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The future of interpretive accounting research

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The future of interpretive accounting research—a polyphonic debate

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From: Thomas.Ahrens@lse.ac.uk
Subject: The future of interpretive accounting research
Date: 30 July 2004 12:54:49 BDT
To: [insert field: email_address]

Dear [insert field: name],

We are writing to invite you to an informal meeting to be held just before the next EAA Congress that will take place in Göteborg, starting Tuesday, 17 May 2005. We propose to meet between 16:00 and 19:00 and discuss the future of interpretive accounting research.

In the literature, interpretive accounting research is frequently characterised by what it is not, i.e., "non-mainstream" or "alternative". We are concerned by this lack of an independent intellectual identity. We thus thought of creating an opportunity to discuss in a small group what we view collectively as valuable in our research. We are looking to establish a more positive identity for ourselves as scholars interested in the interpretation of accounting practices. This seems appropriate because there is, by now, a very significant body of work that could be loosely labeled as "interpretive accounting research". What seems so far to be lacking, however, is a shared intellectual agenda that would allow for a clearer articulation of the achievements of interpretive research. Two issues that we would see important for this effort are to reconnect interpretive research with accounting technique and to explore points of contact with functionalist accounting thought. We propose an open debate in the meeting, however.

We hope that this opportunity for discussion will be of interest to you and that you will be able to join us in Göteborg.

Best wishes,
Thomas Ahrens, Chris Chapman & Fabrizio Panozzo

In 1997-99 the three of us organised a series of European Commission funded conferences aimed at building a network of young researchers in the area of accounting. At the time "young" was defined by the Commission as researchers under 35 years of age (allowing for maternity leave or national service). Over the intervening years our network had grown and we wanted to try and take stock of the field in which we had now been working for a surprising number of years. To that

end we put together the above email and a broad invitation list of people who had been at those first meetings, and others of the same generation (or even younger) whom we had met since.

About half of those originally contacted managed to make the meeting where we spent a stimulating couple of hours of debate on the topics raised below—so stimulating that we developed a collective desire to leave a trace of the discussion. Writing a traditional paper with so many, so widely dispersed authors was not going to work. Instead we came up with a different form of collective writing that mirrored the original debate, and that might contribute to ongoing debates in this journal concerning the nature and status of our research (e.g. Arrington, 2004; Inanga & Schneider, 2005; Macintosh, 2004). We agreed a process in which each of us in turn would have one week to add a target of 300 words to a rolling document, going through the contributors alphabetically. After two rounds we would see what we had got...

Thomas Ahrens

Interpretive accounting research (IAR) has come of age. Considering the journals in which its key debates have developed, its beginnings can be dated to the mid-1970s with the founding of *Accounting, Organizations and Society*, and a number of seminal papers from the 1980s. Through the founding of the *Accounting, Auditing and Accountability Journal* in 1988, *Critical Perspectives on Accounting* and *Management Accounting Research* in 1990, and the *European Accounting Review* in 1992, IAR has grown into a sizeable literature. Yet, it is not uncommon to see it characterised as ‘alternative’ (Baxter and Chua, 2003). The overall impression is that of a poly-centric research field held together by its opposition to a variously defined positivistic ‘mainstream’ (Chua, 1986; Panozzo, 1997).

IAR’s poly-centrism is an important strength. It enables interpretive researchers to adapt their theories, methodologies and methods to highly specific research areas and questions. But equally, defining its identity in opposition to a mainstream ‘other’ and working with a plethora of social science theories makes it difficult to explain the contributions and potential of IAR. Its insider quality frequently leaves business school deans, potential doctoral students, and the world of practice unimpressed.

A potential route out of the vague identity of IAR that would not sacrifice its diversity may lie in emphasising the accumulation of interpretive knowledge on particular aspects of, or perspectives on, accounting practice. IAR papers could usefully make greater efforts to explain their contribution to, say, budgeting, centralisation, public sector accounting, accountability, etc., rather than begin their introductions with the often-heard ‘Little is known about...’ For most topics of accounting research this is simply no longer true; the exploratory orientation of much IAR notwithstanding. Future IAR should define its identity more clearly by building on its past insights.

A greater emphasis on the potential for cumulative IAR could usefully go hand in hand with an overall more constructive outlook. In the pioneer years of IAR important contributions were made by showing the limitations of rationalistic and techno-centric understandings of accounting. 30 years on, there is much agreement that accounting practices are vastly more complex than accounting blueprints.

Accounting is interwoven with social, cultural, and political processes. What IAR should now turn its attention to, are the specific ways in which designers and users of accounting systems work with their constructive potential in the pursuit of specific agendas, and how their systems (and agendas) change in the process.

Albrecht Becker

It is time to have a critical look at the critical stance in IAR. Foucault (1997, p. 29) understood critique as 'the art of not being governed quite so much' and linked it to power/knowledge. As critical research in accounting may no longer be seen as concerned with Truth we may translate 'critical' as enhancing the reflexive potential of IAR¹ – for research, teaching, and practice. This might become a positive force in formulating a (new?) identity for IAR and for its contribution to research as well as for teaching and practice.

This reflexivity of IAR should find its expression in three interrelated concerns. The first concern would be to more systematically connect different studies from different theoretical backgrounds concerning similar research topics and look if they 'can be made to talk to each other' (Lukka & Mouritsen, 2002, p. 808). This is one way that we have the chance to demonstrate to 'business school deans, potential doctoral students, and the world of practice' (Thomas) the value of our insights. The second concern is with the theoretical foundations and has a strong link to the first. We don't have to invent the wheel of organization theory over and over again; a more systematic look into this field might prove helpful in finding theories and data which may in analogy be applied to the field of accounting and which might make our findings more digestible for non accounting researchers. Third, we have to think about our teaching. Is there a textbook on IAR or written from an IAR perspective? In what way do we integrate IAR findings into our lectures and courses? Do we think that IAR is only for postgraduate students? Giving our students the chance to reflect on accounting practice, its prerequisites, and consequences would be a good, though not very fast, way to have an impact on practice.

John Burns

I left our insightful meeting with thoughts that this was not about reactive strategies to 'compete against' the mainstream but more about how (so-called) 'non-mainstream' could further their contribution to knowledge and to move to its next level. The difference here is significant.

We discussed what constituted 'good' interpretive accounting research – can/should we validate what we do? Good interpretive research in the future, it was suggested, would be much more than 'yet another' theoretically-informed case study. A desire to get closer to accounting-in-practice was stressed, and maybe there are synergies to be had in this respect from amalgamating research traditions of different cultures and backgrounds.

Will this (inevitably?) take our research out of the (traditional) remit of accounting? Nowadays, accounting is very much a part of the bigger business picture. But has our

¹ I am indebted to Martin Messner for this point.

research appropriately reflected such fundamental shifts of the goalposts? Moreover, do we understand the stakeholders of present-day and future accounting research?

Another part of our discussion was to give some thought to how different accounting concepts evolve and change through time. The form and use of theory featured strongly in our discussions. The wave of opinion leaned towards ambitions of (existing) theory building rather than yet more new theory invention. In the future theory should less likely condemn a scholar to a particular camp but actually be a powerful tool by which interpretive researchers collectively convey the richness and analytical detail of their work – theory will become an enabler in research rather than the noose and stereotyping stamp that it can often be. Consolidation, hybridisation and theoretical eclecticism would seem to be more acceptable:

‘If it works then use it’ may become a more common cry amongst such [‘young’ management accounting] researchers, rather than an insistence upon theoretical and methodological consistency and purity. In short, the research thrust may lie in attempting to integrate and consolidate the variety of theories and methodologies which have emerged in recent years, rather than seeking to add yet more (Hopper et al., 2001).

