Organising the 'Experimental Life' at the Early Royal Society:
The Production & Communication of Experimentally Based Knowledge

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This paper, based in part on work originally pursued with Dr Alan B. H. Taylor, examines how knowledge claims were manufactured and communicated at the early Royal Society. We approach the problem by studying the organisational features and dynamics of the Society—its organisationally sedimented patterns of decision-making and action-taking. This work cuts across attempts by Shapin to characterise 'the new Experimental Science' in terms of a supposedly new 'form of life' which purportedly broke with the previously dominant culture of natural philosophical contention.

Shapin (and many of his followers) see the Royal Society as having been functionally taken over by this 'experimental life'. Using three case studies of experimental projects at the Society, we argue that the institution was more complex in its internal workings and that the Europe-wide, contested, culture of natural philosophy continued to play through and be played upon within the Royal Society, particularly by dominant actors, such as Boyle and Hooke, who could navigate and exploit the decision/action pathways. We conclude by proposing modifications to Shapin's concepts of the 'matter of fact' and 'epistemological decorum', and by pointing out strong parallels to our findings in recent work by Luciano Boschiero on the Accademia del Cimento.

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Outline

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1. How to Study the Organisational Dynamics of the Experimental Life in the Early Royal Society

This paper deals with the organisation of experimental natural philosophy at the early Royal Society of London—the organisation of 'the experimental life' as Shapin would say. In particular, I look at organisational processes and patterns inside the Royal Society...how they shaped that 'life'--shaped experimental inquiries inside the institution and the representations of those inquiries to the outside public.

I'm not concerned with the origins of the Royal Society or with debates about its sociological essence in, for example, religion, Baconian method or gentlemanly etiquette. The focus is on organisational processes—the routine patterns of decision-making and action-taking that produced experimental inquiries, and formed communications about them.

My former colleague, now retired, Dr. Alan Taylor—a former British Royal marine, early IT executive, and in later life an historian of science--pioneered this line of approach. Our joint interests in experiment, in historical process and in the dynamics of natural philosophy led us to collaborate. We were also motivated by Shapin and Dear reviving essentialist claims that the early Royal Society was where modern experimental Science was born.

The decision/action patterns we're interested in were present above and beyond specific passages of activity. However, they only existed in so far as actors reproduced and sustained them, by acting within them. From an actor's standpoint, the patterns were constraints **and** resources--they might be played slightly differently in terms of actors' skills and agendas. One must identify the decision/action patterns, and also follow different actors as they played within and upon them.

The initiation and subsequent leadership of investigations was 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.

So, at the actual meetings we get a frequently observed pattern: 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 participate fully and to introduce activities and views that favoured their preferred beliefs and interests. So, for an active member interested in presenting his own agenda, the best policy was this: Keep the other Fellows engaged by feeding routine decisions their way, and manage any spin off committees or actions the Fellows might collectively call for. In this way the patterns the Society followed [a] facilitated the contributions of active players; and [b] provided activity for most of the Fellows.

What about news concerning experiments communicated to the public? Well, there was a routine failure to report the Society's in house processes of the construction of experimental knowledge. The Society created the impression that it was merely the impartial reporter of news supplied by others.

To study actors we have to remember that competences and interests are two different things, and that competences and interests are played by actors within fault lines of existing decision/action patterns. Hence processes and products, do not reduce to sums of individual interests or competences In particular we follow how leading players played the patterns and worked within them. The existence of lead players and of routine patterns were inter-defining organisational dimensions at the Royal Society.

Finally, it is important to realise that the Royal Society's policies didn't prescribe a day to day method, or procedure, for investigating nature by means of experiments. Indeed the 'Corporate Policy', as it were, was to encourage the use of disparate views on approaches to experiment. The Royal Society was not Baconian, Boylean or Newtonian: We shall see it was open textured by design and functioning.[1] All three of my cases show that the decision/action patterns involved in experimental investigations allowed for a flexible style of inquiry--differences of approach and interest of the Fellows could be mobilised in order to achieve the Society's aims.[2]

2. Natural Philosophy and its Entourage of Sub-Disciplines in Action

Before our case studies, we need one piece of conceptual housecleaning. The best recent early modern historiography has largely stopped using the word 'Science' to denote some then emerging essence. Historians focus instead on the actual constellation of disciplines in early modern Europe devoted to seeking knowledge of nature. Chief amongst these was natural philosophy.

Early modern natural philosophy was not just the Scholastic Aristotelianism of the universities. I see it as a much wider elite sub-culture and field of contestation. A natural philosopher of any stripe tried systematically to explain the nature of matter, the cosmological structuring of that matter, the principles of causation, and the methodology for acquiring or justifying such claims. Of course, the dominant genus of natural philosophy was Aristotelianism in various neo-Scholastic species. But, the term applied to alternatives of similar scope and aim; that is, to any particular species of the various competing genera: neo-Platonic, mechanistic or, later, Newtonian. Natural philosophers learned what I call the 'grammar of natural philosophising' at university whilst studying hegemonic Scholastic Aristotelianism. Even alternative systems followed these rules of formation. All natural philosophers and natural philosophies constituted one evolving sub-culture.

