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Excerpt

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1 Introduction: from the science of accounts to the financial accountability of science

Michael Power

The Pacioli effect

Luca Pacioli, mathematician and teacher, is often mistakenly credited with the invention of double-entry bookkeeping (Swetz 1987, 24). Nevertheless, in 1994, the quincentenary of his *Summa de Arithmetica*, accountants from Edinburgh to Venice have been determined to celebrate the “founding father” of “scientific” accounting. But how can accounting aspire to be scientific, and what is its peculiar logic? For Max Weber, double-entry bookkeeping was much more than a mere business technique; it was paradigmatic of a pervasive formal-legal rationality that was increasingly calculative in nature. Werner Sombart pushed Weber’s insights even further and argued that double-entry bookkeeping, famously eulogized (without irony) by Goethe as one of the “finest inventions of the human mind,” was a necessary condition of capitalism by virtue of the fact that it enabled abstract economic calculation. Capital as a category did not exist outside the double-entry world view.

The flaws in Sombart’s claims are perhaps too obvious to rehearse. As Yamey (1949, 1964) has suggested, double-entry bookkeeping was used less than Sombart imagines – without detriment to the march of capitalism. And the independent development of the Chinese double-entry method, similar to that of the Italian (Jun Lin 1992), undermines necessitarian claims. But whatever the truth of Sombart’s thesis (Edwards 1989, 61–62), it represents a distinctive tradition of thinking about accounting: “Weber was seeking to differentiate between *calculation* in terms of money, and money’s actual use. Thus he opened up the space for a consideration of the distinctiveness of economic calculation” (Miller and Napier 1993, 635). Weber’s (1978) few brief passages on accounting have had “far reaching consequences for the writing of accounting history” – notably that of Sombart: “From such a starting point, an entire historiography has been built around questions of the causal links

Cambridge University Press

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Excerpt

[More information](#)2 *Michael Power*

between ‘scientific bookkeeping’ and ‘the rise of capitalism’” (Miller and Napier 1993, 636).¹

It is not my purpose to engage these questions of historical scholarship directly but to use them as the pretext for this collection. Even if it is trivial to state that accounting emerged from the same Enlightenment project as science, history – especially bad history and its heroes – can be a contemporary resource. In the figure of Pacioli, accounting has a certain potential for placing itself close to the origins of modern rationality itself. However, it would be too simple to claim a legitimizing role for the Pacioli myth. In the United Kingdom at least, public images of the accountant have moved far beyond that of double-entry bookkeeping. Accountants now work in many different roles: as consultants, auditors, tax advisers and insolvency practitioners. Accordingly, the rise of professional accounting is to a large extent a dissociation from the mechanics of double-entry bookkeeping. The claimed expertise of the financial adviser now has little to do with humble routines that can readily be delegated to clerks. Indeed, the success of accounting in professionalization terms has much to do with the subordination of routine expertise (Abbott 1988, 125). As Hopwood (1992) has put it, accountants are now more significant than accounting.

Accordingly, the 1994 celebrations of Pacioli are ambivalent. A quincentenary dear to the hearts of a small body of accounting historians must be set in the context of a professional practice that is increasingly confident of its economic and social position. Practicing accountants no longer need to trace their conceptual origins to that most legitimate of cultural projects – science. Indeed, despite public failures and criticisms, accountants increasingly promulgate a commercial rationality that looks set to eclipse the cultural authority of science. Whatever remains to be said by scholars about the Pacioli of 1494, in 1994 his reexposure comes at a time when accounting is increasingly implicated in the reorganization of research practice around such ideals as “cost effectiveness,” “value for money,” and “efficiency.”

As mathematicians, architects, and others celebrate those parts of the *Summa* relevant to the history of their discipline, university departments of mathematics and architecture are increasingly subject to the agencies of accounting. Accounting as an academic field is relatively young, but as a practice it is coming to shape organizations in new ways, subjecting them to a more detailed and explicit economic logic than hitherto. This

¹ There is a distinct and extensive tradition of concern with economic calculation and capitalist accounting within Marxist economics. See Bettelheim (1976) and Thompson (1986, chap. 3). In addition, there is a neoromantic tradition of reaction against all forms of economic calculation (Power 1992a).

