On the Invisibility and Impact of Robert Hooke’s Theory of Gravitation[1]

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> Current historiography of science – diverse, wide-ranging, and richly reflective as it has become in recent decades – passes such figures, ambivalated members of sufficient standing and voice to be heard and adhered to, over in near silence. Historians of science show little awareness of their existence and scant interest in exposing and analyzing the sources of their ambivalence and its effect on their peers. Attempting to tell their story raises two related questions: that of their visibility, and that of the nature of their impact.[2]

I. Introduction

Fisch’s *Creatively Undecided* (2017) and Fisch and Benbaji’s *The View from Within* (2011) offer a highly innovative and philosophically technical argument in favor of rational normative change. A basic element of Fisch’s argument is the importance of an environment of normative critics and the centrality of ambivalence. The environment is identified with a modified version of Peter Galison’s “trading zone”: an environment where the adoption of an “inter-language” (a pidgin designed to combine simplified versions of the parties’ professional jargons) enables the mutual engagement of two complex sociological and symbolic systems. Not only to trade, as in Galison’s *Image and Logic* (1997), but to fight, discuss, destabilize. Trading zones, construed *à la Fisch*, entail the necessity to make – as far as possible – tacit knowledge explicit, and, in general, to discuss normative assumptions creating ambivalence, self-criticism, and potential normative change.[3] Furthermore, we are told that the destabilizing effect of ambivalence is the work of individuals: “trading zones are only

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[2] Menachem Fisch, *Creatively Undecided: Toward a History and Philosophy of Scientific Agency*, Chicago: University of Chicago Press 2017, 120.

[3] Menachen Fisch and Yitzhak Benbaji, *The View from Within: Normativity and the Limits of Self-Criticism*, Notre Dame, IN: University of Notre Dame Press 2011, 11, 281–291.

visited by individuals, never by whole communities”~~.~~.[4] Fisch makes it clear that the virtuous destabilizing effect that induces rational framework replacement is the contribution – not recognized by a historiography focused on achievement rather than on creative indecision – of those practitioners he calls, with a happy neologism, “ambivalated individuals”~~.~~.[5] These individuals remain largely “invisible” in official historiography. Fisch’s work is thus an invitation for the historian of science and mathematics to provide alternative narratives; narratives in which the role of “trading zone mediators” in triggering rational framework change is recognized. Fisch has carried out some wonderful research aimed at showing how his philosophical account can be applied to a somewhat invisible and “creatively undecided” figure in British algebra, George Peacock.[6]

The goal of this essay is to probe the fruitfulness of Fisch’s account of “ambivalated trading zone mediators” for the historian of seventeenth-century mathematized natural philosophy. I am not interested in “applying” Fisch’s philosophical theory to a test case in order to verify that “all the pieces fall into place” as much as in exploring the heuristic advantage that, as a historian, I can gain by adopting his philosophical viewpoint. As a historian, I consider myself a humble practitioner. One of the great masters in the history of science, Paolo Rossi, compared the historian to a “straw chair maker” (*un impagliatore di sedie*) who learns his craft by apprenticeship in a workshop rather than by applying the rules learned from a textbook.[7] The relationship between history and philosophy is a vexed issue in historical interpretation: somewhat naively, perhaps, I believe that philosophy is valuable when it provides me with a toolbox to develop innovative, hopefully even surprising, narratives of past events. Like all craftsmen, I am opportunistic: I use philosophy with a purpose in mind and I can sometimes be incoherent in my adoption of philosophical tools drawn from the works of scholars belonging to different schools. Adherence to factual evidence is of course a rigid constraint for all historians, but, to state a truism, facts can be viewed differently. What I appreciate the most about Fisch’s philosophical work is that it is inspirational for my hands-on field work: it allows me to view the past with different eyes, with greater attention to failure, indecision, and ambiguity, a historiographic perspective that is often foreign to the main narrative tools we employ as historians of mathematics, which is to say as people who usually concentrate instead on things like success, achievement, and coherence. Fisch’s view provides historians and philosophers of science with a genuine argument for recovering the

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[4] Fisch and Benbaji, *View*, 293. See also Creatively Undecided, 120: “Trading zones are only frequented by individuals. Whole communities do not travel abroad, so they can only be ambivalated at home, from within, by already ambivalated members of sufficient standing and voice to be heard and adhered to”.

[5] Fisch, *Creatively Undecided*, 120. The concept of “scientific framework” is drawn from the seminal work of Friedman, to which Fisch is deeply indebted: Michael Friedman, *Dynamics of Reason: The 1999 Kant Lectures at Stanford University*, Stanford: CSLI Publications 2001.

[6] Fisch, *Creatively Undecided*, 135–222.

[7] Paolo Rossi, *Un Altro Presente: Saggi Sulla Storia della Filosofia*, Bologna: Il Mulino 1999, 30.

contributions of individuals silenced by the historiography of "achievements": the many historiographical accounts of the incremental progress achieved by "great men". The episode I will consider is a well-known correspondence between Hooke and Newton that took place in 1679–1980. Hooke proposed to Newton a new hypothesis concerning planetary motions. This hypothesis consists of the idea that planets move in a void accelerated by gravitational interactions. Until then, Newton had thought that planets moved because of some sort of interaction with the ether filling the planetary system, as he had learned in his youth by reading Descartes' *Opera*. Newton's ether theory of planetary motion has never been a Cartesian theory based on impact, however: the ether that Newton contemplated in his lifetime is composed of particles that repel one another at a distance. One might say that Newton's is an ether seen from the viewpoint of a natural philosopher who attributes to matter some sort of activity that never would have been endorsed by Descartes. After a failed attempt, which was corrected by Hooke, Newton was able to mathematize the motion of planets gravitationally attracted by the sun. Hooke offered Newton a valuable suggestion; however, Hooke's contribution to gravitation theory was minimized and, as a matter of fact, underappreciated by Newton and his acolytes. In a way, it remained "invisible", until recent historiography.

It is appropriate at this juncture to highlight the features of Fisch's philosophy that I will attempt to handle as historical tools when framing a narrative of the above episode. (1) A first element is the creative role of Hooke's ambivalated framework of the planetary system. In the *Micrographia* (1665), Hooke referred to Descartes as the founding father of the new philosophy pursued at the Royal Society. Indeed, "Descartes is the author to whom Hooke most frequently refers in *Micrographia*, and the *Principia philosophiae* is the work he cites most often"~~.~~.[8] Yet, in his correspondence with Newton, Hooke explored a model of planetary interaction based on action at a distance, a model belonging to an anti-mechanistic framework that was well alive in England at the time and was shared by those who detected in nature the presence of occult "powers" operating at a distance. The strength of Hooke's vision very much depends on the unresolved co-presence of two scientific frameworks, one based on the "rules of mechanical motion" and indebted to Descartes, the other based on the operation of "occult" powers. (2) Second, I am interested in Fisch's notion of mediating interlanguages enabling individuals who visit the trading zone to communicate and influence one another. The trading zone, in our case, is the Royal Society, and the interaction occurs in the brief yet momentous correspondence between Newton and Hooke taking place in 1679–1680. The two correspondents differed in terms of philosophical outlook and mathematical competence (and had already clashed in 1672–1676, partly because of these differences). However, they shared a common mathe-

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[8] Michael Hunter, "Hooke the Natural Philosopher", in: Jim Bennett, Michael Cooper, Michael Hunter, and Lisa Jardine (Eds.), *London's Leonardo: The Life and Work of Robert Hooke*, Oxford and New York: Oxford University Press 2003, 131.