So we resist the identification of natural philosophy with Scholastic Aristotelianism only; and we equally reject the idea that it died and was replaced by a new, essentially different activity, Science. Rather it evolved, under internal contestation, and external drivers, and variously elided and fragmented into more modern looking, science-like, disciplines and domain**s**, plural, over a period of say 150 years from 1650 to 1800.

Natural philosophy had an entourage of subordinate, more narrow traditions of science-like practice: These included —the mixed mathematical sciences such as astronomy, optics, mechanics, music theory and the like; and the bio-medical domains, such as anatomy, medical theorising, or physiology. The members of this entourage obviously changed and interrelated over time as well. Different natural philosophers had different interests and skills within the entourage, and even different lists of what was within or without its boundaries. A natural philosopher had to set priorities amongst entourage members, and link them conceptually to his natural philosophy. This created a pattern of linkages characteristic of a particular natural philosophy. The practice of a subordinate science under the aegis of a particular natural philosophy was coloured by the nature of the conceptual linkage. Natural philosophers competed with each other in part through attempts to co-opt these narrower traditions of scientific endeavour.

Natural philosophising was a competitive enterprise. Natural philosophers did natural philosophy by positioning themselves within the wide and diffuse field of natural philosophy.[3] But they also found themselves doing natural philosophy within the confines of particular sites or institutions—such as the Royal Society or the Accademia del Cimento. In such situations natural philosophers played the institution's organisational patterns in ways advantageous to them in institutional and natural philosophical terms.

In sum, we don't believe in origins or ruptures, but in process within a continuously existing field of natural philosophy. The 'Scientific Revolution' marked not the death of natural philosophy and emergence of Science, but a set of transformations inside the seething, contested culture of natural philosophising and its bubbling entourage. We see institutions like the Royal Society and Accademia del Cimento as nodes in the Europe wide field of natural philosophising--the field played through the sites, and the local dynamics of the sites affected the field.

3. Case 1: An Episode with May-dew

In May 1664, John Pell, a Fellow, proposed that the Society experiment about the weight of May-dew; about a salt that was obtained from it; and about the life "generated from it".[4] Many scholars thought that a 'vital salt' was the clue to the generation of life, and that life spawned not in the dew, but "from" the dew, as Pell noted. Suitably intrigued, others Fellows joined in. Thomas Henshaw, a leading Fellow, Royal confident and Privy Councillor, told the Fellows he'd already carried out experiments with May-dew, tantalising them with the claim, "that he had taken several ounces of pure salt out of a barrel of May–dew; and observed [an] abundance of insects, and particularly of millipedes, bred in such dews".[5] Henshaw claimed not only to have the experiments, but also the results. According to the Minutes, the Fellows requested Henshaw "to bring [in] an account in writing of those experiments...and to suggest new ones."[6]

At a later meeting Henshaw's experimental report was read and recorded in the register book.[7] According to the Minutes, the Fellows instructed the operator to repeat as many of Henshaw's experiments, under supervision, as he could—a routine decision.[8] So, Henshaw's experiments had set the program for the subsequent investigation, but the Fellows had the experiments repeated under supervision. And later, in May 1665, they asked Pell to repeat the experiments. The Fellows also appointed curators to carry out the collection, for experimentation, of various types of May-dew.[9] This too was a routine decision.[10] However, there's no evidence that this program was carried out.

Now, let's reflect on the organisational patterns here. They're typical of the organisation: The path of the investigation was left to those wanting to contribute. Henshaw had the interest, the knowledge and the ability to perform the experiments. His experiments established the routine for the subsequent investigation. However, the Fellows did intervene organisationally, in a manner that was routine: They had the May-dew experiments repeated under supervision, and by Pell. Otherwise they weren't prepared to take Henshaw's experimental findings at face value. In sum, the Fellows present at the meetings were prepared to make a routine decision to begin and encourage the activity, once impetus had been given by a member's suggestion, and, given Henshaw's leadership, the Fellows were willing to engage in further rounds of routine decision making.

What about the patterns for communication? Well, consistent with our general findings, the <u>Philosophical Transactions</u> published Henshaw's report on his experiments, under his authority alone. The <u>Phil Trans</u> tells us nothing about the experimental activity inside the Society.

