

Blind Trust: The Gentlemanly Origins of Experimental Science

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[penultimate draft, final appeared in Social Studies of Science 27 (3) 1997 pp503-536, final pagination in brackets; footnote numbers 4 and above equal final footnotes -3, so Note 10 here = Note 7 in published version]]

Steven Shapin, *A Social History of Truth. Civility and Science in Seventeenth-Century England* (Chicago & London: The University of Chicago Press, 1994), xxxi + 483 pp., \$34.50/£23.95. ISBN 0-226-75018-3.

"I will be focusing on science... and, in general, with epistemic items widely taken to be the hardest and most fundamental elements of scientific knowledge--statements of fact, observation reports and the like." --Shapin, A Social History of Truth p.5

*"My Lord, facts are like cows.
If you look them in the face hard enough, they generally run away"*
--Dorothy L. Sayers quoted by Karin Knorr-Cetina, Manufacture of Knowledge p. 1

"Boylean experimental philosophy was not the high road to modern experimentalism; it was a detour"
--P.Dear, Discipline and Experience p.3

I. Introduction: What is at Stake in A Social History of Truth

Rupture or continuity; origins or process in the history of early modern science? These are the historical issues at stake in Steven Shapin's A Social History of Truth, although many readers and reviewers have not realised it--so effective are Shapin's scholarship and his gentle rhetoric of persuasion. Can experimental science (or "experimental natural philosophy") be defined in its origins and essence in terms of the constitution of codes for relating, handling and accounting reports of "matters of fact"? Or, do early experimental natural philosophy and the emergent experimental sciences raise much more complex problems of human practice, material and [503] discursive, and of historical process? Did Robert Boyle and his friends initiate the culture (hence the effective practice) of (English) experimental science (or experimental natural philosophy), which has provided an "example" to the "entire world"? [p.143] Or, was Boyle a player, one of many, in a non-local European field of natural philosophical contention, and were Boyle's manoeuvrings a small part of an ongoing process?

Steven Shapin's impressive, widely read and admired book takes the former position on all these questions. We have doubts about the factual claims and historical implications involved in Shapin's choices. Our three epigraphs foreshadow almost everything we shall be saying about Shapin's historiography. We shall argue that

although Shapin's historiography claims descent from the noblest fruits of the sociology of scientific knowledge [SSK], the 'Shapin' who authored of A Social History of Truth appears to have forgotten some rather simple SSK premises: that facts are discourse-laden and theory-laden; that facts are pursued for their theoretical relevance; and that if a fact is not trivial, it must always have what Pinch has termed evidential context(s). These are textbook principles, as is the realisation that the complexity of facts far outstrips mere questions about actors' etiquettes for choosing and sifting reports of facts, in the proclaimed absence of theory. And yet, Shapin's historiography of experimental science has become focussed on solely the latter issue, and in doing so has stumbled into three pitfalls: He misconstrues the field of discourse and contention in which his actors actually moved--the agonistic field of natural philosophy; he tends to lose sight of the issues of evidential context and fact-theory relations; and, he neglects the issue of the embodiment of theory in instruments and experimental hardwares--a matter crucial to any understanding of experiment in natural philosophy and science.

II. A Look at Shapin's Argument Structure

Shapin is concerned with the social historical mechanics of the emergence of a new experimental natural philosophy or experimental science, as he indifferently terms it. This new knowledge culture revolved around the proper handling, reporting and assessing of reports of one-off facts, or testimonies about matters of fact, or 'moffs', as we shall call them. Shapin says some very odd things [504] about them, as we shall see. First we must review part of his argument, starting by considering what kind of problem was posed by the social and cognitive constitution of the moff culture. The issue was, of course, one of reliable reporting, of giving and detecting veracious testimony, that is, it was a problem of establishing and maintaining trust.

Shapin deploys theoretical tools from phenomenological sociology and symbolic interactionism. His fundamental, and correct premise is this. The fabric of our social relations is made of knowledge: not just knowledge of other people, but also knowledge of what the world is like. Similarly, our knowledge of what the world is like draws upon knowledge about other people--what they are like as sources of testimony; or as Shapin says, "whether and in what circumstances they may be trusted." [pp.xxv-xxvi] He tells us that,

Different members of a community hold knowledge that individuals may need to draw upon in order to perform practical actions: to manoeuvre in the material world, to confirm the status of their knowledge [and] to make new knowledge. Accordingly, in order for that knowledge to be effectively accessible to an individual--for an individual to have it, there needs to be some kind of moral bond between the individual and other members of the community. The word I propose to use to express this moral bond is *trust*. [p.7]

The new Shapinian experimental science relied upon trust, because of its dependence upon reports of one-off matters of fact packaged in reports and testimonies. The question, then, becomes this: What code, etiquette, moral economy, allowed the players to credit (or not credit) such reports, while at the same time maintaining order in the social system of the new science by preventing disastrous levels of disagreement, confrontation and controversy? Here Shapin's argument takes a

dazzlingly executed turn, guaranteed to win attention from Early Modern social historians. According to Shapin, the wider society contained just such a cultural repertoire of telling and hearing true testimony--in its codes of gentlemanly etiquette or civil conversation. Given this, Shapin can explain

...the origins of the practice known as English experimental philosophy. I say that this new culture emerged partly through the purposeful relocation of the conventions, codes and values of gentlemanly conversation into the domain of natural philosophy. [p.xvii]

Shapin's originating hero, indeed the hero of experimental science (at least until the, to Shapin, puzzling appearance of Newton) was Robert Boyle, who with his [505] natural philosophical dependants and cronies conceived and executed this cultural shift. They literally fabricated the grammar and code of civility and decorum of moff-handling, thus solving the problem of 'credibility' upon which the new experimental science or experimental natural philosophy depended. [p.xxi]

There had been a problem of credibility for two reasons. The first is sociological and eternal: there always is a problem of establishing trust in the basic conversational codes of any social system. The second reason attaches to contingencies of historical time and place: in the later Interregnum and more especially in the Restoration, political and religious constraints and tensions demanded that science and natural philosophy be conducted in a manner that did not reignite the epistemic cum religio-political disputes, sectarianism and dogmatism of the recent English past. Only in this way could a new experimental learning be established that would be both progressive and acceptable to the new post-Restoration Establishment.

Shapin offers a fascinating sketch biography of the younger Boyle. From about 1644 to roughly the mid-1650s Boyle spent time in his chemistry lab and developed "a considerable English natural- and moral-philosophical identity". [p.143] Here the emphasis is on the emerging moff etiquette and the identity of this new kind of experimental scientist or natural philosopher. We do not hear much about the content of Boyle's science, his theories, his natural philosophy, his initially Helmontian and increasingly corpuscular-mechanistic ontological commitments. As we shall see, this is because such natural philosophical content is not important for Shapin, and he suggests that it was not important for his moff-mongering Boyle.

From this platform expunged of natural philosophical content, Shapin launches further claims, but unfortunately never elaborates them in any detail: (1) that Boyle and others in the Interregnum developed this new experimental and empirical science; (2) that after 1660 this new moff science was institutionalised in the Royal Society of London. Shapin seems to take these as obvious facts, for which he is providing the deep cultural historical and biographical background with his useful, erudite descriptions of gentlemanly culture and Boyle's (scientifically and natural philosophically contentless) machinations therein. For example, he simply and tersely asserts that, "The later founding of Royal Society of London, and its effective international information exchange system, distributed [506] Boyle's example

throughout the world" (p.143, cf 121). We shall return to these claims below in Section IX

III. Shapin's Origin Tale--Boyle Breaks with Previous Scholarly Culture and Establishes Experimental Science or Experimental Natural Philosophy

Are we correct in asserting that A Social History of Truth is an origin tale for experimental science or experimental natural philosophy? Shapin is rather coy about the issue: He denies it, but not very vehemently, by simply waving a hand in the direction of the complex path from the seventeenth century to the present:

...I need to insist that we still know relatively little about the complex processes through which seventeenth-century practices were successively transformed into those of the present day. Any such version of this 'origins story' ought, therefore, to be argued with due modesty. (p. xviii)

Yet he also intones,

... insofar as history is taken to be defined by 'origins stories,' I offer this one. It is about the gentlemanly origins of seventeenth-century English experimental and observational natural philosophy. If one wants to read this book as a story about the 'gentlemanly origins of modern science,' I cannot prevent that reading. (p.xviii)

Leaving such rhetoric aside, the simple 'fact of the matter' is that A Social History of Truth is constructed as an origin story. We have already seen part of the basis for this claim, and the remainder of the case will now unfold as part of our critique. Time and again Shapin writes of 'new cultural practices such as experimental natural philosophy' and 'the new domain of experimental philosophy' and also of the 'new culture of English empirical science'. We are also encouraged to read an origin tale by the dust cover blurbs from noted scholars which gush over the 'social origins of modern science', or the origin of the codes that still form part of the 'basis for securing reliable knowledge about the natural world'.

