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The Evolution of Knowledge: A Scientific Meeting in Honor of Jürgen Renn

Rivka Feldhay (ed.)

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A Scientific Meeting in Honor of Jürgen Renn

Edited by Rivka Feldhay

In memory of Nuccio

Ordine (1958–2023)

a dear colleague and friend

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Introduction

Rivka Feldhay[1]

A symposium took place in honor of Jürgen Renn at SISSA (Scuola Internazionale Superiore di Studi Avanzati) in Trieste, July 14–16, 2022, supported by the Organizing Committee: Rivka Feldhay, Andrea Gambassi, Hanoch Gutfreund, Christian Joas, Stefano Ruffo, Donald Salisbury, Matthias Schemmel, Tian Miao, and Zhang Baichun.

Participants at the meeting (left to right – back: Steven Levinson, Kostas Gavroglu, Vincenzo Barone, Peter McLaughlin, Bernd Scherer, Gerd Graßhoff, Klaus Vogel, Jens Braarvig, Jürgen Jost, Carola Sachse, Matthias Schemmel, Jens Høyrup, Sonja Brentjes, Hanoch Gutfreund, José Sanchez Manuel Ron, Ricarda Winckelmann, Ana Simões, Urs Scheopflin, Manfred Laubichler, Tilman Sauer, Elio Nenci, Roberto Lalli, Lindy Divarci, Giulia Rispoli, Antonio Becchi, Thomas Turnbull; front: Birgit Kolboske, Claudia Parma, Mila Bottegal, José Montesinos, Maca Montesinos, Margaret Haines, Jürgen Renn, Leo Corry, Rivka Feldhay, Maria Paula Diogo, Carsten Reinhardt, Matteo Valleriani, Massimiliano Badino, Stefano Ruffo).

[1] Minerva Humanities Center at Tel Aviv University

The sessions of the symposium were organized around the following topics:

1.**Origins of Evolution of Knowledge:** Practical and conceptual transformations, scientific revolutions.

2.**External Representations of Knowledge:** Abstraction and reflection, mental models, origins of writing and arithmetic, diagrams in Greek mathematics, visual and graphical representations, paper tools.

3.**Science and Society:** Preclassical mechanics, evolution of Galileo's science, history of architecture.

4.**Institutional Organization of Knowledge:** Einstein papers, history of the Max Planck Society.

5.**The Spread of Knowledge:** Islamic transmission and addition, China and the integration of mechanics, Latin America, Industrial Revolution, epistemic and social communities.

6.**Knowledge Evolution and the Anthropocene:** Exodus from the Holocene—unintended consequences, knowledge for the Anthropocene, dark knowledge.

A selection of the talks from each of these sections are given in this preprint.

The inspiration to organize the symposium stemmed from Jürgen Renn's publication *The Evolution of Knowledge: Rethinking Science for the Anthropocene.* Since this book is presented by Renn himself as a culmination of the joint research undertaken in his department at the Max Planck Institute for the History of Science in Berlin since 1994, it seemed only fitting to celebrate this milestone achievement at an event that would include many of those who fundamentally shaped this joint research endeavor over the years.

The Department's research is based on a conceptual framework for a historical epistemology—understood as a historical theory of knowledge—that was co-developed on the basis of earlier work on science and its relation to human labor and its societal organization. The studies have been dedicated from the beginning to an investigation of the history of science as part of a larger history of human knowledge. Renn and his colleagues consistently emphasize the role of practical knowledge and historical continuity, even when focusing on the turning points of modern science. The investigations include cross-cultural comparisons, in particular between Western, Chinese, and Islamic science, and a research program on the globalization of knowledge in history.

The book covers a timespan from the origins of human thinking to the modern challenges of the Anthropocene. The Anthropocene is regarded here as the new geological age of humankind, defined by the profound and lasting impact of human activities on the Earth system, and is thus the ultimate context for a history of knowledge and the natural vanishing point for an investigation of cultural evolution from a global perspective. From this perspective, the book brings together multiple historical and geographical horizons, and deals with both the *longue-durée* aspects of the evolution of knowledge and the accelerated changes in the development of knowledge that have brought us into the Anthropocene.