Cambridge University Press

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Excerpt

[More information](#)

From science of accounts to financial accountability of science 3

intensification of economic calculation is perhaps most evident in the changing management of public science. Where once the image of science shaped the legitimacy of other practices, the cultural authority of science is now beginning to unwind. It was scientism as the hegemonic self-understanding of science that so preoccupied the sociologists of the Frankfurt school, and concerns about the authority of scientists and technological experts persist in a variety of areas. But a form of financialism looks set to displace scientism, an irony that provides the underlying theme of this introduction.

The emphasis in this collection on economic *calculation* rather than on economics is deliberate. I shall suggest below that the hopes for a consistent economics of science fall well short of providing an account of the calculative practices within which economic reason is instantiated and through which its impact on scientific practice can be gauged. An “economics of science,” which claims a new orientation for studying science in context, will prove to be a paradoxically disembodied and contextless program in the absence of a consideration of calculative practices. The relationship between economics and accounting is varied and complex (Hopwood 1992). Accounting does not necessarily stand in a subordinate relation to economics (though economists may like to think this). On the contrary, economics draws from accounting in crucial ways (Klamer and McCloskey 1992; Hoskin and Macve 1993). And there can be little doubt that abstract economic models assume away the very contexts of economic calculation that are relevant to science studies.

In this introductory essay, I attempt to provide a schematic guide to an emergent field of inquiry: science and accounting. This guide sketches a prototype for a research agenda which unites the collection of essays which follow and must be articulated with two points firmly in mind: a nonessentialist view of what counts as accounting and a constructivist symmetry in the treatment of science and accounting.

The first point requires us to look beyond Sombart and double-entry bookkeeping to all those “accountings” that are implicated in economic activities: costing, budgeting, cost-benefit analysis, risk assessment, censuses, samples, and so on. We must constantly ask what counts as accounting. The second point requires that the varied “constructivist” sensibilities about science must be extended to the forms of economic calculation that provide its context. Though it will not be possible in this essay to explore the many different senses of “social construction,” this point is crucial. One might consistently be a realist about both accounting and science. One might even be constructivist about accounting while remaining a scientific realist of some kind. But it seems inconsistent to be constructivist about science while remaining a realist

Cambridge University Press

978-0-521-55699-6 - Accounting and Science: Natural Inquiry and Commercial Reason

Edited by Michael Power

Excerpt

[More information](#)4 *Michael Power*

about accounting, although existing attempts to construct an economics of science tend to do precisely this.

In the next section I consider that strand of thinking concerned with the “scientific” status of accounting. This looks initially like a fairly standard story about scientism, about science as a cultural model for other practices, and is articulated by a small number of theorists concerned to make accounting practice and accounting research more “scientific.” However, this standard story can be retrieved as an episode in what Daston (1992,1994) and Porter (1992,1994) have called “the history of objectivity.” In section 3 I consider some developments in the history and sociology of science that open up science to its economic context; and in section 4 I am critical of the extent to which these developments lean toward an economics of science that abstracts from concrete processes of calculation. In section 5 I consider the recent social turn in accounting research, which is partly an internal reaction against the prominence of economics-based methodology and which can be aligned with broader historical and sociological reflections on the rise of quantification. I argue that analyses of science in economic context would benefit from newly contextualized understandings of accounting itself. Indeed, while the social turn in accounting has drawn on social studies in science, the reverse has not occurred. One of the conspicuous contributions of the new accounting research has been the recognition of the symbolic and ritualistic properties of accounting practices and their growing cultural hegemony in making activities visible in economic terms. In the light of this research, I go on to consider the role of accounting for science. In short, the replacement of the cultural hegemony of the scientist with that of the accountant can be traced in the shift from preoccupations with the scientificity of accounts to the financial accountability of science.

Accounting as calculating science

Appeals to the scientificity of practices, and the reception of the image of science in different fields, tell us much about the nature of these fields, their preoccupations and their search for institutionalized legitimacy. For many years financial accounting discourse had been concerned with the nature of economic measurement, in particular income recognition and asset valuation. As academic accounting established itself, it was perhaps inevitable that these questions should acquire an epistemological flavor and that images of science should intrude (e.g., Mautz and Sharaf 1961).