Chris Chapman

Integration and consolidation are precisely the concern of Ittner & Larcker (2001). Their article constructs the notion of value-based management as an “integrating structure” within which Management Accounting research can be evaluated, allowing for the presentation of “consistent results”, “inconsistencies” and “gaps”. They state that their most striking observation in their review of this literature is the extent to which the research is driven by changes in practice. They note that developments in practice (such as ABC) offered ammunition in the battle for recognition of the relevance of Management Accounting research in the context of critiques during the 1980’s. However the faddish nature of management accounting research that followed this stream of innovations has not encouraged theoretical integration, and in some cases has meant that research topics “tend to disappear as the next big management accounting ‘innovation’ appears, even though earlier ‘hot topics’ may not have been fully explored.” p. 356.

Their article sparked a spirited series of exchanges in the literature, beginning alongside the article itself, and continuing in a special issue of *European Accounting Review*. Coming out of these debates it seems that the notion of “integration” in research has a similar status to the notion of “critical” research. Whilst “critical research” research initially appears to be an unarguably good ambition, its very appeal supports its mobilisation for very different ends, with complex implications for our identity as researchers (Mouritsen et al., 2002). Thus, in debating Ittner & Larcker’s article, Hopwood (2002) turns Zimmerman’s (2001) more tightly-prescribed musings on integration of the field into an opposition between careerist-oriented and curiosity-oriented approaches to research.

As already noted, having a career is something to aspire to, not to fear. We are concerned about the impressions that Deans form of our work, and a desire for engagement with practitioners should not carry with it the suspicious whiff of “consulting”. The most damning implication of “careerist” research however is its

implication of integration gone too far. Hopwood implies an abdication of responsibility for the nature and direction of our research. As noted above however, an unfettered personal curiosity beckons a field populated by studies on topics about which “little is known” that draw an unending progression of “exciting new” theories. We are beholden then to exercise at least a part of our curiosity towards each others’ interests. Recent studies of Enterprise Resource Planning systems (e.g. Quattrone & Hopper, 2005; Dechow & Mouritsen, 2005) offer a clear example that faddish technologies need not be studied in that light (Chapman, 2005), and that they can contribute to existing research debates, and so there is cause for some hope. Given the emotive nature of issues of identity, and the diversity of others in relation to whom we seek to delineate an identity as IAR researchers however, it is clear that this debate has much ground still to cover.

Markus Granlund

The common idea in the group seemed to be that IAR is about *understanding* the everyday *practice* of accounting (cf. Chua, 1986). This involves *close contact* analysis of *human interaction*. What seemed to be more difficult to agree on was where and how IAR should proceed. This is perfectly natural if you consider the plethora of ways of conducting IAR. Probably the most important question in this regard relates to the contribution IAR has been able to produce: what have we learned *in toto* from the multitude of interpretive case studies presented and published every year? Has our knowledge accumulated? How? Because these questions tend to be difficult to answer, it seems that despite the potential, we probably have not learned too much from recent IAR. This is directly related to the somewhat vague identity so created for IAR. Being able to clearly identify and explain the contribution made with regard to existing knowledge would probably be followed by a stronger identity (cf. Thomas).

The aim to increase or deepen *understanding* may not be enough in the long run. Some of the participants tended to subscribe to the attempt to contribute more or less directly to practice. This is not to suggest acting on the conditions of practice, but engagement with scientific values. In any case, researchers should also consider their readership and what kind of effects they want to induce. Career and contents of research are tightly intertwined, which may have detrimental consequences in many respects (cf. Chris). Whilst we should avoid being considered as only consultants, neither we should be concerned only with what our colleagues think of the cleverness of our work (Swieringa, 1998; Lee, 2003). Theorizing and theory building should also proceed towards building of *accounting theories*: theories that can more easily be communicated to practice; theories that, for example, suggest something about the feasibility of a MCS under certain circumstances (Malmi & Granlund, 2005).

The role of theory is also very much at stake here. Typical of IAR is often the application of social (meta) theories. However, it seems that we cannot falsify or test most theories we borrow from other disciplines, and are unable to contribute back (Humphrey & Scapens, 1996). On the other hand, if we use these theories, we should probably be more careful in their application. There is a risk that the application of these theories does not make the complicated world easier to comprehend, let alone manage (Malmi & Granlund, 2005).

Michael Habersam

Manifesto?!

- To provoke a constructive outlook for IAR - wherever it may lead to.

Eclecticism is good for you – synthesis is needed! The variety of social science methods used in IAR is NOT the problem. Without this variety at hand, IAR would have to invent it because of the variety of practices it is confronted with in the field. But, while poly-centrism is a strength (cf. Thomas) by enlarging our repertoire of methods, perspectives, questions and purposes, to strive for a monolithic methodology (like mainstream accounting research) would be a paradox.

Research syntheses could be an answer to this paradoxical situation in two ways: (1) it is not ‘yet another’ theoretically informed case study (cf. John), and (2) it opens up a perspective on ‘stronger theoretically based conclusions’ concerning the preconditions and consequences of accounting practices in use, as well as referring to practices of research on accounting from an interpretative point of view.

Probably, subjectivism, credibility, research as value-driven/-laden and political activity, dialogical endeavour, re-contextualisation, open-mindedness towards critique, etc. are or are becoming key labels for what is going on within IAR on a theoretical and practical level – but this development is better to evaluate and to present out of a synthesis-perspective than out of a single case-study-perspective.

‘Brother, Where Art Thou?’ - Mainstream is not the enemy! Is it really worth the time to defend against mainstream research(ers) by falling into the trap of the quality/quantity-debate? IAR is not exclusively qualitative research, but research of high quality, using quantities as far as they support the credibility of the findings. This distinction seems to be important, because quantities are then no longer legitimated by the abstract credibility inscribed to ‘facts and figures’ but part of an integrity-game producing research results. This, in turn, means that the use of qualitative and quantitative approaches has to be legitimated equally by the context and purpose of the IAR conducted.

You cannot not be political! – An ‘institution’ is needed, although institutionalisation may indicate the dead end of a vivid movement because the next step is standards of ‘good IAR-praxis’ (which is a point to discuss, especially if standards necessarily result in sclerosis). But without this step of self-organisation, IAR may not be respected in the political game of research grants, publishing activities, accounting professional bodies’ marketing, consulting contracts, etc. Paradoxically enough, disciplinary boundaries are easier to overcome when you became a discipline.

For research-activities the ‘institution’ is a journal, for teaching it is a curriculum and a textbook, and for the accounting profession, it is (being part of) a professional body. These are definitely mid-term goals, but it does not necessarily mean we should establish always ‘new institutions’.

Where to start? Maybe at all levels, maybe with the synthesis-question, because the latter will take us back to what we all know: that there is a lot already done to build on.

Allan Hansen

To be interpretive seems to be to dissociate one self. It seems to be about being at a distance. IAR is often portrayed as being interpretive rather than positivist, practice rather than theory based, alternative rather than mainstream, social and institutional rather than technical, critical rather than naïve, sensitive rather than indifferent, modest rather than arrogant etc. However, how does the distance affect us? Some claim that IAR is “too far away”. In order to build a stronger programme we have to relate ourselves (cf. Thomas), accumulate knowledge (cf. Markus) and find a common ground (cf. Chris). But the next big question is: what does it mean? To what should we relate? What is it that we accumulate? And what is it that we have in common?

One answer would be to go back – back to accounting – where it all started. And ask ourselves: where have all the modesty and sensitivity taken us? To what extent do the thrilling sociological glossaries and intriguing philosophies that have been so hard to learn contribute from an accounting point of view? One might suggest that paradoxically, in our search for the blind spots that conventional accounting research produces, we may blind our selves by philosophical, social and political discourses and find it hard to get back – back to accounting.

