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This is the final peer-reviewed author's accepted manuscript (postprint) of the following publication:

Published Version:

Zan, L. (2016). Complexity, anachronism and time-parochialism: historicising strategy while strategising history. BUSINESS HISTORY, 58(4), 571-596 [10.1080/00076791.2014.956730].

Availability:

This version is available at: <https://hdl.handle.net/11585/593347> since: 2017-06-01

Published:

DOI: <http://doi.org/10.1080/00076791.2014.956730>

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Complexity, anachronism and time-parochialism: Historicizing strategy and strategizing history

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Abstract: The focus of the paper is the relationship between history and strategy. Though little interest can be found in mainstream management journals, the hidden relationship between history and strategy is looked for in a double sense. On the one hand, the paper will try to historicize strategy, questioning the alleged discontinuity that is normally used to explain the emergence of the notion itself. More than changes at the ontological level, it is changes at the perspectival level, which explain the establishment of the new area of research. On the other hand, the article discusses possible implications for broader historical research when taking seriously into account some of the hidden distinguishing features of strategy studies, strategizing history.

Key words: Business history, accounting history, management history, protoindustrial setting, Venice Arsenal, strategic change, global history, great divergence, China administrative history

Introduction: rediscovering history in strategy research?

An Italian journalist once asked Giovanni Agnelli, charismatic CEO of Fiat during a TV interview: "Do women like you?" With his unique upper class way of talking (totally missing the "r" phoneme), he promptly replied: "Whatever I think in this regard is largely irrelevant. You should ask women if they like me". Discussing methodological aspects in our own research puts us in a similar position: "What we think we do" risks to becoming a meta-bias, a biased perception of our own biases. Given the naivety of mainstream management in relation to historicity, however, such a risk is worth taking in the area of management/organization.

For sure, there used to be an interest in history within the strategy field in previous decades, in many regards: for instance in European traditions prior to the establishment of the American model of business schools;¹ for instance in business education also in the US.² This is particular true for strategy research, where indeed Chandler played a crucial role in the transformation of Business Policy into Strategic Management.³ All of us – as strategic management scholars, not as historians – were quoting Chandler in the late 70s and 80s.

Such an interest has vanished over time, partly as a phenomenon even within the US,⁴ and more generally as a side effect of Americanization (see the whole debate on continental vs. analytical model).⁵ In particular, Kipping and Üsdiken⁶ identify a process of "scientization" within strategic management mainstreams.

That there is a swing back to rediscovering history in management research is an open issue. There are institutional signs in this direction. Special issues on history got some attention in recent

years,⁷ not to mention the emerging of new journals (e.g. *Management and Organizational History* [M&OH]). Attention to historical investigation can be found in international congresses.⁸ Some sort of manifesto and call for historical attention emerged in business education to overcoming short termism which is acknowledged as one of the elements in the worldwide financial crises.⁹ Finally, some scholars identify a "historic turn" in the discipline.¹⁰

Without ignoring this optimistic signs, the lack of historicity is still a serious issue.¹¹ I will turn now more specifically to the strategy area, though indeed the reference to it as a "worldwide" phenomenon would require a paper on its own, with its path dependent institutional history. The description of the evolution of management system according to Ansoff,¹² is a good example of the unproblematic attitude by management scholars.¹³ This is a simplistic linear succession of clear cut "tools" over time (systems and procedures manuals, financial control, operations budget, capital budgeting, MbO, LRP, periodic strategic planning, strategic posture management, strategic issue management, weak signals management, surprise management); a very narrow definition of time span, as if nothing was there before 1900.

If Ansoff was writing almost 30 years ago, a similar lack of historical awareness about the discipline emerged at the Euram conference in Liverpool, in a speech which was particularly fresh and interesting on other aspects by Jeffrey Pfeffer,¹⁴ which was non the less referring to the 60 (sixty!) years of management. This is particularly ironic, given the title of the speech ("Renaissance and renewal in management studies"), for those that believe that indeed Renaissance was a crucial period for the development of what we now refer to as "management" knowledge (see below).

Reasons and consequences of this lack of historicity could be identified.¹⁵ Suffice it to remind here how the phenomenon of fashions & fads in the field of management studies makes the accumulation of knowledge difficult.¹⁶ The whole area of strategic change and longitudinal research (on which I will come back later on in the paper) has achieved interesting results during the mid-70s and mid-80s,¹⁷ without "new" contributions later on, with the risk of being totally unknown by PhD students that seem to be scared to quote anything issued before 2000.

What disturbs in such a situation is not merely the total lack of intellectual curiosity about earlier developments and the historicity itself of management as a body of knowledge. What is worse is that in this way management scholars risk not to understand the essence of the identity of their field; and, in parallel, their potential contributive value to historic research.

The aim of this article is to problematizing the relationship between strategy and history, in a twofold direction. In the second section, I will try to use historicity as a way to address some of the weaknesses of the field, and the need to understand the literature under a historiography lenses (in a word I will try to historicizing strategy). In particular, the focus is on three aspects: the issue of time parochialism of strategy studies, and some problems with regional aspects and disciplinary boundaries. In the third section, I will try to investigate some of the specific ways of doing historical research as a strategic management scholar (in a word strategizing history). Drawing on my fieldwork, and on practical issues in doing research, I will trying to investigate if there is any specific element that distinguishes this research from more general approaches, such as business history. Conclusions follow.

Historicizing strategy

When looking inside the strategic management field, the impression is that the "historic turn" is perhaps more relevant for organizational/management studies as a whole than for this specific community of scholars. Things have not changed that much in terms of both history of thought and history of practice.¹⁸

In terms of history of thought, strategic management appear to emerge "out of the blue", never questioning the debts/links with previous bodies of knowledge (not to say the genealogy of strategic management). Nor is there some sort of reconstruction or sedimentation of early contributions in the field. What is left of tools and notions that were so crucial in the 70s and 80s? Are they intrinsically obsolete, or is it the process of forced innovation that causes such a collective forgetting (think for instance at the role of BCG and McKinsey matrixes; or the crucial issue of "defining the business" by Abell; or the SWOT analysis, or the even more sanctified Porter's approach). Interesting enough, there

are chapters in strategic management books focusing on "what do we mean by strategy", which is a sign itself of the complex status of such a fundamental notion in the field. However, very rarely the issue of why, how and when this notion emerged and evolved is addressed. A situation of fear of history can be pointed out: historical reconstruction is likely to weakening the alleged prescriptive value of the field as a whole, while costing time and energy that cannot be "charged" to the "customer" (do not forget the role of gurus and consultancy firms in the diffusion of strategic management, as rarely in any other field).¹⁹

The situation in terms of history of practice was, and still is, even worse. Very little archival research can be found in the strategic management literature (and indeed in management field as a whole);²⁰ in any case, recent periods are privileged, while the importance/interest of looking at more ancient realities seem marginal.²¹

Drawing on my previous research on these aspects, I would like to focus here on three main aspects that I still find under investigated in the literature: the controversial relation between anachronism, time-parochialism, and the issue of complexity; some "regional" peculiarities of the field; a question about disciplinary boundaries in relation to historical research and management.

Anachronism, time-parochialism, and the issue of complexity

A crucial issue to address is the epistemological statute of strategic management from an historical point of view. To put the question in simpler term, one could argue what is new in the field / in the notion itself of strategy. Are we talking of new aspects at the ontological or the perspectival level? The emphasis on strategy is because new phenomena in business competition and organizational life; or is it mainly an issue of lenses used to look at these phenomena?