Still, it is true that Shapin writes that the new moff culture started when the gentle civil conversational code was recruited "into the domain of natural philosophy" [p.xvii]. This seems to imply that 'natural philosophy' existed in some sense before Boyle and simply continued on with a Boylean twist. In fact, however, Shapin has no [507] such historiographical model in mind. Despite his lingering use of the term natural philosophy, Shapin is talking about an origin, resulting from a break or rupture with the previous scholarly and ungentlemanly natural philosophical culture. What this approach misses is that the culture of natural philosophy continued to exist; that Boyle was a player in it and that his rhetoric and machinating represented one set of utterances or positionings in it. We term Shapin's attempt to depict this rupture with natural philosophy and the birth of (real) experimental science 'the wedge argument', and we discuss it below in Section VI. For the moment we simply flag our contention that Shapin has no coherently expressed notion of what this prior culture was about.

We are going to suggest that Shapin and many of his readers are suffering from a failure to conceptualise sufficiently thoroughly, explicitly and consistently what needs to be conceptualised: the nature and dynamics of the actual socio-cognitive fields of natural knowledge making and breaking in which actors moved before, during and after Boyle and his moff boys pumped air. Before we explicate this in detail, we need to look at Shapin's epistemology, the model of knowledge and its acquisition underpinning his history of the advent of moff culture. His epistemology results in turn from a skewed application of certain types of social theory which, properly used, are of considerable value in the history and sociology of scientific knowledge.

IV. Shapin's Epistemology: The Stamp Album Model of Facts and Discoveries

Facts are rather complicated, as our epigraphs indicated and canonical works in SSK have established. Facts can only be distinguished from theory by virtue of actors' conventions and manoeuvres; and, once constructed, facts have a multitude of possible relevances and relations to theory. Actors may take a fact to 'establish' or 'prove' a theory; to 'confirm' or 'falsify' it; to 'support' or 'weaken' it, and so on. What is a fact, and what is its nature and type of 'relevance' to the theoretical context(s) in play, are all up for construction and negotiation in moff-handling communities (or so we were brought up to think, by the founders of SSK, such as Steven Shapin).

Hence, the history of science can be approached very fruitfully by concentrating on facts. However the complexity of facts far outstrips [508] mere questions about actors' etiquettes for choosing, handling and sifting reports of facts. This meta-'fact' holds even in those odd (and inevitably rhetorically sculpted) cases when facts are presented in the tendentious absence of theory by an actor such as Boyle. Yet, Shapin's interpretation of the rise of experimental science has become focussed on just this issue of etiquettes for handling putative moffs in a proclaimedly atheoretical void.

This brings us to the terrible necessity of looking 'in the eye' those moffs that "are the hardest and most fundamental elements of scientific knowledge". Early on Shapin tells us that his book is "concerned with questions about the grounds of scientific knowledge." [p.xv] These are no slips of Shapin's mouse. He uses similar locutions throughout. For example, he repeatedly states that knowledge comes from taking on board reliable reports, apparently in a mechanical and additive way. Knowledge becomes, dare we say it, a collection of moffs. He specifically says general views of the world are built up through the actions by which testimonies are accepted or rejected [pp. 42,65,124]; and, "by the constitutively moral processes by which we credit other's relations and take their accounts into our stocks of knowledge about the world." [p.8]

What we have, therefore, is a picture quite surprising in an SSK theorist, especially one weaned on the likes Schutz and Luckmann. These moffs are like stamps; the actor's stock of knowledge is like a slowly filling stamp album. This seems to be naive Baconianism run rampant; or rather, it is a Baconian style of rhetoric, also used

by Robert Boyle, which Shapin takes on board directly. Here is Shapin on Boyle, not Boyle rhetoricising about Boyle:

His particular natural-philosophical and natural-historical claims provided much of the factual stuff of the world upon which late seventeenth century experimentalists operated. [p.126]

Theory is just not on the agenda here, nor are the complexity and fluidity of the relations actors may negotiate between theories and facts. Shapin, supposedly following Boyle, simply asserts the unexplained priority of matters of fact in the knowledge business, at one point writing, "Matters of fact were to be made into the foundations for properly scientific procedures." [p.124] On reflection, we find we do not know exactly what that means, but it sounds epistemologically and methodologically rather nasty, not perhaps if Boyle had said it, but certainly when Steven Shapin does. As if to solidify this picture of an atheoretical inductivism, Shapin tells us that, "Boyle arguably entered more matters of fact in the register of [509] the seventeenth-century English experimental community than any other individual." [p.126] Shapin is not saying Boyle tried to persuade his friends of this; rather, Shapin is reporting it to us as a first order historical fact about first order moffs in the seventeenth century. With respect for everything Shapin has taught us, that has to be one of the oddest things he has ever written.

Let's now examine why Shapin is burdened with a postage stamp and album model of facts and knowledge, and perhaps why he seems happy to be there. Certainly, the stamp album or 'register' of moffs is an odd notion of the 'stock of knowledge' for somebody so deeply into phenomenological sociology (from which the term derives). Even in common sense reality, the so-called 'natural attitude', a good Schutzian will see the stock of knowledge as ordered into categories and 'typifications' networked together by the sinews of 'structures of relevance'.¹ Now, in common sense reality a one-off 'factual' report bears to such categories and typifications a set of possible relations similar to those a reported fact bears to a theory in any given science (which is a second order, specialised, realm of category, typification and relevance structure). The issue, in both cases, is the socially constructed and negotiated relevance of a report to already held bodies of theory, systems of typifications, grids of categories, etc. Only naive inductivism sees nuggetty facts, moffs, taken up mechanically to constitute *ipso facto* knowledge. In sociology, only some sort of novel, inductivist/empiricist version of Schutz would see, in everyday reality, or in esoteric fields of science, trustworthy moffs collected into otherwise empty registers, all pre-existent categories, typifications, theories and structures of relevance having disappeared in a puff of naive empiricist smoke.

But, let us be clear, the last things Shapin seems to want to deal with are theories, categories, or networks of typifications carried by an actor into a perceiving, reporting or listening situation, because this would evaporate the picture of a

¹ A.Schutz, Reflections on the Problem of Relevance (New Haven & London: Yale University Press, 1970); A. Schutz and T.Luckmann, The Structures of the Life World (London: Heineman, 1974).

nuggetty moff reality and gentlemanly organised and traded stamp collections. In the case of scientists (or rather, in our period, natural philosophers) clearly what is required is that we attend to their scientific (that is natural philosophical) beliefs, concepts, categories and systems of relevance. One would have thought this was the whole point of the history and sociology of science since Kuhn, Hanson and Feyerabend. We cannot pretend that such conceptual structures and [510] contexts do not exist, even if actors sometimes play that game, thus complicating our analysis of their beliefs and actions.

When one adopts the moff and catalogue image of knowledge, one of the first casualties is any understanding of what discovery is, in modern SSK terms. Surely today 'everybody' knows that discovery involves theoretical contexts. Shapin helped to teach us that. As we tell distance learning students tuning into our nationally broadcast first year subject on the Australian Broadcasting Corporation's Radio National Network,

Discoveries are linkages of certain changes of existing theory with certain specified material practices--they are not just new ideas, nor are they the uncovering of nuggetty 'things' existing in nature.²

For a moff (itself a delicate theory-loaded construct) to be important in this sense, something 'significant' in our present stock of knowledge has to be at stake. If moffs are just pasted into the unstructured pages of an inductivist album of knowledge, they are trivial; only if an ecological change in categories and properties is negotiated and agreed amongst the relevant community is something significant and theory- and discourse- relevant happening--a discovery being 'made'. In such a case what is at stake is not just trust in the communicator and hence in the moff, but issues about the structuring of the moff itself, and about the argument purporting to establish the relation of the moff to suggested alterations in the existing stock of knowledge.

Discovery is a rather central phenomenon in science. Much scientific work would seem to be about just this sort of negotiation of discovery. But note how small a part is played by the issue of trusting the initial report. In our view Shapin's paramount problem is that the codes about reporting moffs (or what he calls the moral aspects of the collective nature of knowledge) do not equate to the truth- making or discovery-producing economy of a culture, although that is precisely what he maintains without the benefit of any sustained argument against the existing consensus in basic, post-Kuhnian sociology and history of scientific knowledge.