What does IAR tell us about ‘target setting’, ‘choice of performance measures’, ‘performance evaluation’, ‘design of incentive system’, etc.? Probably a lot! Sociologies and philosophies might be resources for getting closer to these issues in practice. The sensitivity and modesty embedded in IAR may offer us insights to the performative, variable and contingent nature of the accounting issues, rationales and objects and illustrate the plastic but fundamental character of these phenomena in practice. An obligation to consult the domain again and not to compete with it (cf. John) and consider mainstream research more as an allied rather than an enemy might be one way to revitalise IAR.

Interpretive researchers probably have to disconnect, and take us away from mainstream, conventional, indifferent, positivist accounting research for a while, but this does not imply that we should not reconnect at some point. The polycentrism of IAR (cf. Thomas) represents a significant resource for coming up with something new. Accounting may have been treated as a ‘black box’ in IAR but maybe we should ‘reopen’ it and care about conventional and mainstream problems of accounting. Some might still stay away and for very good reasons. However, questions of how the accounting particularities, issues and logics are formed and reflected in practice could be a resource for IAR in terms of where we are at the moment.

Rihab Khalifa

‘Mainstream’ is a political concept that serves as a reference to an almost imaginary rival. More often, certain attributes of the supposed ‘other’ are made cognitively significant through an overstated contrast to the valued etiquette or manners of the

*newly (in this case IAR?) identified group. By repeatedly calling attention to these disapproved attributes they become the very thing that defines them. In that sense, the notion of ‘mainstream’ betrays a certain ignorance about ‘others’. Those who are identified as mainstream do not necessarily see themselves in those terms. Instead, for them there are other groups, whom they perceive as (unjustifiably) privileged or/and ‘mainstream’.*²

One could not deny the usefulness of operationalising ‘otherness’ as a concept in delineating a political identity for IAR, especially in the context of conceptual extremes between what we call ‘mainstream’ and ‘us’. But creating a *positive* identity for IAR becomes a difficult task if ‘we’ were to be (known as) everything that the ‘others’ are not. Not only will that limit what ‘we’ can do, but also help establish the ‘other’ as the norm by benchmarking what ‘we’ do against ‘them’.

IAR should be concerned with its own knowledge objectives, an important role of which is to advance interventionist and activist research. Currently within IAR this approach or attitude towards knowledge is threatened (or held back) by attempts to mimic the objective stance of mainstream accounting research. This is not to say that everybody in IAR should pursue interventionist research, but it is an important opportunity for what we can loosely call the interpretative project. An overtly ethical stance is a useful way of developing IAR. It helps keep the point of methodological debates in view and subordinates them to the specific knowledge gains of research programmes.

To reiterate, IAR could be more strongly involved in agenda setting and policy formulation with a view to clarifying and acting upon issues in ways that make it responsive to problems in practice and society. This is highly relevant to a strengthened accounting research ethos. Having said that, it is noticeable in its opposition to the ‘main-stream’ IAR has managed to embody some of the very characteristics and structures that constitute ‘mainstream’. Let’s have interested (and interesting) knowledge that is motivated by advancing our own ethical convictions, and let’s remember, having differences enhances our own selfhood. In a way, we owe it to others to know who we are.

Teemu Malmi

What worries me with this debate is that we all seem to know what IAR is, but no one is willing to define it. Based on my current understanding, I cannot define it either. I find it difficult to discuss the future of something that escapes definitions. What are the sufficient requirements for a piece of research to be qualified as interpretative? Is IAR different (and if so how?) from critical research (Chua, 1986) as criticality is often seen as one of the core characteristics of IAR? What is the difference between understanding and explaining? Even if some of us may find answers to these questions straightforward, I suspect that spelling them out clearly would show that others disagree, at least partly. This debate should provide as clear a definition of IAR as possible for other researchers, business school deans and potential Ph.D. students to build on.

² In our meetings there was no unanimous agreement about which groups were ‘mainstream’. In fact one of the participants, whom I thought was ‘mainstream’ thought she was not! *Her* ‘mainstream’ was a different group altogether.

Chua (1986) differentiates an interpretative perspective from other perspectives by referring to different assumptions made regarding beliefs about knowledge, beliefs about physical and social reality and relationship between theory and practice. Looking at IAR as it is practiced, it seems that these criteria do not clearly differentiate IAR from mainstream research (e.g. Kakkuri-Knuuttila et al., 2005). Instead of, or in addition to, trying to argue the uniqueness of IAR based on these assumptions once again we could try to be more specific on why we are conducting IAR. What is the agenda(s) of IAR, what are its objectives, or our objectives as researchers conducting it? Are we adding to the same pool of knowledge as the mainstream accounting research regarding e.g. various accounting issues (see Thomas and Allan)? If not, how will this knowledge pool be different? Will we codify that knowledge to create accounting theories (see Markus)? And importantly, how will this knowledge serve organizations and societies we are studying?

I echo the views of others regarding the need to get back to accounting and the more constructive stance, as I consider these ideas provide us with the potential to develop and define an agenda for IAR. I see the clear definition of IAR, including its purpose(s) and how this research links back to organizations and society, as essential steps if we wish to strengthen its identity.

Andrea Mennicken

IAR covers a vast field of research. But as Thomas and others have pointed out, its vagueness should be viewed as its strength. It gives us the freedom to choose and work with different qualitative methods and social theories that highlight different aspects of accounting practice regarding its roles, uses and limits in everyday life. In view of the vastness of IAR, can there be a unifying research agenda that can serve to ground our identity and strengthen our position vis-à-vis other accounting research traditions?

In my view, the notion of “calculative practices” (Ahrens and Chapman, forthcoming; Kalthoff, 2005; Miller, 2004; Vollmer, 2003) could provide such a unifying lens. The notion is broad enough to encompass the variety of different theoretical and methodological approaches used in IAR. But it is exclusive enough to distinguish IAR from other accounting research traditions and provide it with a focus that can strengthen its identity.

The concept of “calculative practices” stresses one of the major strengths of IAR, namely to provide insight into how accounting works in concrete settings. Paraphrasing Garfinkel (1967: 1), it re-emphasises IAR’s focus on the activities whereby members produce and manage, with the help of accounting concepts and techniques, settings of organised everyday affairs. Second, the notion of “calculative practices” opens up IAR for wider questions of how social order is generated. It helps translate narrow questions concerned with how specific aspects of accounting work (budgeting, target costing etc.) into broader questions of social order, which are of wider academic interest and relevance. As Burchell et al. (1980) emphasised, accounting activities should not only be explored in relation to specific issues of organisational management, but also in relation to larger social and economic processes. In this context, it is important not to overlook the implication of accounting

practices in relations of ruling and the (re)production of social, economic and cultural differences and inequalities.

One of the main purposes of IAR should consist in providing critical accounts. But as Albrecht suggested, the word “critical” should be used to stress the reflexive potential of IAR, not to emphasise that one is in opposition to some imagined mainstream. IAR can help practitioners reflect on their actions. But we have to be cautious not to turn the reflexive potential of IAR into an interventionist mission. Then IAR might lose its distance and, hence, its ability to provide others with careful and probing observations.

Anette Mikes

I would use the adjective ‘reflective’ to clarify what I feel IAR’s strength is. Donald Schon talks of the ‘reflective practitioner’ (Schon, 1983, 1992) as opposed to the ‘technical expert’ otherwise known as the ‘rigorous practitioner’. I find this distinction useful in thinking about the dilemmas that researchers of practices face. With the practitioners whom we study we share a common dilemma: that of rigor vs relevance. Some, out of concern with ‘rigor’ build practices (research projects) around formal, analytic models, convert problematic situations into well-formed problems, so that they can exercise technical rationality.