matical language, that of "organic geometry", which allowed them to talk competently and meaningfully about the new "hypothesis" entertained by Hooke. (3) Third, the result of this dialogue led Newton to make the transition from a conception of planetary motions as caused by some sort of interaction with the ether to gravitation theory. Newton "changed his mind" rationally after trading ideas with Hooke, ideas framed in terms of a shared mathematical language. (4) Fourth, Fisch's model of the trading zone is based on the assumption that this is a space in which individuals meet. Fisch's Popperian background distances him, in this respect, from the positions shared by Science and Technology Studies (STS) scholars, who develop their study of scientific change and scientific controversies at the level of social, rather than individual, actors. Contrariwise, Fisch reformulates an idea that was central to Popper: the idea that scientific creativity is the work of individuals framing bold hypotheses. (5) Fifth, I will give prominence of place to the notion of "invisibility". Of course, this notion is central not only in Fisch's work but also in the philosophy of another major protagonist in the history and philosophy of science, Steven Shapin. Shapin, however, sees "the making of scientific knowledge as a fundamentally social activity" and studies those social forces that exclude "technicians", as a social group, from the historical record, shying away from describing this process of exclusion in individualistic terms, as Fisch does.[9] This leads me to my ~~sixth and~~ last point. I shall cursorily attempt to indicate the reasons behind Hooke's centuries-old invisibility in the official historiography of science and the reasons why we have recently changed our historical narratives, so much so that today Hooke's contribution is considered essential for the development of planetary theory. I will claim that the scientific culture of the early third millennium is propitious for a rehabilitation of Hooke's role, and in general for a rehabilitation of the role of technicians, as Shapin has made clear in his seminal paper. In doing so, I will position myself outside Fisch's perspective, which consists of a philosophical analysis that is strictly confined to an internal account of rational scientific change. Here, the practitioner of the craft of history will claim some freedom from the need to follow a philosophical theory too consistently: perhaps the humble "impagliatore" has, very marginally, something to offer the theoretician!

II. Hooke's Theory of Gravitation

Robert Hooke's contribution to the theory of gravitation is an interesting case study for probing the fruitfulness of Fisch's ideas as briefly outlined in the previous section. Indeed, the nature of Hooke's contributions to mathematized gravitation theory was contested during his own lifetime and gave rise to problematic evaluations until the last century. Hooke often has been contrasted with Newton and Christiaan Huygens

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[9] Steven Shapin, "The invisible technician", *American Scientist* 77 (1989), 563.

as a practitioner and unfavorably compared to a theorist – or as a "mechanic of genius rather than a scientist".[10] Yet, from the point of view of his contemporaries, Hooke, because of his dexterity in trading and moving between the diverse locales of London – between watchmakers' shops, the chambers of the Royal Society, and the laboratory in Gresham College – was better suited to the enterprise of mathematized natural philosophy as envisaged by the Royal Society than was the lonely Newton.[11]

Hooke's social status was lower than that of the other members of the Royal Society. The orphaned son of a Royalist minister, he made his way up the social ladder by entering Westminster School and then gaining a position as chorister at Christ Church in Oxford. Here, his meeting with Robert Boyle – an aristocrat who saw religious apologetics and pious natural philosophy as his mission – made his fortune, so much so that Hooke became an influential and respected *gentleman* of broad learning, who was accepted in the salons of noblemen and who could dine with archbishops. The Oxford group, gathered around Boyle and John Wilkins, needed Hooke's experience as a mechanician and his privileged access to the world of painting, mechanical drawing, glass working, and clockmaking. He was a man who could converse with natural philosophers such as Christopher Wren, Isaac Newton, and Boyle but who could also competently interact with and instruct the practitioners active in London's workshops.

In 1662, the newly founded Royal Society enrolled Hooke as their paid Curator of Experiments, an important, pivotal position. True enough, he did remain somewhat of a servant to the other fellows, who could request him to perform experiments; and all too often, much at his frustration, he was forced to fight to uphold the uniqueness of his role. Hooke's lower social status did not allow him to identify with "gentlemen free and unconfin'd" like, for example, Boyle.[12] Nonetheless, he wished to distinguish his purposes from the utilitarian aims of the craftsmen with whom he conversed so successfully. He powerfully served the function of relating the world of natural philosophy, dominated by philosophical and theological agendas, to the technological enterprises of mechanical practitioners. Indeed, as Jim Bennett and Ofer Gal have shown, Hooke conceived mechanical tools as instruments of knowledge; objects with which he could think about the microscopic mechanisms (such as the "spring of the air") underlying macroscopic phenomena accessible to our senses.[13]

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[10] Brian Vickers, *English Science, Bacon to Newton*, Cambridge: Cambridge University Press 1987, 99–100; A. Rupert Hall, "Robert Hooke and horology", *Notes and Records of the Royal Society* 8 (1951), 175.

[11] Steven Shapin, "Who Was Robert Hooke?" in: Michael Hunter and Simon Schaffer (Eds.), *Robert Hooke, New Studies*, Woodbridge: Boydell Press 1989, 253–285.

[12] To use Thomas Sprat's words as cited in Hooke, *New Studies*, 13.

[13] Jim A. Bennett, "The mechanics' philosophy and the mechanical philosophy", *History of Science* 24 (1986), 1–28; Ofer Gal, "Producing knowledge in the workshop: Hooke's 'inflection' from optics to planetary motion", *Studies in History and Philosophy of Science* 27 (1996), 181–205; and *Meanest Foundations and Nobler Superstructure: Hooke, Newton and the Compounding of the Celestiall Motions of the Planets*, Dordrecht: Kluwer, 2002.

In 1679–1680, Hooke addressed letters to Newton in which he advanced a new hypothesis concerning the planetary system. Hooke proposed viewing the planetary system as composed of mutually gravitating bodies. The hypothesis in question was not new; it had already been presented in part by Hooke in some lectures to the Royal Society as early as May 1666 and had later been published – in 1674 – *in calce* to an essay titled “An Attempt to Prove the Motion of the Earth by Observations”, dedicated to his astronomical observations aimed to determine stellar parallax. Hooke brought to the attention of his readers a “System of the World” that differed “in many particulars from any yet known” and that was in compliance with the “common Rules of Mechanical Motions”.[14] Hooke’s 1674 essay was reviewed in *Philosophical Transactions*, and his planetary theory was thus given wide circulation.[15]

The new Hookean system depended on three “suppositions”. in which Hooke merged two opposing scientific frameworks: the “mechanical philosophy”, most notably as expounded by Descartes, and the “magnetic philosophy”, proposed in England by John Dee, Francis Bacon, William Gilbert, and Christopher Wren, among others.[16] The first supposition was that all celestial bodies have an “attraction or gravitating power towards their own Centers[,] whereby they attract not only their own parts […] but that they do also attract all other Celestial Bodies that are within the sphere of their activity”~~.~~. It seems that Hooke conceived of this “sphere of activity” as finite and that, in 1678, he perhaps changed his mind on the universality of the attracting gravitational force, for in that year he gave one of his Cutlerian lectures to the press in which he argued that comets are acted upon by the sun’s “gravitating power” in a different way (one that in certain conditions might cause “protrusion” rather than attraction) compared to the planets.[17] The second supposition, drawn from Descartes, was that all bodies move in straight uniform motion until they are “deflected and bent” by some “effectual powers” in “a Circle, an Ellipsis [sic], or some other more compounded Curve Line”. The third supposition was that the “attractive powers” are “so much the more powerful in operating, by how much the nearer” the bodies are to the centers of attraction. Hooke described the way in which the planets of the solar system are attracted by the gravitational power of the sun and how they attract each other, influencing “considerably” their motions, and hoped that astrono-

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[14] Robert Hooke, “Motion in a curve: a statement of planetary movements as a mechanical problem”, In: R. T. Gunther (Ed.), *Early Science in Oxford, vol. 6: The Life and Work of Robert Hooke* (Part 1), Oxford: Author, 1666/1930, 265–268, and *An Attempt to Prove the Motion of the Earth from Observations*, London: John Martyn 1674, 27.