Shapin creates the impression of an atheoretical and argument-less context of learning or discovery by trading mainly upon rather trivial moffs, where trivial means reports couched in ordinary language and of relevance only to everyday,

² J.A.Schuster, *Introduction to Science and Technology Studies, Program 3: 'Theory, Fact & Discovery in the New History & Sociology of Science'* (Open Learning Australia SCI 14, Australian Broadcasting Corporation, Radio National) broadcast, July 1995, Dec 1995, March 1996.

common sense stocks of knowledge. Even here, of course, a Schutzian should analyse the interplay between a new report and the existing content of an actor's everyday stock of knowledge. Trivial reports are only atheoretical in the sense of being irrelevant (until further notice) to whatever second order scientific theories the actor may hold. However, we [511] shall see below in Section VIII that the situation is even more parlous in Shapin's case. In a number of his extended examples of Boyle's work, Shapin tries to establish the commonsensical, and hence to him atheoretical character of the situation, by suppressing mention of the actual natural philosophical theoretical contexts that were in play. It is true that Boyle did often play a native's game of 'hide the theory'. Historians of science need to deconstruct that game and contextualise it; not enrol in it themselves.

In sum Shapin's approach to moffs involves a two fold displacement from what a reasonable reading of phenomenological sociology would recommend: (1) Even in common-sense reality, the natural attitude, actors' shaping, communicating and reception of facts depends upon the stock of knowledge to hand which includes but vastly surpasses the mere existence or not of a trust code; (2) Stocks of knowledge are socially segmented, and at the very least we can speak of specialised, expert, professional and disciplinary stocks of knowledge, which, of course are sedimented upon experts' commonsense stocks during the course of training. Again, the shaping, communication and acceptance of facts in any such specialised realm depends crucially on the complexion of the relevant stock of expert knowledge, a complexity which includes but vastly surpasses the existence of a trust code in one or another expert realm.³

Shapin reduces all knowledge issues to the level of common sense reality, and tries to establish that gentlemen were the privileged truth tellers in that realm. He thereby ignores the existence of specialised stocks of knowledge in seventeenth century English society and hence the cleavage of 'trust' relations along such professional, disciplinary and social boundaries.⁴ Let us leave aside the fact that most of the evidence Shapin proffers about gentlemanly truth telling deals with oaths, promises, and the contexts of law and social obligation, rather than facts of the matter in any practical or expert setting. [pp. 65-68] It remains the case that when, for example, Shapin tells us that "All 'normal' gentlemen were deemed to be perceptually competent" [p. 78], he is riding roughshod over the patently obvious fact there existed segmented domains of competence. Gentlemen might have been

³ On the dependence of the hearing of accounts as 'possibly true' within the domain of commonsense stocks of actors' knowledge see Harvey Sacks, "On the Analysability of Stories by Children" in J.J. Gumperz and D. Hymes (eds.), *Directions in Sociolinguistics* (New York: Holt, Rinehart and Winston, 1972), 325-45. For an application of this approach to the issue of scientific experts 'hearing' methodologically couched stories as 'possibly true' within their stocks of expert knowledge see J.A. Schuster, 'Methodologies as Mythic Structures: A Preface to the Future Historiography of Method', *Metascience*, Vol. 1/2 (1984), 15-36.

⁴ Shapin tells us that technical competence and terms of art were to be avoided in gentlemanly civil conversation.(p.118) In that case what kind of medium for specialised, expert human inquiry was constituted by gentlemanly decorum?

the 'truth tellers' in early modern England; but, if you were a gent, or even if you were not, which gents did you believe (and why) on the relevant issues; for example, Presbyterians, or Independents, or Episcopalians on church government; those for divine right or parliament on the [512] constitution; Paracelsians or Gassendists or Boyleans in natural philosophy and so on? Similarly, common sense surely recognised that not all gentlemen had studied, say, fortification, and that in such a specialty those that had studied, gents as well as master (but non-gentle yet literate) artisans, were competent sensory agents in that domain. Shapin constantly harks back to the contemporary rhetoric of gentlemanly disinterestedness grounded in lack of constraint and need to work. [p. 83] This again assumes no natural philosophical agon and no other segmentation of beliefs. Did not gentlemen and those hearing gentleman take stock of others' expertises, and their own stock of knowledge, when the subject was facts about commerce, trade, theology, politics or natural philosophy?⁵

Let us be clear, in general social theoretical terms some trust code or other is always implicated in social interaction and cohesion, and indeed in whatever stock of knowledge is in play. But the existence of a trust code completely under determines the working of any particular stock of knowledge. Moreover, the differentiation of specialised stocks of knowledge in any socially and institutionally differentiated society entrains differentiations in trust codes and/or their modes of deployment. Shapin ignores that differentiation under the pretence that gentlemanly trust and decorum, established at best on evidence for ordinary interactions, somehow constituted, or better reconstituted whole swathes of socially segmented knowledge, creating a vast epistemological desert of flattened, atheoretical, domain-less 'matters of fact'. In our view this is unconvincing on social theoretical and historical grounds.

We return, then, to the surprising conclusion stated earlier: Shapinian historiography has squandered some of its SSK capital, including the notion that if a fact is not scientifically trivial, it must always have 'evidential contexts' in one or more domains of scientific discourse. In order better to understand what Shapin is missing about these evidential contexts, we now turn to the most problematical of Shapin's categories--that of 'natural philosophy'. Shapin's argument ultimately depends upon the notion that Boylean experimental practices broke with a dying and sterile culture of natural philosophy. Before we examine Shapin's 'wedge strategy' for demonstrating this remarkable originative act on the part of Boyle, we must stop and consider the possibility of a frankly diametrically opposed conception--the

⁵ Shapin tells us (p.101) that decorum dictated that a gentleman adapt to the circumstances in which he found himself, and that hierarchical relations shaped that adaptability. He adds (p.102) that principles of civil behaviour might be universal but manifestations were taken to be highly local. And, he continues (p.103) obligation to truth telling was relative to setting. One had to tell the truth to equals 'so far as prudence dictated'. This suggest to us that there was a "decorum of expertise" keyed to the actor's 'knowledge about the social segmentation of the stock of knowledge' (a fine Schutzian concept by the way. Schutz and Luckman, op. cit. note 1 pp.301, 321) We can speak of a "decorum of expertise" to explain why young gentlemen deferred to philosophy tutors; or why "Master" Thomas Harriot is listened to by his gentlemanly employers: he was the expert and they knew it.

possibility that Boyle did not kill off natural philosophy, but was just a player within it, a [513] player with a particular, and less than hugely successful set of stratagems and rhetorics for attempting to establish dominance within it.

V. The Field of Natural Philosophy--Process and Continuity in Explaining the Scientific Revolution Period

We readily admit that many of our criticisms of Shapin are set in the context of our development of a quite different conception of the culture of natural philosophy, which requires some explication before we return to Shapin's text. We believe that Shapin's bid for a grand explanation of the rise of early experimentalism can only be effectively explored and criticised from the standpoint of historiographical projects of similar scope and intent. We do not insist on the correctness of this particular model of natural philosophy, but we do hold that future understandings of early experimentalism will be better served by descendants of this model than by offspring of tales of 'origins' and moffs.

As a result of our earlier independent work, we premise that the chief object of inquiry in the history of Early Modern science is not some abstract category of 'Science', nor even the then existing or emerging narrower sciences, such as mechanics, optics, astronomy, anatomy, hydro or aero-statics, astrology, medical theory, etc; but rather a larger enterprise that encapsulated and transcended them--a long-lived, variously institutionalised, internally contested, yet coherent discursive formation--the field of 'natural philosophy'.⁶ The 'history of science' in the early modern period was very much the history of natural philosophy, consisting largely in actors' struggles for cognitive and social hegemony in this evolving field of contention. This involves looking at the numerous competing claims and stratagems of natural philosophers, as well as the structures that held together this common 'domain of discourse'. The traditional literature on the Scientific Revolution certainly missed this category, although the term 'natural philosophy' was endemically employed in varied atheoretical ways as a synonym for Science or (some of) the sciences. However, to see the Scientific Revolution as a conflict of systems of natural philosophies is not a new insight.⁷ The deeper issue is this: how precisely to characterise natural philosophy, as an evolving field of socio-cognitive contention in which the Early Modern debates over [514] particular systems took place? Our current work involves modelling natural philosophy as an evolving discursive formation, the structure and dynamics of which supplies the answer to the question

⁶ J.A.Schuster 'The Scientific Revolution' in R. Olby et al (eds.), The Companion to the History of Modern Science (London: Routledge,1990), pp. 217-242; J.A. Schuster and G.Watchirs, 'Natural Philosophy, Experiment and Discourse in the 18th Century: Beyond the Kuhn/Bachelard Problematic', in H. Legrand (ed.), Experimental Inquiries (Dordrecht: Kluwer, 1990) pp. 1-48.; J.A.Schuster, 'Descartes Agonistes: New Tales of Cartesian Natural Philosophy, Perspectives on Science, Vol. 3 (1995), 99-145.