This ‘positivist’ epistemology of practice (and research) has increasingly been contrasted with an ‘alternative’ (but alas, nameless) epistemology which takes full account of the competence practitioners display in situations of uncertainty, complexity and uniqueness. This alternative epistemology deliberately immerses itself in what Schon calls the ‘swampy lowland’ of practice: confusing but crucially important situations, which are beyond the reach of ‘rigorous’ practices (research projects) built around the use of formal, analytic models.

I believe that IAR’s distinguishing factor is its belonging to this alternative epistemology. Our interest in what lies beyond the technical rationality of the practices we observe may take the form of on-the-spot problem solving, theory building, or re-appreciation of the situation. IAR is endowed with an ability to display a corresponding ‘reflection-in-research’ that allows it to capture this reflective feature of practice.

We can follow theories of practice as they become ‘theories-in-use’ (Argyris, 1995). Second, we can illuminate the fundamental, systematic mismatches between espoused and in-use theories, even if the practitioners we observe are unaware of the mismatch. Further, we can observe how traditional normative theories filter up as rationalizations of practice, how these get criticised as not corresponding to reality and how new theories of practice are generated, which then become (espoused) theories of best practice.³

Reflection-in-action, and the cycles of espoused, in-use and newly espoused theories constitute the swampy lowland of research on calculative (or whatever) practices. Messy and confusing problems force some practitioners to descend into this swamp

³ I owe this to Mike Power

from the high ground of manageable problems and technical solutions. IAR invites researchers to follow them there.

Fabrizio Panozzo

I think/fear this is a matter of discipline. On different grounds and with a couple of meanings.

Emotionally and socially, first, at least for me: our discipline is at stake, the one that enthused us, allowed us to share with others our intellectual passions and feel part of a community. This discipline is losing strength and appeal. We still practice it but we find it increasingly difficult to nominate, let alone predicate, what it is that we do. Also, this lack of enthusiasm, I think, leaves others, especially “the authorities”, unimpressed.

Second, the device that we seem to identify for rescuing our academic field is the fabrication of a proper disciplinary space. We’d like to be more disciplined, sharpen our discussions, focus on the production of knowledge that really counts and accumulate it. In order to do that we call for more precise definitions, quality measures, performance indicators to be brought together in a matrix that promises to add relevance to the knowledge that we produce. We place trust in these neutral, “objective”, regulatory devices because we believe that it is precisely this that relevant others (deans, editors, etc.) will appreciate. Indeed, this is a very wise move and I eagerly endorse it. However, I do believe that this is not enough. As interpretive accounting researchers we should remember that even the most “coherent” and “cumulative” disciplinary matrix will be re-produced and re-interpreted by the people thinking and acting within it. The context in which we operate as academics is social and political, and attempts to engineer it should not come at the expense of its “cultural” characteristics. What I am trying to say is that a new relevance for IAR will not be regained solely by the fabrication of a disciplinary space that meets international academic standards. What we will achieve through that is a strengthening of our position in the labour market and in the citation industry. But, I believe, the role of social scientists who seek to be “interpretive” demands more than that. Interpretation is an exploratory endeavour and exploration requires curiosity and vision. We must parallel the structuration of our discipline with a commitment to exploration of calculative practices in contemporary societies and anticipation of issues on which knowledge is far from being accumulated. A difficult combination which might require some kind of division of labour in our academic community but will indeed be facilitated by the socialization and self reflection that the present debate has activated.

Martin Piber

IAR is based on the idea of understanding accounting practices in the field (cf. Markus). However, this is not meant to scapegoat the mainstream or “the other” (cf. Rihab). It is simply meant as a thorough exploration of the field. Understanding in an interpretive way is pragmatic, ambiguous and provisional. Pragmatism suggests we concentrate on the very situation, its perception and all related processes. This opens multifaceted doors to different and sometimes ambiguous interpretations of different recipients. The provisional character of an interpretive study shows that the process of

understanding never stops. Indeed, the researcher has to create an artificial end. Therefore only some aspects of a given problem can be elucidated. Others have to be omitted – also to keep the conclusions meaningful.

This contested process of understanding has two consequences: Firstly, it is not enough to map something. Understanding needs a reflective reconstruction of the interacting forces in the situation. And secondly, it requires a personal judgement. Doing an interpretation always means that there are other (possibly as fruitful) options of making sense out of given data or a given case. IAR makes a contribution (also for practitioners), because it fits the very situation of the organization.

In this context IAR follows the reflective practitioner to the ‘swampy lowland’ (Schön 1983; cf. Anette). However, excellent IAR should be able to replace the notion of swampy lowland with ‘thrilling fields of social and organizational life’. IAR does not act as a wiseacre coming from the ivory tower. Researchers are people, who are there, look, and reflect on what is happening – with curiosity and vision (cf. Fabrizio). The forming of a theory out of a reflection process is a tricky issue. As soon as the reflection is standardized or ends up in a theory, the reflective moment freezes. Therefore it makes sense to hold the suspense – ambiguous, provisional, and open for debate – as long as possible. When we think about theory building, synthesis, common standards, and new institutions, these points should be considered. On this basis I argue for a cautious strategy. This debate could be a good starting point in order to reach a critical mass for the building of relationships between different streams of IAR. Then the practice of established relations will be followed by structures and the institutionalization of our discipline.

Paolo Quattrone

The genesis of this piece is quite unconventional and thus I propose a further departure from academic orthodoxy. Let’s think of a research trip to Rochester, Chicago, or Harvard. Then ask ‘practitioners’ of mainstream accounting, i.e. academics who publish in the US top accounting journals, to define ‘accounting’. We would probably get as many definitions as the interviews carried out. I may be wrong... but may be not.

An implicit assumption seems to underpin the contributions which have preceded mine: IAR (whatever it is) is fragmented, polycentric and varied (which we all agree is good) while ‘the mainstream’ is not. But are we sure this is the case?

If the findings of my imaginative field study are right (a deconstructivist accounting researcher could corroborate them without leaving her/his chair) then there is at least one consequence: when we write, talk, and debate about the ‘mainstream’, or even IAR, we do so in its *absence* (Quattrone, 2006). We always refer to something which is not there ready to be seen. It is there for we (and others, not necessarily humans) engage with it. Like an ‘inscription’ (Latour, 1987), we speak of the world in its absence.

So the mainstream and IAR have something in common. There are some differences though. Think of rankings of journals and the effect they increasingly have on academic career patterns. These create a hierarchical order and establish a power

relation. Thus the difference between IAR and ‘mainstream’ is that the mainstream *is* mainstream while IAR *is not*.

Why is IAR not powerful? (And do we want it to be?) One possible answer relates to the postmodernist approaches which have inspired some versions of IAR.

Deconstructing accounting is no longer enough. Postmodernism has become a sort of *postmortem*: it arrives too late on the crime scene and leaves us with no answers on how to do things and carry on. Many of us have taught IAR issues in an MBA class. Students have listened to us, some got interested, others got bored and fell asleep, but in the end all said: ‘OK that’s all very interesting, but what shall I do tomorrow when I go back to work?’

What IAR has failed to do is to instil hope (of solving a problem, of seeing things differently, of ‘getting things done’) in those whom IAR wishes to engage with. It has failed to engage the Other in order for it to make references to ‘IAR’ and make it happen, make it *present*. Whatever we term ‘mainstream’ has instead managed to be present. The idea of IAR as a fragmented, polycentric, critical, and reflexive study may be appealing for us but it is not for many (or enough) others. At least in its current form.