Booth and Rowlinson²² reference to presentism is well posed, and requires some additional development, drawing on two "dangerous" issues in historical research: anachronism and time-parochialism. Anachronism is perhaps one of the capital sins in historical research. Putting things "out of their corrected historical time" is the worst thing you can expect from historians. Yet things are not that easy, as a discussion at the general level of historical research has pointed out.²³ What I am particularly interested here is a phenomenon which is in a sense the opposite to anachronism, or perhaps an unusual form of *present centeredness*:²⁴ that is, taking the present as unique and totally different from the past, as if our ancestors were living in an easy and unproblematic world compared to our times – a sort of time located *bon sauvage*, so to speak. It is an unusual example of anachronism because normally anachronism tends to projecting the present and its characteristics over the past. Here, on the contrary, the presumption of uniqueness of the present is projected in a representation of the past perceived as radically different. Ansoff is again a perfect example here, when explaining the emergence of the notion of strategy as a reply to a new world, "turbulent", "discontinuous", "uncertain", as if previous worlds were by opposition calm, continuous predictable and so on.

Indeed the discovery of the Similaun mummy has proved that technological knowledge characterizing his society 5.300 years ago was all but trivial.²⁵ Eighteen kinds of wood were used in his equipment showing skills in relating to mother nature that would seriously challenge any of our sever orienteering and survival games; the beautiful axe reveals a sophisticated capability of melting cupper that is impossible to achieve for "experimental archaeology" (isn't it a nice oxymoron in itself?) of our time. Indeed whenever one opens the archives or discovers objects of the material culture of the past, the initial attitude of "arrogance of posterity" tends to vanish rather quickly. The past was not necessarily easier, more stable, simpler, and so on. Complexity, uncertainty and ignorance (echoing Loasby's work)²⁶ were there, even for Oetzi in his time, for the Romans, the Qin emperor, the Venetians, the Ottomans and so on. From that point of view, the radical lack of any link between management/organizational research and heritage/archaeological disciplines is an enormous waste of intellectual, theoretical and methodological opportunities. This would allow testing our tools, approaches and notions in radically different contexts, while learning to accept radical contextual differences, overcoming simplistic and "presentist" obsessions, as for instance the focus on Fordism and post-Fordism as "the" meaningful discontinuity.

At this point, however, the issue of how strategy field evolved in relation to the more general debate on decision-making needs be taken into account, from the means-ends model to more problematic understanding.²⁷ To be sure, the link of such a debate with broader philosophical and epistemological movements is not sufficiently addressed: just think about the crisis of the rational choice model, and the emphasis of unanticipated consequences of human action by Merton, or the unintentional consequences by Popper and Hayek.²⁸ Nonetheless this debate, from the earlier discussion on bounded rationality to the more articulated considerations of both political²⁹ and cognitive aspects³⁰ is perhaps one of the most important debates in our fields, whatever boundaries between organization, management and strategic management one prefers in processual research.³¹ In the strategic management literature, this has resulted in the establishment of a rather consistent research area on strategic change, with a particular understanding of strategy as actual behaviour, as conducts, as opposed to choice and decision,³² an aspect I will come back later in this article.

Despite differences characterizing the decisional field in any specific case, according to the decision making debate one would expect that, *in principle*, issues such as uncertainty, ambiguity, bounded rationality, complexity, emerging strategies and behaviours, unanticipated consequences of human actions indeed are constitutive elements of decision making. Of course, any context (in space and time) and any “decisional field”³³ are unique. Of course, the world today is significantly different from the world just 10 years ago, no doubts. Yet, some commonalities characterizing decision making are still there. In other word, while Ansoff and others would refer to new changes affecting the world of business and organizations as ontological, my suggestion is that these changes are more perspectival, i.e. changes affecting the way in which we – as a community of knowledge producers – are looking at social phenomena in less simplistic ways.³⁴

As such, the perspective could be used and applied to decision-making fields well before our time. This is not anachronism in that we are not “putting in the mouth” of actors of other time our notions (we are not projecting, say, the prisoner dilemma in Robinson Crusoe conscious understanding of his context). We have the right to make sense of his situation and his sense making too, utilizing a game theory approach, if we like it. On the other hand, one can refer to the discovery of the Americas as one of the most important example of “emerging strategy” in human history.

At first sight, this could sound strange, in a field – like management studies – which is biased by its way of representing things “in the mind of CEOs” or “in the shoes of managers”. However, complex notions we are dealing with as “strategy” can be applied out of this radical (and apparently naïve) hermeneutic view. Indeed Axelrod speaks of “strategy of viruses”,³⁵ as a notion to make sense of the succession of events that characterizes their behaviour, whether or not viruses do think or plan (Mintzberg would say “stream of actions”, overcoming his previous misleading expression of “stream of decisions”)³⁶.

In a way, the separation of actions from thinking, the problematization of the relation between these concepts and the implication of such an intellectual position is one of the most interesting elements of innovation in strategic management, though largely unnoticed by scholars themselves. The evolution of the notion of strategy itself, a signifier for many significances, is here involved, though rarely addressed by the literature.

The initial development of the strategic field following Chandler 1962 work is well acknowledged (see above). However, the development of the new area of research soon overcame the limit of Chandler’s way of looking at strategy (in a linear and mono-directional way, as choice, as the whole strategy/structure debate)³⁷. Indeed an important literature was giving increasing emphasis at strategy as results, as action, as conduct, as behaviour. Again, think of the notion of emerging strategy by Mintzberg; the business idea by Normann; the notion of strategy as configurational order of the firm by Miller; or the dialogue between Pettigrew and Mintzberg on decisions.³⁸ In a word, strategy as a way of making sense of distinguishing features of a firm (or more in general of an organization), as an ex-post category of the analyst, with an interest on “implicit” or de facto strategies.

Two elements are here involved from a theoretical point of view, not always explicit: on the one hand the issue of the analytical complexity; and on the other the relational complexity. While the latter is usually present in the discussion, the former risks being lost somehow (see for instance Mueller et

al.³⁹ and their focus on “making sense of politics and power issues”, i.e. what is here referred to relational complexity). “Analytical complexity is here intended as the inner complexity that the context can assume from the actors' viewpoint, given systemic interrelationships between its components which are difficult to understand and master conceptually for the actor in terms of clear-cut and stable cause-and-effect chains”.⁴⁰ What matters is the relative freedom in combining input, throughput and output, echoing the language of the systemic approach (see also the concept of causal ambiguity by Rument⁴¹ as one of the isolating mechanisms in firms' competitive inimitable behaviours.)

An important element in this discussion is the specific ways in which “strategy” is referred to in the strategic management field as opposed to more general approaches in sociology or economics. “Thus, in abstract terms, a game is defined as ‘strategic’ when one actor's behaviour takes into account the moves and reactions of others... (i.e Goffman) ...In economics, this perspective underlines oligopoly theories and other particular applications of game theory”. In the field of strategic management, however (and here focusing on firms to make it simpler), given “the complexity and individuality of the systemic configuration of each firm (the ambiguity of relations between components and single aspects of the analytical complexity of the firm), the conceptual category of ‘strategy’ plays the role of interpreting the particular *logic* in the functioning of the system, that is, the role of reconstructing and synthesizing the overall specificity of firm/ environment relationships. In other words, within this conceptual context, strategy tends to provide a representation of the *order* of such complex system”.⁴²

In the incredible melting pot of disciplinary perspectives taking part to the field (from sociology to psychology to economics), the strategic management research was transforming a simple/simplistic notion of choice into a more problematic one of conduct. Its reconstruction is based on a difficult sense making process about the analytical/relational complexity; where, put it simply, the notion of “fit” played – and still plays – a crucial role, well beyond mechanistic uses in the “contingency approach” era.⁴³

Regional aspects (or ethnocentrism)

The worldwide dominance of North American management is a phenomenon that gave rise to a very interesting research stream on the Americanization of business education, and the establishment of the business school model.⁴⁴ Still, the numbers provided by Engwall⁴⁵ are astonishing: 6 % of “writing space” is left to Europe; Asia; Mexico, Central and South America; and Africa.