Note how the decision-making and action-taking patterns actually brought this project into being and informed it: In asking Henshaw to present his experiments, the Fellows avoided taking any non routine decisions and actions. To think experiments on May–dew were important was one thing, but knowledge about what experiments to do and competence to perform them was another. Interest and competence were disjoined, and the gap between was bridged on terms dictated by the organisation, by its patterns and processes.[11]

Note also that the culture of natural philosophy played right through this affair. We're not dealing with some break from natural philosophy as some would have it, nor are we dealing with atheoretical "matters of fact".[12] There are two important points here: First, as Alan Taylor has already established elsewhere, the publication was not atheoretical: For readers interested in alchemical or chemical matters, Henshaw's published report bristled with overtones of theories related to generating substantial change, and indicating that change had occurred. For alchemically and chemically informed readers, the theoretical issues would have been obvious.[13] Natural philosophical interests were played through the affair, showing up in the output, if only in a muted way: The Society, in other words, allowed for, and catered to, that diversity of natural philosophical perspectives by means of its decision/action patterns—a very important finding for those hypnotised by Shapin's picture of the experimental life.

So, natural philosophising was in play. This prompts further insights: Firstly, the actions that took place, the trajectories they formed, and the outcomes that arose did not necessarily reflect the background beliefs and agendas of Henshaw or other particular Fellows. Many had natural philosophical axes to grind, but all geared in with the unfolding process. The processes of manufacturing and communicating experimentally based knowledge existed beyond the mere summation of individuals' socio-cognitive interests and agendas—studying the patterns is thus important. And secondly, the participants, each acting in view of his own awareness of the decision/action patterns, reproduced and enforced those patterns. This rendered the events at the Society intelligible to all players and manageable by the active participants.

4. The 1680 Metals 'Project':

In our second case, the 1680 Metals Project, the experiments involved melting, mixing and finding the specific gravity of several metals and their mixtures. The experiments were carried out and witnessed under the supervision of a committee at a select venue.[14] Hooke, as Curator coordinated the activity, brought the members of the committee together and arranged for the experiments to take place.[15] At the weekly meetings of the Society, Hooke would read a formal report on the experiments performed the previous week. It would be registered and the Fellows were then encouraged to make decisions about the selection of metals for the next experiment or to suggest practical applications. At the end of the course of experiments, the President moved that the experiments should be written up. In response, Hooke presented the Fellows with a table of the experimental results. The results were not printed in the Philosophical Collections—temporary successor to the Philosophical Transactions.

Each experiment in the investigation was performed, witnessed, reported, discussed, registered in the Society's register book, and data from it included in Hooke's final table of results. This might look quite Shapinian, concerned with atheoretical matters of fact. But let's inquire further.

The metals project had begun with some discussions in 1679 about the cause of 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,[16] 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.[17]

This is all based upon 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 adduced just such principles and deduced a prediction: 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.[18] Hooke offered to table experimental evidence "mak[ing] evident" his premises, and the Fellows immediately accepted.[19] 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."[20] On 29 January Petty chimed in with a similar idea and example.

Many subsequent experimental results supported Hooke's premises.[21] But, when a mixture of tin and lead showed a specific gravity smaller, than it "really ought" to be,[22] Hooke suavely declared that the result challenged "the invention of Archimedes"[23] He meant that a result violating Archimedean statics couldn't be accepted and so couldn't count against his corpuscular-mechanical premises either.

Hooke's organising of this project depended on his ability to impose his matter theory and the warranted power of deductive reasoning within natural philosophical discourse. The other active members accepted Hooke's premises, based on the mechanical philosophy. The experiments were intended to demonstrate Hooke's propositions, not to investigate them or to 'test' them. [24] The authority vested in the mechanical philosophy--and apparent deductions from it--could override any unacceptable experimental results.

Notice the rhythm of Hooke's engagement: The project had grown from rambling discussions, crystallised by Hooke's early moves. He then guided the usual routine actions, and as his control tightened, the project took shape.

So, in the end, Hooke was able to control the organisation of the inquiry, and to impose a disciplined structure on the events at the Society, He managed the series of experiments, directed how the individual experiments were to be carried out, and he also controlled how the results of the experiments were evaluated**.** But, at all times he worked under the routine decision/action pattern for meetings set by the Society. That decision/action pattern did promote witnessing and group responsibility, as Shapin would claim, but it allowed for, indeed required, bravura organisational performances, such as Hooke's.

Finally, this project arguably conduced to the improvement of natural philosophy: like the May-Dew project, it did provide empirical support for the authority of the premises. But, it didn't supply atheoretical matters of fact; it did not repress natural philosophical commitments; and à fortiori it did not avoid the deductivism and mathematical articulation so often found in the natural philosophical utterances of men concerned with the mixed mathematical sciences, such as Hooke.