Tobias Scheytt

We agree that IAR is somehow different to other approaches. But it remains rather unclear in our discussions, what is meant by the ‘I’ in IAR: a specific approach, a perspective, a paradigm, a political agenda, a bunch of case studies, a network of people? I argue that in order to discuss the future of IAR, it is useful to remind ourselves of the origins of interpretive approaches. The results can then also be used as a springboard for identifying the future potential of IAR. Maybe this is too simple, but sometimes there is nothing so practical as a good theory about theories, (to paraphrase Kurt Lewin).

At a very basic level, the differences of IAR and, say, positive accounting research (PAR) originate in the epistemological assumptions and the methodological choices that are related to them. Given the tradition of interpretive research in organisation studies (cf. Burrell/Morgan 1979) and in accounting research (cf. Chua 1988), it is common knowledge that PAR puts its emphasis on the deductive development of nomothetic theories, while IAR performs acts of understanding/interpretation which result in an ideographic theory.

This has far-reaching consequences. With respect to epistemology, PAR observes what it observes depending on its theoretical model. Therefore it is very precise in a specific respect, but ‘blind’ to other aspects. Second, without direct observation it cannot change direction and focus once the research process has begun (cf. Eisenhardt 1989). Third, important aspects sometimes deliberately ‘hide’ behind the empirical methods employed in PAR (e.g. power structures), as results in social research can be biased by ‘knowledgeable’ actors without direct observation as a possible corrective. Fourth, the formal logic of PAR forces empirical studies to suppose that actors follow the same formalism, that preferences and goals are clear, that situations are free of contradictions, etc. Hence, PAR cannot regard constellations that do not ‘obey’ this logic, like ambiguities, paradoxes, recursive effects, side effects, etc., which,

however, form an important aspect of today's organisation theory (cf. Poole & Van de Ven, 1989; Clegg et al., 2000, Lewis, 2000). These logical 'problems', however, turn an organisation into a complex, context specific social entity with its own history (cf. Czarniawska-Joerges 1992). And, these problems can be rediscovered in organisations in the form of culture, history, values, development, change, sensemaking, to name just a few.

For epistemological and methodological reasons I argue therefore that these factors cannot be sufficiently conceptualised by PAR, as they are intrinsically processual and context-specific. On the other hand, these factors in their conjunction with 'accounting in action' form the natural playground for IAR (cf. Soin and Scheytt, 2006).

Thomas Ahrens

This is one of the most productive projects I have known! We're halfway there in our efforts to outline the future of IAR as a shared vision.

We know that cumulative knowledge is better than a field of creative loners and that our preoccupations with the mainstream are something of a red herring. The mainstream is less homogeneous than imagined. Focusing instead on our own identity, IAR is good at studying process ('accounting in action') and it can be sensitive to Tobias' context, ambiguities, paradoxes, recursive effects, side effects, etc. We have broad agreement that IAR should be concerned with accounting as practice and many of us want IAR to have an ethical dimension that has a bearing on its practical uses, e.g., in social reform, management, consulting, and teaching. IAR's focus on accounting as social and organisational practice works with notions of culture, history, values, development, change, sense making, etc. and presumes reflexive actors.

Those are strong visions relating to the substance and the process of research and theorising! For my taste, they establish what IAR is about. So, no more 'we-don't-know-what-we're-doing' complaints, please!

Still, important difficulties remain.

IAR does not have strong institutions for communicating IAR's value to the world. I am not sure what would work best to instil Paolo's hope of 'getting things done'.

Another important issue relates to the practice of research. What I find most difficult when drafting a new interpretive accounting paper is having to choose, from all the interesting things I found out about accounting practice, those which are *really* interesting for the readership, the ones that allow me to shed light on presumed knowledge or existing puzzles. What I need most in that situation is an excellent understanding of the literature to be able to develop illuminating angles on the data. (Here I agree with Allan that portraying IAR as practice based rather than theory based is difficult. Without the theory, how do you know which practices to study and what's interesting about them? There is no pre-sociological-discourse-accounting to go 'back' to.) It is difficult, but I think we can keep Martin's reflective moment open

with careful use of theory, by developing ideas that are not artificial endings to understanding but feed into future debates.

This brings me back to cumulative knowledge. I image not Fabrizio's disciplinary matrix but a resource that enables us to hybridise our diverse pockets of knowledge such that it sheds light on our observations. Unlike Fabrizio, I think that anticipating and understanding new phenomena benefits from cumulative knowledge, because without it we are doomed to find novelty anywhere, simply because we have failed to understand the extant literature. Needed is cumulative knowledge as a product of ongoing debates that recognise certain forms of commonalities and relationships connecting different spaces of the accounting arena.

Albrecht Becker

Déjà-vu: 'On trying to study accounting in the contexts in which it operates' (Hopwood, 1983 [!]) and accounting 'as a *social and institutional practice*, one that is intrinsic to, and constitutive of social relations' (1994, p. 1). Tobias reminds us that it is exactly this emphasis which distinguishes IAR from MAR (mainstream accounting research). Thomas extracts three 'difficulties' from the first round which I tend to see as two broader topics.

(1) Most of us have stressed the (theoretical) multivoicedness of IAR and the problem of dealing with the diversity and number of (empirical) case studies and how we may form something like an IAR body of knowledge without reducing it to too narrow a disciplinary space. I think the best way to do this is to draw on the empirical material we have already gathered and will produce in the future. I strongly agree with Thomas that this is an inherently theoretical endeavour which has the potential to produce a multifaceted picture (or shall I say, construction) of accounting practice. One of the greatest challenges is that to be able to communicate with each other within this multivoiced discourse, we have to be knowledgeable about many theoretical perspectives. But I still appreciate Lukka & Mouritsen's (2002) idea of a multitude of cases as a common basis of the community. Did anybody ever have the idea of setting up a qualitative accounting case database?

(2) What about the problems of 'communicating IAR's value to the world' (Thomas)? Being a pessimist, I would say that this is an inherent problem of IAR. By definition we neither have recipes to 'solve' practical problems nor neat models to 'solve' theoretical puzzles. This makes it more difficult to be heard particularly by accounting practice. And I always suspect that there is a negative correlation between being an interpretative researcher and effective engagement in professional and department politics - so much for impact on the academe. Being an optimist at the same time I believe that quality matters in the long run. Concerning impact on accounting practice, an important point is that understanding accounting practice as a powerful means and discourse in the social construction of organizational reality, the Other of the accounting perspective comes into the focus. This may have very practical consequences raising issues of dysfunctional effects, justice, and power - and, yes, issues of better design of management control systems. I tend to see these issues very much as topics for an IAR teaching agenda at all levels of programs. Concerning the impact in academic institutions, seeing accounting practice as deeply involved with other organizational and management practices opens up the chance to engage in

research across functionally defined boundaries, e.g. HRM, production management, marketing etc. This may deepen our integration into academic institutions and may at the same time inspire new ideas for research.

John Burns

I agree wholeheartedly that we are about knowledge accumulation, and warm to claims that it might be an opportune time to take stock of IAR.

But I still see potential problems in some of our labelling. For instance, consider our discussion of what ‘interpretive’ means. There are some who, in hearing the term “interpretive”, might think “not necessarily factual, judgement-based, non-scientific, therefore (?) less rigorous research). This might be damaging to your research. In a similar way, I wouldn’t advise introducing yourself as a “critical” accountant if you are trying to secure access for a new case study!

Novelty, curiosity, knowledge-seeking, innovativeness and similar terms have been used by several contributors - this I *really* like! As others have hinted, we are answerable to Deans, the profession, heads of department, research councils, editors and more. But most (Europeans, at least) will still pursue their type of research, and will still carve out successful careers. Without curiosity and innovation how can we pursue the knowledge accumulation that we stress? In this respect I agree with Rihab in that we should continue to pursue new and exciting areas (she gives ethics and the pragmatics of accounting as examples).

