allows me to claim that I am contributing something substantive to the field. If some findings and, hence, investigations, were totally unexpected, then I explain them in free form in a latter section of the paper, which is titled something like "Further Explorations." Any material deviation from this paper-writing outline makes it harder for mainstream researchers, at least, to follow the logic and to understand the contributions.

A second problem causing communication difficulties is that many IAR researchers seem to value ambiguity. Ambiguity is not a desirable quality of research papers. We need precise understandings of the phenomena observed in at least an important subset of settings. If an IAR paper claims that there are complex interacting forces at work, I want to know precisely what those forces are, what causes them, what affects their incidence and effects, and eventually how we might go about measuring all of these concepts so that we can take the next research step—empirical testing of the theory. If all that is not known at the end of a study, the researchers can provide a list of limitations. But this lack of knowledge is a limitation, not a strength, of the IAR approach. Researchers can muck around in the detail for some time, but at some point they need to decide, and focus on, what is important.

A third problem causing communication difficulties is IAR papers' tendency to make excessive use of unnecessary (?) jargon. Researchers in all fields communicate through their own preferred vocabulary, which is referred to, often disparagingly, as "jargon." I accept that jargon facilitates communication with those who are part of the field. It provides an efficient, shorthand way to communicate precise meanings concisely. But it hinders communication with people outside the field. In order to be able to communicate with researchers in the U.S. mainstream, I have had to become comfortable with terms such as principal, agent, decision rights, moral hazard, adverse selection, information asymmetry, and informativeness. But I believe that is necessary if I want my arguments to be widely understood.

Although some IAR insiders might not even perceive it, IAR writings tend to be heavily laden with technical jargon that mainstream accounting researchers are not familiar with. In the literatures that I read on a regular basis, I rarely if ever encounter words such as ontology, epistemology, positivism, postmodernism, constructionism (or deconstructionism), ethnomethodology, dialogical endeavour, re-contextualization, polycentrism, nomothetic, reflexivity, idiographic theory, or even interpretive research. Using these words probably allows IAR insiders to communicate effectively and efficiently with one another, and it certainly makes them appear learned. But it doesn't do much to communicate to me and, presumably, to other outsiders to this literature. The jargon makes the IAR papers difficult for outsiders to read. Reading these papers requires considerable effort, which few people are willing to exert. And even if they were willing to expend considerable effort, they would undoubtedly miss a significant proportion of the content. I will invest the time needed to learn the IAR jargon if I perceive it to be worthwhile either in improving my understanding of the phenomena in which I am interested or in increasing my chances of getting my research accepted by the highly ranked journals. But so far I have not seen the value.

4. Conclusion

My advice to IAR researchers is simple. If you have something to offer, and my guess is that you do, show us (and now I am speaking as if I were a mainstream, positivist researcher). Tidy up your best findings and communicate them to mainstream researchers in ways we can understand. I don't get the sense that many of you have ever tried. If you can debunk some common wisdom, everybody will stand up and take notice.

For example, I am intrigued by the idea that there is a difference between managers' espoused and in-use theories. I have heard this argument before, and I believe it to be true. But I have no good examples in my head. Hit me over the head with one. More generally, give me some packaged teaching modules, such as real world teaching cases supported by teaching notes written in terms that I can understand, that would allow me to introduce the most important IAR findings to my students.

I have no doubt that the "rigorous" adherence to theory blinds mainstream (positivist) researchers to important issues. But IAR researchers have to admit that progress is being made by positivist researchers. The positivists' theories are adapting, which suggests that data, and yes even direct observations of the real world, are being used to inform the theories. If IAR can help accelerate that progress, explain how.

The positivists' research findings should also be incorporated into the IAR research agenda. Mainstream and IAR researchers should not be enemies. They have the same goal, to create knowledge, and on many of the same topics. There is considerable overlap between the literatures, and these literatures should be informing each other. You should not ignore the positivists' writings. An interdisciplinary, multi-method approach to research should lead to new insights.

Unfortunately, the primary burden of connection is on groups like IAR researchers who are less powerful because they do not control, and indeed are often not even represented on, the editorial boards of the journals generally perceived to be of the highest quality. So IAR researchers must actively reach out to researchers outside of IAR. My impression is that this is rarely done and, in fact, IAR researchers sometimes push outsiders away. For example, some years ago I submitted a paper to an "interdisciplinary" accounting conference. The call for papers mentioned a number of topics (e.g., budgeting, management control) that looked interesting to me. The paper I submitted seemed relevant to me. But I got a cordial reply from one of the conference organizers suggesting that this was not the right conference for me to attend, that I would not feel comfortable there. That strikes me as a very destructive attitude. IAR researchers should be trying to communicate with mainstream researchers, not pushing them away. It is unfortunate that the burden of outreach is on the less powerful groups, like IAR, that do not control the schools, departments and journals. But much in life is not fair. The situation is the way it is, and it must be dealt with.

There are multiple avenues that IAR researchers can use to try to communicate with mainstream researchers. Go out of your way to cite works in the mainstream literature and try to link your findings to theirs either in a complementary or alternative sense. Write some papers that use "mainstream" and IAR methods in parallel. How would the mainstream researchers have viewed the phenomena, and what would they have concluded? How about the IAR researchers? Why is the IAR approach superior, or at least what does it add? Is what it adds important? Maybe some IAR topics cannot be easily related to mainstream research, but some clearly can. Pick the easiest ones first. Or organize an IAR conference aimed specifically at mainstream researchers. But conferences with concurrent sessions allowing the researchers with different traditions to segregate themselves will not work. The conference must force the communities together. It is hard work to try to communicate to multiple audiences, and it is just as hard work to try to understand a presentation being given in a "different language." But if IAR research is to be recognized in mainstream circles, this communication gap must be closed.

I think that these are doable tasks. I think that the best minds in the IAR community can do them. It is unfortunate that the burden of outreach falls mainly, if not exclusively, on the power-challenged IAR community. But if done successfully, I think that the payoffs, both to IAR and mainstream researchers, could be significant.