[15] *Philosophical Transactions*, January 1674, 12–13.

[16] For a study of Hooke’s sources in the so-called “occult” philosophy, see Xioana Wang, *“Though their Causes be not yet discover’d”: Occult Principles in the Making of Newton’s Natural Philosophy*, PhD thesis, Edinburgh 2019.

[17] Robert Hooke, *Lectures and Collections, Made by Robert Hooke, Secretary of the Royal Society. Cometa […] Microscopium […]*, London: John Martyn 1678, 12–13.

mers could determine the law of variation of the gravitational powers in order to reduce “all the Coelestial Motions to a certain rule”.[18]

The correspondence between Hooke and Newton conducted in the winter of 1679–1680 shows that these three suppositions caught the Lucasian professor totally unprepared. Until then, Newton had envisaged the motion of the planets as caused by an ether filling the planetary system, as is apparent in his “An Hypothesis Explaining the Properties of Light”, which he had sent Henry Oldenburg in December 1675. Newton then reiterated the concept in a famous letter to Robert Boyle in February 1679, in which he proposed a different ether model compared to that of the “Hypothesis”.[19] In both cases, Newton envisaged the ether in ways that could not be defined in Cartesian mechanistic terms: the particles composing the Newtonian ether were indeed endowed with some sort of activity, whereas for Descartes, matter was passive. However, Newton shared with Descartes the idea that the interplanetary space is filled with matter. In contrast, according to Hooke, the motions of the planets ~~occur in empty space~~, and the mechanician can predict their orbital motions by “compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body [of the Sun]”~~.~~.[20] In addition, Hooke assumed that the attraction of the sun decreases with the inverse square of the distance, and in January 1680, he asked Newton to provide a demonstration of what would be the curve traced by a planet subject to a force of this kind.[21]

Hooke was very tentatively proposing to Newton a hypothesis on the causes of planetary motions indebted to the explanation in terms of action-at-a-distance magnetism that had been considered by several natural philosophers, from William Gilbert and Johannes Kepler to Christopher Wren.[22] In the “magnetic philosophy”, however, planets move because of a magnetic rather than a gravitational interaction. Hooke's planetary model was based on the hypothesis that what causes planetary motion is action-at-a-distance gravitation, an occult force banned by the mechanical philosophy as propounded by Descartes and Thomas Hobbes. However, Hooke described his system as based on the “common rules” of mechanics. One might be tempted to characterize his model as “ambivalent”, or at least, sufficiently complex to address the desiderata of a broad range of natural philosophers.

Certainly, Hooke was uncertain about his hypothesis, and that is why he asked Newton's expert opinion. His letters to Newton have a very tentative character. In the first place, Hooke was unable to provide a mathematical proof of gravitation theory,

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[18] Hooke, *Attempt*, 28.

[19] Newton to Oldenburg (7.12.1675), in: H. W. Turnbull, J. F. Scott, A. R. Hall, and L. Tilling (Eds.), *The Correspondence of Isaac Newton* (7 vols.), Cambridge: Cambridge University Press 1959–1977, vol. 1, 362–386; and Newton to Boyle (28.2.1679), in: Turnbull, Scott, Hall and Trilling, *Correspondence*, vol. 2, 288–296.

[20] Hooke to Newton (November 24.11.1679), in: Turnbull, *Correspondence*, vol. 2, 297.

[21] Hooke to Newton (6.1.1680 and 17.1.80), in: Turnbull, *Correspondence*, vol. 2, 309, 313.

[22] For Hooke's sources, see Jim A. Bennett, “Hooke and Wren and the system of the world: Some points towards an historical account”, *The British Journal for the History of Science* 8 (1975), 32–61.

as he candidly made clear, asking Newton for one. Further, he suggested several experiments with pendulums aimed at verifying his hypothesis. He hoped to measure a variation in the period of oscillation at different heights (for example, at the foot and the top of St. Paul's Cathedral).[23] We are confronted here with a momentous framework shift promoted by an actor – Hooke – who was very appreciative of the mechanical philosophy (as is apparent from his Preface to the *Micrographia* [1665]) but who was also interested in considering action at a distance, a characteristic of the alternative magnetic philosophy. Hooke's position was indeed a hybrid between two competing philosophies, the mechanical and the magnetic.

Until 1679, Newton had embraced planetary models whereby planets move around the sun because of the action of a medium filling the interplanetary spaces.[24] The shift to the new model based on ~~void and~~ gravitation is unanimously considered in the literature as a decisive revolution in Newton's intellectual development. But how could Newton, to make use of a turn of phrase that characterizes Fisch's philosophical lexicon, "change his mind"? I have no space to enter into the details concerning the making of the *Principia* from 1679 to 1687, and I will not even attempt to broach the complex historiographical issues concerning the nature of Newton's ether hypotheses; what I would like to underline here is that this is a change that implied new norms of what can be considered as a valid explanation of natural phenomena. In the framework dominating Newton's mind before the correspondence with Hooke, a medium filling the interplanetary spaces was causally responsible for planetary motions. In the new Hookean framework, instead, a gravitational interaction acting ~~in void~~ was accepted as a causal explanation, insofar as it could be mathematically deduced from the planetary phenomena. Newton was soon to discover that Hooke's model could be mathematized in a very successful way. One might contend that it is mostly because of such mathematical fruitfulness that Newton was eventually led to embrace Hooke's hypothesis, which, of course, is at the basis of the *Principia*.

It is the correspondence with Hooke that tore a veil from Newton's eyes, allowing him to see very far. From the available documentary evidence, however, it is not clear how far Newton could see in 1680. According to some scholars, it was at this time that he first developed an outline of the theory of gravitation. It is considered likely that in early 1680, Newton managed to prove that the first two laws of Kepler imply that the planets are attracted to the sun by a force that varies with the inverse square of the distance. According to others, things are not so straightforward.[25] It is often said that although the credit goes to Hooke for having turned Newton away from his ether

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[23] Hooke, *Motion in a Curve*, 266.

[24] However, in his correspondence with Thomas Burnet and John Flamsteed in the years 1680–1681, Newton was still considering the idea of an ethereal terrestrial and solar vortex outlined in terms that one would define as Cartesian. Hooke transformed Newton into an "ambivalated" individual who mathematized action-at-a-distance gravitation but, with other correspondents, still contemplated the role of an interplanetary ether.

[25] For an evaluation of this issue and a bibliography, see Niccolò Guicciardini (Ed.), "Open forum: Newton vs. Hooke on gravitation", *Early Science and Medicine* 10 (2005), 510–543.

model, he cannot claim the merit of having provided a mathematical formulation of the new model. It is one thing – we are often told – to advance a qualitative hypothesis (the planets move in a vacuum in which they are deflected from inertial straight trajectories by a gravitational force directed toward the sun) and quite another to provide a mathematical demonstration. The weakness of Hooke's mathematics would also be evident – according to some scholars – from the fact that, as appears from his correspondence with Newton, he believed that the speed of a planet was inversely proportional to its distance from the sun, which is not compatible with Kepler's law of areas.[26] Thus, in the end, we should agree with Hall, for whom – as we know – Hooke might at most be called a "mechanic of genius" rather than a "scientist".[27]