5. Optics: 1672 Newton/Hooke optical debate

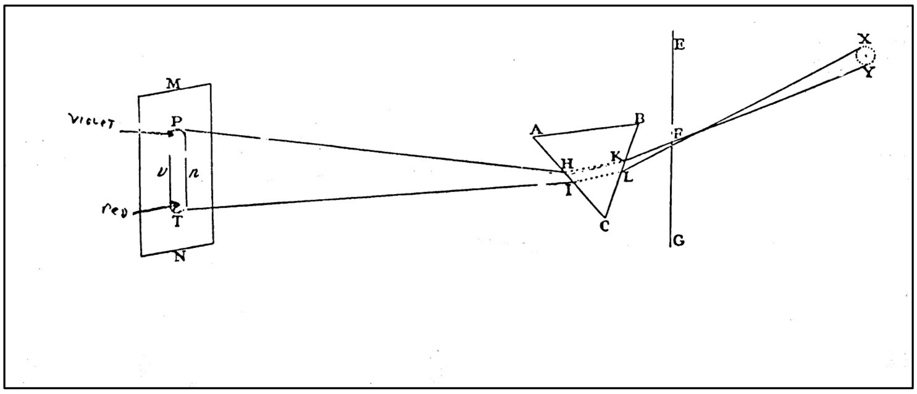
The third case study involves the 1672 Newton/Hooke optical debate. This was a contest in the natural philosophical field; but it also took place in a site suffused by that field, the Royal Society, and it obeyed the site's decision/action patterns, as well as the rule of an active directing agent, Hooke again.

Newton was a Fellow, but his understanding of the Society was still an outsider's view, shaped by reading the Phil Trans. On 6 February 1672, Newton submitted to the Society a purportedly amathematical, experimental report wherein a new theory of light and colour was supposedly 'deduced' from experiments, thinking that was what the Society required. Newton was suppressing the fact that this work belonged to the traditional mixed mathematical science of geometrical optics.[25] Robert Hooke managed the affair within the Society's organisational patterns, and what was published was as usual, not the in house work, but heavily filtered and edited accounts.

The fusion amongst mathematics, theory and experiment that underpinned Newton's work on colour has drawn comment by leading analysts, such as Bachelard and Kuhn. More recently Peter Dear claimed that Newton here achieved a novel marriage of experiment and mathematics that broke with the now passé culture of natural philosophy and conjured the "spirit of modern experimental Science", which, he says, the Royal Society immediately embraced.

Well, at base, Newton's work was traditional mixed mathematical optics, not a fracture with the past—and the Royal Society's experimental life functioned as normal in this case.[26]

Let's look first at Newton's earlier Cambridge optical lectures. Here Newton clearly signalled that his approach wasn't novel, but merely a significant set of discoveries within the mixed mathematical domain.[27] "Concerning light", Newton begins, "I have discovered that its rays differ from one another with respect to the quantity of refraction...".[28].[29] . Newton follows the deductive pedagogical style of the Schools, announcing, "I will at once present the reasoning and experiments that support" the claim.[30] This leads to his famous elongated spectrum experiment, which he claims proves that rays of light entering a prism with equal angles of incidence suffer differing refractions, since if they had equal refractions a circular image would result. That experiment is based upon a masterful phénoméno-technical realisation of the then accepted relevant geometrical optical laws: Kepler's [1604] theory of pin hole images and the Snel/Descartes [1637] law of refraction.[31]:



Newton's (1672) Novel Elongated Spectrum of Colours:
You must ***know*** and know ***how to use*** these theories:

Kepler's (1604) Theory of formation of pin-hole images;

Descartes (1637) Sine law of refraction.

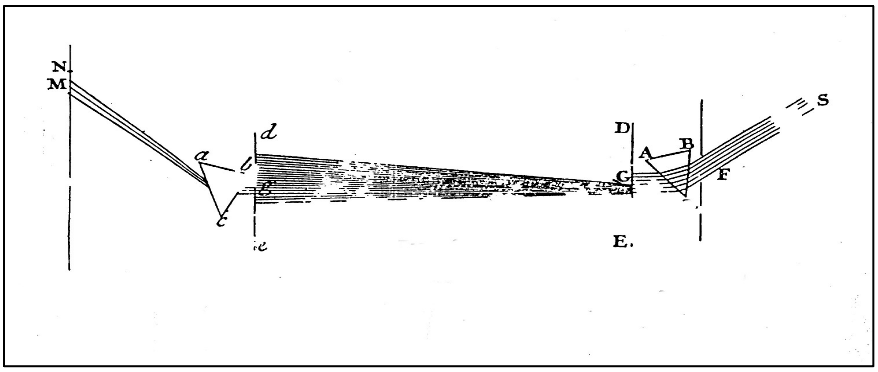
Newton justifies mixing mathematics and experiments by stating that, "The relation between the properties of refraction and those of colours is certainly so great that they cannot be explained separately".[32] And he articulates this by harking back to traditional understandings of the mixed mathematical sciences and their relations to natural philosophy:

> [A]lthough colors may belong to [natural philosophy], the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning.[33]

This joining of mathematics and experiments wasn't novel: Newton reminds us that this occurs routinely in "astronomy, geography, navigation, optics, and mechanics...",[34] even if this approach hadn't previously been followed, as Newton asserts, in studying colour or natural philosophy generally. So, Newton lectures us on the routine grammar of articulation between natural philosophy and the entourage of mixed mathematical disciplines.[35]

In contrast, in his 1672 letter to the Royal Society, Newton purported that his theory was deduced from experiments and that it was amathematical. His letter contains no supporting diagrams. However, in a passage suppressed when the letter was published in the Phil Trans, Newton proclaimed that his colour investigations were as mathematically certain as any part of optics

In the letter Newton claims that white light is an heterogeneous collection of "rays" differing amongst themselves in respect of certain original qualities, such as their reflexibility and refrangibility, and their "colorific qualities". Rays produce colour either singly or by their mixture. Two "experiences" were the basis for the induction of this theory: In the first, as we just said, Newton ingeniously produced a prismatic spectrum of sun light whose elongation exceeded that to be expected on the basis of the then accepted relevant geometrical optical laws. However, as noted, the geometrical background to Newton's experiments was not mentioned. In the second experiment, the experimentum crucis, a narrow beam of putatively homogenous light was selected out of the original spectrum and refracted through a second prism, there undergoing precisely the same degree of refraction as characterised the originally selected beam.



The *Experimentum crucis*: Not consensually taken as such, open to re-interpretation (Hooke), does not/cannot 'prove' Newton's position.

Newton was challenging the modification theory of colour to which both Hooke and Boyle adhered. Hence Newton ran the risk that his non routine theory claim would be forced to run the gauntlet of organisational decisions and actions ultimately grounded in the routine accepted theory view. This risk was fulfilled.

At the meeting which first received Newton's letter, the Fellows minuted Newton's theory with no mention of the supporting experimentation[36], thus focusing attention on the theoretical issues. The Fellows also called for comments, from Hooke, Boyle and Ward, but only Hooke responded, introducing his own views on colour. He was Curator of experiments: his behaviour would have been seen as routine and helpful.

In his report, one week later, Hooke in one sense aided the acceptance of Newton's crucial experiment, by stating that he himself had achieved this result hundreds of times; but, Hooke added that the experiment was not sufficient to account for all colour phenomena. Hooke argued that white light is nothing but a pulse or motion propagated through a homogeneous medium and that colours are produced by the mechanical disturbance of that pulse. [This report was never published.] The Fellows minuted Hooke's arguments for his own theory, which they specifically saw,[37] "as a refutation of a discourse of his [ie Newton's theory]". But they also requested, with praise, Newton's permission to publish his letter[38] Note the degree of attention the Fellows were willing to grant to theory.[39] The Fellows also requested that Hooke perform experiments to back his theory. Later, Hooke added more supporting experiments, with soap bubbles and two pieces of glass[40].

On 4 April the Fellows also followed routine procedure and requested a witnessed performance of Newton's crucial experiment. Hooke cleverly reported that while the experiment confirmed Newton's theory it was not an "experimentum crucis", as the phenomenon could also be explained by his own modification theory.

Newton was sent Hooke's comments on his first letter, which rejected Newton's claim that his work possessed mathematical certainty. In his response, 11 June 1672 , Newton made explicit that he was talking about a geometrisation of refraction/colour phenomena--the material being as certain as other previously achieved parts of geometrical optics.[41] Newton then expounded for the Fellows Aristotle's concepts of the superior and subordinate sciences, the conceptual structure of the mixed mathematical sciences, as well as the traditional rules of methodological exposition.[42] [text 1—See Appendix] So, as in the Metals Project case, we have the continuing efficacy of the traditional grammar of natural philosophical utterance. No rupture had occurred within natural philosophical culture: for Newton, as for Hooke, all their optical/experimental utterances were business as usual in mixed mathematics/natural philosophy, meaning of course that differences of theory and of method rhetoric could exist.

When Newton's two letters were published in the Phil Trans, Henry Oldenburg's editing verged on censorship. Whole paragraphs were deleted,[43] including any reference to mathematics and some of Newton's radical empiricist rhetoric. [Text 2-see Appendix] [44] [45] Newton's Aristotelian response to Hooke's denial of the 'science of colours [being] most properly a Mathematical Science', was also omitted. Also ignored was the activity that took place at the Society, including the witnessed experiments generated by Hooke and Newton. By not publishing the details of the witnessed experiments, the Fellows avoided providing public support to either Newton or Hooke. Additionally, however, all suggestion that anyone from the Society had been critical of Newton's work was deleted. [46]

Clearly, the Society didn't want to publish a theory debate; nor did it want to be identified with or seen as endorsing Newton's theory, his method claims, or his Aristotelian lecturing about the structure of the mixed mathematical sciences. However, Oldenburg did publish Newton's offhand remark in the first letter about the possibility of a corpuscular hypothesis.[47] But, he didn't publish Newton's retraction in his second letter.