An issue that is not totally addressed in historical debate is how this have an impact itself on ways in which we do (and publish) historical work. In accounting history in the recent decades, a huge debate took place against the Anglo centrism characterizing the accounting history literature.⁴⁶ Nothing really changed, to be honest, if not that papers by English speaking (and usually only English-reading) authors are now more or less carefully specifying in their subtitle or in a footnote that they are focusing on English-speaking literature.⁴⁷ What happened before the Americanization of the world is substantially ignored (see for instance the ignorance amongst North American OT scholars of the French tradition, such as Crozier and Friedberg⁴⁸). It would be nice to address the question: to what extent a similar cultural colonialism is affecting individual areas of historical research (economic, business, management and organizational history)? To what extent a critical questioning of such Anglo centrism has been developed?

The question is not that colonialism has a tendency to be disturbing for those that are colonized. Indeed ignoring/removing what was before the process of Americanization (or what could be out of this process) is likely to make it difficult to understand elements of differences at the content and institutional levels of the new situation: the overall process of forgetting by doing makes it hard to compare the present situation with other possible alternatives.

Curiously enough, even critical management scholars are not so warmly interested in these aspects.⁴⁹ Very little, if any, attention is paid to business “thought” in previous periods and elsewhere, may be with different ways of structuring sub fields, may be with less separated areas compared to what we are now familiar with (accounting as separated from management, from marketing etc.). Some exception can be found,⁵⁰ but curiously enough ignoring parallel debates taking place at the margin of Anglo-dominated accounting history. Even more this is the case with management practices: that there

are different ways of running entities and long term administrative trajectories over history is something totally ignored a part from some naïve reference to Eastern patterns during the period of Japanese boom. The small number of papers about China and the Far East is also a problem with business/management historians. To be honest, this attitude takes place already within the Western world, as an example of internal imperialism, well before totally ignoring what is outside the West.

Disciplinary issues

Investigating the relationship between strategy and history – as the focus of this article – opens up some serious questions from a disciplinary point of view regarding "strategy", especially if we refer to specific approaches or research areas (as for instance strategic management). Suffice it to underline here two main aspects concerning the historical location of strategy. First, the establishment of similar approaches and research areas is part of the process of Americanization on business education:⁵¹ the notion of strategy was not used explicitly as such in the *Economia Aziendale* tradition, without meaning that similar issues were not treaded under different perspectives and language. Second, even today in the Anglo-American context a clear distinction of sub field (management, general management, strategic management, organizational theory, marketing, accounting) can be largely questioned, especially for those working "at the margins" of these institutionally defined areas. For, usually, categorizations and organizing principles are consistent when varieties within classes or entities are fewer/weaker than varieties across them: which happens always, out of the mainstreams.

Such an intrinsic ambiguity about contents versus institutional establishment of research areas has of course a greater impact when investigating the relationship inside historical research. Are differences between economic history, business history, management and/or organizational history, accounting history related to clear cut specialization on different topics, or are they the result of a largely random institutional process in their own histories? In addition to academic sociology, is there any "real" reason to distinguish between these aspects as if they were "objectively" distinct and separate? Is it a question of similarities and differences of levels alone? When going back to read De Roover⁵² – or the work by Forsellini, with its beautiful title⁵³ – I have troubles in seeing differences at all in the kind of historical research I would like and try to do, as a management scholar with historical interest. On the contrary, the total lack of interest on European bureaucracies in the Renaissance period makes me so distant from Chandler and its "vested interest" in ignoring the work of Lane. His friend Fredrick Lane had already discovered and published in English since the 30s what was happening in Venice some 3-4 centuries before the industrial revolution.⁵⁴ Indeed, these fields of historical research tend to ignore each other, working with individual and separate institutional tools, such as journals, conference, literatures, where cross quotations are rare.⁵⁵

The ambiguity seems to me particularly intriguing when addressing the issue of the relationship between Business History and management/organizational history, even when acknowledging the impact of the former (and Chandler) on the developments of the latter during the 60s-70s,⁵⁶ and the lack of historical interests of the great majority of strategic management and organizational scholars. (Indeed, the difference between "management" and "organizational" history is hard to understand, unless we refer to the obsession with the history of thought that has characterized so far North American approaches within the former, compared to the historic turn in organization field: for instance compare Wren with Booth and Rowlinson⁵⁷. On the other hand, I am aware that the term "management and organizational history" is a very ambiguous neologism. Nonetheless, I will use it freely for its promising impact on the debate: see the relative new journal *M&OH*).

A recurrent, somehow implicit, position seems to refer to a sort of division of labour between the two communities of scholars, business *historians* versus management *theorist*.⁵⁸ Even when calling for more interaction between the two communities, the first is expected to developing history, the latter theories: "Business historians...address issues of central concern to researchers in management studies and business administration".⁵⁹ Though a bit more articulated, this is also the position of Kipping and Ûsdiken,⁶⁰ with their reference to "management history" merely as history of management thought, and they call for greater interactions in the sense of how (business) historians could benefit more from development in management research.

My disagreement on this specific point is manifold. First, I do not agree that management scholars develop or should develop theories. This is mainstream understanding of management research, which is, indeed, theory driven (Hambrick critically refers to the "devotion to theory"⁶¹). There is an obsession of finding what the theoretical contribution of any piece of work is. "The" theory, a sort of reification of literatures, debates and conversations about a variety of topics is never problematized. If you are working in sociology looking at new forms of racial segregation, people will not necessarily ask what is your contribution to theory, but rather your contribution to a better understanding of the phenomenon. Indeed, several times when doing field research it is easy to get the referees' comments "so what", or the charge of being "descriptive" (would anyone charge that sociologist of being descriptive on new forms of racial segregation?). There is a growing concern on this aspect of mainstream management. Pfeffer himself, in the paper I was previously criticizing for the lack of awareness on history⁶², provides a very good criticism of this attitude, and calls for the need of fresh, phenomenon driven kind of research in management. "Our field's theory fetish...prevents the reporting or rich details about interesting phenomena for which no theory yet exist".⁶³ "The target of phenomenon-based research is the early capture of a phenomenon for theoretical work and the development of effective research designs. However, this approach is a forgotten step in management and organisation science. Although important contributions to our discipline began here, many authors claim that we have become theory-driven and thus tend to forget how to view a phenomenon in its totality".⁶⁴

In this sense, Kipping and Üsdiken⁶⁵ are right in saying that management is theory oriented. However this can be seen as a degenerative process by some management scholars, which in turn is related to the lack of any interest in understanding phenomena in themselves (contrary to the title of the Academy of management conference in 2010, we do not care), where description is irrelevant *because* historicity does not matter, and where the only legitimized way of presenting research is generalization. (Here I am in agreement with Kipping and Üsdiken, when referring to the scientization of strategy).