Recent documentary discoveries and a different sensitivity toward the complex meanings of the terms *mathematics* and *scientist*, when such terms are evaluated in their historical context, have led to a dismissal of Hall's radical judgment. Patri Pugliese discovered that Hooke resorted to graphic constructions of trajectories (see Figure 1) that allowed him to mathematize central force motion.[28] Nauenberg has detailed Hooke's use of experiences with pendulums and balls rolled onto concave surfaces in order to verify the shape of the trajectories traced by bodies accelerated by central forces.[29] These geometric constructions and experiences operate as a kind of graphical and mechanical simulation of planetary motions and should be viewed by the historian as methods belonging to the mathematical sciences, in the broad sense that the term *mathematics* had in the seventeenth century. An important feature of the mechanical philosophy was the use of artificial instruments such as pendulums, springs, and inclined planes as a means of shedding light on the causes of natural phenomena, because the latter were thought to be generated by mechanical causes.[30] Hooke investigated the mathematical structure of the planetary system using graphical models and mechanical devices, tools that were familiar in the practice of the

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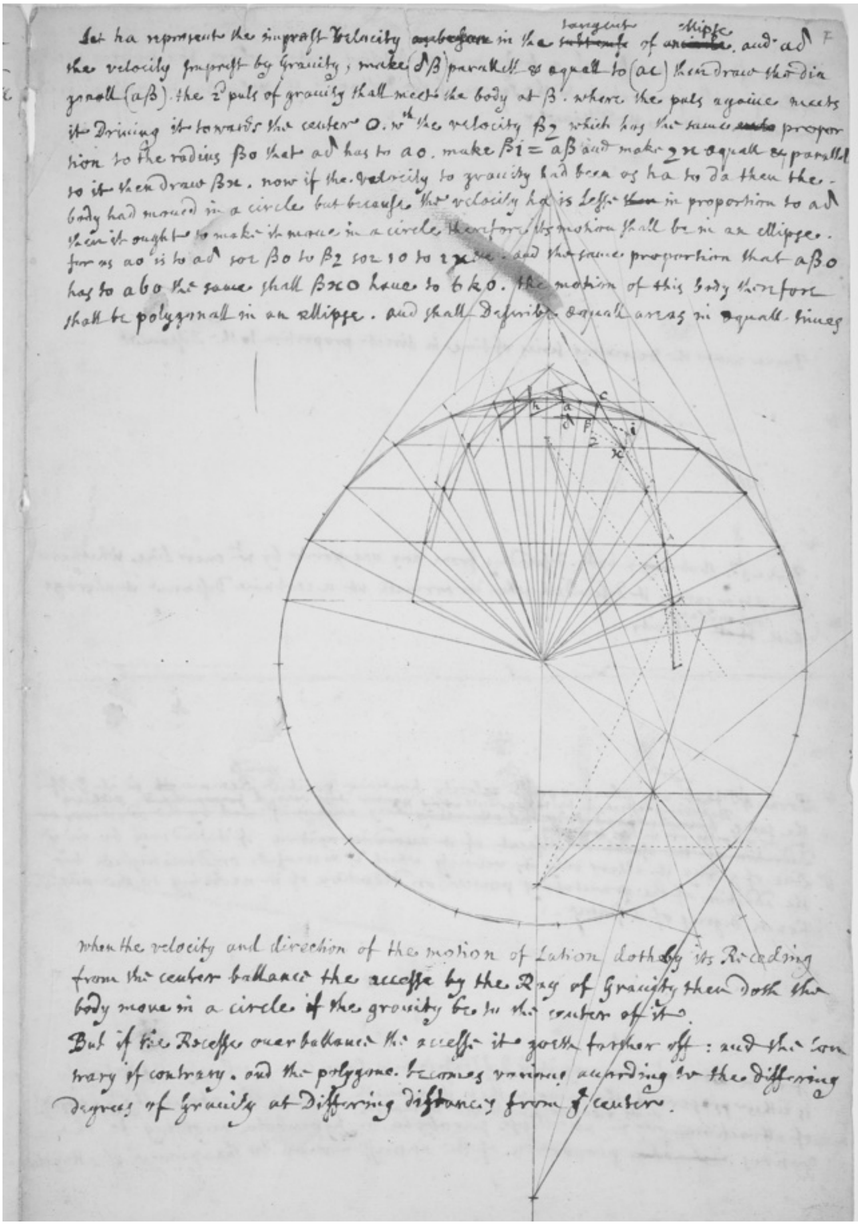
[26] I. Bernard Cohen, *The Newtonian Revolution: With Illustrations of the Transformation of Scientific Ideas*. Cambridge: Cambridge University Press 1980, 244–245.

[27] "It is worth noting too that whereas Huygens had approached horological invention through his studies in pure mechanics, and left the work of construction to professional clock- makers, Hooke's attitude is that of a mechanic of genius, rather than that of a scientist" (Hall, *Horology*, 175).

[28] Hooke's technique, which is similar to the first theorem of Newton's "De motu" (Cambridge University Library, MS Add. 3965~~.7~~, fols 55r–62bisr), consists of compounding tangential inertial motion with displacements directed toward the center and whose length is proportional to the force's intensity. This manuscript is discussed in Patri Pugliese, "Robert Hooke and the Dynamics of Motion in a Curved Path", in: Michael Hunter and Simon Schaffer (Eds.), *Robert Hooke, New Studies*, Woodbridge: Boydell Press 1989, 181–205, and in Michael Nauenberg, "Robert Hooke's Seminal Contributions to Orbital Dynamics", *Physics in Perspective* 7 (2005), 4–34. Jed Buchwald recently shared his ~~new~~ ideas about this manuscript with me.

[29] Patri Pugliese, "Robert Hooke and the Dynamics of Motion in a Curved Path", in: Michael Hunter and Simon Schaffer (Eds.), *Robert Hooke, New Studies*, Woodbridge: Boydell Press 1989, 181–205; Michael Nauenberg, "Robert Hooke's seminal contributions to orbital dynamics", *Physics in Perspective* 7 (2005), 4–34.

[30] Domenico Bertoloni Meli, *Thinking with Objects: The Transformation of Mechanics in the Seventeenth Century*, Baltimore, MD: Johns Hopkins University Press 2006.



Let ha represent the impresst velocity ~~[illegible]~~ in the ~~[illegible]~~ tangent of an ~~[illegible]~~ ellipse. and ad the velocity impresst by gravity, make (dβ) parallell & equall to (ac) then draw the diagonall (aβ). the 2d puls of gravity shall meet the body at β. where the puls againe meets it Driving it towards the center O. wth the velocity β2 which has the same ~~[illegible]~~ proportion to the radius βo that ad has to ao. make βi = aβ and make 2x equall & parallell to it then draw βx. now if the velocity to gravity had been as ha to da then the body had moved in a circle but because the velocity ha is less ~~than~~ in proportion to ad then it ought to make it move in a circle therfore its motion shall be in an ellipse. for as ao is to ad soe βo to β2 soe 10 to 2x[illegible]. and the same proportion that aβo has to abo the same shall βxo have to bko. the motion of this body therfore shall be polygonall in an ellipse. and shall Describe equall areas in equall times

when the velocity and direction of the motion of Lation doth by its Receding from the center ballance the accesse by the Ray of Gravity then doth the body move in a circle if the gravity bee in the center of it.

But if the Recesse overballance the accesse it goeth farther off: and the contrary if contrary. and the polygone becomes various according to the differing degrees of gravity at Differing distances from ye center.