The Evolution of Knowledge, first published in English (Princeton University Press, 2020), and subsequently in French (Les Belles Lettres, 2022), German (Suhrkamp, 2022), Italian (Carocci Editore, 2022), Chinese (Post Wave Publishing, 2023), and Vietnamese (Phanbook, 2023).

Consciously working against the current of "micro history" and joining the emerging field of "world history," the book is characterized by six major directions that together mark a breakthrough in the way that history of science has been perceived until now. In terms of its insights, creativity, and originality, the book may be compared to Thomas Kuhn's *The Structure of Scientific Revolutions*, even though it differs diametrically from Kuhn's theses and worldview. These aforementioned six major directions are as follows:

1) Taking its point of departure from the conviction that "science" is not an intellectual product *sui generis* that was born in the West, but a kind of knowledge rooted in basic experiences of human beings (and even animals), the book traces structures of knowledge acquisition, drawing on a variety of disciplinary traditions: from evolutionary biology, experimental psychology, geology, earth science, and

archeology to history, philosophy, anthropology, sociology, cognitive science, and economics.

2) The book delineates in detail, and in an original manner, the mechanisms of knowledge acquisition, consolidation, transmission, transformation, innovation, and dissemination of science. Among its foci are practices with objects; first order symbolization/representation in language; application to new contexts resulting in the creation of new objects such as simple machines (external representations); second order reflection on practical experiences and inventions leading to abstraction or conceptualization; creation of mental models (internal representations) and their inscription in texts; the creation of social and institutional niches for the survival and transmission of knowledge; integration of old and new knowledge, and its re-organization; creation of intellectual and institutional structures necessary for the dissemination of knowledge; and finally the sum of practical, intellectual, sociopolitical, and market conditions allowing for the construction, integration, consolidation, reorganization, and dissemination of knowledge, analyzed in terms of "economies of knowledge."

3) The book exposes and reconstructs the above-mentioned mechanisms through the examination of a plethora of deeply researched historical case studies that are not limited to a certain historical period or geographical territory. Quite the opposite: in an astonishingly daring way, the book includes case studies from the depth of global history such as the Neolithic age; the emergence of writing and basic mathematical thinking in Mesopotamia and in ancient Egypt; Greek science, its adaptation and translation to Syriac, Arabic, and Hebrew and later on into Latin; pre-classical and classical mechanics developing in early modernity in Europe; mechanical praxes and thinking in China; the eighteenth century chemical revolution; early and later industrial revolutions; relativity theory, and more.

4) From the discussion of historical case studies within a well-developed conceptual framework emerges the theoretical language that guides and enables a long-term narrative of global knowledge. The main terms of this new theoretical language are: *challenging objects*, namely those objects whose nature and mode of behavior challenge accepted intuitive and practical assumptions about the world such as that of motion implying force. *Internal* and *external representations*: the first

develops into signs of things in the mind and "mental models"; the second consists of texts, tools, machines, institutions, social, and political contexts for the reconstruction and constitution of shared public knowledge.

5) The book treats knowledge as being always on the move, resulting in the conclusion—on the broad basis of historical analysis of the case studies—that knowledge has always been global. The historical examples of processes of the globalization of knowledge span from the third millennium B.C.E and up to the present; from ancient Mesopotamia, through the Arab empire, Europe, China, and South America.

6) Finally, the motivation for writing the book is rooted in an analysis of the era of the Anthropocene—a new human geological age, according to some contemporary scientists—and the challenges it poses to humanity. Thinking about the Anthropocene takes into consideration the impact of human activities on the Earth system. Such impact is contingent—as a result of mechanisms of variation and selection—potentially inducing progress but also the destruction of the planet. The book is rooted in the view that knowledge may provide a key to solve contemporary problems. It points out the possibilities opened up by digital globalization and its dangers; relates to artificial intelligence and the risks it entails; promotes consciousness of the need to connect knowledge of the natural sciences to knowledge of the humanities and to develop a new economy of knowledge. It seems that in the author's view history of science can provide a vision for coping with such huge challenges and may serve as a model for new types of knowledge required by the new era.

The book consists of seventeen chapters, each of which is organized around one epistemic key concept, with all of them combining into a cluster constituting the theoretical backbone of the book. Such are the concepts of: Abstraction and Representation; Scientific Revolutions; Economy of Knowledge; Epistemic Evolution; among others—each is clarified and illuminated through one or more historical case studies, yet each of them functions as an analytical tool for presenting the historical cases.

