

Chapter 28: Schuster and Taylor vs Shapin on the 'Origin of Modern Science' and Nature of the Early Royal Society of London¹

[1] Shapin's Historiography: Trust, Matters of Fact and the Origins of Modern Science

In his *Social History of Truth*, Stephen Shapin built upon his work with Simon Schaffer to sculpt a rupture and origin story: The defeat of traditional natural philosophy—pedantic, scholarly, ungentlemanly—and the origin, the birth, at the Royal Society, of an essentially new genre of experimental science at the hands of Robert Boyle and his friends. Shapin's 'natural philosophy' is mainly identified with scholastic Aristotelianism, but also includes mechanists such as Hobbes and Descartes. Boyle defeated natural philosophy by devising the code for the proper handling and assessing of reports about atheoretical 'matters of fact' [hereafter 'moffs']. This solved the problem of 'credibility', required by the new Experimental Science, and has provided an "example" to the "entire world" [p.143] He did this by drawing upon etiquettes of gentlemanly behaviour.

Shapin thus offers an origin story of experimental Science--capital S; and, he claims to identify the essence of that practice. Shapin's discovery is widely touted in cultural studies, STS, and public understanding of science. Shapin never admits that the field or subculture of natural philosophy continued to exist, as civil war raged within it. And that his hero of anti-natural philosophy, Boyle was a player, one of many, in a European field of natural philosophical contention.

[2] A Look at Shapin's Argument Structure

[2a] Handling Matters of Fact

Shapin is concerned with the social historical mechanics of the emergence of this new experimental science. 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. For Shapin the key problem involved in the origin of the new experimental sciences was the problem of reliable reporting of facts, of giving and detecting veracious testimony, reports of fact, and inside that it was a problem of establishing and maintaining trust in what people reported to each other as 'matters of fact'.

[2b] Sociological Underpinnings

Shapin deploys theoretical tools from sociology. 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,

¹ References: readings for relevant tutorial

John Henry, *The Scientific Revolution*, pp.33-41

Steven Shapin, *The Social History of Truth*, pp.119-125

Steven Shapin, *The Scientific Revolution*, pp.85-112

J.A.Schuster and A.B.H.Taylor, "Blind Trust: The Gentlemanly Origins of Experimental Science", *Social Studies of Science* 27 (1997) 503-536.

A.B.H.Taylor and J.A.Schuster, "Organising the Experimental Life at the Early Royal Society" *Joint US/British/Canadian History of Science Societies Meeting*, St Louis Missouri, August 2000.

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]

[2c] Where to get the code or rules for trustworthy handling of matters of fact?

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. 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]²

[2d] Robert Boyle as exemplar

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 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]

[2e] Why there was a problem of credible witnessing and reporting of facts

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.

[2f] More Big Historical Claims

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

² This is where Shapin shows his debt to Robert Merton. He has transcribed Merton's sociological strategy of explanation into a new key: instead of Puritan values or norms being imported from the larger society into 'science' to form it up as an institution; we have Shapin's idea that codes of trust and civil reporting of facts were brought in from gentlemanly culture, to form the sociological essence of the 'new science'. An exact explanatory analogue for Merton's famous tactic?

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 Boyle's example throughout the world" (p.143, cf 121).

[3] Taylor, Schuster Critique: Historiography of Scientific Revolution and of the Royal Society

[3a] Matters of fact are strange, atheoretical 'things'

This brings us back to Shapin and his matters of fact: about which he makes some very odd claims : "matters of fact are the hardest and most fundamental elements of scientific knowledge." Knowledge becomes...—a collection of moffs He says general views of the world are built up through the actions by which testimonies are accepted or rejected. Or, " by the constitutively moral processes by which we credit other's relations and take their accounts into our stocks of knowledge about the world."

[3b] Matters of fact as stamps in an album

So, the moffs are like stamps, the stocks of actor's knowledge like stamp albums—Baconianism run rampant. Shapin takes Boyle's posturings on board directly and uncritically: 'Boyle arguably entered more matters of fact in the register of the seventeenth-century English experimental community than any other individual.' I love that quote, it has to be one of the silliest things Shapin has ever said. Matters of fact are atheoretical, they are not theory laden, they cannot be for Shapin and he does not want them to be.

[3c] What happened to theory-loading of facts?