In September 1685, Hooke wrote a manuscript on central-force motion that graphically implemented his 1674 hypothesis on planetary motions. Here, Hooke constructed a polygonal approximation to the elliptical trajectory traced by a body attracted by a central force whose intensity is proportional to distance (Hooke's law). Source: Trinity College, Cambridge. MS O.11a.1/16 courtesy of the Master and Fellows of Trinity College (Cambridge).

mechanicians active in London in his times. Rather than criticizing Hooke on the basis of anachronistic normative values about what "good" mathematics should be, it is more appropriate for the historian to accept that his mechanical practice was considered the right way to proceed within the community of inventors and virtuosi who were pursuing natural philosophy by resorting to the "mixed mathematical sciences".[31]

The case of Hooke's theory of gravitation, its invisibility for official historiography, and its impact on Newton that we have briefly considered above raises interesting questions that can be broached within the framework of Fisch's theses on "trading zones" and the creative role of "ambivalence", because Hooke was literally trading between natural philosophers accustomed to handle astronomical data and mathematical formulas and practitioners skilled in drawing diagrams and manipulating machines. In doing so, he fostered a mediated dialogue and cooperation between individuals who belonged to socially separated groups and who adopted ways of life and languages that rarely mixed with each other. Hooke thereby promoted the professional training he had received in the smoky workshops of London as something that amounted to much more than just manual dexterity. His familiarity with mechanical tools was an instrument for discovery that, as we have seen, was inspirational for Newton. The "mechanic of genius" had a great deal to teach to the true scientist and natural philosopher.

One might claim, at this juncture, that the category of the "ambivalated" individual can be applied to Hooke. In the Preface to the *Micrographia* (1665), Hooke had defended Cartesian mechanisms as a grounding feature of the new philosophy pursued at the Royal Society. Hooke shared the Cartesian view, according to which matter is ultimately composed of corpuscles, and the microscopic "texture of matter" and the interactions of corpuscles could explain all phenomena. The inner workings of nature, he claimed, could be accounted for in terms of interactions between "compounding particles of matter" that eventually might even be observed through the help of some improved microscope.[32] Hooke's planetary hypothesis, however, was based both on the Cartesian "rules of mechanics" and on the idea that powers acting at a distance are the cause of planetary motions. A second model, familiar to the followers of "occult"

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[31] In the Aristotelian tradition, the mixed mathematical sciences, such as astronomy and optics, could not attain causal explanations and were thus subordinated to natural philosophy. It was natural philosophy, framed in terms of syllogistics, not in terms of mathematics, that could uncover secondary and even first causes. The rising status of the mixed mathematical sciences as appropriate tools for causal explanations is a feature of seventeenth century natural philosophy that should not be underestimated. For a study of this topic see, for example, Gary I. Brown, "The evolution of the term 'mixed mathematics", *Journal of the History of Ideas* 52 (1991), 81–102.

[32] "Tis not unlikely, but that there may be yet invented several other helps for the eye, at much exceeding those already found, as those do the bare eye, such as by which we may perhaps be able to discover living Creatures in the Moon, or other Planets, the figures of the compounding Particles of matter, and the particular Schematisms and Textures of Bodies"~~,~~ (Robert Hooke, *Micrographia: Or, Some Physiological Descriptions of Minute Bodies Made by Magnifying Glasses, With Observations and Inquiries Thereupon*, London: J. Martyn and J. Allestry 1665, sig. ~~b4~~).

forces, such as John Dee, William Gilbert, and Francis Bacon, was superimposed upon the mechanical one in a way that is characteristic of quite a number of English natural philosophers in the seventeenth century.[33] One might claim, with John Henry, that this merging of occult philosophy with the mechanical one is what paved the way to Newton's gravitation theory.[34] Without this ambivalated copresence of two competing, apparently conflicting frameworks, we would not have had the creative shift, the "change of mind", that lay the groundwork for the *Principia*.

Yet, I would like to stress another point here concerning the language that allowed Newton and Hooke to exchange views on planetary motion. The mathematical language they employed was that familiar at the time to those mathematical practitioners intent on studying the mechanical generation of curves via the handling of instruments made of sliding rulers and strings. The mathematical practitioners active in the early modern period often saw a curve-tracing device not so much as a theoretical construct but as an instrument to be applied in one's workshop. The curve traced by an instrument could serve as the conic surface of a lens, the hyperboloid surface of the fusee of a clock, the cycloidal shape of the teeth of a wheel, or the stereographic projection of the lines of equal azimuth of the celestial sphere. Knowledge of the construction and handling of mechanical instruments was part of the mathematical repertoire not only of Hooke, the mechanical practitioner, but also of Newton, the mathematician. And one might contend that it is such an overlapping between the mathematical practices of Hooke and Newton that allowed them to enter into a fruitful dialogue: they were deploying – so to speak – a mathematical "interlanguage".. The two met in a trading zone in which they (litigiously) dialogued by using a mathematical pidgin language that was part of the mathematical toolboxes of both the Lucasian professor, mathematically trained by reading the works of Descartes, Barrow, and Wallis, and the Royal Society secretary, instructed in the mechanical practices of the London mathematical practitioners.

The interest in the use of mechanical instruments in geometrical constructions was so alive that a discipline called *geometria organica* (from the Greek word *organon*, for instrument) was atop the agenda not only of mechanical practitioners such as Hooke but also of many highbrow mathematicians, including Descartes. The latter filled his *Géométrie* (1637) with curve-tracing devices (composite compasses, rulers equipped with strings and pulleys) that he deployed in the *Dioptrique* as parts of lens-grinding machines he discussed with skilled opticians such as Jean Ferrier.[35] The *geometria organica* was cultivated also by one of the most prolific Dutch mathe-

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[33] Wang, *Occult Principles*.

[34] John Henry, "Occult qualities and the experimental philosophy: Active principles in pre-Newtonian matter theory", History of Science 24 (1986), 335–381; "The fragmentation of Renaissance occultism and the decline of magic", *History of Science* 46 (2008), 1–48.

[35] Henk J. M. Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction*, New York: Springer 2001; D. Graham Burnett, *Descartes and the Hyperbolic Quest: Lens Making Machines and Their Significance in the Seventeenth Century*, Philadelphia, PA: American Philosophical Society 2005.

maticians, Frans van Schooten, who right from the title page of his treatise (1646) made it clear that he was not simply interested in pure geometry, for his work, he emphasized, was useful not only to geometers but also to opticians, designers of sundials, and mechanicians (the title reads *De Organica Conicarum Sectionum in Plano Descriptione Tractatus: Geometris, Opticis, Praesertim Vero Gnomonicis & Mechanicis Utilis*). Christiaan Huygens, with his *Horologium Oscillatorium* (1673), offered another influential example of interaction between the study of mechanical curves (such as the cycloid) and geometrical constructions applied to natural philosophy as well as to mechanics (horology). Organic geometry was the interlanguage deployed by Hooke and Newton in the trading zone of the Royal Society.

As a mathematician, Newton was very much interested in organic geometry, in the tracing of curves via mechanical instruments. Most notably – as he informed John Collins, a prominent mathematical practitioner active in London, in 1672 – he devised a new method for tracing conics and higher order curves.[36] Therefore, he shared with Hooke mathematical competences and interests that allowed the two natural philosophers to exchange ideas and "communicate across a cultural border"[37] that separated the practitioner from the university professor. Organic geometry was their inter-language, to use Fisch's terminology, and the Royal Society was the conflictual zone where they traded their views on light (from 1672–1676) and planetary motions (from 1679). But Newton was a mathematician who had other languages and concepts in his toolbox that were foreign to Hooke's background knowledge. Newton was the discoverer of a method to calculate the radius of curvature of a plane curve, a technique that allowed him to graphically approximate planetary trajectories according to Hooke's hypothesis in a powerful way. It is this technique that, as Nauenberg has demonstrated, allowed Newton to formulate Hooke's hypothesis in a more satisfactory way.[38] Instead of graphing the trajectory by means of the composition of motions via the parallelogram rule (as Hooke did; see Figure 1), Newton drew the trajectory by calculating the radius of curvature (given his knowledge of the law that "the radius of curvature is proportional to speed squared, divided by the normal component of central force") and approximating it piecewise as a series of circular trajectories. This technique afforded Newton a better approximation. If we follow closely the correspondence between Hooke and Newton, we discover that at first, Newton made a mistake, which Hooke corrected: Newton calculated that a body attracted by a constant gravitational force toward the center of the Earth ("a body B let fall & it's gravity will give it a new motion towards the centre of ye Earth") would spiral down toward the center ("describing in its fall a spiral line").[39] Hooke

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[36] MS. Add. 3977.10, fol.1v (Cambridge University Library). Turnbull, *Correspondence*, vol. 2, 230–231.