The spectrum and richness of this structure is simply breathtaking. Similar attempts to tell the history of world knowledge often suffer from simplification, flatness, or a superficial view of complex phenomena: this book represents the opposite. It provides the reader with a general view of the evolution of human knowledge from its roots, without giving up on detailed analyses and complex explanations of particular phenomena. Yet, it achieves an unusual narrative clarity that attracts the reader to not only start this long journey but also to continue through it until the very end.

The greatest achievement of this book lies in its great synthetic power. As claimed by the author in the preface to the book, in the last decades history of science has suffered from the collapse of great narratives about the growth of knowledge, the progress of science, or even about paradigm shifts, all adapted to the story of science in the Western world. The result was a fragmentation that has impoverished the capacity to tell a story about the role of knowledge in human life and culture, and a lack of vision about its capacity to solve problems and give meaning to humanity at large. The *Evolution of Knowledge* is an attempt to fill in that absence. It offers a story of evolution, namely of a continuous development depending on conditions of variation and selection, thus emphasizing the contingency of knowledge development towards possible—but not inevitable—progress or destruction.

Acknowledgements: Thanks go to Stefano Ruffo and Andrea Gambassi for the initiative to hold this conference and their great generosity in hosting this event at SISSA. Thanks also to Luis Melendrez Zehfuss for his support in copyediting the papers and to Lindy Divarci for managing this publication. All participants are indebted to Lina Schwab and her colleagues in Trieste, Claudia Parma and Mila Bottegal, for the wonderful organization of this event.

Part I. The Role of Practical Knowledge

The Tangled Roots of the New Mathematics of the 17th Century Scientific Revolution

Jens Høyrup[1]

In the *Discours préliminaire* to the *Encyclopédie* (*Encyclopédie* I: xxvi), Jean le Rond d'Alembert writes as follows:[2]

> Finally Newton arrived, for whom Huygens had prepared the road, giving to philosophy a shape that it seems she will keep. This great genius saw it was time to ban from physics conjectures and vague hypotheses, or at least not take them for more than they were worth, and that this science should be submitted to nothing but the experiences of geometry.

We notice how an echo of Nicolas Boileau's *Enfin Malherbe vint*, "finally Malherbe arrived" (ed. Chéron 1861, 93) gives extra weight to the praise of Newton as the definitive culmination of the scientific revolution (d'Alembert knew his *belles-lettres* just as well as his mathematics). A naive reading might further find a specific reference to Newton's use of *geometric* proofs in the *Principia*, in contrast to the application of infinitesimal calculus, but the defining contrast with conjectures and vague hypotheses shows that this is overly naive. Infinitesimal analysis was perfectly at home in the *Classe de Géométrie* of the Académie des sciences, to which d'Alembert belonged, as it was in d'Alembert's own writings. D'Alembert does to Newton what Sainte-Beuve (Chéron 1861: ix) claims Boileau has done to Malherbe's prescription: *il l'étend et l'approprie à son siècle*, "he extends him and takes possession of him for his own century." And in d'Alembert's 18th century that meant that the permanent shape of natural philosophy brought about by Newton was

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[1] Section for Philosophy and Science Studies, Roskilde University, Denmark.

[2] My translation, as all translations in the following. The original runs:

> Newton, à qui la route avoit été préparé par Huyghens, parut enfin, & donna à la Philosophie une forme qu'elle semble devoir conserver. Ce grand génie vit qu'il étoit tems de bannir de la Physique les conjectures & les hypothèses vagues, ou du moins de ne les donner que pour ce qu'elles valoient, & que cette Science devoit être uniquement soûmise aux expériences de la Géométrie.

now expressed in the new infinitesimal analysis, whereas the arguments of Galileo, Kepler, and Huygens had indeed been just as geometric as those of Newton.[3]

That the miscellaneous infinitesimal considerations we find in the 17th century gave rise to *infinitesimal analysis* was conditioned by the preceding creation of the new *algebraic analysis.* This, as well as the preceding consideration concerning d'Alembert, I shall leave as a postulate, easily verified however by a glance at Leibniz's mathematical texts. My topic, less worked out by others, is the complex process from which emerged the first,+ algebraic level of the new analysis of the 17th century.