There is also a second disagreement with the "division of labour" between business historians and management scholars, as seen above: the fact that management/organizational scholars could be bearers of specific ways of looking at historical research is not perceived as relevant in this position. (I must confess some difficulties in understanding whether Kieser⁶⁶ could be positioned amongst the two parties. Not only OT could benefit from historical research, but there are rooms to "use of organizational Theory in Historical analysis". To what extent this means referring to "organizational" historians is somehow unclear.)

An alternative view is however in the air. Kroeze and Keulen explicitly refer to "organizational" history and historians.⁶⁷ Even more directly, Thomson raises "The case for management history", with the idea of treating it "as a discrete subject area, although one closely related to business, accounting, labour, and industrial history...It seeks to concentrate on the nature, process, and practice of management as an activity within the organization".⁶⁸

A part from a disturbing reference to "British managers" (who cares? isn't it a much broader issues?), and some aspects that could be questioned – such as the alleged specificity of management history for its focus on salaried managers as opposed to the focus on boardroom by business history – the call for a focus on decision making is interesting. "An ideal perspective of management history starts from those decisions, identifies their evolution and dispersion through a timeline, observes the nature and characteristics of the people making them, and broaden out into studying the wider implications of this occupational group".⁶⁹

To me, more than an issue of *who* (owners/managers), the serious elements addressed by Thomson is the *how*: how decisions are made, how things happens, looking at management as practices and as a body of knowledge; or, in other words, an emphasis on managing more than management (whoever manages).

The hidden implication in this position is who has the knowledge to do a similar investigation? Is the management expert bearing some specific additional knowledge when doing historical investigation? Thomson's view is radical, though not really spelled out: "Ideally, management history

should come from within the study of management".⁷⁰ Indeed, I will draw on these concepts in the second part of the article, reflecting on some of the specific features of doing historical research from a management study perspective.

Strategizing history

Drawing on my own research and experience, I will try to reconstruct how similar questions crossed my work as a (management? organizational? accounting? business? economic?) historian: in any case, with a deep concerns about the non-linear, unpredictable political and cognitive essence of decision making processes. In a word, coming to historical research from the background of "bounded rationality" and the related interest as an analyst on "fits and misfits", how were my studies influenced by such a position?

I am forced to such an exercise of self-referential investigation (still with Mr. Agnelli in mind) because what is interesting is not only (or that much) our own perception of differences in abstract, general terms: rather, it will be useful comparing how we – from different backgrounds and disciplines – solve very practical research questions in doing our historical investigation.

Toward a History of Accounting Histories

I first approached the field of accounting history when I was commissioned an item for an encyclopaedia on business administration by some Italian colleagues. What was supposed to be a rather simple task of putting in popular terms within 10 pages the content of a (supposed unproblematic) long debate by Italian accounting historians happened to become a rather interesting research on historiography and nationalism.⁷¹

What is shocking is that histories of accounting history tend to differ radically in different contexts. Periodizations, notions, authors have very little in common when comparing for instance the Italian tradition and the English written textbook in this area. "Accounting is Italian (*La ragioneria è italiana!*)"⁷², you can find in the text of Italian scholars, while English speaking ones have a disturbing attitude to quote only works written in English (in a word: "after Pacioli, nothing"). This suggests first that ideas do not take place and diffuse in conditions of "perfect competition": competition of approaches and traditions has a lot to do with political issues (the North American dominance after WWII)⁷³; and with difficulties in coping with knowing, understanding and comparing a variety of local approaches and traditions. The analyst too – the accounting historian in this case, as an actor in his/her own organizational content, the community of knowledge producers⁷⁴ – is boundedly rational: both the chauvinist Italian scholars and the imperialist English speaking ones.

When focusing on the Italian historiography, bounded rationality is missing from the dominant narratives in other important concerns. On the one hand, there is a linear description of the evolution of the present "doctrine" from the "precursors" and the "founding fathers", as if the present state-of-the-art could not be other than deterministically shaped by the past, in a situation where neither a significantly different future is perceivable. On the other hand, there is an implicit unproblematic linear relation between ideas and actions. Theories matter and whenever theory can be found no question is posed about their relations with practices. Indeed what such a view misses is, for instance, that accounting theory is something that emerged after a couple of centuries of a practice (double entry) without a literature, followed by a century or more of literature without a theory, and only at this point, in the 17th century, earlier accounting theories emerged. What kind of accounting practices were at place in the 17th, 18th, and 19th centuries is totally out of interests of the dominant accounting historiography in Italy. With a few exceptions,⁷⁵ accounting history is reconstructed more in libraries than in archives. Theories more than practices are looked at.

What to say about the Anglophone historiography? No interest at all on what was written elsewhere, and in other languages? Curiously enough, this strong "power" element is not even perceived by critical scholars, whether Foucauldian, new accounting historians or whatever.

Management and Accounting Discourse at the Venice Arsenal, 1600

This is perhaps the most important piece of history I have been working on.⁷⁶ The starting point was the

research by Venice historians, which already addressed the issue of important administrative changes toward the end of the 16th century.⁷⁷ While management scholars have been totally uninterested in a similar debate, our aim was to reconstruct the evolution of debates and conversation about what we would call nowadays “management” issues, and what was referred to as *discorso del maneggio* at that time (at least this could teach to hundred thousand students the etymology of the word “management”⁷⁸).

We first look to the four main archival sources referred to by Venice historians (about 1586-1594). Second we discovered that these were part of a broader and systematic *series* of reports that were established in 1580 to give account of the degree of achievement of the goal of having a reserve of hundred galleys in the Arsenal ready for war, in addition to the fleet in the sea. Third we tried to make sense of the contents of these reports, looking at discursive regularities and changes across a period of 70 years, up to 1640. The evolution of a discourse about managing the shipbuilding activity can be described, with the inventions of new concepts and metrics such as the notion of cost, work in progress; the explicit design of organizational aspects such as the work gang; the establishment of forms of planning practices; and other logistic aspects.

What my analysis – as a management historian, or less ambiguously, as a historian with deep knowledge of management as a body of knowledge; or, again, as a management scholar with strong historical interests – add to the picture by general historians? What is really curious to see is the differences between Venice historians themselves in their judgment about changes in the organizational-administrative capability. According to Lane, they simply invented modern management (“with “ideas of efficiency which might be expected of factory managers in capitalistic industry”⁷⁹); according to Concina, the mess that still was going on can lead to an opposite conclusions: “administrative capabilities seem to be lacking”.⁸⁰ Both are statements that would be difficult to share by a “good” strategic management scholar. Unanticipated and unintended consequences are always at stake; chaos and disorder and not the negation of order. It is the dialectical relationships between them that address an issue of “degrees” of order (or between intended and emerging strategies, in Mintzberg words). Out of a functionalist attitude, an interesting research question can be addressed at that stage: the contradiction between the relative success in dealing with the relational complexity (gaining control over labour force), compared to the weaker achievement in conceptually dominating the analytical complexity of the whole production process needs to be understood. To make sense of this antithetical results in dealing with relational vs. analytical complexity, I used a kind of strategic change explanation.⁸¹ It was the choice driven by a military logic (to build a reserve of 100 galleys) that was causing an unforeseen change in the production regime: from production on order to production on stock, to use the contemporary management vocabulary. And this was causing the explosion of coordination needs (producing 100 galley “in parallel” is infinitively more complex than producing few boats, “looking” at completion stage in much less intuitive ways), despite the efforts by people running the Arsenal to “better manage”. It is this crucial unintended consequence that explains the persisting chaos in the Arsenal, despite sophisticated organizational innovations. Curiously enough, these consequences were unanticipated by both the actors of the time, and by Venice historians of our days.