Let me now show you why Shapin is stuck with a postage stamp and album model of facts and knowledge, and why Shapin is happy to be stuck. The stamp album or register of moffs is an odd notion of stock of knowledge for somebody so deeply into sociology of scientific knowledge and HPS. Only a naive inductivism sees nuggetty facts, moffs, taken up mechanically to constitute knowledge. What happened to the theory-ladenness of facts, all facts, all the time?

Well last thing Shapin wants to deal with is theory, and in particular the natural philosophical beliefs of his actors. This, I think, is because it would force him to acknowledge two general topographical features about knowledge which do not suit his thesis here.

[4] Theoretical Crunch Issues

[4a] A significant claim, which if accepted = 'a discovery', always involves a bid to make some change in the previously accepted conceptual grid.

For a moff to be important in this sense, something 'significant' in our present grid of knowledge has to be at stake. In that case what is at issue is an argument from the moff to the claimed alterations in existing grid of knowledge (including knowledge embodied in hardware as a result of such negotiations). Note how small a part is played by the issue of trusting the initial report about the moff. Shapin's problem is that the codes about reporting moffs do not equate to the

truth- making or discovery-producing economy of a culture, although that is precisely what he maintains.

[4b] Knowledge is always socially segmented—expertise, and trusting people other than gentlemen, or experts who happen to be gentlemen.

The segmentation and differentiation within the stock of knowledge. Now, everybody knows we live and act in cognitive-moral universes, involving shared stocks of everyday concepts, norms and theory-loaded facts. Trust in some claim-makers is part of that socio-cognitive weave. But note I said trust in some **claim makers**, not **fact tellers**. The claims we trust need not be limited to what members chose to label facts of the matter. We can take on trust theories, norms, long narratives, judgements etc. This tiny refinement of Shapin erodes his whole position, because it follows that we have to take very seriously the segmentation of knowledge: Trust **is** endemic, a glue of any interaction, Shapin is right. But he ignores the equal truth that stocks of knowledge are segmented and differentiated, as are stocks of knowledge about the segmentation and differentiation of knowledge.

[5] Whom do you trust in 17th century England?

So, gentlemen might have been truth tellers in early modern England--but, if you were a gent, or even if you weren't, which gents did you believe on the relevant issues—Presbyterians, or Congregationalists, or Episcopalians on church government; those for divine right or parliament on the constitution; Paracelsians or Mechanists in natural philosophy and so on.

[6] Boyle's rhetoric of presentation and legitimation took Shapin in (like Merton before him)

Boyle may have pretended to be of no natural philosophical sect, to eschew theory and to depend upon facts, but that was just his way of posturing in the natural philosophical contest of the day: In Boyle's positional rhetoric, moffs exist independently of natural philosophical explanations. But, that is only his story his framing rhetoric—and he doesn't stick to it. He is a corpuscular mechanist, and he opposes other natural philosophers who are not mechanists or who are mechanists not to his taste or under his control. And we will see that makes a difference to his work. Yet, poor old Shapin has swallowed Boyle's positional rhetoric hook line and sinker, mistaking it for the guts of some de novo form of efficacious scientific practice with a vast and portentous destiny.

When you look at the examples of Boyle's work that Shapin presents (see Schuster & Taylor essay review of Shapin, "Blind Trust..."), you do not find that the etiquette of trust was essential to some new science. You see that the basic decisions about acceptance and non-acceptance of other people's 'factual' 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 dare not question, and which fully accord with his modes of explanation. The subordinate rhetoric for accounting such acceptances or rejections is what Shapin has mapped.

[7] So let's turn to our own work on the Royal Society:

[7a] A new approach—decision/action patterns—what typically happened and how it affected the construction and publication of knowledge claims

Alan Taylor pioneered the study of organisational dynamics of scientific institutions. He emphasised that we should examine routine patterns of decision-making and action-taking. Through such decision/action patterns experimental activities were conducted inside the institution and communications about experiments were produced for outside consumption.

Alan's main findings about decision action patterns in the early Royal Society are these:

[1] The initiation and leadership of investigations was use left to those who chose to be active; but, once initiated, a course of in-house investigation was subject to routine patterns, routinely ordered by the Fellows—chiefly the repetition, re-testing and witnessing of experiments as a condition for their acceptance.

[2] This situation imposed a pattern on the actual meetings: When material emerged at the Society's meetings outside the sphere of competence of most of the Fellows, they were left with little opportunity to participate. But, for the few Fellows who had the required skills and motivation, a window of opportunity opened for them to introduce activities and views that favoured their preferred beliefs and interests. Lead players and routine patterns interacted.