Miranti (1986, 1988) explores the idea of “scientific accountancy” in the early years of the American accounting profession and in the writings of Haskins, one of its founding members. Haskins appealed to the

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Excerpt

[More information](#)

From science of accounts to financial accountability of science 5

epistemological affinity between accountancy and the apparently indigen-
ous traditions of statistical thinking in Emerson's writings. Accountancy
was linked to ideals of social progress and order, a view that contrasted
with the "British" emphasis on the limitations of statistics in the face of
professional judgment and the "approximate" nature of accounting.²
These early reflections represent attempts to lift accounting and audit
practices beyond the status of craft knowledge and to connect them with
relatively established forms of scientific thinking. But these ways of
talking about accounting were at best symbolic. According to later
theorists, such as Chambers (1991) and Sterling (1979), accounting prac-
tice suffered and continues to suffer from the pervasive subjectivity of its
calculative operations. In their view, accounting practice could become
more scientific only by concerning itself with the objective economic *mea-
surement* of independent phenomena.

Stamp (1981) rejected these appeals to science, preferring instead to
explore jurisprudential models for understanding accounting. From
Chambers' and Sterling's points of view, Stamp is a kind of relativist. But
from Stamp's point of view, Chambers and Sterling misunderstand the
fundamental nature of both accounting and science. Where Sterling
desires "tests rather than tastes," Stamp argues that the accounting pro-
ject is fundamentally judgmental and that the myth of objective mea-
surement hides the forms of social agreement that support calculative
practice.³ It would be a mistake to see this accounting debate purely in
philosophical terms, although such terms provide its currency. As I have
argued elsewhere (Power 1986), the Sterling–Stamp debate is difficult to
understand if we seek to identify it too closely with philosophical debates
about realism and relativism, notwithstanding the use of key philosphi-
cal terms. Stamp is not simply a "methodological anarchist," and
Sterling is not a "naive realist." Rather, both are deeply preoccupied with
the nature of stability and consensus in accounting practice. Arguing that
they have got their physics wrong (cf. Stamp 1993), though strictly
correct, is to miss the broader significance of these preoccupations.
Sterling's and Stamp's exchanges must be understood in relation to a

² Such differences may explain why the United States, as contrasted with the United Kingdom, was fertile ground for the development of statistical sampling in auditing. Indeed, scientific accounting finds its counterpart in the hopes for auditing to ground its inferential claims in the authority of statistical sampling (Vance 1950; Power 1992b; Carpenter and Dirsmith 1993). Even though sampling remained controversial within mainstream statistics, it could acquire practical certainty as it moved into other fields and lent those fields their own peculiar scientific authority. As Collins (1985, 145) reminds us, claims become more certain the further they travel from their origins.

³ Sterling's appeal to science to support the elimination of judgment looks strange. Contra Popper, Sterling regards science as a basis for *eliminating* controversy rather than for institutionalizing "conjecture and refutation."

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Excerpt

[More information](#)6 *Michael Power*

growing self-consciousness about the purpose of financial accounts (American Accounting Association 1966) and the project to develop a conceptual framework for financial reporting, a project that has consumed a great deal of time and money in North America in recent years. As Lyas (1984, 109) puts it, “Accounting seems to be in a Cartesian frame of mind and to be engaged on a fundamental inquiry into its own foundations.”

In other words, a debate between a small group of theorists reflects and articulates wider regulatory concerns about the state of accounting. What is accounting for? What is its basic structure? How can consensus for accounting policy be secured? While academics could recast these preoccupations in epistemological terms, regulators were confronted by the inevitable compromises of practice and the need to carry the support of a critical mass of practitioners. Consequently, the conceptual framework project has been heavily constrained by the need to appear rational while retaining the bulk of existing practice as it is. This quest for foundations in the form of a conceptual framework is philosophically rather unfashionable and manifestly unscientific, at least in Popperian terms. But the conceptual framework in accounting is more an administrative than a philosophical enterprise. Indeed, it can be regarded as an attempt to prevent the overt politicization of the process of setting accounting standards by appeal to an agreed set of axioms. Yet this has only exacerbated the politics of the process, especially as the conceptual framework leaves most substantive accounting issues untouched. Thus any suggestion of philosophical realism by ideals of “representational faithfulness” within this framework must be regarded with caution. Such ideals are simply one value among others and must be traded against other practical concerns.⁴

These debates about the status of accounting practice and the nature and purpose of economic measurement constitute a distinctive tradition of theorizing about accounting, one with discernible connections with the interests and aspirations of practicing accountants even when informed by philosophical reference points. But another more pervasive development has taken place that has served to marginalize Sterling, Stamp, and other theorists concerned to reconstruct the mission of financial accounting. This is what can be described as an “empirical turn” in the social sciences, of which the rise of so-called “positive” research in accounting and related disciplines is an example. Though the history of this empirical turn is undoubtedly complex, within accounting research it has become evident that questions of scientificity have shifted away from

⁴ See the essays in Mumford and Peasnell 1993 for a further exploration of this claim.