One of the reviewers in the publication process was posing a question that made me crazy, asking me to “verify” whether this discussion was transformed into actual changes. That is a naïve question, which I refused to answer. Even today, if we consider important consultancy reports and internal debates, how can you say whether the report had an impact? With hundreds of multiple cause-and-effect relationships the answer would be, even in current day research, “who knows?”, or at least it would be an issue of reconstructing the debate and discourse itself, in less deterministic a way⁸². What I think was particularly interesting is the discourse itself, the process before (and in any case, separated from) its impacts. For instance, Molin in 1633 argued “if we were asked how many ships we have produced in the last decade, we would not be able to give an answer”.⁸³ The fact itself that such a question was introduced in the conversation about managing seems to me so interesting, in cognitive terms and as an early historical example of management knowledge in itself. Finally, the whole research shows how the inner nature of management as rhetoric is not as a current day issue, but one of its constitutive elements (*discorso del maneggio*).

A couple of interesting elements of distinction from other approaches in critical accounting history can be pointed out in this regards. First, I am not referring to any "genesis" of managerialism:⁸⁴ that "modern" management can be identified exactly is questionable, especially when comparing forms of administration across countries (managerialism itself being a product of managerial rhetoric). In any case, that one could trace back "the" genesis, rather than expecting a more pluralistic establishment of forms of "modernity" in management is a more general epistemological issue. Furthermore, and even more interesting with the above discussion on "strategy", if human accountability is surely an interesting element of the picture, there is something else. In my understanding of the Arsenal archives I was also so much interested in making sense about the process of organizing experienced by key actors in the Arsenal and the Republic (the cognitive dominance of the analytical complexity intrigues me, more than simply the emphasis on the relational complexity).

Second, while I am sympathetic with some of the positions of the so-call "new accounting history", its ethnocentrism (the focus on UK), the lack of interest on early State bureaucracies, and above all its present centeredness is what keeps me distinct from it.⁸⁵

A final point about the community of management scholars: at that time (first submission 1996, eight years to get published) no journal in the management field was interested in such an archive based article; accounting journals were more open to historical research, but were then asking "more accounting stuff than management". In any case, when I submitted the paper at the Academy of Management at the Management History division it was rejected because it was too long!

The International Museum of Ceramics in Faenza (MIC)

In reconstructing the 100 years of history of MIC from a management study point of view – and with the above-mentioned attitude of taking bounded rationality seriously into account in dealing with complexity, and with attention on "streams of actions" – we faced a rather bizarre situation.⁸⁶ Two relevant literatures could be potentially identified, with no relation at all between them – which is already in itself an issue of lack of (or bounded) rationality by scholars.

On the one hand, there is a literature by arts historians and curators, giving a crucial emphasis to the founder of the museum, Gaetano Ballardini, a mythical figure, with a sort of very "modern" project for the museum, as a living institution promoting local economy and craftsmanship. On the other hand there is a literature on the transformation of public sector in recent years, lacking interest in historical antecedents in general and for sure on arts organizations and museums like the MIC.

In our work, we were putting together these two unconnected literatures within a critical view. Indeed the myth can be deconstruct in various ways: whatever the initial "imprinting" could be crucial, all in all Ballardini only covered the first 40 years of the museum (ironically put, just a 40%), and the history of the museum presents consistent changes afterwards: in this regards, the narrative about the museum by curators can be in fact pluralized. Moreover, one thing is the vision Ballardini had in mind; another thing is to what extent such a vision was transferred into reality ("getting things done", to use the macho view of management): another good opportunity to see how much of the strategic design was actually transformed into realized strategy. The degree of "modernity" tends to weaken in such an analysis, with "ups and downs" that risk to be missed by the curators' narrative.

Indeed, there are elements of "modernity" in the MIC history that the literature on NPM would simply forget. One of the most intriguing aspects in this story is the institutional evolution: born as a private entity (association), after almost 70 years the museum was transformed into a public entity, than transformed into a more autonomous legal form 20 years later, and into a private law foundation 4 year after. Institutional transformation is not a new experience, thus: there is a repertoire of them that can be tracked back. The linear development of these transformations could be questioned to some extent, for even the last two changes – mostly in tune with the NPM movement – seem to be bounded: the MIC never experienced a business plan over its 100 year of existence! However, what the NPM literature would ignore is that "privatization" – surely a crucial phenomenon in most public sectors in the world now a day – is not coming out of the blue: there is a "history" also in this regard. One of the fascinating elements here is to go back and reconstruct something that is so strange for us in these years: the process of "publicization" around 1974, the discussion and motives that were moving the management

of this entity to ask to get inserted in the public sector (irony of history: basically the idea was to get more funding!). Totally unexpected, unpredicted by the actors, the new institutional settings then emerged in its “perverse” effects, with the lack of autonomy, control, and even accountability, and of course cuts in resources, leading *in the end* to a situation that is so familiar for us now.

Accountability itself is not something new; it was there before the transformation into a public entity. Forgetting this element would mean not fully understanding the inner dynamics of public sectors that we are dealing with.

China administrative transformation in the last decades

The transformation of China in the last 30 years is perhaps one of the most striking processes of change ever experienced by a country.⁸⁷ This event is an incredible sort of laboratory for those interested in investigating the formation of social order (spontaneous vs. deliberated, in Hayek word; or deliberated vs. emergent in Mintzberg’s terms). In such a short period, the whole structure of a centrally planned economy was transformed into the second economy of the world. To understand what happened (how was it possible?), I would suggest putting ourselves “in the mind of CEO”: are we able to imagine what would we do if we were in the position of the Chinese prime minister in 1978?

There are of course several ways of looking at such a huge process of change (from a political, military, social point of view). Even when taking an organizational/administrative viewpoint, things tend to be described in a variety of separated ways.

Our research was firstly reconstructing the representation of changes at different levels: the reduction of the central apparatus; the fiscal reform(s); the transformation of national budgeting and accounting changes. One thing that scholars in the field tend to ignore is the risk of lack of consistency amongst these different levels: there is an issue of fit between *macro* and *micro policies*, which is never questioned by these divided literatures. There are “mistakes”, unanticipated phenomenon that are likely to question any linear evolution of such a huge transformational process. One would wonder whether at any point in time anyone had a clear idea of where the transformation process would bring the country as a whole (indeed, such bounded rationality was perhaps something making change possible!)

Moreover, not only unforeseen results can emerge in the translation of (ambiguous?) aims from macro to micro polices: the actual enactment from *micro policies* to *micro practices* play another crucial role, making things more “creative” than one would expect. Even in terms of technicalities, it was curious to discover that accounting practices at the shop floor inside China public sector are totally different from those that one would expect reading documents and agreement from a Washington point of view. According to our field investigation, the income and expenditure format does not exist in accounting practices in Chinese public sector entities (*shy ie danwei*), a shocking result if one has a naïve understanding of management (and accounting) discourses, forgetting their intrinsic semantic ambiguity as rhetoric. Once again, management as rhetoric is a very recent kind of approach (from McCloskey onward)⁸⁸: but it could work as a perspective, applied to previous histories.

China: Management and the Great Divergence debate

The great divergence debate is perhaps one of the most fascinating topic in this period, questioning so many hidden assumptions in mainstream historiographies at different levels, and above all their strong Western bias.⁸⁹ Curiously enough, a common aspect characterizing the debate is the total lack of reference to the “micro” level, i.e. the importance of looking at “management”, the ways in which organizations were run.