[3] For an active member interested in pushing his own agendas, the best policy was to keep the rest of the Fellows engaged by feeding routine decisions their way, and to manage any spin off committees or actions the Fellows might collectively call for. It's useful to follow how leading players played the patterns and exploited them.

[4] As to news about experiments communicated to the public, this routinely failed to report the Society's in house processes of the construction of experimental knowledge. The Society systematically created the impression that it was merely the impartial reporter of news supplied by others.

[5] Finally the decision/action patterns formed sedimentary layers. The deepest level was the corporate policy settings of the institution as laid down in its Charters and Statutes. These announced the Society's product, claims about natural knowledge, and they discussed in a general way the Society's strategy—how its founders saw its operations would achieve its aim—the improvement of natural knowledge. Importantly, the Royal Society's corporate policies didn't include a detailed day to day method, or procedure for investigating nature by means of experiments. Indeed the 'Corporate Policy' was to use disparate views on approaches to experiment. So, in terms of 'Corporate policy on method' the Royal Society was neither Baconian, Boylean nor Newtonian: it was open textured by design. The decision/action patterns allowed for a flexible repertoire of inquiry—differences of approach and interest amongst the Fellows could manifest themselves under the umbrella of the Society's stated aims..

Some sociological articulation needs to be add to Alan's original insights:

[a] The decision/action patterns were present above and beyond specific passages of activity, but existed only in so far as actors reproduced them, by acting within them. For an actor, the patterns were constraints and resources—played slightly differently by actors with differing skills and agendas. It's important to identify the decision/action patterns, and also to follow actors as they play within and upon them.

[b] When following actors, remember that competences and interests are two different things. Actors deploy their competences and interests within given fault lines of decision/action patterns. Hence the resulting processes and products inside the institution never reduce to sums of individual interests or competences.

[7b] Natural philosophical agendas and theories are in play; mathematics is indeed sometimes used

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. 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.

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. 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.

[7c] Findings from Taylor/Schuster case studies of 'courses of experimental inquiry at the Royal Society

An Example: The Royal Society's Metals Project of 1680 It involved melting, mixing and finding the specific gravity of several metals and their various mixtures. The project began in 1679 with discussions about variations in atmospheric pressure. Some speculated that an increase results from an influx of air from elsewhere, raising the height of the atmosphere locally; others suggested that whilst the quantity of air remains the same, there's an influx of "steams, fumes or saline substances" which dissolve in the air, making it heavier without increasing its volume. Hooke, Wren and Croune discussed these matters, and towards the end of one discussion, Hooke said that, he had . . . "[carried out] an experiment proving the [inter-] penetration of liquors...by putting oil of vitriol into water" with the result that the two liquors together took up much less room than when they were separated. From this Hooke adduced a principle: When "bodies really penetrate into the texture of each other;" both together take up less room than they did before they mixed, making a body with a higher specific gravity than either reagent.

Hooke offered to table experimental evidence "mak[ing] evident" his premises, and the Fellows accepted. At a meeting on 4 December 1679, Hooke melted copper and tin together into one mass with a specific weight higher than the average of the specific weights of the ingredients. He boasted that the cause was "the penetration, which those bodies made into one another."

In the Project, experiments were performed and witnessed under the supervision of a committee, with Hooke coordinating the activity, bringing the members of the committee together and arranging the experiments. At the weekly Society meetings, Hooke reported on the previous week's experiments. This would be registered and the Fellows encouraged to decide upon metals for the next

experiment or to suggest practical applications. At the end of the Project, Hooke presented a table of the experimental results.

Three key remarks about all this:

[1] Natural philosophical commitments and procedures were woven throughout this project. Hooke and the Fellows granted authority in this inquiry to the mechanical philosophy and to the power of mathematical deduction: Hooke's "premise" expresses Boyle's corpuscular-mechanical matter theory. The corpuscles of one substance are dispersed into the myriad voids existing amongst and within the complex particles of another substance, hence total volume decreases and specific gravity increases. Hooke organised the project by imposing his theory and the warrant of deductive reasoning within natural philosophy. The experiments were intended to demonstrate Hooke's propositions, not to investigate them or to 'test' them. Of course, many experimental results supported Hooke's premises. But, when a mixture of tin and lead showed a specific gravity smaller, than it "really ought" to be, Hooke suavely declared that the result challenged "the invention of Archimedes". He meant that a result violating Archimedean statics cannot be accepted and so it cannot count against his corpuscular-mechanical premises either. This was accepted without question. Clearly, unacceptable experimental results couldn't destroy the authority of the mechanical philosophy and apparently rigorous deductions from it.