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Excerpt

[More information](#)

From science of accounts to financial accountability of science 7

concerns about first-order accounting practice. The emergence of quantitative programs for the *explanation* of practice has displaced the question of scientificity from practice to research. Scientificity is no longer a question of legitimacy for accounting *practitioners* but for a growing body of *researchers*. One important condition of possibility for this transformation has been the advances in information technology that have made available data on capital market prices. These data bases have enabled researchers to conduct empirical studies of the impact of accounting information, and, particularly in North America, accounting research has professionalized and normalized around social scientific canons of experimental method (Whitley 1986).

Across the social sciences, meta-debates about methods have borrowed heavily from those in the philosophy of the natural sciences. The recent history of accounting research is instructive: the emergence of “positive accounting theory” (Watts and Zimmerman 1986) as a research handbook is supported by views, albeit crude ones, of what it is to be properly scientific. According to this view, accounting theorists must no longer concern themselves with “better” accounting, a normative issue of little empirical interest. Their role is to explain practice as they find it. Methodological discussion in these contexts is the public relations arm of a new empirical research style. As a consequence, theorists such as Mattesich (1989) who have attempted to reconnect this science of accounting research to the ends of practice have generally found themselves isolated. More generally, concerns have begun to emerge about the “gap” between academic research and accounting practice.

To the casual observer, these developments provide evidence of the reach of scientism. But their interest may end there. Neither first-order concerns about scientific accounting, whatever that could mean, nor second-order preoccupations with research methodology need to be taken too seriously. Indeed, it could be argued that these debates are hopelessly muddled and that the invocation of the likes of Popper, Lakatos, and Feyerabend can be explained away as so much intellectual fashion and at worst as philosophically incompetent: methodology debates within accounting research have given accounting academics an excuse to play with philosophy to demonstrate their intellectual credentials. But if this were all there was to the various accounting fascinations with the image of science our story could end here. Interestingly, it would be a story with an underlying scientism of its own, insofar as it reinforces traditional intellectual territories: incompetent accountants can never contribute to the philosophy of science.

However, another interpretation suggests itself, one that locates the reception of the scientific image in the accounting field as an episode in

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Excerpt

[More information](#)

the “neglected” (Daston 1992) history of objectivity. Scientism and the exported images of science are important components of this history, and an understanding of objectivity debates in context must remain indifferent to such qualms about the philosophical expertise of those who appeal to philosophy. Even epistemologically “flawed” reasoning reflects the preoccupations of a field of knowledge and may have some bearing on programs for social and administrative control enacted in the name of science. It is with this in mind that Porter (1992, 636) revalues the “scientific” debates in accounting during the 1960s as “unsung classics in science studies” and suggests a starting point for developing a broader appreciation of the scientificity of accounting.

Concerns about objectivity in accounting contexts must be seen in terms of preoccupations with the stability and replicability of professional judgment processes. Replication is a regulative ideal, both for knowledge production and for administration and, as Collins (1985, chap. 6) has shown, for the determination of the boundaries of expert communities. Hence the concept of objectivity is implicated in professional sensibilities and in the negotiated balance between expert discretion and impersonal rules. Seen in such terms, philosophical debates reflect competing views on rule making and authority. Within financial auditing there are recurrent debates about the role of structure in the audit process. In such contexts, the claimed objectivity of formal structure has little to do with realism in any traditional sense and much to do with defendability in a court of law if things go wrong. As Van Maanen and Pentland (1994) show, auditing records have a rhetorical function, and formal methods of “writing up” practice provide a public face for the consumption of lawyers. Accordingly, the objectivity of procedure (Megill 1994) is closely related to its adversarial potential. More generally, “objectivity” in this sense of a publicly visible code of practice may be constituted for complex regulatory and professional purposes. From this point of view, standardization projects, such as the conceptual framework for financial reporting, have less to do with accurate representation and more to do with preserving institutional structures of self-regulation. Conceptual framework projects simply dress up questions of procedural fairness as cognitive issues.