According to the Phil Trans, the Society played no role in this affair and the debate had no mathematical content. But, as we've seen, insiders knew the first proposition to be false, just as expert opticians knew the latter to be so. As a result of this policy, outside readers would have seen the Society as an impartial distributor of information, with only the experiments providing support for Newton's theory, a view that has infected much of the historiography.

Another significant outcome is the lack of publication of Hooke's work with the suppression of all in house research.[48]. But Hooke's experimental reports on thin plates and soap bubbles would have been first rate grist for his theory mill—and they had been written up in an acceptable form, even with named witnesses. Consider what might have happened had Hooke not been Curator and constantly present, but had simply submitted those experimental reports from the outside, as had Newton. We can see here how the outcome of the affair, both what happened and what didn't happen, forms a visible surface pattern of evidence, fossil vestiges, shaped and deposited by the deeper techtonic forces of the Society's decision/action patterns and publication policy.

6.Reprise, The Three Cases and the Shapinian Story:

For Shapin the essence of the new science championed by the Royal Society under Boyle was epistemological decorum: a decorum of gentlemanly, civil reporting, weighing and warranting of purely atheoretical, purely amathematical "matters of fact". This avoided natural philosophical sectarianism and conflict. Our cases indicate that you could have natural philosophical divergence without conflict, and that mere natural philosophical differences did not necessarily entail conflict--if you organised the situation. That's what the Royal Society was doing.

The variety of natural philosophical agendas was real; the presence of theory, tacit or overt, was inescapable. The Royal Society functioned so as to produce warrantable claims about experimental results in the midst of these realities.

The Royal Society accepted the necessity of natural philosophically shaped input, and it accepted the natural philosophical theory-loading and agenda relevances of outputs [by making them personal]. In between it insisted on in house witnessing, repetition and testing. So, epistemological decorum does not mean atheoretical facts exist or that natural philosophising died so that method-driven Science could be born. It means that natural philosophical commitments and differences were held within civil limits inside the institution, and occluded in public communication outside of it.

So, in this new sense, there was 'epistemological decorum' in the May-dew case, with lots of natural philosophical theory and interest expressed and communicated; There was epistemological decorum in the metals case--a decorum involving assertion of corpuscular matter theory and mathematical driving of results—a decorum imposed by Hooke's management of the organisational processes. And in 1672 decorum was maintained by Hooke's management of the decision/action patterns in the interest of existing views of geometrical optics and its relation to corpuscular-mechanical natural philosophy. If Newton wanted to breach this decorum and set up his own 'decorum', he'd have to control the Royal Society or attack elsewhere in the field of natural philosophising—both of which he later did.

7. A Confirmatory Comparative Study: Boschiero on the Accademia del Cimento

Luciano Boschiero is a former student of mine. He did his doctoral thesis on a more Schuster/Taylor than Shapin study of the Florentine Accademia del Cimento, and then post-docs at Columbia and Johns Hopkins. Boschiero has worked in Florence with manuscript sources, with correspondence, diaries and the surviving annotated drafts of the Saggi. He published a prize winning essay about his work in *Annals of Science* and last year published his book of the thesis.

In his work he attends closely to the mathematical and natural philosophical training, skills, enterprises and products of the academicians, before, during and after their period of engagement in the Cimento. This includes Viviani and Borelli, the leading mixed mathematical/mechanistic members, the two stalwart Aristotelians, and Prince (later Cardinal) Leopoldo di'Medici, one of the two patrons, who was a fully committed corpuscular-mechanist.

Boschiero carefully distinguishes between the in house natural philosophical contestation at the Cimento, and the constraints exercised on the communication of the Cimento's results in its only publication, the Saggi. He documents the deep and endemic conflicts of natural philosophical values, theories and agendas amongst the members, and the ways this shaped the courses, and outcomes, of their experimental natural philosophising.

Three principles are applied and confirmed in Boschiero's work: [1] there continued to exist a European culture of natural philosophy; its entourage of subordinate specialist disciplines, and deep ferment and contestation therein; [2] no rupture occurred in that culture at the Cimento (or anywhere else) into a modern world of 'experimental method', a protocol for handling and gleaning knowledge from atheoretical 'matters of fact' ; [3] a rhetoric of such a method, and even a censorship policy (in the case of cosmology and 'hard core, explicit' anti-Aristotelian claims) was exercised by the patrons over public communications in their name, that is, in the name of the Cimento; but this neither signalled, nor caused, a rupture from the culture of natural philosophising into a mythical modern culture of 'method' and 'atheoretical matters of fact', either in Florence or anywhere else, and certainly not at the Royal Society, as we have seen in the rest of this paper.