⁷In the 1940s Robert Lenoble put the conflict of varieties of natural philosophy onto the map of the Scientific Revolution, followed in the 1960s by key articles of P.M Rattansi. Later Brian Easlea and J.R.Ravetz tried to popularise this view, but it is not the dominant view, nor has it been further theorised.

"Of what was the Scientific 'Revolution' a process of change?" We hope thereby to avoid the traditional fascination with specious 'revolutions', de novo 'origins' or heroic 'discoveries', in favour of a dynamics of continuous historical process in interpreting the period of the 'Scientific Revolution'.

Our model takes into account these and other factors:

(1) Each system proposed by a claimant purported to explain in a 'systematically coherent' way the nature of matter, the cosmological structuring of that matter, the principles of causation and the methodological principles for acquiring such knowledge, as well as the relation of that cosmos to God and the place of humankind in nature.

(2) At the basis of any system of natural philosophy resided one or more privileged images or metaphors, the articulation of which underlay one or more of the elements and/or their modes of systematic interrelation. Because a natural philosophy was, so to speak, selective in its choice of constitutive metaphor, it might seem that it necessarily embodied and represented certain interests and values at the expense of others.⁸ However, the values, goals and meanings 'belonging' to a given natural philosophy were necessarily open to variation and reinterpretation. Any constitutive metaphor might embody a rich array of potential meanings, differentially understood and stressed by readers.

(3) A large portion of the fundamental level of the grammar of natural philosophical utterance was carried and communicated within one dominant variety of natural philosophy--Scholastic Aristotelianism. The continuing, institutionalised legacy of Aristotelian natural philosophy and logic was deeply sedimented into university trained natural philosophers--the vast majority--regardless of whether they later advocated alternative systems and indulged in anti-Scholastic rhetoric as part of their field-tactics. This common deep structure provided notions of method, and some of the cognitive aims of natural philosophical systems in general, right across the contested domain of natural philosophy.⁹ For natural philosophers other than Aristotelians, Scholastic Aristotelianism [515] was neither 'dead' nor 'hegemonic'. Rather, it had implanted part of the grammar of just about any new systematic utterance.

(4) There were two other main features of the grammar of natural philosophical utterance. First, the field of natural philosophising was surrounded by, and variously linked to, a number of 'contextual' enterprises and discourses, such as theology, university teaching, and the practical arts, which natural philosophers variously perceived as important to control, shape or co-opt. Natural philosophers had varying views about the relevance of the contextual features, about their relative

⁸This belief has always licensed simplistic 'external' explanations of natural philosophies in terms of value homologies to larger social, political or economic structures or interests.

⁹A.B.H.Taylor, *The Early Royal Society of London*, (Ph.D diss. Univ of Melbourne, 1991).

weighting, and about the 'linkages' between their natural philosophy and their preferred configuration of contextual features. Our field-in-process notion of natural philosophy allows us to conceptualise, in the manner of Foucault in The Archaeology of Knowledge, that natural philosophers contended to 'articulate' natural philosophical discourse onto such 'exogenous', structures and fields of discourse.¹⁰

Secondly, we envision the contested articulation of varieties of natural philosophy onto certain selections, shapings and orderings of those traditions of narrow technical practice that we above termed the 'sciences' in the period. Each 'science' was variously considered to be part of, or conditioned by, one or another system of natural philosophy. The shape of a science, its development and its very legitimacy depended upon the character of its natural philosophical 'cover' or articulation. And, the shape of a natural philosophical system depended in part upon its manner of articulation upon a certain selection and grading of existing 'science-like' sub-domains.¹¹

Hence, high stakes were placed upon establishing the 'correct' system of nature, for it was expected to yield a social and cognitive hegemony over the other players, whilst also enabling the victors to enforce their own notions about the proper content, order and goals of the 'sciences', and about the correct relations between natural philosophy and the 'contextual' discourses.¹²

(5) The long term dynamics of the field may be mapped. One does not see sudden ruptures in the field, or its sudden collapse and replacement by experimental science.¹³ Moreover, different natural philosophers may be mapped as players in

¹⁰ For example, we can read Paolo Rossi (*Philosophy, Technology and the Arts in the Early Modern Era* [New York: Harper and Row, 1970]) as having described how a body of non-natural philosophical literature on practical arts and its values---itself articulated upon structural changes in sixteenth century Europe--was co-opted and redeployed by Bacon and Descartes into core debates inside the natural philosophical field, as part of their respective strategies of advancing their overall claims in the natural philosophical agon.

¹¹The history of the more narrow scientific traditions cannot be narrated apart from detailed attention to process in the field of natural philosophising and vice versa, and this holds as well for the newly emergent traditions of experimental sciences of the 18th century, as we show in Section VII. cf Schuster and Watchirs, op. cit. note 6.

¹² We are developing in this connection: (1) the Foucaultian notion that in large measure a field of discourse is continuously defined by discourse about and upon its boundaries; and (2) the Bourdieuan insight that an agonistic field is continuously defined by the play of the contending actors as they acquire and expend field-specific resources. cf Schuster, op. cit. (1995) note 6.

¹³We shall see, in Section VII below that from the mid 17th century experimental natural philosophy may be defined, generically, by contested attempts to articulate competing natural philosophical utterances onto artefacts and hardwares imported into the field of discourse; that is, by an imperative to articulate natural philosophical utterances onto 'hardware-objects' of discourse. Furthermore, by extension, and most interestingly, the model provides a basis for describing the largely unintended long term processes by which such experimentally oriented natural philosophising led in the 18th

this field. We can, for example, put the field model to work to reconcile and explain two competing impetuses in Descartes' natural philosophical project, embodied in two systematic treatises, *Le Monde* and *Principia philosophiae*, which have spawned competing historical interpretations [516] of his career.¹⁴ Rather than searching for the real Descartes and his real system (and thus acting like players in that discursive agon), we should note that Descartes enthusiastically wrote both treatises, in differing circumstances, in the continuing service of his campaign to win dominance in natural philosophy. His shifting evaluation about what relevant peers might make of his bids helps explain why he moved from the former text to the latter.¹⁵

Not surprisingly, then, a field approach to natural philosophy is central to avoiding the pitfalls haunting Shapin's account. Shapin takes Boyle's rhetorical tactics as the essence of his position, and as constituting a break from the pedantic, and ungentlemanly culture of natural philosophy out into the wide, modern space of experimental science. That is, in our terms, Shapin takes certain of Boyle's posturings about moffs in the agonistic field of natural philosophy over against Scholastics, Cartesians and Hobbesians as decisive evidence that Boyle was not a natural philosopher at all! However, just as we would not now view Descartes' late-career legitimation stratagems simply as the peculiar, brilliant private gymnastics of a detached and aloof mind, but rather as gambits in the micro-politics of natural philosophical persuasion and contention; so Boyle's ploys should not be read as marking a break with the field, but rather as plays within it. Boyle's posturing about matters of fact was done for rhetorical, tactical purposes, within the natural philosophy game, which he intended to dominate in the interest not only of his fact rhetoric, but more centrally in the interest of his mechanistic ontology and voluntarist theology. To grasp all this requires, of course, a well designed conception of natural philosophising adequate to its tasks of historical narrative and explanation. Such a conception is rather unlikely to arise through re-broadcast of Boyle's own tactical rhetoric.

century to the crystallisation of relatively more discrete, smaller, emergent domains of experimental work--new, emergent experimental sciences, if you will.

¹⁴ Schuster, op. cit (1995) [note 6], 138-41. One version of Descartes' natural philosophy was expressed in his project of *Le Monde*, completed by 1633 but unpublished in his life time. It emphasises physical explanation and problem solving at the expense of explicit metaphysical legitimation and grounding via articulation of scholastic categories. The other, better known version, manifested in the *Principles of Philosophy* (1644) stresses the requirement of textbook-like systematisation and presents metaphysical and theological grounding as 'essential' to the constitution and quality of a natural philosophy.

¹⁵ Ibid., 138-41. Similarly, despite the whiggish leanings of some historians of science, Descartes' correspondent and ally, Marin Mersenne, was not an anti-natural philosopher, proto-positivist or first 'physico-mathematical' scientist; but rather a natural philosopher with a fully explicable, radically minimalist notion of natural philosophical utterance, who arguably did not win the day, but who did provide some claim-shaping rhetoric that subsequently circulated in the wider field.

VI. Shapin's Wedge Argument: Recruiting Gentle Codes to Break the Continuity of Natural Philosophical Culture

Shapin's entire approach pivots on one fundamental argumentative tactic which embodies a set of finely honed, persuasive but arguably quite false historical contentions. We have termed this tactic the 'wedge argument'. It asserts that Boyle was indeed the originator of a new experimental science which superseded or rendered irrelevant the old culture of natural philosophy. Shapin uses all his scholarly [517] resources to set up this argument, and it literally provides the ligaments and sinews of his entire book. From within our continuity and process model of natural philosophy the wedge argument lacks persuasiveness. We maintain that no defeat, marginalisation or supersession of the culture of natural philosophy occurred; that Boyle was simply a clever (if ultimately rather unsuccessful)¹⁶ natural philosopher whose somewhat novel rhetorical tactics and agonistic posturing have been read by Shapin as marking the origin of a modern, 'post-natural philosophical', new experimental science.