Once we understand that certain preoccupations with the scientificity of practices have little to do with representing reality but reflect complex episodes of institutional change, then the “science of economic calculation” emerges in a different light. An intellectual *volte-face* is required, from questions about the scientificity of economic calculation to questions about the implication of calculative practices for the self-understanding of science. Such a shift necessitates a recognition of the deep

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Excerpt

[More information](#)

From science of accounts to financial accountability of science 9

affinities between quantitative practices, such as statistics and accounting, and administrative and managerial concerns. As one accountant put it (Rorem 1927), “Accounting and statistics are similar in their use, for both are tools of control”; and Miranti (1989) argues that the new regulatory order of the Interstate Commerce Commission, which emerged at the end of the nineteenth and beginning of the twentieth centuries, depended for its efficacy on the construction of accounting and statistical uniformities to enable comparative analyses to be made. In this respect accounting sharpens the “mind’s eye” of administrative reform or, in the words of Porter (1992, 641), the “ambit of accounting . . . is first of all administrative and not cognitive.” Defining objectivity as the statistical variance of measurement practices (Ijiri and Jaedicke 1966) may be privately supported by realist intuitions about the ultimate elimination of this variance and a convergence upon *the* measure. But such a definition implies that the greater agreement among accounting measurers, the greater the objectivity of the measure. Accounting objectivity is an administrative rather than an ontological product.

The emergence of state financial apparatus in eighteenth-century Germany, and the varied needs for a “science” of state finances, the *Kameralwissenschaften*, illustrate the interpenetration of statistical, financial and administrative practices. Cameralism, concerned centrally with forestry and mining practices, aspired to unify the fields of science, technology, and finance as elements of *polizei* science (Forrester 1990, 291). For example, Lowood (1990, 321) describes the resource management initiatives for forestry, which require the forest to be described in quantitative terms before subjecting it to economic reason. In this context, difficulties in establishing the mass of wood and the use of estimation techniques for irregular bodies led to the standardization of the *Normalbaum*. The physical forest balance sheet, with its conceptual roots in forestry science, was then linked to the financial balance sheet of the cameralist by the concept of “yield.” Regulation of the forest in this way required a new configuration of administrative expertise to keep the forest’s books in terms of both wood and gold. As Lowood (*ibid.*, 337) puts it, “the recurrent themes of equilibrium and the balance sheet harmonized with those of administrative convenience and scientific resource management.” The forest became objective and normalized for management purposes.

The scientific aspirations of economic forms of calculation, and their implications for rethinking the history of objectivity, must also be set in the context of an intensification of the use of numbers in modern political argument. As Rose (1991) argues, political choices are involved in decisions about what to measure, and numbers have the capacity to con-

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Excerpt

[More information](#)10 *Michael Power*

stitute new realities and new possibilities for surveillance. In this way numerical ideas of the world are constitutively social within it. Like Porter, Rose draws attention to images of procedural “fairness” constituted by the faith in numerical representation. Similarly Barry (1993) argues that a sociology of measurement and calculation provides a link between the history of science, the sociology of knowledge, and social theory more generally. According to Barry, “a range of technologies historically associated with the engineering of spatial relations” (*ibid.*, 463) have constructed new objectivities around forms of calculation that are solutions to both the problem of long-distant control and the construction of new hyperspaces of number. Measurement and other forms of scientific representation are deployed “in the regulation of social and economic relations” over large distances.

This theme is echoed in Porter’s contribution to the present volume, which focuses on the role of impersonality in projects of quantification. He notes a strong preference for standardization, via agencies of “quantitative impersonality,” in contrast to ideals of accuracy and precision. It is a relative preference traceable to regulatory initiatives in North America between the 1920s and 1950s. Realist intuitions played little role in these regulatory developments. For example, in the search for costs and benefits, Porter claims that numbers “emerged . . . with only a modest degree of thingness” and that therefore numerical abstraction from qualities is an imperative of control rather than accurate representation. “To create new concepts is to create new things,” and these new things embody possibilities for regulatory action. Furthermore, “Numbers react back on the processes they are designed to measure, and are themselves important actors in the economic process.” Thus, according to Porter, crime and death rates and other public statistics can describe reality because they define it. In defining it they create newly “rational” forms of behavior, new incentive structures and motives. Indeed, “the numbers can just as well be self-undermining as self-vindicating: precisely because they have become powerful, it is attractive to circumvent them.”

The spread of quantification also corresponds to a reconfiguration of expert knowledge and stimulates projects of professionalization. Alborn’s contribution to this volume emphasizes the administrative nexus between science and forms of calculation, and its implications for forms of collective mobility. Centers of calculation as networks of allies in Latour’s sense emerge. For example, Victorian actuaries formed an alliance with scientific values in the early part of the nineteenth century only to abandon it later when they were more occupationally established. The history of actuaries’ professional mobilization demonstrates that appeals