[2] This Metals project met the Corporate aims expressed in the Charters and Statutes: It provided empirical support for the authority of the premises; arguably, it conduced to "the improvement of natural philosophy". But, it didn't supply atheoretical matters of fact; it didn't repress natural philosophical commitments; and mathematical deduction could warrant the authority of claims, even when "matters of fact" spoke against them.

[3] Here we see both the routine patterns and the driving of a project through the patterns by a lead actor. Hooke's early moves crystallised a project out of rambling discussions. He then guided the usual routine actions, took control of them and shaped the project. Hooke managed the series of experiments, directed the individual experiments, and controlled how the results of the experiments were evaluated. But, at all times he worked under the routine decision/action pattern for meetings. That pattern promoted witnessing and group responsibility, as Shapin would expect, but it allowed for, indeed required, bravura organisational performances, such as Hooke's. Additionally, and crucially, it allowed for the explicit role of natural philosophical theory and the authority of mathematical deduction.

All of this prompts a very important observation: The role of group witnessing and responsibility—and indeed the role of the whole pattern of decisions and actions—was not to produce atheoretical matters of fact. Of course, Shapin and co are absolutely right that the Royal Society was not about the sponsoring of the natural philosophical interests and agendas of individuals. But that didn't mean that there were no natural philosophical agendas and interests in play. The patterning of witnessing, re-testing, and having the Fellows comment on or suggest routine experiments served precisely to channel and mute contrasting, even competing natural philosophical interests and competences toward the production of experimentally based claims. But the claims were in and of the natural philosophical domain.

When it came to publishing these claims, matters were different. There was no publication in this case, but as we see in our other cases, publication involved

heavy censorship and spin doctoring, producing discursive items that might look like atheoretical matters of fact, but their inescapable theoretical contexts and penumbra were never far away, or always successfully buried.

[7d] How the Royal Society functioned—correcting Shapin

[1] We've seen no evidence to support the idea that the Royal Society became functionally identified with a regime of gentlemanly exchange of atheoretical matters of fact. It's true that in house these institutions repeated and witnessed experiments; but, there was clear loading of theory and of personal agendas in all these cases.

[2] Things that look like the sort of 'matters of fact' that Shapin envisions were published, but there were always theory-loading, evidential contexts and sometimes mathematical articulation behind them. We suggest that the unit of communication that the Society aimed at wasn't the impossible ideal of the atheoretical matter of fact. Rather, in each case some neat communication package was devised. Theory-loading; contexts of theoretical relevance; and mathematical articulation were tacitly or overtly recognised in the package. But the package was shaped to be the neatest, "matter of fact" looking message possible, given the in house activities and debates, and the wider knowledge politics of the case.

[3] All of our cases reinforce our primary contention that to understand how the Royal Society produced and communicated experimental natural knowledge, one should analyse its organisational strategies. Additionally, and this is the key: whilst we study the organisational strategies for making and communicating experimental inquiries, we always remember the actors were also working within, and contending about, claims and agendas in natural philosophy, a Europe wide, dynamic and evolving elite culture of making and breaking systematic knowledge of nature. So, it is clearly incorrect and ahistorical to pretend that natural philosophy whatever it was, had died, and a new Science was born in these sites.

[8] Conclusion—getting Bobby Boyle back in perspective

Shapin sees Boyle's experimental 'science' 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. (p.185) Shapin believes the Royal Society was a passive factory churning out experimental knowledge, first entirely under the Boylean, then entirely under the Newtonian experimental regime. Shapin does not document, as opposed to assert the existence of these totalising regimes of experimental activity, and the reason may be that no such Boylean, then Newtonian regime existed.³

The moral of our story for Boyle, compared to Shapin's story: Robert 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

³ Shapin's argument on this point builds to a climax at pp.122-4. Viewed with a sceptical eye, informed by our '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.

mechanists of different tempers. Boyle did indeed sometimes down play explicit corpuscular-mechanical systematics (matter theory and theology linkages). He did sometime persevere 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 contest to play in a new, really scientific field. What Shapin has mapped is some of Boyle's rhetorical apparatus for accounting for successes and failures in the actual natural philosophical game, rather than the fundamental social and cognitive mechanisms of his making and breaking of knowledge claims in natural philosophy.

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