Finally, I do not see mainstream as ‘the enemy’, nor as being a great concern for us. I think we should channel our energies more to developing our own footings – to better develop *our* “so what”. We share (I think!) a dissatisfaction with a rational/optimising-premised mainstream. But let us stop it there and move onwards in our own agenda. With this in mind, and at risk of making a complete idiot of myself, may I take the liberty of suggesting some of the key underpinning tenets that might describe who we are and what our research is about?

- ✓ a processual lens (history and path dependency matters)
- ✓ social, economic, institutional, cultural, political, legal (and more) contexts matter and should be endogenised into our explanations (holistic and not overly-premised on assumptions and caveats)
- ✓ understanding (better word?⁴) and explaining the dynamics and interplay of accounting practices in organisations and society
- ✓ use different theoretical insight to assist in such explanations (eclectic)
- ✓ adopt multiple research methods
- ✓ where possible, to extrapolate our understandings to engage in dialogue with practitioners (but neither statistical generalisation nor prediction being prioritised)

Chris Chapman

⁴ Some of you have already raised problems with this term and have suggested some interesting alternatives, like adopting adjectives like pragmatic, ambiguous and provisional. I especially like Fabrizio’s ‘exploratory endeavour’.

It seems we may be at an “is the glass half full or half empty?” point in our debate. We have lots of ideas, but perhaps too many, since we also like to translate each other’s contributions into our own preferred vocabularies (does that represent the richness of our ideas or our failure to agree?). We recognise the fallacies behind the idea of a “mainstream”, but we still find it difficult to drop the habit of negative definition (I had to smile when John went straight from developing “*our* so what” to our dissatisfaction with the mainstream). I am going to pitch in on the half full side of things however.

Before worrying about the mountain still to climb (a good thing surely if we grant ourselves the status of being “young” researchers just a little while longer), let’s remember how much has already been achieved. Framing ourselves against unfriendly institutional structures, or facing with the “so what” question with reference to individual papers, or even projects, it is easy to forget just how far our understanding of *Accounting, Organizations and Society* has moved since 1976. The term “alternative” may make me want to grind my teeth, but Baxter & Chua’s (2003) review charts a series of foundational developments in our thinking, each backed up by impressive bodies of work. Our continued enthusiasm and energy are at least partly due to the cumulative effect of this past work, both in terms of individual studies that we might have found inspirational, but also as they collectively offer us a context of interpretation. Let’s keep this momentum in mind.

Markus Granlund

I have little to add to the insightful ideas presented above, although I may not totally agree with all of them. For example, I think there is space in the IAR agenda for interventionist research approaches (see Jönsson & Lukka, forthcoming). Indeed, interventionist approaches can be (and have been) applied in the field of IAR to produce theoretical *and* practical contributions. Interestingly, if we exclude interventionist research approaches from the agenda of IAR, we are one step closer to mainstream research and the view of accounting as a pure science rather than an applied (social) science.

According to Mattessich (1995) IAR can be labelled as ethical (not pragmatic) normative research (including also the radically critical genre of research). In his view the major contribution of this type of research has been to point out certain concerns (ethical, environment, gender, etc.) while arguing that accounting is never value free, contrary to what positive accounting theory assumes. But I guess my question is whether this is enough to create a successful future for IAR (cf. Paolo’s description of MBA students.)

Some further remarks on the role of theory. I agree with Thomas that we need theories to guide us to see patterns and “really interesting” things in the mass of interesting details. Sadly enough, I have also seen researchers to use theory just because one is needed to get the paper published in a certain journal. Moreover, it seems that the field of IAR is in some respect starting to resemble the mainstream as certain theories seem to have become standards to be used in order to get research on certain topics published. I have also experienced confusion and difficulty in comprehending what the results of some of these studies actually *are* (not only what they mean). And believe me these people also include brilliant academics who know a

lot about social theories and the philosophy of science. Too many times the results are also being criticized for only illustrating the theory and its concepts and for repeating things that have been said earlier in another context. So my worry is whether we are moving in the right direction, and, moreover, what kind of theorizing IAR would actually benefit from to make its future look brighter. As I tried to say in the first round, I consider this as a crucial question, both in terms of *what theories* to apply and *how*. In my view IAR should be open to all kinds of relevant explanations (cf. Michael). As Allan points out, IAR may sometimes have become blind to certain important issues (e.g. supplementary economic explanations) and been carried away with socio-ideological frameworks.

Michael Habersam

What would we like to read/write in an IAR-textbook?
(Some ideas for organizing/structuring our research in the next 2-3 years).

Possible title: Foundations of Interpretive Accounting Theory – exploratory research into theories and practices of calculation

Preface (outline of motivation):

IAR is not simply an applied methodology borrowed from social science but an exploratory endeavour in the field of accounting, i.e. into calculative practices. It is innovative research that leaves space for metaphoric examples and surprising findings. Case studies very often give hints for interventions as well as to probable (side-)effects of these. Therefore practitioners looking for relevance may find well argued reasons for, e. g. changing accountability routines. Well argued could mean ‘valid from different perspectives’ and based on different methods, entrenched sets of values, political power relations (this depends of the scope of research already done or still to do). To argue poly-centrism as a strength would be supported by a comprehensive review of the existing body of knowledge.

The book could ‘present’ and ‘instruct’:

I.

Presenting the history of thought of the last decades years of IAR, its basic idea of studying accounting in the context in which it operates (cf. Albrecht) and why this task is still relevant. ‘Did the challenges change and/or did the methods change within the last 30years of IAR?’

II.

Asking different ‘key researchers’ to present their point of view: main question(s), underlying theoretical understanding including theory-praxis relation, research methods, interesting pieces of work, some comments by them looking back on the progress of ‘their’ field, including critical perspectives on others (within and beyond the IAR-community).

Maybe there are different ‘generations’ becoming visible, from ‘setting the stage-years’ to approaches of differentiation and attempts to make IAR more transparent to its own scientific community, students, practitioners and interdisciplinary research partners.

III.

Common ground

There is a common theoretical background (ontology, epistemology, methodology), which is visible by analyzing the constructivist approach/vocabulary in use by us. There is a common task which is not at all a-historical: to explore practices of calculation, i. e. going back to the roots of accounting (and IAR as a distinct (deconstructive) perspective on it). Different studies talk ‘to each other’ across diverse conceptualisations of IAR-research, theoretical backgrounds, methods in use, and insights when being ‘translated’ on the basis discussed.

IV.

What is the future of IAR as theory-driven research when practices are not developing out of the blue and we are part of the problem? I guess, it is necessary to judge about the status of ‘intervention’/‘impact’ concerning our impetus to do and to present IAR(-findings). This may be a most instructive part of the book!

Allan Hansen

Stimulating stuff. Just a brief remark on our theories and definitions that we all seem to be concerned about, and a challenge for the ‘I’ and the ‘A’ from a theoretical and definitional point of view that seems to be hard to ignore at least as long as we do not want to throw away the ‘R’ (and I presume we will not).

The ‘I’ seems to stand for how we approach accounting research. But what does it mean – subjectivism, criticism, realism, relativism, constructivism? It is for sure that vagueness can be a resource (as Andrea argues) and polycentrism fuel (like Thomas declares). It reveals the multifaceted nature of accounting practices and leads us to places where we would never have been without it. And maybe it is the ‘powerlessness’ which Fabrizio refers to that will give us the freedom to go there. But on the other hand, why should we go there? If we want to make a difference then we also have to find some kind of I-principles. At least if we consider IAR to be a boundary object. Following Star and Greisemer (1989) IAR must be “plastic enough to adapt to local needs” (ibid.: 393) but also “robust enough to maintain a common identity across sites” (ibid.: 393). Thus, if we want to make a difference – relate our selves – robustness is also a part of the story. Another question is, to whom is it that we want to connect? To whom is it that we want to make a difference? Chris and Teemu got their ideas in their MBA classes. Rihab has others. And some of us are probably more attracted to the ‘A’ than to the ‘I’. What it is that drives the reflexivity that Anette and Albrecht suggest we pursue?