8. Conclusions:

[1] We've seen no evidence to support the idea that the Royal Society or the Accademia del Cimento 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 natural philosophical theory and 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 public communication that the Royal Society and the Cimento 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.

[3] All of our cases reinforce our primary contention that to understand how the Royal Society and the Cimento produced and communicated experimental natural knowledge, one should analyse their 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 knowledge of nature. So, it is clearly incorrect and ahistorical to do any and all of the following: To pretend that events in the field of natural philosophising were local only, and/or that precious micro-cultural apercus about local sites can be uttered forgetting the natural philosophising, and/or pretending that natural philosophy whatever it was, had died, and a new Science was born in these sites.

APPENDIX:

Text 1

Newton's reply was, "That the Science of colours is most properly a Mathematicall Science", and in support he added,

> I said indeed that the Science of Colours was Mathematicall & as certain as any other part of Optiques; but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from Experiments & so but Physicall: Whence the Propositions themselves can be esteemed no more than Physicall Principles of a Science. And if those Principles be such that on them a Mathematician may determine all the Phenomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in wch severall colours are originally inherent; I suppose the Science of Colours will be granted Mathematicall and as certain as any part of Optiques. And that this may be done I have good reason to believe, because ever since I first became acquainted with these Principles, I have with constant success in the events [in his lectures on optics] made use of them for this purpose.

Text 2

> A naturalist would scearce expect to see ye science of those [phenomena of colours] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely infering 'tis thus because not otherwise or because it satisfies all phenomena (the Philosophers universal Topick,) but envinced by ye mediation of experiments concluding directly & without any suspicion of doubt

What was printed was:

> "the origin of Colours is unfolded: concerning which I shall lay down the Doctrine first".

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[1] Here we mean 'Baconianism' as a more or less systematically articulated natural philosophy as recoverable from the published texts of Bacon. The Royal Society was in many ways accountably 'Baconian' in a range of looser, more generic senses of the term .

[2] Not just to mobilise' but to contain without conflict--Shapin therefore overdramatises: you can have divergence without conflict and mere differences do not necessarily entail conflict--you can have different decorums.

[3] Think Bourdieu (and Latour, if you must)

[4] Birch, History , i, 418.

[5] Birch, History , i, 418; we have inserted the emphasis for "in".

[6] Ibid.

[7] Birch, History , i, 422, 425.

[8] Ibid; see also Ibid, ii, 45. The Fellows carried out some experiments at the weekly meetings with May-dew, Ibid, i, 427. One reason for the operator carrying out experiments concerning May-dew outside the meetings would have been related to the time, as reported by Henshaw, required for the investigations. T. Henshaw, "Some observations and experiments upon May-dew", Philosophical Transactions, 3 (1665), 33-36.

[9] Birch, History , i, 419.

[10] The background assumption that the species of dew would vary with the micro-environment was given support by the writings of Hooke. When he referred to mites, Hooke argued that different substances might have varied properties that nourished small creatures. Taylor, "An episode with Maydew', 174f.

[11] Including the roles they leave, or invite, for leading players, and the organisation's modes of participation in wider, supervening cultures or discourses—such as that of natural philosophising.

[12] Referring to the May-dew experiments, Dear, for example, argued that: "The [alchemical] theoretical justification for the experiments could not properly be presented; what took its place was a correctly accredited experience."

[13] Indeed, the plain language of Henshaw's account probably aided in the communication of such theory-loaded messages, in contrast to the obscure style of the traditional Alchemists.

[14] Birch, History, iv, 7-9, 13f, 23, 25, 29, 30.

[15] Birch, History , iv, 16. Hooke did not always call the committee together. The operator on one occasion was given the responsibility for not only calling them together but was also ordered to report any one who was absent. Birch, History , iv, 20.

[16] Birch, History , iii, 509.

[17] Ibid, We have inserted the emphasis.

[18] Ibid.

[19] Ibid,

[20] Birch, History, iii, 511.

[21] Birch, History, iv, 11f, 25.

[22] Birch, History , iv, 6.

[23] Ibid.

[24] Birch, History , iv, 6. Interestingly, when Hooke first announced to the Society his intention to follow the above procedure in order to "prove" his statements or propositions, the President supported this approach and argued that the "best method" of carrying out experimental investigations "was to proceed synthetically by first making the proposition what was designed to be proved, and then proceeding with the experiments to make the proof." Hooke, however, used the term synthesis for the inductive process from experiment to theory. Oldroyd, The Arch of Knowledge, 115f.