So, if the research on the Venice Arsenal is moving backward the time of change affecting economic developments in the West (more than the industrial and management revolution, modern management it is a Renaissance phenomenon in this regard), what are the implications for the Great Divergence debate?⁹⁰ Is that proving that the West was better off in earlier periods than commonly acknowledged, or is it simply an issue that even historians working on the East – similar to their colleagues working on the West – tend to overcome the relevance of knowledge and managing at the micro level?

There are very serious issues with such a research question, first of all related to the lack of archives comparable to what we have in the West. More than an accident, this in itself deserve more reflection, being a signal of a different attitude toward reporting and controlling, which is already a part of the problem. But the economic developments of Chinese economy at least until the Song dynasty was so rich that missing any notion or procedures for managing seems simply impossible.

Why do scholars (in both the West and the East) ignore such an intriguing research question? My impression is that the under-problematicized relation between thought and action, between design and conduct, is the responsible. Looking at outcomes would be a possible alternative to look at the decisional process itself; and the lack of actual results would undermine any relevance of a conversation of management. Here again, it is a question similar to my referee asking what happened "after" the flows of documents we found in Venice: in a sense I am more interested in the sense making process of Molin, and his doubts about the lack of metrics for the periodical production output, more than looking for cause-and-effects relations. Similar, I would look for conversations about the issue of managing by Chinese actors, more than looking at impacts.

Discussion

One of the distinguishing elements in a similar set of research projects is that perhaps a "true", full time historian will never cover such a vast set of periods and issues. As a management scholar, I am a part time historian, so to speak, and more than working full time on history, historical projects, and for historians, I am taking the historical perspectives in any of my research projects, wherever opportunities and accidents bring me.

Trying to link some of the elements of the archive/field research with the previous discussion early in the paper, the various aspects can be found to some extent in all the research projects I have presented in the second part of the article.

Ethnocentrism

The emphasis on American driven definition of boundaries within business studies, and the total lack of attention to previous articulation, makes the case of accounting historiographies a bit "stranger" for English speaking reader perhaps. A similar discussion will not be so strange for people that have worked on continental traditions.⁹¹ The similar lack of interest for continental contexts in the case of the Venice Arsenal is in turn difficult to explain, given the general acknowledgement by historians of the importance of the "Venetian method" in the development of economic activity (even when not necessarily one agrees with the Sombart thesis on the importance of double entry bookkeeping in the development of capitalism). Discovering that there was much more than simply "bookkeeping" at that time, that calculative practices, human accountability, and complex processes of sense making in organizing should raise the question "what is new" in current day's management, a part from its institutional features. In the case of the MIC one could refer to the small dimension of this entity to reach interest from an international audience. However, that China is so absent from the attention of management, accounting (and business) historians – in terms of both long trajectory in economic development and administration, and the recent, epochal transformation – is a measure of the ethnocentric syndrome of our fields. Indeed, this refers to the twofold kind of ethnocentrism we were referring to: the "internal" one, where the Anglophone domination forces to ignore the rest of the Western world itself (see data provided by Engwall); and the "external" one, where, all in all, so little research is still done out of the Western bias. The serious delay with the broader establishment of global history must be underlined in our fields.

Time parochialism

The present centeredness is a recurrent element my research has to fight with. The "founding father" metaphor of the Italian tradition of *Economia Aziendale* is in this sense disarming. The Venice Arsenal – and the "rediscovery" of the etymology of management in action, in the *discorso del maneggio* – corroborates to a great extent the idea that managing/organizing has "always" been an issue of

understanding complexity (both analytical and relational), uncertainty, more than being these elements characterizing the present period.⁹² A curious difference in time reference can be found in the MIC case. Art historians tend to anachronistically project the heroic past (Ballardini) into the future, as if the whole century of the museum's life was determined by the first 40 years. In turn, New Public Management literature is underlying the need of "new" elements that, searching carefully, one will find already at work to some extent (the statute already in place well before any "mission statement"; the attention to visitors and to crafts etc.). Time centeredness in the case of recent reforms in China takes a striking form: i.e. the lack of interest by historians (management, accounting business, economic) in reconstructing one of the most fascinating process of economic (and social, and political) transformation, its roots in the proactive transformation of the State and public sector, which made it possible the construction of the market economy. Finally, the total lack of interest of management, accounting (and possibly business) historians from the debate on the Great Divergence is perhaps the best example of the naivety of management, accounting and more generally business literature to understand their differences and specificities, and their hidden roots.

The issue of complexity: the contribution of strategy scholars

In this regards what is distinctive (if anything) in the ways in which a strategic management scholar do historical research? Following the previous discussion, two elements seem to me crucial. First, there is the acknowledgment of "bounded rationality" as one of the basic features of decision-making. Second, there is a way of dealing with complexity in itself. Indeed the management (and particularly the strategic management) scholar has a strong attention toward the intrinsic logic within the stream of actions, the internal logic of the specific behaviour of an organization, which is looked after investigating the various possible meanings of interactions (fits, or mis-fits if you do not like them).

Without considering individual talents, what a strategic management scholar could be good in doing historical research seems related to the capability/skill/attitude of looking at interdependencies, consequentiality, links and fits between individual elements of a decision or of a composite action.⁹³

While *intentions* and *power* elements are crucial and widely used by other scholars as well, they are just part of the picture. The internal *fit*, the relation between various components in the ways in which things are done – the stream of actions; the business idea, the configuration of strategy, the business model, as sometimes referred to in the literature – plays a role which is specific to the strategy scholar. In turn, this gives a particularly interesting content to notions such as *sense making*, and decisional *processes* within organizations, which is not necessarily used in other fields.⁹⁴ Hereafter I will refer to strategy and strategizing as the consistence of these five elements (intention, power, fit, sense making, and processes).

Without re-examining in details the "case studies" provided before, few comments can be added about the ways in which the strategizing perspective – as here defined – adds some specific insights:

- Accounting historiographies: the field itself (accounting in its historical evolution) is understood as an intellectual struggle between different positions, strategizing at once the subject and the actors and their vested interest (in Italy or abroad).
- Shipbuilding at the Venice Arsenal: the investigation of the analytical complexity, the deep analysis of hidden interdependencies, of fits and misfits within the organizing of production activities is what distinguishes this study, while sharing with others (e.g. Venetianists or even some Foucaultian scholars) notions of power. In addition, it is the sense making process that was intriguing me so much: how the reading of original archive documents can tell you about ways in which actors of the time were understanding, debating, perceiving problems, organizing priorities, inventing notions in their conversation about managing.
- MIC: not only the analysis was overcoming the reified approach that tends to characterize NPM literature, while paying attention to the complexity of curatorial discourse (and their professional view on part of the "fitting" issue). Our view of the fit is much broader (including

financial feasibility and institutional sustainability), focusing on longer term concerns, though unanticipated in the mind of the funder, and later actors, within a much broader view of time and processes.

- China administrative reforms: the analysis overcomes the linear conception of change that characterizes a set of divided literatures, which ignore each other as if they were talking about clear cut and independent phenomena. Our analysis problematizes the relationships across different levels: the consistency within macro policies themselves; the consistency between macro and micro policies; and the consistency between micro policies and micro practices.
- Great divergence: the study calls for a more serious attention to micro phenomena in organizing as a promising perspective to this interesting debate. How different ways of conceptualize, understand, counting, archiving have had a role in processes and discourses, even well before results and impacts (the latter being incredibly difficult to reconstruct given the situation of archives in the Country).