To define (as John, Teemu and Markus and others are calling for) and theorise seems to be inevitable at least if we want to be connected – if we want to make a difference and even to accumulate knowledge! However, to define and theorise does not mean that we have to abandon reflexivity and vagueness as a research strategy. They seem to be two sides of the same thing as Star and Greisemer suggest. At least if we believe that IAR should be considered as a ‘boundary object’.

Rihab Khalifa

I find it easy to agree with most, if not all, of what has been said above, and that is perhaps because we are still discussing issues in broad terms, which might be useful in a debate of this kind. Instead of delineating a political identity to IAR in contrast with non-IAR, I am wondering if it is at all possible that accounting could be an interesting (and perhaps independent?) field of knowledge and theory. One that overcomes commonly held categorisations within the research community.

My increasing interest in what is widely regarded as non-IAR has tempted me to seek to transcend the boundaries of what I know about the IAR versus non-IAR debate to argue that perhaps IAR should be conceived of as part of a broader history of accounting knowledge. Ideas from different research traditions could then be seen as mutually constituted and not exclusive. Many research traditions, which, for interpretivists may appear as "mainstream"⁵, have been seeking to add to our understanding of accounting in the names of usefulness, validity, interest in knowledge, debunking of received wisdom, all of which are of concern to interpretivists. For example, they have sought to derive accounting theory and prescriptions for financial reporting standards using economics (e.g. market efficiency research), understand contracts and managers' and agents' choices (e.g. positive accounting research), explore the value of financial reporting alternatives in experimental settings (e.g. accounting information processing research), apply symbolic logic to accounting settings (e.g. modelling research in information economics), develop economic models of behaviour to test in experimental settings (e.g. laboratory and experimental accounting research), etc. Like interpretivists, those research strands addressed gaps or absences in the literature. With respect to research objectives, is it not too artificial to distinguish IAR from non-IAR? Are we underestimating the overlap of vocabularies and research objectives?

I would like to come back to my earlier point about activism broadly conceived. It is all nice and well that we "reflect", "understand", and "interpret" but I do share Paolo's view that IAR, and perhaps non-IAR, have "failed to instil hope (of solving a problem, of seeing things differently, of 'getting things done')". For accounting research to make a difference to accounting, and to the world in general, we would need to articulate our values and communicate them in the choice and presentation of research and teaching topics.

Teemu Malmi

Let me be the devil's advocate for a while. I can not help smiling when I read about problems of communicating the value of IAR to the world. What value? Aren't we assuming here that we are smart and the rest of the research community, administrators, MBA students and managers are unintelligent as they do not understand the value of IAR. We have done this for years and achieved a lot (cf Chris). But still, others do not understand the value? Is it a problem of communicating the value? Or is it a problem of creating value (and to whom)?

I think this debate has been very productive in outlining who we are, what we do and what we have achieved so far in many respects, although many of these accounts

⁵ For interesting reading on the different strands of research, see (Tinker and Puxty, 1995; Bricker, 1993)

seem to represent what we would like to be and do, not necessarily how things manifest in research practice (cf. Markus).

I will briefly comment on the role and nature of theory. The question seems to be what theories to use and how. I am not sure if this is a good enough question in case we would like to instill hope. To continue Rihab's wondering, could it be that accounting is an interesting field of knowledge in its own right, in need of its own theories? Could we think of not only using theories to understand, but to combine and develop these theories further based on our interpretative research to better explain accounting related issues? Or to put it differently, what should be the outcome of an IAR program? Are we happy with multifaceted picture of accounting practice (cf. Albrecht)? An ideographic approach to research will certainly add a constant stream of new shades to this already complex picture. Ever more complex pictures hardly instil hope however. Complexity and explanations of uniqueness, case by case, will never make IAR appealing to practitioners. What is needed is cumulative knowledge on certain commonalities and relationships (cf. Thomas), formulated as theories of accounting practice. It is hardly a choice between nomothetic and ideographic approach. We could position ourselves somewhere in between those extreme ends of a continuum. Even if the first steps in the research program are purely idiographic, the next phase could develop a theory out of these findings. IAR theories capable of explaining accounting-related issues should be of interest to a wide audience and should facilitate the communication of the value of IAR.

Andrea Mennicken

Back to accounting

I agree with most contributors that one of the major strengths of IAR lies in its close engagement with those fields where accounting is actually done. Not theory, but practice – and connected ethical dilemmas, political controversies, challenges, organisational innovations etc. – should inform our research agendas. Theory plays an important role in making sense of what we observe (e.g. through providing us with useful analytical categories and concepts), but it should not predetermine the questions that we seek to answer. For me, “back to accounting” does not mean to develop new accounting-specific theories. As Albrecht has said, we don't need to reinvent the wheel. Rather, it means to look at how accounting is done, and what kind of actions, organisational and societal developments it enables (or disables). In so doing, accounting is inherently political (cf. Rihab).

Cumulating knowledge

To create a recognisable field of IAR, of course it is not enough to accumulate a series of isolated case studies. The main challenge lies in inter-linking them. It is important that more linkages are created between case studies dealing with similar issues but from different perspectives. On the other hand, we have to be careful, not to overdo it and create too much path-dependency for ourselves, because I don't want to end up identifying my own research agendas exclusively through the lens of gaps that others have left or defined.

Striving for relevance

What sort of relevance do we want to achieve, and vis-à-vis what world? Do we want to speak to practitioners? Other accounting researchers? Other disciplines? Deans?

Editors of American accounting journals? Whatever audience we want to speak to, for me relevance should not become prescribed by a set of standardised quality criteria used in positivist accounting research or by American business schools. We should not forget that we already have a series of respected journals (AOS, CPA, EAR, AAAJ, Economy and Society) that publish “less disciplined work” and are open to experimentation. To gain institutional strength it is important not only to look at those who don’t respect us. It is also important to see who already recognises us. IAR has already gained considerable recognition by some disciplines (e.g. sociology, organisational studies, political science, studies of science and technology). It might be worthwhile to build on this and seek further linkages not only within but also outside accounting research.

Anette Mikes

Our discussion is converging towards identifying some important challenges that IAR faces. Two main challenges worry me in those fearful moments of existential angst. One concerns the relevance of what IAR scholars do, the other concerns the accumulation of knowledge gained.

As for *relevance*, the first good news is that IAR is particularly well suited to stay relevant. It is true that many studies kick off with the observation ‘little is known about...’, however, to me, this is quite refreshing. One of the necessary conditions of ensuring the relevance of IAR (or any research body) is that we study the role of novel practices within contemporary settings. Given the pace of change surrounding calculative and control practices (social, organisational, technological, etc.), novel studies of accounting will always be required to address emerging issues.

The second good news is that there is a growing audience of researchers and non-researchers to whom the findings of IAR are of interest. Maybe I should have put it differently: the interest lies in selective, filtered-through-IAR findings. The rise of benchmarking organisations and senior practitioner networks tells me that there is a need for the exchange of practice stories that allow these practitioners to vent their frustrations and anxieties, and to ruminate over the complexities they face in the ‘swampy lowlands’ of practice. In my limited experience of communicating the results of some classic IAR studies (as well as of my own humble efforts) outside the accounting conference circuit, there is indeed an interest in what we do. Or to be more accurate, in what other practitioners (whom we observe) are really doing. This is because senior, experienced practitioners can also understand, appreciate and be curious about the espoused and in-use theories of the other...