[37] Fisch, *Creatively Undecided*, 111.

[38] Michael Nauenberg, "Robert Hooke's Seminal Contributions to Orbital Dynamics", *Physics in Perspective* 7 (2005), 4–34.

[39] Turnbull, *Correspondence*, vol. 2, 301.

replied that, rather, the body would move between a pericenter and an apocenter, describing a curve akin to an ellipse ("my theory of circular motion makes me suppose it [the curve] would be very differing and nothing att all akin to a spirall but rather a kind of Elliptueid").[40] After a few days, Newton was able to solve this rather thorny problem, setting the record straight: he showed that the trajectory would be neither a spiral nor an oval (for example, an ellipse) but rather a precessing orbit with a large apsidal advance.[41]

It is a reasonable guess that Newton "changed his mind" – a phenomenon of rational-framework transition on which Fisch strongly insists – when he realized that his mathematics allowed him to mathematicize Hooke's hypothesis; this is what made the hypothesis so interesting for Newton. To mathematicize an ether theory of planetary motion is a Herculean task, one that not even Leonhard Euler was able to fulfill almost a century later. Indeed, vortex fluid motion could be mathematically tackled only in the 19th century in terms of the Navier-Stokes equations. Physical models can be tackled by mathematical tools that are not always available when they are first conceived. Quantum fields are a wonderful model for studying the interaction of matter with radiation, but until the techniques of renormalization were developed, the calculated amplitudes in quantum electrodynamics were all meaningless infinities (I am thinking of the notorious divergences that plagued theoretical physics in the mid-20th century). It is reasonable to surmise that the force that led Newton to move beyond the ether model and to accept an action-at-a-distance model was the realization that Hooke's hypothesis was approachable by mathematical tools at his disposal. Notice that I am not reiterating here the thesis according to which Hooke was a poor mathematician compared to Newton. This might be true, but it is not the issue at stake here. The difference between the two correspondents that afforded Newton an advantage over Hooke was slighter than one might think. Newton's advantage boiled down to a detail: a technique to calculate the radius of curvature of a plane curve – a technique, undoubtedly, that Newton had mastered since his discovery of the fluxional method. One should not too hastily conclude that Hooke was incompetent in mathematics and that Newton's superiority as a mathematician explains his success in the 1679–1680 correspondence.[42] As a matter of fact, Newton was deploying a very simple graphic technique. Newton was not integrating a differential equation, of the sort we might find in a textbook of "Newtonian" mechanics nowadays; rather, very much like Hooke, he used pen, straight edge, and compass to carefully draw an approximating curve on paper.

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[40] Turnbull, *Correspondence*, vol. 2, 305.

[41] Turnbull, *Correspondence*, vol. 2, 307.

[42] Hooke's competent notes to Guillaume F. A. de Hospital, *Analyse des Infiniment Petits pour L'intelligence des Lignes Courbes*, Paris: Imprimerie Royale 1696, show that the secretary of the Royal Society was perfectly able to digest the intricacies of the infinitesimal calculus. See the Royal Society Library London (MS Cl.P/20/87) copy consulted by the author after discovering these annotations at the website http://www.hookesbooks.com/ (last accessed 20 May 2020).

It might be contended that it is exactly the mechanical character of Hooke's mathematical methods (his *geometria organica*) that led many historians in the past to disqualify Hooke's intellectual endeavor as not pertaining to “true science”~~.~~.[43] These historians have projected the modernity of “Newtonian” mechanics onto Newton, failing to realize that the two correspondents were speaking the same mathematical language. This might be an example of the phenomenon of the “invisibility in official historiography” of trading-zone mediators that has been underlined by Fisch. As we have seen above, Hooke was quite invisible in the works by I. Bernard Cohen and Rupert Hall, two mid-twentieth-century masters in the history of science who, influenced as they were by Alexandre Koyré's interpretation of the “scientific revolution” as the outcome of the mathematical visions of a few great giants (Copernicus, Galileo, Kepler, Descartes, and Newton), were prevented from attributing great importance to the Royal Society's Curator of Experiments. When Koyré, Cohen, and Hall were writing their influential historical narratives, the popular models of what it is to be a scientist were the likes of Einstein and Heisenberg: theoretical physicists who, basing their mathematical constructions on philosophical principles and abstractions, could deliver predictions that received spectacular confirmation. It is only recently that Hooke's reputation has been “restored”, as Lisa Jardine puts it.[44]

Lately, there has been a proliferation of studies on Hooke, and at this juncture, it is fitting to ask why Hooke's fortune has changed recently, so much so that a commemorative plaque to him has been unveiled in Westminster Abbey, just in front of the funerary monument of his great enemy, Sir Isaac Newton.[45] There is no easy answer to this question, and in searching for one, I will complement Fisch's internal account of rational change with a historiographical perspective based on the idea that the context in which the historian lives determines his narrative: we always view the past – to state a truism – through the conceptual frameworks provided by our present standpoint. The fact that Hooke is given pride of place in recent accounts of seventeenth-century natural philosophy is a consequence of a re-evaluation of the role played by engineers and mechanicians in the development of early modern science and of a shift of interest from mathematics and astronomy (the two disciplines that informed the Koyrean and Sartonian narratives of the scientific revolution) to mixed

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[43] As a matter of fact, notwithstanding the unique fortune of Hooke's intellectual career, the merging of mathematics, natural philosophy, and mechanics characterizing Hooke's scientific practice was not unusual for the mathematicians of the 17th and 18th centuries. See Dominique Tournès, *La Construction Tractionnelle des Équations Différentielles*, Paris: Blanchard 2009.

[44] “Robert Hooke: A Reputation Restored”, in: Michel Cooper and Michael Hunter (Eds.), *Robert Hooke: Tercentennial Studies*, Aldershot: Ashgate 2006, 247–258.

[45] Ofer Gal, *Meanest Foundations and Nobler Superstructure: Hooke, Newton and “the Compounding of the Celestiall Motions of the Planets”*, Dordrecht: Kluwer 2002; Stephen Inwood, *The Man Who Knew Too Much: The Strange and Inventive Life of Robert Hooke 1635–1703*, London: Macmillan 2002; Lisa Jardine, *The Curious Life of Robert Hooke, the Man who Measured London*, London: Harper Collins 2003; and Michael Cooper and Michael Hunter (Eds.), *Robert Hooke: Tercentennial Studies*, Aldershot: Ashgate 2006.