To the (small) extent that Shapin recognises the existence of the field of natural philosophy, he characterises it as possessing all the crabbed, pedantic, tendentious, petty and unproductive qualities colloquially attributed to the Scholastic Aristotelianism of the universities by its seventeenth century opponents; that is, by proponents of alternative natural philosophical approaches (such as the Hon. Robert Boyle).¹⁷ There is evidence, and Shapin uses it, to the effect that the qualities and virtues of a gentleman were contrasted favourably to those of closeted scholars and scholastics. Since Shapin is arguing that the new experimental science is founded upon the recruitment and institutionalisation of gentlemanly modes of civil conversation and codes of trust, he can easily suggest that the constitution of Boylean science marks the effective end of the now superseded scholarly and rather melancholic culture.

There are problems, however. Shapin never justifies or defends in detail his assimilation of all pre-Boylean natural philosophising to the crabbed Scholasticism represented in the contemporary rhetoric he cites. This means he has trouble assimilating to his schema bold and brilliant natural philosophical innovators such

¹⁶ T. S. Kuhn, 'Robert Boyle and Structural Chemistry in the Seventeenth Century, *Isis*, Vol. 43 (1952), 12-36.; P. Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago and London: University of Chicago Press, 1995), 3, 242.

¹⁷ See Shapin pp. 170,175-6,178-180,191. Shapin attributes to Boyle advocacy of values for natural knowledge such as openness and intelligibility. As Rossi (op. cit. note 10) long ago showed these were promoted into the natural philosophical agon much earlier than Boyle, by figures such as Bacon, Descartes and Hobbes; but Shapin recognises no prior natural philosophical endeavour other than scholastic Aristotelianism. At p.191 Shapin remarks that, "The rejection of the rule of Aristotle, and the consequent opening up of nature's possibilities, precipitated enormous problems of practical authority for the new experimental enterprise..." This is hardly an adequate picture of the field of natural philosophy in the early and mid- seventeenth century in respect of the issue of experimentalism and the values increasingly advanced to promote and reflect it.

as Descartes and Hobbes.¹⁸ Similarly, he is mystified by Newton, who, coming after Boyle, seems to rewrite the newly founded rules of modern science.¹⁹ There is no mystery, however, if one places Descartes and Hobbes in the context of the field of natural philosophy, along with the Aristotelians they sought to displace. Nor is Newton mystifying if he is seen as a towering, rather idiosyncratic and unanticipated player in natural philosophy²⁰, who supersedes Boyle, the natural philosopher, not Boyle the (almost) founder of modern experimental science. In short, Boyle's rhetoric actually provides Shapin with his historical categories, and this simply will not do, provided, as we maintain, Boyle like everybody else was a rhetorically armed and adept player of natural philosophy, not its modern conqueror. [518]

Finally, the limitations of Shapin's procedure become quite apparent when one examines closely his depiction of the sources and qualities of gentlemanly culture, bearing in mind our earlier observations about the segmentation of knowledge. Shapin is at great pains to ignore or down play the role of education in the making or marking of a gentleman. (p. 57) Yet he must admit that education was among the two or three most important (and hence contested and debated) markers of gentility. (pp.63-64) Consequently, Shapin must ignore the educational commonalities shared by his heroes and villains in order to drive the wedge between mere pedants and scholarly natural philosophers on the one hand, and new gentlemanly scientists on the other.²¹ But, this wedge has no point and no force whatsoever. Beyond the contemporary debates about how, why and whether education conferred or bespoke gentility, the salient fact is this: The most commonly shared property of seventeenth century seekers after knowledge of nature was university training.²² And this

¹⁸ Shapin pp. 170-1. Shapin has Boyle creating his new experimental science against the background of the scholar/gentleman distinction. This assimilates all non-Boylean natural philosophical players, even non-Aristotelians to the defunct 'scholar' category.

¹⁹ Shapin p. 185: "Until the public emergence of Isaac Newton as the paladin of a newly mathematicised natural philosophical enterprise, no single individual was so widely pointed to as the pattern of what it meant to be an English philosopher of nature." On the issue of mathematical natural philosophising and mathematicised experimentalism in Newton, and before Newton, Cf J.A.Schuster and Alan B. H. Taylor, 'Seized by the Spirit of Modern Science' Essay Review of P. Dear, op. cit. note 16, *Metascience* (forthcoming)

²⁰ Schuster, op. cit. (1990) note 6.

²¹ Shapin (pp.58-9) shows, correctly that there was much contention and worry about the blurring of social categories and about social mobility. But then why would anybody in the natural philosophical agon (other than the Boylean gambit players themselves) think gentility solved the 'endemic problem' of credibility? Following Lawrence Stone, Shapin correctly concludes that by the 1640s real problems existed in telling who was a gentleman--exactly what should be expected in the super-heated, contentious, highly segmented political, religious and natural philosophical atmosphere of the time. But, we should note, as Shapin is not wont to do, that there was much less of a problem about determining who was a natural philosopher: He was somebody who uttered propositions in the field of natural philosophy, whether one agreed with him or not, and regardless of the rhetorics one might use to try to exclude or marginalise competing players.

²² Taylor, op. cit. note 9.

training was not so much initiation into chapter and verse of Scholastic Aristotelianism, which some accepted and some did not; but rather training into the foundations of the grammar and aims of natural philosophising, conveyed through Scholastic training and practiced by Aristotelians and alternative natural philosophers alike, as we observed earlier in Section V.²³

VII. Experimental Natural Philosophy and the Evolving Culture of Natural Philosophy

Thus far we have argued that Shapin has an origin story of a new culture of reporting of atheoretical moffs. Against this, we have recalled that facts are theory-loaded, social accomplishments, not simply nuggets traded on trust. We have also suggested that changes in this period occur within a culture of natural philosophical discourse, a culture that Shapin claims, on the basis of the wedge argument, was superseded by Boyle and his friends. These are serious problems, but they are about to get worse. We wish now to raise the stakes for Shapin by exposing, in contrast to his view of experimental science/experimental natural philosophy, what experimental natural philosophy looks like within our historiography of the dynamics of the field of natural philosophy. Certainly for us the advent of experimental natural philosophy was not a question of the defeat of the culture of natural philosophy, and certainly not an [519] issue about atheoretical moffs. Indeed, the problem of experimental science, and experimental natural philosophy raises the issue of theory-loading of facts to new levels. This is because we must take seriously the ways in which experimental instruments, apparatus and hardwares themselves embody theoretical commitments, and the ways that embodiment of theory in hardware is fundamental to the nature and existence of experimental science.

No matter how experimental natural philosophy (and experimental science) have been viewed in the literature of the Scientific Revolution, before Shapin they certainly have never been identified with the onset of a particular code or etiquette for reporting and receiving claimed facts, and this has been for very good historical and philosophical reasons. Shapin's idea of a claim handling code centred on trust holds (by his own admission) in all circumstances of effective communication. So, wherever there exists experimental science (whatever it may prove to be), moral codes for report handling will be present. This is merely a background or necessary condition for communication. The problem of conceptualising experimental natural philosophy as an historical category and explaining its rise and ramification into

²³There was a wide variety of natural philosophically literate men. Natural philosophical culture housed priests, monks, gentlemen, doctors, lawyers, some master artisans, etc. Thus what gentlemen at university shared with 'non gents' and 'quasi gents' at university was the culture of natural philosophising--a culture they shared and contested with privately educated toffs such as Boyle, and with self educated upwardly mobile elite craft masters and practical mathematicians. On Shapin's own showing gentility was a fluid and disputed category and marker. Exposure to the educated culture of natural philosophising was clear and unequivocal. What natural philosophical scholars and natural philosophically educated gentlemen shared was just that, the general lineaments of the culture of natural philosophy. The history of natural philosophy and of natural philosophers must start with social historical premises such as these, not from Robert Boyle's 'player's' rhetoric.

experimental sciences therefore is likely to extend a bit beyond the issue of moff codes.

