Avoiding self-deception in the study of academic accounting: A commentary about and beyond Endenich and Trapp’s article

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Abstract

The differences shown in the Endenich and Trapp article help us formalize the cartography of publishing opportunities in accounting. In this commentary, I offer some ways that the paper could have offered more insight. I also propose a broader agenda for the sociological study of the social organization of academic accounting. For this purpose, elements of elite reproduction, the rhetoric of quality and the politics of journal production are proposed as core elements.

I am encouraged by the decision of the editors to Critical Perspectives on Accounting to publish the paper entitled “Signaling Effects of Scholarly Profiles – The Editorial Teams of North American Accounting Association Journals” (Endenich & Trapp, 2018). The examination of the accounting profession should include its academic wing, even if a major schism exists between it and practitioners. My commentary on the paper concerns several dimensions, but accepts as a given the general importance of work exploring this area. I subdivide these remarks into two categories – the micro and the macro. The former questions the evidence produced by the paper as an academic exercise. The latter seeks to reflect on the broader purpose or contribution of work in the area.

1. What do we learn

Stripped to its essence, the paper tells us that the credentials of editorial boards can be taken as a signal to authors about the value placed on certain types of research. As such, the paper makes it clear that Contemporary Accounting Research (CAR) presents a more open, eclectic opportunity set than does The Accounting Review (TAR). The paper also offers evidence that a tighter match exists between the research methods favored by editorial board members and what their journals publish, than between the subject areas favored by editorial personnel and their journals. The paper is based on a content analysis which ascertained the necessary classifications that spanned a quarter century of observations and deployed regression analysis to test the research questions. Thus, the paper has strong ambition, ample empirical legwork and a rigorous means of evaluation.

The first question I wish to pose is whether we should believe the paper’s message of the existence of a signal. Here we should separate the prospects of a consistent communication from that of a purposeful communication. The authors have the burden of proof here since a signal implies much that has to be in place in contrast to the absence of a signal. I certainly agree that what a journal has published in the past communicates much to those inclined to make submissions. In fact, many authors make special effort to cite works previously published in a journal to demonstrate a stronger fit with the interests of a journal. Endenich and Trapp do not disagree with the existence of that signal, but go on to further assert the existence of
a separate signal in the publishing records of those likely to be involved with the review of submitted manuscripts to that journal. The use of such information by authors does not align well with my personal experience but certainly is not illogical or impossible. My anecdotal experience may not be a good guide since I have not regularly pursued publishing opportunities in the mainstream of the accounting discipline, especially as such is represented by TAR and CAR. Therefore, who signals what is an empirical question.

Unfortunately, the paper never demonstrates how the incremental and less-visible signal of what editor and board members publish can be important or even just salient in the presence of the much more visible signal of what gets published. Given that the results offered demonstrate that editorial board publishing and overall journal publishing are highly correlated, the simpler explanation is that what a journal publishes in each edition explains the lion’s share of the signal strength or bandwidth available for potential authors who are intended in reading these tea leaves. When the strong signal is also easy to access, a weak signal that is difficult to dig out is of little value. Ultimately, this is an empirical question for which multiple regression was built.

There is much more to say than what the authors have said that might be useful in painting the necessary context. Most importantly, the issue of why these particular journals are compared needs elaboration. Although most would conclude that TAR and CAR are among the best journals in the accounting discipline, there are others as well. TAR and CAR stand out as the flagship journals of the U.S. and Canadian academic accounting associations. We need to discuss the meaning of such, given that they do not necessarily represent the absolute two best journals of the discipline. One wonders what implicit beliefs are held by the authors about free-standing academic organizations and the publishing opportunities they present to their membership. Should such be led by elite elements to create best practices in a quite narrow sense, or should it be more open to good work of all sorts as such is found in the preferences of its membership? In other words, a comparison of the editorial characteristics of TAR and CAR has meaning only if one has a working theory of who publishes within their pages vis-à-vis the careers of the broader memberships. Put differently but succinctly, should TAR editors be any different than those of say, the Journal of Accounting Research, a private journal run by the University of Chicago or those of the Journal of Accounting and Economics out of the University of Rochester? With many journals of reasonably equivalent importance to an academic accountant, the point of comparing two of the set is rather difficult to appreciate when the tradeoffs invoked by authors are much more robust.

The learning offered by a paper that is essentially a content analysis exercise has much to do with the strength and non-obviousness of its categories. I have to admit being rather disappointed by the essential failure of this paper’s classification scheme. By offering substantive areas that are no more precise than the four primary sub-disciplines (and used by the Hasseback directory since the 1970s) the paper offers us an abbreviated version of how just about anyone would do it. Financial, managerial, auditing and tax also are how the curriculum and the academic job market are organized, at least in the U.S. The contours of this division are reasonably well known, with financial accounting in a long-term consensus ascendency. Although to a lesser extent, the methodological categories are also predictable and well-known in advance of this paper. Here, the rise of archival and experimental at the expense of analytical and survey is not news. The paper would have been well served to work inside of the categories that all readers would be expecting to see. This would have brought something new to the reader. With the use of such well-known categories that are also very broad, the result fails to offer much surprise value.

I believe that the paper has chosen to ignore some of the inconvenient politics of publishing that bear upon the contents of TAR and CAR. I can offer a non-definitive rendition of what could be relevant truths. Scholars in the U.S. are very conscious of their leadership in academic accounting.1 As such, elite components within the U.S. are absolutely convinced in the righteousness of whatever direction they have led the discipline. Most recently this has been financial in substantive topic area and archival in methodology. Other journals must decide how much they seek credit for the emulation of these “approved” directions, or bravely go their own way through a purposeful differentiation strategy. CAR’s situation is made more complex and perhaps rendered sui generis by its location in Canada. As part of the Commonwealth, the accounting discipline is somewhat influenced by British traditions (and perhaps also French ones). The academy is open to the intellectual traditions of critical accounting in ways and magnitudes uncommon in the U.S. Located outside the U.S. also allows (and perhaps obligates) Canadian factions to pursue more independence from U.S. academic hegemony. Proximity to the U.S. allows CAR to capture a high volume of U.S. submissions produced in accordance with technically demanding U.S. standards and preferences. Thus, the results of the paper I comment upon are considerably driven by the varying geographic origins of the two journals.

The bottom line about the paper is that it tends to belabor the obvious. Ignoring whether the signal is redundant or not, there is a strong parallelism between the type of reputational capital possessed by editorial personnel members and the type of papers preferred by journals such as TAR and CAR. That we all “knew” this before is now strengthened with the publication of this paper. For those of us who work in this end of things, we now have a good citation for this fact. We also “knew” that TAR was less ecletic and intellectually upon-minded than CAR. Now we have that knowledge documented. Good research does not always have to change what we know, but it is certainly better if it does.

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1 Although buttressed by rankings that feature so many U.S. schools at their top, the righteousness of leadership here seems to be more a matter of critical mass and historical presence. Rankings are too much of a recent phenomenon to be considered as more than an objectification of existing stratification beliefs. In a general sense, the tendency toward careerist objectives of faculty and the recent monoculture of scholarly value also drive these results (Humphrey & Gendron, 2015; Willmott, 2011).
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