

## EAA PhD Forum 2015

### Session: Trends in management accounting: Editors' perspectives

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#### Learning objectives

- Evaluate the relevance of your research (in management accounting)
  - The “So what?” and “Who cares?”
- Articulate your research in relation to a “big unanswered question” in your area
- Evaluate the “primitive motivation” for your research (in management accounting)
  - The “puzzle”, the “tension”
- Elaborate the “substantive contribution” of your research (in management accounting)

#### Brief overview/summary

Those coming to this session to hear “which (‘hot’ or other) topics in management accounting they should research to maximize publication success” will be disappointed because that should not be the principal driver of an earnest researcher. Instead, we will exemplify the breadth of research in this area of accounting and apply generalizable criteria that make a study “interesting” and therefore enhance its likelihood of publication. Some of these criteria can be phrased in questions as follows:

- What is (are) the big unanswered question(s) (in the area)?
- What is the main “primitive” motivation for the research? Where is the “tension”?
- What is the “conventional wisdom” in the area, and does it make sense? Can, or should, it be questioned?
- Are there any “puzzles” that don’t fit the conventional wisdom?
- Is there anything different the study does that would lead you to expect the insights (if not the results) to be different? Does it seem appropriately “different” given the tension or puzzle the study seeks to address?
- To whom is the research (ultimately) of interest?

In other words, this session provides an overview of not just indicative areas of management accounting research broadly conceived, but more importantly a more generalizable “way of thinking” about doing management accounting research.

#### References (background reading)

Van der Stede, W (2011), Management Accounting Research in the Wake of the Crisis: Some Reflections, *European Accounting Review*, 20(4), 605-623.

Van der Stede, W (2015), Management Accounting: Where From, Where Now, Where To?, *Journal of Management Accounting Research*, 27(1) ([papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2446106](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2446106)).

Evans H, Feng M, Hoffman V, Moser D & Van der Stede W (2015), Points to Consider When Self-Assessing Your Empirical Accounting Research, *Contemporary Accounting Research* 32, in press ([papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2120181](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2120181)).

#### Who should attend?

This session aims at PhD students who have an interest or specialize in management accounting research, although the word “management” is merely a delimiter because we will use examples from this area of accounting; the session could be more broadly of interest beyond just management accounting.

## Research Impact and Relevance



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- Mintzberg:
  - “We glorify ourselves by describing our own age as turbulent”
- Of course ...
  - Doing research has never been as difficult;
  - And, getting it published is near impossible.

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## Relevance is key

- Research that has potential impact and that “**smart practitioners**” (and hence, academic reviewers most likely, too) can get excited about
- Research that is “**appealing**” in that sense, but also (a bit) creative
  - **When we do such research well, we will find out that it will get published**

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## Beware of “means-end reversion”

- Stop talking about “publications”; worse even – “**hits**”
- Start talking about “**research**” again
  - **Good, interesting research is a precondition for publications**
- Note: *Publications, of course, are important*

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## What is “interesting research”?

- **Don't know**, really!
  - One knows it when one sees it (and it can be ... **diverse**)
  - Still, **for you to determine**; preferably at the start of your project rather than by means of some *ex post posturing* about relevance
- There sure are “**biases**”
  - But will all the journals keep turning away research that is interesting and well done?

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## What is not going to work?

- Just tweaking existing research
- Just doing something because it hasn't been done before
- **Academic journals might not be the best place to start looking for ideas**



- Note: *Prior academic literature, of course, is not to be ignored*

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## What might work?

- Maybe the following questions can help as a guide

Illustrated with examples from an area with which I am familiar; but hopefully applicable across various other research (sub)areas

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## What are the big unanswered questions in the area?

- e.g., **Pay-for-performance?**
  - Do firms structure pay packages to provide executives with **incentives to increase the value of the firm** in the long run?
  - When they do, should they do so regardless of any **sociological or moral considerations** on the amount of compensation granted?
  - ...