As a 'matter of fact' our historiography, centred on the category of the field of natural philosophy in process, was constructed in part around the problem of explaining experimental natural philosophy, and its correlate, the Kuhn-Bachelard problem of explaining the emergence of new experimental sciences across the eighteenth century.²⁴ Our own perspective builds upon aspects of the work of Rossi, Kuhn and Bachelard, linking some of their tantalising insights with the field model of natural philosophy, along with a strong dose of SSK analysis of experiment and testing, as developed from Collins, Pinch, Barnes, and indeed, Shapin. In our view experimental natural philosophy, as it emerged in the mid seventeenth century, consisted in a particular set of developments of possibilities inherent the generic field of natural philosophy which unfolded in a unique temporal sequence over the next century and a half. That is, as a mode of natural philosophising, experimental natural philosophy had a complex diachronic structure--it was not all of one piece, nor did it originate all at one time. Moreover, it had complex and ill-understood outcomes, as over time it variously debouched upon, or ramified into, new domains of theoretical-experimental practice that one might care to denominate [520] 'experimental sciences' (or, to shift metaphors, as such domains thickened and dropped out of a still living field of natural philosophising).

It is a curious fact that recent influential writings on the history of experimental science, such as those by Shapin and Dear, tend to ignore the rich heritage of Gaston Bachelard's approach to the subject. We contend that in this area it is essential to work with Bachelardian concepts, or rather with a sociologised form of Bachelard, a Bachelard reinterpreted through spectacles provided by contemporary studies of the sociology of experiment. For Bachelard a *phénoméno-technique*, the germ of an experimental science or discipline, is a coupling of mathematically articulated theory and hardware. We envisioned a simpler more qualitative form of a Bachelardian *phénoméno-technique* as a coupling of discourse and hardware, and further loosened up Bachelard, sociologically and discursively, with a sense of the fluidity and reinterpretability of the hardware-discourse couple. We viewed apparently stable, 'closed' couples as contingent products, outcomes of social processes of closure, and subject to the permanent possibility of re-negotiation of the coupled elements. We arrived at the following principles for dealing with experimental hardwares:

- (1) Theory is negotiationally embodied in hardwares: Instrumental and experimental hardwares are the accountably 'adequate' materialisations of theories and concepts agreed/enforced as 'adequate' at some point in the past and 'until further notice'.
- (2) The 'results' of experiments are doubly constructed: The performance and output of a hardware is doubly discursive and socially negotiated. Firstly, because the hardware is consensually frozen discourse and, secondly, because any output

²⁴Schuster and Watchirs, op. cit. note 6.

requires further interpretation, evaluation and deployment in its intended argumentative contexts. (So much for moffs.) Nevertheless, in conceiving of the situation of early experimentalists in a way not constrained by Bachelard's overly rigid conception of the *phénoméno-technical* couple, something was still missing, despite the help we derived from the sociology of experiment. In these early struggles to construct and impose hardware-discourse couplings, what was at stake were initial embodiments within hardwares of qualitative conceptions and theories--not mathematics, but discourse. So the issue was, "Where did the discourse come from for the first experimental couplings"? [521] And the answer was our third principle:

(3) The 'theory' in early experimentation was natural philosophical discourse. In particular it was that generic mode of natural philosophising which after about 1650 termed itself 'experimental' but which also needs to be called corpuscular-mechanical: a consensual melding of Gassendi and Descartes with other ingredients including Baconian method and experiment rhetoric emphasising experimental grounding, tentative theorising, exploitation of instruments and possible technological benefits. This experimental-corpuscular-mechanism [hereafter ECM], constituted a large generic sector in the field of natural philosophy. ECM's Baconized rhetoric of experiment hardly sufficed to guide the details of natural philosophical utterances or their linkage and articulation profiles with the 'sciences'; yet, like any such otherwise ineffective and vague method doctrine, it did help to shape the way knowledge claims were assembled, negotiated and entrenched or rejected. It also functioned at the institutional level in providing some of the rhetorical resources for solidifying and delimiting legitimate practitioners and practices, as in the apologetical and programmatic rhetoric of the Royal Society. In these two related guises the method rhetoric of ECM helped constitute the sense in which natural philosophy had become 'experimental', at least in an initial 'declaratory' sense.²⁵ That is, the key imperative of experimental natural philosophy was to forge new hardware-discourse couples or co-opt old ones from other areas, for example craft practices. But at first ECM was largely a declaratory doctrine asserting its command over existing constellations of couples without having much to show for the effort, except the declaratory rhetoric. Hence the excitement over nodes of actual embodiment, as in the air pump. But, that does not mean experimental science starts with the air pump episodes, as our fourth principle entails.

(4) Part of experimental natural philosophy tended to dissolve into 'sciences'--over the course of 18th century: In general terms, the imperative in experimental natural philosophy had been toward co-option of existing couples or creation of new ones by implanting natural philosophical discourse in hardware. But, this global aim exerted subtle pressures toward creating domains of relative specialisation: that is, sets of related couples, bathed in increasingly domain-specific swathes of discourse. Increasingly dense clusters or nodes of hardware-discourse couples emerged within

²⁵ Ibid., 21, 33-4.

the natural philosophical field--dealing [522] with and constituting such areas as electricity, magnetism, heat, and the physical properties of light. These crystallised into something like semi-autonomous fields of experimental research (which Kuhn read as 'first paradigms' in the experimental fields and Bachelard as breaks from 'pre-science' to 'science'). In that situation, natural philosophers had more and more to bend what otherwise would have been the needs of systematic utterance to the requirements of engaging the ongoing flow of practice and explanation in such nodes. Not only did this affect the grammar of natural philosophical systematics; it also tended to destroy natural philosophy as a cultural enterprise, as the nodes branched into fields which required no natural philosophical 'cover stories'.²⁶

Our model explains the indisputable facts of the heterogeneity of the 18th century experimental accomplishments, their wide dispersion in time, and the fact that qualitative command of *phénoméno-technics* always preceded the possible later accomplishment of a mathematico-experimental practice. Theory (natural philosophical discourse), hardware and outputs were the key variables and determinants of a closed achievement in experimental natural philosophy. Natural philosophical utterances varied; hardwares and their outputs carried charges of ordinary language description (as well as non-philosophical craft dialects). All these materials needed to be mutually adjusted, agreed and crystallised into a node of natural philosophical discourse-of-the-hardware-and-its-outputs in order for an achievement in experimental natural philosophy to have taken place. Codes and etiquettes for moff handling rather under determine how specific swathes of natural philosophical discourse might be consensually articulated onto specific hardwares and their outputs.

The implications of this sort of model for Shapin are clear: Historians of experimental sciences in this period should not seek the mythical origins of experimental Science or sciences. We do not need tales of the sudden discovery of proper methods, metaphysics or models, (or now etiquettes) which supposedly shattered the debilitating fascination exercised by putatively non- or pre-scientific beliefs and enterprises such as 'natural philosophy'. We need to know how the field of natural philosophy functioned and what early experimental natural philosophy was, as well as how and why the pursuit of embodiment of discourse in hardware promoted the crystallisation out of more narrow hardware-discourse domains. We are not watching the right thing if we study one case only and call it an [523] origin; or, if we study rather trivial cases of reportage of atheoretical fact, rather than domains of hardware-discourse couplings, especially as they thicken and drop out of the wider natural philosophical field. For all these reasons it would obviously be unwise to argue that the debate over the air pump, or any other specific event in experimental natural philosophy (as Peter Dear does with Newton's prism

²⁶Hence we see that without the earlier advent of declaratory experimental natural philosophy we might not have had later experimental sciences, although we may well have had further evolved domains of hardware-discourse in technics and the practical arts, domains standing apart and over against the elite culture of natural philosophy, as has been customary in the West until the seventeenth century.

experiments) sets a transferable model or exemplar for other emergences of efficacious experimental practice.²⁷

We have been talking about experiments and hardwares, but what about cases where no hardware was in question--no couple or phenomeno-technic was in the offing, and where the reports were couched simply in ordinary language? Here the candidate moffs sat very loosely in relation to high natural philosophical talk. Shapin sometimes appears to suggest the whole of experimental science or experimental natural philosophy consisted in this sort of case. As we are about to see, some of Shapin's most elaborate examples are of this type. Boyle quite often retailed such claims, and they do look like relatively stable, tradeable, postage stamp moffs. What has to be said is that this sort of case is rather trivial, representing what Kuhn long ago called 'random Baconian fact gathering'. To be precise, it is not the case that these reports had no theoretical (natural philosophical) relevance. It is rather that these reports tended to sit well adrift of their natural philosophical evidential contexts; or, as we would say, they fit under a loose declaratory corpuscular-mechanical motivation, rationale and legitimation. Their triviality consisted in the facts that they were not phénoméno-technically embodied, and that they did not prompt focussed, concerted renegotiations of natural philosophical discourse. So it is only by concentrating on rather common sense reports, and by tending to ignore the small but real theoretical relevances that they may have had, that Shapin can begin to persuade us of the existence of the new moff culture.