Complex process? Isn't it quite simple? Al-Khwārizmī created algebra in the 820s; Abū Kāmil refined it; Fibonacci reordered it in Latin in 1202 (or 1228); it survived with little change and no progress for three centuries—and then Cardano (in some nasty interaction with Tartaglia) brought it to a new level, inspiring Viète and Descartes. That is the standard story.[4]

Standard stories are not necessarily wrong, but this one is.

There is not much reason to discuss the Arabic developments in any detail, since algebra was received in Catholic-Christian Europe only through three channels—efficiently through two only.

One was Gerard of Cremona's translation of al-Khwārizmī's algebra, made in Toledo around 1170. It did not circulate much—there was not really space for it within the world of university learning, but it did circulate modestly; 15 manuscripts survive (Hughes 1986: 221).

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[3] *More* geometric, we may say: according to Guicciardini (2016), it seems that Newton was right when claiming in later times to have possessed the fluxion technique already when producing the *Principia* in 1687.

[4] The second half of the story underlies this passage from (Karpinski 1929):

> From the mathematical point of view this treatise by Jacob of Florence, like the similar arithmetic of Calandri, marks little advance on the arithmetic and algebra of Leonard of Pisa. The work indicates the type of problems which continued current in Italy during the thirteenth to the fifteenth and even sixteenth centuries, stimulating abler students of mathematics than this Jacob to researches which bore fruit in the sixteenth century in the achievements of Scipione del Ferro, Ferrari, Tartaglia, Cardan and Bombelli.

If even Louis Karpinski knew no better, who in his time would have known better?

Another translation was prepared by Robert of Chester (ed. Hughes 1989); we know it from three 15th-century manuscripts produced in southern Germany; its particular terminology has left no traces whatsoever. The algebraic problems contained in the *Liber mahameleth* (ed. Sesiano 2014) and the few pages introducing the topic in the second part of the *Liber algorismi* (ed. Burnett, Zhao, and Lampe 2007, 163–65) were equally ineffectual.[5]

In 1202, Fibonacci wrote a first version of the *Liber abbaci*, in the last chapter of which an algebra is contained. A revised version was made in the late 1220s. The best guess is that the basic introduction was produced independently by Fibonacci under inspiration from Gerard's translation;[6] the illustrating problems were borrowed from many sources, some of them indirectly from Abū Kāmil (Høyrup 2022b). A large cluster of problems borrowed together, probably inserted in the revised edition, was based on a Latin translation of an Arabic treatise drawing upon Abū Kāmil while revising his approach; neither the Latin translation nor the Arabic original are known to have survived. Fibonacci's algebra, on its part, did survive as part of the larger treatise; its impact, however, was negligible—Jean de Murs drew on it as one of several sources for the algebraic books of his (scarcely influential) *Quadripartitum numerorum* in the mid-14th century (ed. L'Huillier 1990), and it was partially copied by Benedetto da Firenze and a few others in the mid-15th century, yet without affecting their own algebraic work.

The essential reception was effectuated by a handful of abbacus masters in the early 14th century.

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[5] Probably during the second quarter of the 13th century, Guglielmo de Lunis made another translation of al-Khwārizmī's algebra into either Latin or (rather) the vernacular (Høyrup 2022a, 313–317). Guglielmo *may* have drawn upon Gerard's translation, but certainly also had direct acquaintance with Arabic material. Longer or shorter stretches from its beginning were quoted by Benedetto and two more around 1460, but it has left no other traces. A redaction of al-Khwārizmī's work known as *Liber restauracionis* (Moyon 2019) is probably also to be dated to the 13th or 14th century (possibly, as pointed out by Moyon, a Latin translation of an Arabic redaction). It awoke enough interest to be translated into the vernacular around 1400—but since its particular notation did not spread further, the interest seems to have subsided soon afterwards.

[6] This seems to follow from Nobuo Miura's analysis (1981).

The immediate source area (a single source can be excluded) must have been Romance-speaking[7] and located somewhere in the Ibero-Provençal area. Compared with Abū Kāmil, the level is modest.