[25] At the heart of Newton's use of mathematics in colour is his work on different refrangibility. Consideration, he argued, had not been given by others to how light is refracted and produces different colours, Shapiro, op. cit. 435, 437., a focus that encourarges a mathematical mode as in optics.

[26] Normal here does not mean 'ideal' according to some stipulated norms of scientific behaviour, as proclaimed for example by Merton or Shapin. It means the decision/action patterns functioned in the manner we have routinely come to expect. Whether what the RS thus produced and communicated can be normatively be judged as 'best practice' by contemporary or modern standards is another issue and one that we can address only after seeing what in fact transpired.

[27]. There are two extant sources of information on Newton's lectures, the 'Lectures opticae' and the 'Optica', and while there are differences; they, in many respects, are the same. In fact, the 'Lectures' appear to be a shorter version of the 'Optica'. The exact period in Newton's thought that these two series of lectures represent is confused by the lack of precise dating, although it seems that the Lectures were first presented in 1670 and the Optica was ready for publication in early February 1672. Shapiro, op. cit., p.18f The dating of the 'Optica' is especially auspicious for this work as it represents Newton's considered view on how he intended his colour work to be made public, mathematically, at the time when he chose to present his views to the Society amathematically. To add strength to the view that Newton's intent was to present his work to the public mathematically, the content of the 'Optica' is ordered differently to the 'Lectures'; it is arranged to facilitate mathematical treatment with all the mathematical work on refractions being brought to the fore, in contrast to the 'Lectures'; prior to direct engagement with as Newton termed it "The Origin of Colours". Shapiro p. 24; 'Optica', Part II, p. 433. in Shapiro, op. cit.

[28] Shapiro, 283

[29] S 283

[30] S285

[31] S285f

[32] reference?

[33] S 439 . Indeed, since an exact science of them seems to be one of the most difficult that philosophy is in need of, I hope to show . . . how valuable mathematics is in natural philosophy. I therefore urge geometers to investigate nature more rigorously, and those devoted to natural science to learn geometry first.

[34] S Ibid;

[35] And, in his pointed remarks, he may have been reacting to those who had excluded mathematics in experiments with colour and who in general saw no place for mathematics in experimental natural philosophy, such as Boyle

[36]. Birch History iii, 10

[37] [get reference for this point] It is Birch iii 10.

[38]Birch, History (ref. 15), iii, 9.

[39]. Ibid, iii, 10.

[40] Birch, iii, 20, 41. [<==Hooke also wrote report on this—our ms 'Case Studies' approx pp.30-31

[41] Newton, Correspondence (ref. 152), i, 187f.

[42] Some idea of the role that Newton visualised geometry playing in his experimental work on colour and its particular emphasis drawn from the grammar of natural philosophy can be obtained by comparing Newton's comments with Aristotle's sentiments on the subject. Aristotle argued that in optics, an investigator "investigates mathematical lines but qua physical not qua mathematical" PA, 46a, 18-21 that is visual lines but not geometrical lines. For Aristotle the optician studied visual lines, as Newton studied visual lines. For Aristotle and Newton, it was the geometrical style of treatment that first decided how the observed facts were visualised and expressed, and second determined the framework, system and technique used in the

demonstration of the "reasoned fact". Aristotle presented optics as a subordinate science and geometry as a superior science. PA, 78b5-79a5. Newton concurred with these statements of Aristotle when he referred to "the Science of Colours" as a "part of Optiques" that "depend as well on Physical Principles as on Mathematicall Demonstrations".

[43] We take editing to be the changing of the text to ease what the writer is saying. Whereas, censoring is altering the meaning of what the writer has said by either deleting passages of text by erasing, or amending key words or phrases, to negate an impression or information that was not acceptable to the interests the editor, Oldenburg, represented – the Royal Society.

[44] Newton, Correspondence (ref. 152), i, 96f.

[45] Philosophical Transactions, (1672).

[46] Newton had responded to Hooke's comments by name, but what reached the public, was a version of Newton's reply in which all personal references to Hooke as a critic were omitted. For example see the following: "Mr Hook" [Newton], Correspondence (ref. 152), i, 171 is replaced by "The considerer", Philosophical Transactions (1672), 5084. "Mr Hook" [Newton] Correspondence (ref. 152), 174 is replaced by "the Objector", Philosophical Transactions (1672), 5087. "Mr Hooks concessions" [Newton] Correspondence (ref. 152), 176 is replaced by "the Animadversor's Concessions" Philosophical Transactions (1672), 5089 and Philosophical Transactions (1672), 5093. . It is however the case that Newton had conceded in a covering letter that the Society could 'mitigate' any references that were seen as troublesome to the Society. (Corr I 193)

[47] Correspondence I 94; Phil Trans Feb 1672 p.3078. "…if the rays of light be possibly globular bodies."

[48] Although Newton's published second letter discusses some of it, without of course naming Hooke