As for the problem of *content accumulation*, there is a tension between this, and our parallel efforts to observe and track diverse and emerging practices. The more directions we branch out to and the more diverse research frameworks we adopt, the more difficult it becomes to synthesise. However, similar concerns trouble other research paradigms too. Despite the diversity, I believe we could try to identify a number of themes in IAR that allow us to pull together some propositions, in a similar vein to Chenhall (2003)’s overview of research findings from contingency-based research. For example, as a start (or experiment even?) one could track some popular research themes, e.g. control systems working under conditions of high uncertainty /

environmental hostility etc. No matter how ad hoc the clustering is, once thrown together, we might well find that our IAR studies start ‘talking to each other’.

Martin Piber

After all these stimulating ideas I will summarize aspects of common ground on what we – as interpretive accounting researchers – think that we are and what we want to do. Furthermore I will try to give some hints for an answer to the question of MBA-students and other practitioners raised by Paolo: “What shall I do tomorrow, when I go back to work?”

IAR tries to understand accounting practices in different social and organizational contexts (Hopwood 1983). The main differences to Positive Accounting Research (PAR) are different ontological and epistemological assumptions. These differences concern knowledge, the understanding of social reality and the relationship between theory and practice. IAR uses a processual lens and constructs idiographic theories. Therefore it is possible to handle paradoxes, ambiguities and recursive effects.

On this basis, various proposals have been made on how to proceed with the promotion of IAR: forming theories, cumulating knowledge, generating textbooks, launching a journal, fabricating a disciplinary space, strengthening our position in the labour market and the citation industry, developing quality standards and performance measures, and establishing a database for qualitative case studies. However, these ideas are mainly oriented towards academia. The most tricky issue seems to be how to communicate the value of IAR to practitioners asking the question raised by Paolo?

On the surface it seems to be a problem that we have no recipes and no models to deal with problems in a standardized manner. On closer examination it can be seen that practitioners are often frustrated with the use of these recipes and models. As a consequence they suppose that the problem is a limited understanding. But in the end it turns out that these kind of theories are never “ready-made” for practical problems. Trivial recipes only exist in the fiction of unreflected theories. Therefore a whole industry takes care of “implementing” those recipes and models. With idiographic theorising IAR takes these issues into account. It can open black boxes, which are, and have to be, closed in predefined models. So IAR can, and should be, more self-conscious about its ability to take into account complex contexts and unique problems – especially in relation to practice.

Paolo Quattrone

What is needed to instil hope? I am happy to attempt giving an answer. In many of the voices heard in this pieces (included mine) there is an aspiration to become a discipline and to have some institutional forms through which IAR can become visible and referred to as something which has an effect, as something which is able to gain a place in the space of accounting academia, practice and why not politics? What is sure though is that we do not want to become a ‘discipline’ in a modern sense, i.e. in a way which views theory as an attempt to subsume a series of empirical instances at the expenses of that Other which does not fit into the picture. This is, for instance, what the stakeholder approaches in accounting do: they depict actors (e.g. ‘the investor’) and assign them a role (in the investor’s case maximising the return on

the investment) and accounting is supposed to serve this role objectively and neutrally. Never mind if the investor is close to some religious organisations, political parties and other constituencies which have a different agenda (see the Parmalat scandal): these ‘others’ are excluded from the understanding and accounting is simply a tool. We all know this (including the mainstream accountants and the practitioners, even without IAR). What this sort of theory does is to highlight what is normal (an investor seeks profit and accounting serves to calculate it) and exclude what is diverse (it actually may not and accounting may indeed serve thousands of other purposes).

Where do I want to get to? I would like to support those calls in this piece to go back to accounting. We do have a discipline and one with a multi-centenary history and practice, but, we are not able to propose an alternative way of accounting and accountability. Instead of taking objectivity and impartiality as the lighthouse which guides our journeys into financial transactions (see the IFRS project) we need a different theoretical approach, a different way of reporting which makes the impossibility of giving an objective account of financial transactions the cornerstone principle of theorising.

I agree with those before and after my piece who call for accumulation. I may be wrong, but I think IAR is mature enough to build on the (relativist) epistemological, sociological and political understanding of accounting developed in these past thirty, and then some, years. Why do not we begin to develop forms of accounting practices which assume the impossibility of representation (pace IFRS)?

Then we would have something to offer, something to write in textbooks, something to build institutions upon (I fully agree with the need for institutionalization if the institution can learn from IAR). In other words we need something people can refer to in positive but not positivist ways, and we may begin from rethinking the label this piece has referred to: IAR.

Tobias Scheytt

Well argued, Paolo: an argument that finally leads into a complete deconstruction of our topic. And, clearly we could finish the discussion on IAR at this point, find another label and start to develop and promote, say alternative or ‘truthful’ (what is this from a (de-)constructive perspective?) accounting tools. However, I doubt that we, or even the ‘whole’ community of non-mainstream accounting researchers, are powerful enough to establish such alternative tools that practice could relate to in a positive way. We would underestimate the ‘intelligence’ of accounting practice and accounting practitioners if we think that the process of developing such tools would be disregarded. Rather, such a movement would be recognised, the proposals analysed and the systems and ideas adopted in a different and, say, instrumental way by the accounting industry. The history of accounting shows numerous examples (e.g. Intellectual Capital Statements) of how developments at the margins of the discipline develop into elements of the standard repertoire of accounting techniques. The idea behind the research programmes of professional bodies is, among other things, to explore future developments in their respective fields. More important, I don’t know whether the invention of new techniques and tools is possible, given the methodological and epistemological position ‘IAR’ (or how ever it might be labelled) refers to.

Hence, I think that the novelty of IAR lies not in its – questionable – potential to develop new tools or instruments. Rather, I'd like to take up and extend Anette's argument on relevance that we should see who already recognizes us. One observation that I made in contacts with practitioners is that telling a good and interesting story about the experience of an implementation process of a specific accounting system in a specific organization is worthwhile for, at least some, practitioners, and not only for the small community of interpretive accounting researchers. This interest stems from the curiosity and innovativeness that is also a characteristic of knowledge-seeking practitioners who aim – in perhaps a very simple way – to improve their praxis.

But this observation can – once again – be countered by clear observations that the impact of IAR is generally weak compared to MAR. Hence, structurally expressed, I conclude that a number of paradoxes lie at the heart of our discussion (and the heart of IAR). We argue that much has been achieved in IAR, that there is a well-developed research tradition and a community working on common grounds of values, methodologies etc. On the other hand it is common sense that IAR has no fixed *gestalt*, has no impact, is somewhat muddled, not accepted by MAR etc. However, as we all know, these 'flip-flop' arguments are only a problem for normal science approaches. If we consider the sociology of science, these flip-flops can be identified as a characteristic of a mature discipline that nevertheless has the potential for future development – isn't it a good outcome, hence, that our discussion has to be continued?

Final thoughts: Thomas Ahrens & Chris Chapman

We have arrived at the end of our second round. If nothing else we have shown that one thing that characterises IAR is a willingness to engage in argument over what is best. It is difficult to say 'no' to knowledge, critique, reflection, relevance, (= influence?), etc., but they continue to mean different things to different people. We are not in a position to 'conclude' at this point (without causing a minor bloodbath). We stopped after two rounds of our quick-fire exchange, not because we felt like leaning back in the comfortable knowledge to have said it all, but because we felt that the sequential 300-words-plus medium would not yield many surprises in a third round. The debate must continue, perhaps in more focused papers with more focused groups of authors, to further explore some of the many connections whose contours became visible in outline through this debate: connections between relevance and experimentation, rigour and activism, embedding in wider literatures and discipline, translation and the accumulation of knowledge, teaching and identity formation, action and critique. This was *a* beginning—one amongst many that have sought to contribute to an emerging map of the future of interpretive accounting research

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