mathematics, hydrology, pneumatics, biology, medicine, microscopy, geology, and alchemy.[46] But perhaps, even more deeply, our perspective has changed because of the recent advent of new scientific paradigms dominated by computer simulations and performative technologies. Hooke, the practitioner who tested his new theory of planetary motion by letting spheres roll on conical surfaces or observing pendular motion, the "organic" geometer who graphically reconstructed central-force motion might be viewed as a precursor of computer modeling. His social status and scientific practice somewhat resemble those of practitioners of nanotechnology and genetic engineering, who often find themselves active in "extra-mural science", scientific enterprises located outside academia, driven by a know-how that is nurtured in the manipulations performed in the laboratory and shared with technicians and entrepreneurs rather than in theoretical conversations with post-docs and grown-up colleagues in the department seminar. Such analogies are of course perilous: they can lead to Whiggism as much as the parallel of Newton with Einstein once did. Yet, such analogies act, mostly tacitly, today in orienting the historians' perspective along angles that allow the emergence from oblivion of Hooke as an important, ambivalent mediator between the "dark shops" of glass- and clock- makers and the rooms of aristocrats and high-ranking clergymen who turned to natural philosophy for the pursuit of religious apologetics. Hooke's ambiguous position between these two groups, one might further contend, was in part responsible for his invisibility. He occupied too low a position to figure as a conspicuous collaborator of his patrons, Christopher Wren and Robert Boyle. Hooke was aware of the important role as a mediator between the "cultures of skill and those of learning" that he was playing.[47] In the dedication to Sir John Cutler in the *Micrographia*, he wrote the following:

> This Gentlemen [Cutler] has well observ'd, that the Arts of Life have been too long imprison'd in the dark shops of Mechanicks themselves, & there hindred from growth, either by ignorance, or self-interest; and he has bravely freed them from these inconveniences.[48]

III. Hooke's and Newton's Contrasting Views on Mathematical Method

As we saw in the previous section, what is at stake in evaluating Hooke's contribution to gravitation theory is a conflict about what can be counted as good or "true" mathematics. It is in part this conflict that generated the anxieties that inflamed the clash between Hooke and Newton, and it is this conflict that generated diverging readings of the relative importance of the two English natural philosophers. In this section, I

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[46] Domenico Bertoloni Meli, *Thinking with Objects: The Transformation of Mechanics in the Seventeenth Century*, Baltimore, MD: Johns Hopkins University Press 2006, 218–223, 242–247.

[47] Robert Iliffe, "Material doubts: Hooke, Artisan culture and the exchange of information in 1670s London", *British Journal of the History of Science* 28 (1995), 285–318; Larry Stewart, "Science, Instruments, and Guilds in Early-Modern Britain", *Early Science and Medicine* 10 (2005), 392–410.

[48] Hooke, *Micrographia*, "Preface", sig. ~~g2~~.

will suggest that Hooke and Newton clashed not only because of a conflict generated by priority claims. They diverged on rather deeply felt issues concerning the nature and role of mathematics in natural philosophy. One might say that the Royal Society was the conflictual "trading zone" where Hooke and Newton confronted opposing norms concerning how natural philosophy should be practiced. Unfortunately, it is often the case that the confrontation between Hooke and Newton is reduced to simply an issue of priority. This is a reading that the two protagonists of the dispute themselves helped to shape.

Indeed, when Hooke learned about the imminent publication of the *Principia*, he was vocal in claiming his priority: he could find very little appreciation of his contributions in Newton's opus magnum. To those who sided with Hooke, it was plain that the Royal Society's secretary had been robbed of a decisive contribution in the understanding of the planetary system. On September 15, 1689, Hooke's friend, diarist John Aubrey, pleaded for Hooke's case by writing in despair to the antiquarian Anthony à Wood, who was then composing his *Athenae Oxonienses*:

> Mr Wood! This [gravitation] is the greatest discovery in nature that ever was since the world's creation: it never was so much as hinted by any man before. I know you will doe him [Hooke] right.[49]

Yet, Hooke's contribution to gravitation theory was forgotten in the 18th century, a period in which the status of practitioners of analytical mechanics, who conceived themselves as the heirs of the Newtonian program expounded in the *Principia*, raised high in the scientific academies.

For the mathematicians of the Enlightenment, Newton's disparaging evaluation of Hooke's contribution to gravitation theory was non-problematically true:

> Now is not this very fine? Mathematicians [like Newton himself] that find out, settle & do all the business must content themselves with being nothing but dry calculators & drudges & another [Hooke] that does nothing but pretend & grasp at all things must carry away all the invention.[50]

Newton was writing the above lines in 1686 to Edmond Halley, who had informed him about Hooke's claims in the discovery of universal gravitation. These claims, as

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[49] John Aubrey, *Brief Lives: Chiefly of Contemporaries: Set Down by John Aubrey between the Years 1669 & 1696*, edited by Andrew Clark, Oxford: Clarendon Press 1898, vol. 1, 415; Turnbull, *Correspondence*, vol. 3, 42.

[50] Turnbull, *Correspondence*, vol. 2, 438. One reads in an appendix to Du Châtelet's French translation of the *Principia*, "Il ne faut pas croire que cette idée jettée au hazard dans le Livre de Hook diminue la gloire de M. Newton […] L'exemple de Hook & celui de Kepler servent à faire voir quelle distance il y a entre une vérité entrevue & une vérité démontrée, & combien les plus grandes lumières de l'esprit servent peu dans les sciences, quand elles cessent d'être guidées par la Géométrie". Du Châtelet, "Exposition abregée du système du monde, et explication des principaux Phénomenes astronomiques tiré~~é~~ des Principes de M. Newton". Appendix to Isaac Newton, *Principes Mathématiques de la Philosophie Naturelle par Feue Madame la Marquise Du Chastellet*, Paris: Desaint & Saillant 1759, vol. 2, 6.

we know, were not accepted. But Newton’s victory was far from being as straightforward as one might think when projecting on historical actors conceptions about the role and nature of mathematized science that were accepted as uncontroversial only much later. If we look somewhat more closely at Newton’s interactions with the Royal Society, we discover that his conceptions of mathematized natural philosophy were initially rejected. Furthermore, Newton himself was at pains to integrate the new mathematics he had so successfully developed in his youth within the philosophical agenda that he had been endorsing from the mid-1670s onward. Anxiety and ambivalence were not foreign to the greatest hero of the scientific revolution.

Newton had become a member of the Royal Society ~~in 1672~~ after having presented his reflection telescope to the society. This innovation fitted well with the desiderata of the newly established institution, which assigned great importance to microscopy and the improvement of telescopic observations. As is well known, in 1672, Newton submitted his famous essay on the *experimentum crucis*, in which he claimed to have proved a new theory concerning light and colors. The theory had ~~already~~ received a thorough treatment in the Lucasian lectures on optics that Newton had deposited in 1672 and dated retrospectively from 1670. In the third lecture, he stated that by the use of “geometry”, the science of colors, and natural philosophy in general, could achieve the “greatest evidence”~~.~~. He also expressed his annoyance toward those natural philosophers who were confining themselves to “conjectures and probabilities”~~.~~.[51] Newton might have had Hooke in mind here, since in his *Micrographia* (1665), the latter had warned readers to consider any “small Conjectures” concerning the “causes of things” contained in the book as simply “doubtful Problems, and uncertain ghesses”~~.~~.[52] Hooke was expressing values deeply felt in the Royal Society.

One should bear in mind that just after the Restoration, many natural philosophers belonging to the Royal Society wished to make it clear that no “unquestionable” or “dogmatic conclusions” should be feared from them. Politically opinionated philosophers, or dogmatic theologians, were not admitted into the society, which instead promoted a mitigated skepticism that could protect its fellows from the politically risky positions epitomized by Hobbes’s or Spinoza’s metaphysics.[53] As one could read in the influential manifesto by Thomas Sprat, *The History of the Royal Society*, experimental philosophy “never separates us into mortal Factions”: it is by avoiding the “enthusiasm and dogmatism” dominant during the Interregnum that the Royal Society’s fellows were free to “raise contrary imaginations […] without danger of Civil War”~~.~~.[54] That is why any discourse aimed at reaching certainty was looked upon with

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[51] Alan Shapiro (Ed.), *The Optical Papers of Isaac Newton, Volume 1. The Optical Lectures 1670–1672*, Cambridge: Cambridge University Press 2012, 88.