Geometric proofs are absent. Rules are given for the six basic first- and second-degree "cases" (equation types) and for those cases of the third and fourth degree that can be reduced to these or solved by means of a root extraction. Soon, however, (false) rules were also offered for such higher-degree cases that cannot be resolved in these ways. At the conditions of the time, they were not easily controlled: the proposed solutions all contain radicals, and radicals were never approximated (and higher-degree equations were never used for any practical purpose). Such false rules might therefore be useful in competitions for positions in municipal abbacus schools and for students.

Very soon we also see incipient use of abbreviations for the algebraic powers, used in particular in formal calculations—for example[8] the reduction:

$$\frac{360}{1\rho} + \frac{360}{1\rho\ m\hat{e}\ 6} = \frac{1080\ m\hat{e}\ 2160}{2\ censi\ m\hat{e}\ 6\rho}$$

(ρ stands for the thing; *mê* for *meno*, "less"; + and = are modern; the fraction lines are in the original). Since very similar notations had been created in the Maghreb in the outgoing 12th century (too late for Fibonacci to know them), it seems almost certain that the ideas were borrowed.

However that may be, some abbacus writers used abbreviations in a way that effectively barred their use for symbolic calculations; nobody used them systematically (Høyrup 2010; 2015). Nor was there any agreement about what the abbreviations should be. This was still the situation in 1494, when Luca Pacioli (1494: 67^{v}) summed up the situation in the words *tot capita tot sensus*, "as many heads, so many opinions."

Beyond using the abbreviations in formal calculations (which, by the way, could also be performed with the names written in full, as Biagio does with *censi*), in the outgoing 14th

[7] There is not a single Arabic loanword in the texts; for the second power of the unknown *cosa*, "thing," they use *censo*, rendering the Latin translation *census* of Arabic *māl* that had been the Toledo standard in the later 12th century.

[8] Siena, Biblioteca Comunale L.IV.21, fol. 404^{v}, Benedetto da Firenze rendering what Biagio "il vecchio" had written before ca. 1340.

century we also encounter schemes (emulating those used in the arithmetic of Hindu-Arabic numerals) for the addition and multiplication of polynomials—even they agreeing with Maghreb models. From the later 14th century onward, we know some scattered instances of the use of several unknowns (essential in Viète's and Descartes's algebras), and from Pacioli we know that more must have existed; even this, however, was never systematized—Pacioli just informs us so that we may know, so he says.

The source area that inspired the beginning of abbacus algebra must have understood the nature of the sequence of algebraic powers as a continued proportion—that is shown by the rules for reducible higher-degree cases. This is not strange; this insight had been well described by al-Karajī and had spread from him to Arabic algebra. Also other aspects of early abbacus algebra make one think of a "diluted al-Karajī" (Høyrup 2011). It is far from certain, however, that the first generation of abbacus-algebra writers understood what they were borrowing (if they had, the acceptance of the false rules is hard to explain[9]). In 1344, however, Dardi of Pisa showed in his formulation of rules for a huge number of cases involving roots of powers that he understood to the full, at least practically[10]—but he never explains the principles involved; that had to wait for Antonio de' Mazzinghi's work half a century later.[11]

Although the system shows its first cracks, Antonio's naming of the higher powers is generally multiplicative—his "cube of cube" is the sixth, not the ninth power. In other words, the powers *thing*, *censo,* and *cube* are entities, not functions.[12] That was to change over the next century, but once again not systematically and not in all writers. Pacioli has

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[9] For instance, understanding would reveal that the problems:

$\alpha C = \beta t+N$ and $\alpha K = \beta t+N$

(t being the unknown *thing*, C its second and K its third power) can only be solved according to the same rule if $\alpha C = \alpha K$, that is,

– if $\alpha = 0$ (which would be meaningless at the time and is in any case excluded since β, t, and N are all presupposed to be positive)
– or if $C = K$, that is, if $t = 0$ (still excluded by the number concept of the time) or $t = 1$; that is, all in all, if $\alpha = \beta+N$.

[10] (Van Egmond 1983) contains an overview.

[11] Antonio's explicit understanding may have links to his production of the first tables of composite interest.