Conclusions

While general common philosophical aspects can be identified as conditioning our ways of doing historical research in general (e.g. the reference to the linguist turn), then other specific elements in the approach of each historian are likely to be at work, in relation to backgrounds and disciplinary belongings.

Organizational theories – as we know them now a day – are crucial to understand history before further methodological concerns. Allison's analysis on the Cuba missile's crisis⁹⁵ is a good example of general investigation on decision making, in a sense not focusing on the substantive elements of the crisis, which simply becomes a context in his "generalizing" analysis, or before other specific, substantive aspects. To put it radically: nothing happens (nor happened) outside organizations; history (and historiographies) included. The ways we think and theorize about decision-making have an impact in our ways of conducting historical research. In turn history, as a representation of the past, contains (implicitly or explicitly) a theory of decision-making. This opens up huge opportunities for organizational scholars in historical research.

From this point of view management scholars in general have an interesting attitude to bring in related to their typical "bias": this is their approach "in the mind of CEOs", or "in the shoes of managers". When reading the document written in 1586 by Baldissera Drachio in Venice, my attitude was not that different from the one I have when reading a current day report written by the internal staff, or a consultancy report. In any case, I am trying to make sense of inner complexity and behaviours. Particularly from a strategy perspective – as defined above – the articulated focus on intention, power relationships, issues of fit, and organizational processes have an impact on ways of doing historical research, making sense in sense making processes of the actors involved.

While anachronism remains an unacceptable sin for historians, the need to overcome our time parochialism is the main point of this article. Bringing in a sense of historicity in the understanding of management, organizational and strategy studies – in a word historicizing strategy – will allow us to play a role in historical investigation, strategizing history.

Acknowledgments

The paper has benefited from the warm discussion during the workshop on Qualitative Historical Methods in Management and Organization Studies, run at the Centre for Management & Organizational History, Queen Mary University of London, September 8-9, 2011.

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Endnotes

- ¹ Zan, "Toward a history"; Zambon, "Accounting and Business"; Engwall et al., "Management education"; Üsdiken et al., "Academy, Economy and Polity".
- ² Augier et al., "Notes on the Evolution".
- ³ Zan, "Interactionism", "Future directions"; Kipping et al., "Business history".
- ⁴ van Fleet, "Wren Daniel", "Doing management history".
- ⁵ For a short review in English language see Mulligan, "The great divide". For implications on the Business History tradition, see Toninelli, "The Atlantic divide".
- ⁶ Kipping and Üsdiken, "Business history".
- ⁷ Craig, "Strategic use"; O'Sullivan et al., "Moving forward"; Carter, "The Ages of strategy".
- ⁸ For instance, a new track on history is now available at the Egos Congress; a plenary session on "Rediscovery history in management" was run at Euram in 2011 (<http://www.economia.uniroma2.it/euram/sarea.php?p=10&sa=116>). See also the debate at the 2011 AoM annual conference: "The Concept of Strategy 40 Years Later: What Happened to Andrews's Vision for Business Policy?" (<http://program.aomonline.org/2011/submission.asp?mode=showsession&SessionID=351>).
- ⁹ Warren et al., "How business"; Smith, "Management history"; Bisoux, "A return"; Clegg et al., "Making strategy".
- ¹⁰ Clark and Rowlinson, "The treatment"; Booths and Rowlinson, "Management and organizational"; Durepos et al., "Theorizing the past".
- ¹¹ Kieser, "Why organization"; Leblebici et al., "The birth"; Caswill et al., "Doors and boundaries"; Berthod, "Using historical"; Kroeze et al., "The relevance of history"; Thomas et al., "Constructing the history".
- ¹² Ansoff, *Implanting Strategic Management*.
- ¹³ For a criticism see Zan, "Interactionism".
- ¹⁴ Pfeffer, "Renaissance".
- ¹⁵ Whitley, "The fragmented"; Zan, "Writing management".
- ¹⁶ Abrahamson et al., "Management fashion"; Carmona et al., "Vogues in management".
- ¹⁷ e.g. Huber et al., "Longitudinal field research".
- ¹⁸ Zan, "Future directions".
- ¹⁹ While critical studies are right in addressing issues of power in the strategy discourse within the "real" world, within organizations, this element in the construction of disciplinary jurisdictions amongst professionals (academics, consultants, "experts") tends to be overcome.
- ²⁰ E.g. Newton, "From freemason".
- ²¹ Ruef et al., "Agrarian origins", is one of the few exceptions.
In the newly established track on history at Egos in Barcelona, 2010, a discussion emerged about the relevance of studying situations prior to the industrial revolution. That management/organizational historians themselves are not hundred per cent in agreement about the relevance of renaissance period for the development of knowledge in doing business is in itself very telling.
- ²² Booth et al., "Management and organizational".
- ²³ Knuuttila, "Memory, anachronism".
- ²⁴ Asplant et al., "Present-centred history".
- ²⁵ BBC, "Ice Mummies".
- ²⁶ Loasby, "Choice, complexity and ignorance".
- ²⁷ Simon, *Administrative behaviour*, "Rational decision making", "Bounded rationality"; Cohen et al., "A garbage can"; March, *Decisions and organizations*. For a review see March, "Epilogue".
- ²⁸ Merton, "The unanticipated consequences"; Popper, "Predictions and prophecy"; Hayek, *The counter-revolution*.
Though I am sympathetic with tentative reconstructions of broader evolutions – e.g. the linguistic turn by Kroeze et al., "The relevance of history", or Brown et al., "A narrative approach" – I must confess I have some troubles in connecting Merton, Hayek and Popper to postmodernism. Moreover, I am interested in impacts that are more direct in our day-to-day work on historical research.
- ²⁹ Pettigrew, *The politics*.
- ³⁰ Weick, "Enactment processes".
- ³¹ Ropo et al., "Reflections on conducting"; Rehn et al., "Processual approaches".
- ³² Normann, *Management for growth*; Pettigrew, *The management*; Mintzberg, "Patterns in strategy formation"; for a reconstruction see Zan, "Looking for theories".

³³ Gherardi, "La struttura del campo".

³⁴ Of course, there are indeed serious differences as the ontological level. However, more than uncertainty, discontinuity or turbulence, what is interesting is the increase in the variety of competitive and organizational solutions, with more degrees of freedom in "making business". This in turn makes the competitive field more opaque, hard to understand, and makes a notion such as strategy useful in cognitively dominating the differences at play.

³⁵ Axelrod, *The evolution of cooperation*.

³⁶ Mintzberg et al., "Of strategy, deliberate and emergent".

³⁷ Miles et al., "Organizational strategy"; Boschken, "Strategy and structure"; Amitabh et al., "Research in strategy-structure".

³⁸ Mintzberg, "Patterns in strategy formation", "The design school"; Normann, *Management for growth*; Miller, "Configurations of strategy"; Mintzberg and Waters, "Does decision get"; Pettigrew, "Studying strategic choice".

³⁹ Mueller et al., "Politics and strategy practice", 1168.

⁴⁰ Zan, "Interactionsim", 269.

⁴¹ Rumelt, "Toward a strategic".

⁴² Zan, "Interactionism", 276.

To better illustrate the specific ways in which a strategic management approach would work in terms of analytical and relational complexities, the example of Robinson Crusoe can be taken back. "My impression is that talking about Crusoe's strategy would be helpful first of all in understanding the logic in the human/natural system relationship (i.e., focusing on the systemic view): how he made sense of the context, of its way of functioning, and how he made sense to survival and found possible sustainable patterns of relationships with the context. The sense of what he thought and did, can be summarized as his 'strategy'. The further aspect, that is, the interactionist view, only came in a second phase, when Robinson met Friday, thus enriching his 'strategy' with a human/human relationship" (Zan, "Interactionism", 269).