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## What is the main “primitive” motivation for the research? What is “puzzling”?

Banks are reviewing junior bankers’ pay in a move by investment banks to boost salaries following public and regulatory pressure to reduce lavish bonuses. The moves come in response to new EU rules that cap bonuses for senior bankers at twice salary. They are increasing their salaries to offset lower bonuses, so there is no impact on total pay. The changes leave the junior bankers in a stronger financial position, since a larger proportion of their total pay is guaranteed, even if the bank performs badly.\*

- **What about pay-for-performance?**
- **Might something else be at work?** (Clearly, regulation.)
- **What about pay-for-performance (motivation)? Selection?**

\* The Financial Times, 17 Feb 2015

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## What is the root of the issue?

- Likely (*inter alia*) a lot to do with **governance** ...
  - Who is getting which share of the pie for what?  
(performance or power)



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## What is the “conventional wisdom” in the area, and does it make sense? Can, or should, it be questioned?

- The conventional view is that of “**arm’s length bargaining**”
  - Executive compensation in publicly-traded firms is the product of a negotiating process between:
    - The **executives**, seeking the best possible deal for themselves
    - The **board**, seeking to get the deal that maximizes shareholder value

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## What is the “conventional wisdom” in the area, and does it make sense? Can, or should, it be questioned?

- The alternative view is that of “**managerial power**”
  - Could it be that executive pay is a byproduct of the agency problem between managers and shareholders rather than its remedy?

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## Are there any (persistent) **anomalies** that don't fit the conventional wisdom or paradigm?

- There are a series of common pay practices that, while **hard to reconcile with the arm's length model of contracting**, seem consistent with the managerial power view
  - e.g., Why are departing executives often given gratuitous benefits beyond those that are contractually required?

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## Can cross-paradigmatic insights provide (better) answers for the anomalies?

- A number of **legal, organizational, sociological** studies suggest that various factors may cause directors to have **"incentives to go along"** and hinder directors' capacity to deal with the executives at arm's length
  - e.g., "cognitive dissonance" (psychology)  
"group think" (sociology)

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## Why would you expect the results to be different/interesting?

- Evidence of **"rents"** rather than **"pay-for-performance"**?
- **"Moral objections"** or **"system inadequacies"**?

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## Who are the ultimate consumers of the research?

### Impact!

- Hopefully *not only* journal reviewers and editors
  - Even for them, relevance ranks high, if not highest
    - The purpose of publishing in [an academic] journal should include **informing a larger constituency of the significance of the findings in the paper**

→ **There can be no impact without relevance**

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## Tips (for what they are worth)

- Be a jotter – take in puzzles and jot them down.
- Read sometimes outside of what you *have to read*.
- Read practitioner material in your area. (*That, you must.*)
- Remember: Academic journals are *not* the best place to start looking for *impactful* ideas.
- Have an “elevator story”. (*If it doesn't seem to make sense, it probably doesn't.*)

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## Tips (cont.)

- It's okay to be quite micro. (*As long as there is a bigger point.*)
  - Social science is subject to many imponderable contingencies, and so you may (and probably should) feel that you have precious little to say. (*But try to say it well.*)
- Relate to the macro. (*Make the bigger point.*)
- Don't overstate (*when making the bigger point.*)

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### Tips (cont.)

- No perspective is ever full-purpose. *(Nor is any perspective ever entirely worthless.)*
- Don't bash *(when arguing for your perspective).*
- Tension is good. *(Being normative is not.)*
- Do research by conviction. *(Not convenience.)*
- Research is not an excuse to use your method. *(Research should be problem-driven; not method-driven)*

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### Tips (cont.)

- Nirvana fallacy: Imprecision and imperfection are not an excuse to not do the research. *(Imprecise ideas may well be the most potentially impactful.)*
- Learn to peel an onion.  
↓
- *(But be prepared to cry.)*

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