VIII. Shapin's Case Studies: Moff Culture as Pretence or Triviality; Or, 'Let's Pretend there is no Culture of Natural Philosophy'

Let us now consider two of Shapin's case studies of moff culture. We want to show the overarching presence of the natural philosophical [524] in these cases, and the subordinate nature of the moff codes. The first case involves the problem of reports about the hydrostatics of icebergs. (pp.247-258) In a rare acknowledgment of Boyle's mechanical philosophy Shapin begins by conceding that Boyle aimed in his cold

²⁷What then of the air pump case? Indeed looked at closely the air pump case does display a struggle to constitute it as a discourse-hardware couple in the loosened up Bachelardian sense mooted earlier, where the discourse comes from the competing natural philosophical utterances of Boyle and Hobbes, rather than being a case of Boylean 'matters of fact' versus Hobbes' old fashioned natural philosophical discourse. Shapin makes this point, despite himself. It is entailed in his talk about Boyle on the air pump--when he concedes that for Boyle sometimes the spring of the air is a cause of lower level reported phenomena, and sometimes it is the moff revealed by the air pump. (Shapin, 'Pump and Circumstance: Robert Boyle's Literary Technology', *Social Studies of Science*, Vol. 14 (1984), 481-520 at 486,501; Shapin and S. Schaffer, *Leviathan and the Air Pump* (Princeton: Princeton University Press, 1985), 51, 203.) So, Shapin admits what our model would predict, that Boyle shifts moffs up and down. Reports about the outputs of theory- or discourse-embodying hardware are themselves theory-loaded and renegotiable in theory-relevant ways. Certainly Boyle positions himself against Hobbes, making the spring of air a moff, when it suits him. But we must note the grammar of his moves. Only one of Boyle's dupes would take Boyle's rhetoric seriously as unveiling of a true, ie really real moff. That is why the air pump case is interesting-- but it certainly is not the "Origin of Experimental Science" where atheoretical moffs are made. It is as we said, one small case, one small moment in an endemic discourse-hardware dialectic set in train by the experimental imperative in late seventeenth century mechanistic natural philosophy.

researches at offering hypotheses which assimilate the nature and effects of cold to the basic principles of the mechanical philosophy. However, this is just about the last we hear about this corpuscular-mechanical cognitive interest and evidential context.

Boyle's credible witnesses in this case were Messrs. Thomas James and Samuel Collins. The former was a reliable sea captain, the latter a gentlemanly physician who had travelled in Russia. They supplied factual testimony whose competence and sincerity Boyle saw no adequate reason to doubt. They reported that some of the icebergs they had seen sat up above the water line higher than one would expect on hydrostatical principles. These principles were further backed up by experience in temperate climes, with fresh water ice. There was no question here of Boyle challenging the principles of hydrostatics as he took them from Archimedes and Simon Stevin. Nor, Shapin points out, would Boyle dispute James and Collins reports: How then, asks Shapin, would Boyle be able to use them in future or "look them in the face". [p.256] This point relates to Shapin's characteristic stress on the theme of disaster and chaos in the scientific community if putatively good truth tellers were not handled with kid gloves. For Shapin the situation therefore is that the two gentleman had privileged testimony. He writes,

I have indicated, how Boyle warranted James and Collins as credible persons. I need now briefly to show how he wove their credible testimony into an ontological and moral fabric." [pp.252-3]

Before proceeding let us note with care that last phrase. "Ontological" will not mean the theoretical realm of corpuscular-mechanism; it will mean a kind of common sense world of networks of everyday objects and persons.

Shapin shows Boyle casting around for possible answers, speculating about the makeup and formation of icebergs, about the properties of salt water, about the possibility of additional reports. Eventually Boyle has to leave the matter there, leading Shapin to conclude,

For the present Boyle's ingenious mundane reasoning produced a [525] picture which contained sincere and competent sea captains, true Archimedean hydrostatical principles, and icebergs with too much of their mass above the sea...etc. Civil conversation about these matters might continue. [p.258]

Here then is the Shapinian 'ontological fabric', attributed to Boyle, and meaning 'world picture in common sense terms in which these reports and the theories of Archimedes can live side by side'.

However, let us recall the fact that Robert Boyle was a committed corpuscular-mechanical natural philosopher. We may ask what happened to corpuscular mechanism in this episode, for on Shapin's telling it has more or less slipped from view, by virtue of the simple expedient of Shapin neglecting to consider it or factor it in. If we take seriously Boyle's mechanistic natural philosophy and his explorations of cold within it, we notice the following: When Boyle accepted the gents' reports, a

possible realm of (mechanical) theorising (hypothesising) about the formation and constitution icebergs opened up, and there was an opportunity to learn (or 'discover' in the social sense [section IV]) more about cold in corpuscular mechanical terms, just what Shapin initially described as Boyle's concern. Clearly, Boyle's acceptance of the reports occurred in a theory laden context. Had he denied the reports, he might or might not have been able to look Captain James in the eye; but he certainly would have had nothing interesting to learn or 'discover' in corpuscular mechanism about icebergs and cold, because everything would have been seen to occur just the same at home in Stalbridge. Did Boyle accept the (theoretically anomalous but theoretically inviting) reports from the nice gents just so he could continue to look them in the eye; or, was it also (indeed mainly) in order to hold open an evidential context where one played with corpuscular mechanical explanations of cold. Unlike Shapin, we readily grant the first reason, but insist upon subordinating it to the second.

The second case study reveals again Shapin's marginalising of the natural philosophical context. It involves divers' reports and hydrostatics; or, we should say, hydrostatics interpreted and legitimated from Boyle's mechanistic standpoint, not the competing standpoints of his fellow mechanists, Hobbes or Descartes. [pp.258-66] The problem involves testimony from divers who report no perception or feeling of pressure on their bodies at considerable depths. Shapin points out we have an inverse case to that of the icebergs and Captains: Potentially troubling testimony came not from trustworthy gents like James and Collins, but from people who might not necessarily be regarded as creditworthy. Accordingly, [526] Shapin asks, "How might disqualifications--physical and moral--to perceive and reliably report upon nature be used in deciding nature's make up?" [p.258] But again, "nature's make up", "ontology", is not an issue in Boyle's corpuscular-mechanism, but an issue of everyday categories about people and objects.

Shapin cites Boyle's opinion that these divers reports were a 'noble objection' to mechanical hydrostatics. [p.261] But once again the field of disputation in natural philosophy is only barely visible, and so Boyle's natural philosophical need to get this testimony set aside is played down. Shapin does tell us that the wily Hobbes accepted the divers' reports and gave a mechanical explanation of them. And later Shapin says [pp.262, 260] that some anti-mechanist natural philosophers also accepted the divers' reports. This surely entails that the issue was the credibility of a big piece of Boyle's mechanical philosophy--his inference (or prediction) of pressure from corpuscular-mechanical principles, which had been challenged by his natural philosophical opponents, mechanists and non-mechanists, who eagerly seized upon the testimony of 'no perceived pressure' as 'evidence' for their respective articulations (in our sense of Section V) of their respective natural philosophies upon hydrostatics.

Shapin does not deal with this as a contest of natural philosophical claims and modes of articulation upon hydrostatics, in which Boyle, as usual, is a player. In his version, we learn that eventually further evidence showed up, in the form of artefacts, pewter bottles from wrecks, crushed, apparently by the water pressure,

and that Boyle was happy with the new evidence. Let us note carefully Shapin's assessment:

The redistribution of credibility between vulgar divers and pewter-bottles was meant to secure assent to a picture of the world which contained the empirical fact of water-pressure.
[p.264]

Note here how bland Shapin's description is with respect to the natural philosophical agon. Recall Hobbes, another different kind of mechanist had accepted the divers' reports and given a mechanical explanation of why their feeling was reliable. In the realm of mechanical natural philosophy that move challenged Boyle on a mechanically produced pressure. Shapin underplays this by depicting Boyle as concerned to network pewter bottles and "the empirical fact" [only] of water pressure. Where, we might well ask, [527] is the evidential context, the theory, the natural philosophical agon, apparently neglected in this account?

Shapin then proceeds to describe Boyle's ruminations over the years about this issue. He concentrates on Boyle shifting the boundaries and categories between reliable and non reliable reports. Shapin is fascinated by what we would term Boyle's early and later framing rhetoric. After he had heard about pewter bottles [and bleeding ears etc], Boyle could come out against the testimony of the vulgar and unreliable commercial divers. But Shapin is uninterested in the natural philosophical agon and the related cognitive interests and tactics that seem to be driving the affair. We suggest that Shapin's account, properly reinterpreted does not show experimental practice constituted by gentlemanly etiquette. Rather it shows that when some vulgar witnesses wound up not supporting what Boyle needed for natural philosophical reasons and in a natural philosophical evidential context, Boyle wheeled out available social-epistemic markers of the type Shapin well describes. Moreover, what Boyle needed by way of evidence was determined by natural philosophical theory, interests and the current state of debate with rivals. The social markers, grounded in etiquette and civility labels, were one subsidiary tool kit in this overriding natural philosophical agon.