⁴³ Miles et al. "Organizational strategy"; Venkatraman et al., "Exploring the concept of fit"; for a recent reconstruction see Garlich, "The concept of strategic fit".

⁴⁴ i.e. Engwall et al., *Management education*; Caswill et al. "Doors and boundaries"; Tienari et al. "Editorial"; Wesley, "Getting too close".

⁴⁵ Engwall, "Research note: Asterix".

⁴⁶ Zan, "Toward a history"; Zambon, "Accounting and Business"; Biondi et al., *Accounting and business economics*.

⁴⁷ e.g. Carnegie et al., "Exploring comparative".

⁴⁸ Crozier et al., *L'acteur et le système*.

⁴⁹ This was the topic that with a colleague of mine (L.M. Sicca) we wanted to address at a track on "Criticizing CMS" for the 7th CMS International Conference, 2011. "The aim of the track is to investigate to what extent a similar language domination/imperialism is characterizing CMS as well. Problematizing the problematizers, criticising the criticism, we would like to focus on forms, ways and degrees of Anglophone domination in the field, looking at reasons and consequences. In short, what are the potential developments, future possibilities of CMS when/if the strong ethnocentric focus is questioned (Anglo-Saxon first of all, and Westerner second)." Though the track was accepted, unfortunately, we got just two submissions.

⁵⁰ Üsdiken et al., "Academy, Economy and Polity".

⁵¹ Zan, "Future directions".

⁵² e.g. De Roover, "New perspectives", *Business, banking and economic*.

⁵³ Forsellini, "L'organizzazione economica" (The economic organization of the Arsenal).

⁵⁴ e.g. Lane, *Venetian ships and shipbuilders*. About the role of Lane see Bullard et al., "Where history and theory interact".

⁵⁵ From the point of view of academic politics, the weakness of each of these areas in mainstreams economics, management, organization and accounting would suggest a more careful attitude towards bridging and overlapping more than working in isolated towers. From the methodological point of view, similar processes and debates that are taking place in various areas are ignored.

⁵⁶ Kipping et al., "Business history".

⁵⁷ Wren, *The history of management*; Booth et al., "Management and organizational".

⁵⁸ O'Sullivan et al., "Moving forward".

⁵⁹ Jones et al., "Introduction", 1.

⁶⁰ Kipping et al., "Business history".

⁶¹ Hambrick, "The field of management".

⁶² See footnote 14

⁶³ Hambrick, "The field of management", 1346.

⁶⁴ Von Krogh et al., "Phenomenon-based Research ", 291

⁶⁵ Kipping et al., "Business history".

⁶⁶ Kieser (1994).

⁶⁷ "A different attitude of organizational historians and a renewed interest in combining organizational and historical theories with practical historical studies is necessary": Kroeze et al., "The relevance of history", 10.

⁶⁸ Thomson, "The case for management", 99.

⁶⁹ Thomson, "The field of management", 107.

⁷⁰ Thomson, "The field of management", 108.

⁷¹ Zan, "Toward a history".

⁷² Masi, "La ragioneria è morta".

⁷³ See also Augier et al., "Notes on the evolution"; Bottom, "Before the Ford Foundation".

⁷⁴ Whitley, "The fragmented state".

⁷⁵ e.g. Besta, *Bilanci Generali I, Bilanci Generali III, La ragioneria*.

⁷⁶ Zan, "Accounting and management discourse".

⁷⁷ Lane, *Venetian ships and shipbuilders, Money and banking*; Concina, *L'Arsenal della Repubblica*; Davis, *Shipbuilders of the Venetian*.

⁷⁸ Few years ago, a previous director of one of the most prestigious journals in strategic management asked me with innocence: "Can you please elaborate on what you know regarding the origin of the term management...have long wondered!" Indeed the term "maneggio" as a substantive of the verb "maneggiare" (literally: handling) was widely used at that time. The translation and diffusion of Pacioli's book since 1494 throughout Europe is likely to have been a vehicle of the migration of the terms in other languages. While the terms have later on assumed different meanings in Italian (maneggio now only means horse-riding stables), the old meaning is preserved in Spanish "manejo". Yet, the English synonymous "handling" still echoes the original etymology of the Italian term.

⁷⁹ Lane, *Venetian ships and shipbuilders*, 213.

⁸⁰ Concina, *L'Arsenal della Repubblica*, 175.

⁸¹ Zan, "Accounting and management discourse", "Future directions".

⁸² For instance, referring to the Edwards Report about the British Museum in 1996, it is not so easy to trace back to this text what happened later with the hiring of a "managing director". Indeed this was an argument used by some of the actors to legitimize this decision (starting from the government), though this is more an issue of rhetorical stratagem. More than linear cause-and-effect, it is an issue of conversation about managing, which addressed issues and concerns not necessarily in a consistent way. For an in-depth analysis, see Zan, *Managerial rhetoric*, Chapter 2.

⁸³ see Zan, "Accounting and management discourse".

⁸⁴ as in Hoskin et al., "The genesis of accountability".

⁸⁵ See the abstract of the paper by Miller et al., "Genealogies of calculation" (italics mine): "The delineation of the domain of traditional accounting history is illustrated by reference to three sets of issues: the links between double-entry bookkeeping and capitalism in the writings of Weber and Sombart; the links between bookkeeping practice and decision making in the writings of Yamey; and the quest for examples of "early management accounting" in the writings of those such as Edwards and Fleischman & Parker. *In contrast to such concerns* of accounting history, four genealogies are presented: the promotion of discounted cash-flow techniques for investment decisions in the U.K. in the 1960s; the emergence of costs in the late eighteenth century; the accounting for value added event in Britain in the late 1970s; and the construction of standard costing in the early decades of the twentieth century".

⁸⁶ Lusiani et al., "Change and continuity".

⁸⁷ Zan et al., "Budgeting China".

⁸⁸ McCloskey, "The rhetoric of economics".

⁸⁹ For an overview see Deng, "A critical survey"; Vries, "The California school".

⁹⁰ Zan et al., "Searching for managerial".

⁹¹ Üsdiken et al., "Academy, Economy and Polity"; Biondi et al., *Accounting and business economics*.

⁹² Another curious example, a joint result of "inner ethnocentrism" and time parochialism is the phenomenon of double translation that can be found in Italy; we are now importing the term management from English

(actually we are using it in English) after it was invented in Italy (*maneggiare*) and then "imported" in English as management, as seen above.

⁹³ As mentioned, very few of us in the management as well as in the accounting field are full time historians; but even more interesting, in the rest of our time – though contradictory it may seem – all in all even as a “critical” management scholar I still teach management and planning, budgeting etc. It is another kind of planning, what Normann would refer to as “process view” as opposed to the “goal view”; but still, it is planning. As management scholar, the focus is on potential consequences, whatever unintended they may be (indeed, more unintended they might be).

⁹⁴ The notion of fit seems indeed something that distinguishes strategy within the strategic management field. It is alien to the definition of strategic behaviour as taking into account the other actors' behaviour, typical of a game theory approach, both in sociology and economics; but it is alien also to the over emphasis on power that often characterizes critical studies, paradoxically with results not that different from a game theory approach: the issue of making sense of (analytical) complexity is not addressed.

⁹⁵ Allison, *Essence of decisions*.