[52] Hooke, *Micrographia*, “Preface”, sig. ~~b1~~.

[53] Barbara J. Shapiro, *Probability and Certainty in Seventeenth-Century England: A Study of the Relationships Between Natural Science, Religion, History, Law and Literature*, Princeton, NJ: Princeton University Press 1983.

[54] Thomas Sprat, *The History of the Royal Society of London, for the Improving of Natural Knowledge*, London: T. R. [Roycroft] for J. Martyn and J. Allestry 1667, 56.

suspicion, while skepticism and probabilism were approved of in some of the most influential Royal Society manifestos, such as Hooke’s masterpiece on microscopy, Sprat’s *History*, and Joseph Glanvill’s *Scepsis Scientifica* (1665).

In his Lucasian lectures on optics and his 1672 paper, Newton broke with this code of behavior by stating that the theory of colors he was proposing – a topic that he knew was regarded as “belonging to physics” – was not “an hypothesis but most rigid consequence”.~~.~~[55] The fact that Newton’s mathematized optical theory of colors was presented in ways not acceptable to the Royal Society is made manifest by the fact that the above statement was censored by Henry Oldenburg, the secretary of the Royal Society. To most fellows of the Royal Society, Hooke’s mechanical mathematics was a much more congenial enterprise.

Hooke was always careful to present his mathematized natural philosophy as conjectural and hypothetical (he made it clear that his gravitation theory was a hypothesis based on suppositions). Moreover, his mechanical and graphical mathematics, contrary to Newton’s, could be understood by all the fellows, as it did not require mathematical training and could be made persuasive through well-orchestrated public displays, rather than a solitary exercise of reason. All these features of Hookean organic mathematics resonated with values such as experimentalism and the public accessibility of demonstrations that were promoted at the Royal Society.

When Newton produced his bulky *Principia*, very few could read his demonstrations and grasp the usefulness of the abstract mathematized planetary theory. Halley had to make a serious effort to have the book accepted at the Royal Society by insisting on its purported usefulness for the arts of gunnery and navigation.[56] One should not conclude, however, that Newton, his robust methodology notwithstanding, viewed the new mathematized sciences of colors and gravitation that he had immensely helped to shape without anxiety. On the contrary, an analysis of his manuscripts, correspondence, and publication policies reveals how difficult it was for him to integrate the new algebraic methods of series and fluxions within the complex web of beliefs that he had come to endorse in the mid-1670s. He began framing his ruminations on mathematical methods in terms of a sharp opposition between the methods of the moderns (epitomized by Descartes) and those of the ancients, as revealed by Pappus’s *Collectiones*, which he read avidly. Newton began searching for the hidden method of discovery of the ancients, the *Analysis Veterum*, more beautiful and concise, and more powerful, he claimed, than the Cartesian approach to geometry in terms of symbols. The Cartesian algebraic geometry in which he had so wonderfully excelled in his youth now gave him “nausea”~~.~~.[57] Algebra, he conceded – according to

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[55] Newton, *Optical Papers*, 87; Turnbull, *Correspondence*, vol. 1, 96.

[56] For Halley’s review of the *Principia* published in the *Philosophical Transactions* for 1687 and 1697, see Isaac Newton, *Papers and Letters on Natural Philosophy*, edited by I. B. Cohen and R. E. Shoefield, Cambridge, MA: Harvard University Press 1958, 405–424.

[57] D.T. Whiteside (Ed.), The Mathematical Papers of Isaac Newton, Vol. 4, Cambridge: Cambridge University Press 2008, 276–277.

a memorandum by David Gregory – is “fit enough to find out, but entirely unfit to consign to writing and commit to posterity”.[58] These methodological convictions of Newton the mathematician resonated in complex ways with his views on the wisdom of the ancients and his philosophically pronounced anti-Cartesianism. Although historians interested in these aspects of Newton’s complex intellectual biography risk drawing oversimplified implications between diverse aspects of Newton’s intellectual endeavor (those pertaining to mathematics and those pertaining to history and religion), this historiographical challenge is worth facing, as it allows us to probe Newton’s authorial strategies. As time went on, Newton lost no chance to depict himself as an heir of the ancient geometers rather than as a follower of the “mathematicians of recent times”, whom he despised.[59]

The contrast with Hooke is striking. The Curator of Experiments, and from 1679, the secretary of the Royal Society, portrayed himself as a representative of the new burgeoning class of mechanicians and entrepreneurs active in London for the promotion of useful knowledge and profiled himself as a follower of both Baconian experientialism and of moderate Cartesian hypotheticism. He viewed himself as the promoter of a new philosophy which in principle could restore part of the knowledge of the natural world that mankind had lost after the Fall from Eden.[60] The Lucasian professor, instead, aimed at certain knowledge, despised conjectural hypotheticism, and viewed himself as the restorer of a pristine Noachian wisdom corrupted because of idolatry. As a natural philosopher, he rejected those Cartesian mechanical conjectures that, he claimed, were “blazoned about everywhere”.[61] Hooke’s and Newton’s contrasting views on mathematical methods and the role of mathematization in natural philosophy are related in complex ways to the conflicting narratives that the two men constructed and divulged. Indeed, part of the passionate polemic between Newton and Hooke was framed in terms of different historical narratives on the development (and corruption) of natural philosophy and mathematics.

As we have seen in this section, in many ways, Hooke and Newton endorsed different norms on how mathematics should be deployed in natural philosophy. Yet, as was earlier detailed, Hooke was able to “destabilize” Newton, as Fisch would put it,[62] and induce a momentous framework transition in the Lucasian professor’s mind.

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[58] Turnbull, *Correspondence*, vol. 3, 385. Translation from Latin by Herbert W. Turnbull.

[59] Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method*, Cambridge, MA: MIT Press 2009.

[60] “And as at first, mankind fell by tasting of the forbidden Tree of Knowledge, so we, their Posterity, may be in part restor’d by the same way, not only by beholding and contemplating, but by tasting too those fruits of Natural Knowledge, that were never yet forbidden”. Hooke, *Micrographia*, sig. ~~b3–b4~~.

[61] Newton, *Optical Papers*, 88–89. Here Newton cites from Isaac Barrow, *Lectiones Opticae et Geometricae*, London: Godbid 1674, Lectio XVIII, § XIII, 125.

[62] On Fisch’s concept of “destabilization”, see Fisch, *Creatively Undecided*, 4, 33.

IV. Conclusion

The historical case we have considered – the influence of Hooke on Newton’s theory of gravitation – is emblematic of the anxious confrontations on mathematical methods and natural philosophy that characterize the long seventeenth century. If we allow ourselves to make full use of Fisch’s and Benbaji’s nomenclature, we can conclude by saying that Hooke, an “invisible” individual for official historiography, forged a hypothetical explanation of planetary motions in terms of action-at-a-distance gravitation, and he did so “from within” a Baconian agenda that “ambivalatingly” merged features of the mechanical and the occult philosophies. Hooke was successful in this proposal because of his ability to talk and trade between different groups, the natural philosophers of the Royal Society and the practitioners of the arsenal and shops of London. The open questions on planetary motions of the natural philosophers found an answer in the mechanical and graphical instruments of the practitioners. Hooke’s new “Hypothesis” on the “System of the World” was the gift he handed Newton, who transformed it and brought it to a level of mathematical sophistication that has been received since the 18th century as the crowning moment of the “Scientific Revolution”~~.~~. There is no doubt that it was Newton who achieved the mathematization of the theory of gravitation. Yet, as Fisch has taught us, we should not only focus on achievements but also appreciate the creative role of individuals who, by visiting a trading zone equipped with a “mathematical pidgin”, a language fit for the trade of ideas, fostered a rational- framework change.