Let us summarise, then: In the case of icebergs, Boyle had plenty of good testimony. He therefore was interested in the debate at the level of (corpuscular-mechanical) iceberg models, not in questioning the principles of hydrostatics or the reports: He might have interestingly varied his (mechanical) hypotheses about the nature of icebergs and so learned and theorised more about cold. But, if Boyle had denied the gents' reports, the science of Archimedes would still have had to be preserved, and, in addition previous understandings of icebergs would have been confirmed, and hence Boyle could have learned nothing new in corpuscular-mechanical terms about 'cold' and in relation to icebergs: natural philosophical play would have been blocked. So natural philosophical conflicts and interests were paramount. The social markers of trust and civility were auxiliary tools to hand for positioning, selecting and shaping subsidiary utterances about evidence for natural philosophical claims, arguments and goals.

In the divers case, Boyle had already gotten his corpuscular mechanistic explanation of pressure. This he would really have liked to maintain, but presumably not because sacred atheoretical [528] moffs were at stake or because the social order of the moff collecting club would otherwise collapse into chaos. Rather, it was because Boyle's reputation for claim making in natural philosophy was partially invested in this particular arena. So, the reports of evidence arguably relevant to these natural philosophical gems would just have to be found consistent with them. Credibility was at stake at the level of natural philosophical claim making and explanation. Again natural philosophical conflict and interests were driving utterance and action.

In our view these two cases do not tend to show the etiquette of trust as essential to some new experimental science or some new cognitive practice of any kind. They suggest that the basic decisions about acceptance or rejection of reports were determined by Boyle's interests in the realm of theory, corpuscular-mechanical explanation, and in the bits of mathematical science (hydrostatics) that he dared not question. What Shapin has mapped is some of Boyle's subordinate rhetorical apparatus for accounting such acceptances or rejections, rather than the fundamental social and cognitive mechanisms of his making and breaking of knowledge claims in natural philosophy. Boyle was a committed mechanical philosopher and voluntarist theologian, and he was also a master of the rhetoric and protocols of mobilising experiments and reported fact for his natural philosophical positionings, against non-mechanists and other mechanists of different tempers. That was all that was happening, and it was happening inside the contested field of natural philosophy, and not over against it in some bright originary dawn of "English experimental science".

IX. Boyle, Matters of Fact and the Early Royal Society

Shapin (followed by Dear)²⁸ sees Boyle's experimental natural philosophy as fully institutionalised and practiced in the early Royal Society,

The later founding of the Royal Society of London, and its effective international exchange system, distributed Boyle's example throughout the world. (p.143)

Boyle's approach reigned at the Royal Society until his death in 1691 (p.291) when it was replaced by Newtonian mathematicised natural philosophy.²⁹ (p.185) Both Shapin and Dear believe the Royal Society was a passive factory churning out experimental knowledge, first entirely under the Boylean, then entirely under the Newtonian experimental regime. [529] Shapin does not document, as opposed to

²⁸ Dear, op. cit. note 16, 242.

²⁹Or, as Dear tells us early in his book, "When the Royal Society gave itself up, at the end of the century, to the self-labelled "mathematical" natural philosophy of Isaac Newton, it reconnected with the enterprise that [John] Wilkins had earlier identified as its intended calling." Dear, op. cit. note 16, 2-3

assert the existence of these totalising regimes of experimental activity, and the reason may be that no such Boylean, then Newtonian regime existed.³⁰

We are engaged in a detailed study of the interplay in the early Royal Society of organisational features and dynamics on the one hand, and modes of production and communication of knowledge claims on the other. It is important to treat an organisation such as the Royal Society seriously in organisational terms, rather than taking the institution to consist of the summed sociological characteristics of its members, and its cognitive activities to consist, magically, in the full implementation of what, after all, were only rhetorical codes of presentation and communication of results. In the era of Boyle, for example, we find a complex, and evolving pattern of organisational decision-making and action-taking, which was always partially constitutive of the varied modes and manners in which natural knowledge was presented, solicited, communicated and legitimated. Indeed we find that it was the very design and functioning of that organisational "decision/action" patterning that allowed for and encouraged this variety. We find that a rhetorical regime of civility and decorum, and the presentation of matters of fact were certainly to the fore; but much overlaid with other codes and practices.³¹

For example, mathematics did play a part in the shaping of some experimental natural philosophical activities and discourses, which were presented in more traditional, non-Boylean, methodological garb. In 1680 that arch Boylean, Robert Hooke, conducted at the Royal Society an extended set of mathematically and experimentally articulated investigations on metals. He performed experiments to 'prove' and 'make evident' his premises, and when an experimental result did not thus conform to theory, he ignored it: So much for the moral integrity of matters of fact, compared to the drive and trust of theory directed research on a mathematical deductivist model.³² Theory was at times coded just below the surface of superficially matter of fact material. At other times it was present, despite what some scholars have recently claimed, for example, in the extended investigations of 'may-dew' orchestrated by Henshaw.³³ Witnessing did validate in house performances, but experimental findings were published in the name of individual authors. There were various types of experimental actions and activities at the

³⁰Shapin's argument on this point builds to a climax at pp.122-4. Viewed with a sceptical eye, biased by the field model of natural philosophy, he seems to be rehearsing (some) rhetoric of actors, not accounting for social-cognitive actions and decisions inside the Royal Society.

³¹ It is mainly at meetings where we can detect witnessing of rather non theory-relevant matters of fact. Public representations of research tended to be on the authority of the individual author and reports of experiments often betray large slabs of theory-relevance.

³²T. Birch, *The History of the Royal Society of London: For Improving of Natural Knowledge From Its First Rise* (4 vols, London, 1756-7); reprinted (New York, 1968), vol. iii, 509; vol. iv, 6.

³³ A.B.H. Taylor, 'An Episode with Maydew', *History of Science*, Vol. 32 (1994), 163-84.

meetings with quite different styles, contents, and organisational implications and functions. [530]

Shapin offers an example of the Boylean gentility code at work in the controversy regarding the observations of comets.³⁴ He argues that the Society was mobilising the codes of trust and decorum to produce a solution to the social and cognitive turbulence involved in the dispute. Now, on Shapin's own evidence a dispassionate reader might well conclude that the controversy was resolved through expert consensus formation on technical astronomical grounds, with a rather twitty top dressing of hand wringing by certain Royal Society heavy weights. Shapin also fails to stress evidence as to what was going on in the background as Oldenburg coordinated communications in this affair. Oldenburg was often provocative in his attempts to generate critical replies. For example, he passed on to Hooke a letter from Auzout which he, Oldenburg, peppered with remarks to provoke Hooke (unsuccessfully) into a reply. Included in the notes, in the margin of the letter Oldenburg wrote: "Non sequitur, You must rally with him again.", "What say you to this?"; "A handsome sting again will be necessary"; "Me thinks, here you may tosse railleries wth him."³⁵ Oldenburg was quite ready to use what Boyle would have argued was an ungentlemanly style and therefore to generate tension by counterpointing uncivil behaviour against accepted gentlemanly behaviour. His uncivil behaviour extended to his editing of the correspondence, where he deleted what he did not approve of and preferred to publish what he stated 'pleases me'.³⁶

Our point is not that examples and counter-examples can be multiplied ad infinitum, although that does tend to deflate Shapin's grandiose picture of a Boylean moff hegemony. The real issue is that all these examples need to be studied carefully in the contexts of the natural philosophical agon; the existence of expert knowledges; and the organisational patterning of actions and decisions in institutions such as the Royal Society. Moff rhetoric and its conversational and textual codes has a place in all this, but it hardly supplies the deep structures of historical explanation and understanding.

X. Conclusion.

We are left by Shapin with a take on Boyle, or on a part of Boyle that is prominent, because of how Boyle coyly played the natural philosophy game. Boyle did indeed sometimes down play explicit [531] corpuscular-mechanical systematics (matter theory and theology linkages). He did sometime perseverate on atheoretical 'matters of fact' and the culture of trust involved in reporting and trading them. But, this does not mean he and his friends broke free of the natural philosophical agon to play in a new, really scientific field. Shapin constructs his case by systematic neglect of

³⁴ Shapin, pp.266-91

³⁵ [H. Oldenburg] *The Correspondence of Henry Oldenburg*, edited and translated by A. R. Hall and M. Boas Hall (13 vols; vol i-ix, Madison and Milwaukee, 1965-1973; vol x-xi, London, 1975-977; vol xii-xiii, London, 1986), ii, 474.

³⁶ *ibid.*, 474

the natural philosophical aims and tactics of Boyle. Properly interpreted, the examples illustrate garden variety manoeuvring within a well understood actors' world of natural philosophy. But we do not hear much about that world, or about the scholarship from Lenoble to Rattansi and Rossi and beyond that could explicate it. And that, as a matter of fact, is a great pity.