[12] Some writers also give pseudo-multiplicative names to higher roots, speaking, e.g., of the fifth root as "root of cube root"; others are aware that root-taking is *an operation* and roots therefore by necessity functions (evidently not using this much later term). Antonio introduces the term *radice relata* for the fifth root, and in parallel (that is the crack just mentioned) speaks about the fifth power as the *cubo relato*. Otherwise, his naming for powers remains multiplicative.

come to see the powers as functions, which evidently entails the question of how to name the fifth and higher prime powers. An alternative system explained by Pacioli therefore identifies the powers with their number in the sequence—*number* being the first, which means that Pacioli's number-names are not exponents, and that the easy rule for multiplying by adding exponents does not apply.

Some writers, understanding that the false rules *were* false, tried to find better ways. One method consisted in transforming homogeneous equations—for instance, taking in a problem about a capital growing over 3 years from £100 to £200, not the value after one year but the interest per month as unknown; in mathematical principle this is a linear transformation, and the one who did it must have had a very good understanding of polynomial algebra (*very good* indeed since the transformation had to be done without symbols).[13] Whether the inventor understood that the resulting rule was not generally valid is not clear, but Dardi (from whom we know about these rules) knew.

Another way to advance consisted in the invention of specious "roots"—the "cube root of 44 with added 5" being 4 because $4^3 = 44+5\cdot4$. In itself this is just a name for the solution to the case "cube equal to roots and number," but at least one treatise from around 1400[14] shows that it can also be used to solve the case "cube and *censi* equal to number"—even this is achieved by a linear substitution and thus asks for mastery of polynomial algebra.

The texts do not explain the methods—only the non-reduced coefficients reveal to us how the special rules and the case-transformations were obtained. In consequence these ingenious methods apparently did not spread in the environment, and Cardano had to reinvent. There was absolutely no impact on the German *coß*, the next phase in the story.

The *coß* descended from abbacus algebra but in an intricate and protracted process. Beginning around 1450, a number of German mathematical writers—Friedrich Amann, Johannes Regiomontanus, and several anonyms (Amann and Regiomontanus at least with background in university culture and astronomy)—took interest in algebra,

[13] Analysis in (Høyrup 2022a, 224–226).
[14] Florence, BNC, fondo princ. II.V.152. (Franci & Pancanti 1988) contains an edition of the extensive algebra section of the manuscript. Analysis of this aspect of the algebra in (Høyrup 2022a: 246*f*).

apparently as a new mathematical discipline they wanted to learn about. The first decades of the reception mirror the messy state of abbacus algebra. The terminology not only reflects inspiration from northern Italy (whose "thing" was *cossa*) as well as Florence (where it was *cosa*); both Amann and Regiomontanus would use several different sets of abbreviations, evidently corresponding to their source of the moment; some of the anonyms are more messy. There is no reason to be scandalized: what these pioneers drew on was equally confused (none of them seem to have had the good luck to stumble upon a high-level abbacus algebra); before the Germans could produce coherence of their own, they had to make sense of whatever they had been able to find.

Eclecticism did not last many decades, however. In 1489 Johannes Widmann, university educated, published the first large-scale *Rechenbuch*. It contains no algebra, but already in 1486 Widmann had held algebra lectures at Leipzig university. We do not have any text showing what he taught except for the announcement referring to "the 24 rules of algebra, and that which they presuppose"—the latter specified to include "algorism for fractions, ratios and surds." He can be supposed to have built on a manuscript in his possession that still survives—a manuscript which taken as a whole is quite eclectic. But we may further suppose that his lectures were in the style of his book, and thus systematic (as also suggested by the announcement). Widmann probably used the standard notation for powers that we know from manuscripts dated from the following years (+ and – were in any case used in his *Rechenbuch*).

University lectures were held in Latin. A Latin algebra from no later than 1504 was almost certainly written by Andreas Alexander (Folkerts 1996), one of the first specialized mathematics lecturers in Leipzig. A related German text about the topic (the *Initius algebras*[15]) may also be from his hand—if not, somebody else profited from Alexander's work (as actually suggested by some of the formulations).

Both of these works reduce the number of rules to 8, taking advantage of reducibility through division. Neither circulated much; the latter at least was used by Adam Ries, who produced the oldest surviving manuscript copy and probably used both for his *Coss;*[16]

[15] (Ed. Curtze 1902, 435–609).
[16] Thus spelled (in agreement with the title page) so as to distinguish it from the general discipline *coß*.

even that work, however, did not circulate, so the only lasting influence of Alexander's work was the inspiration Christoph Rudolff received from it. We may observe, however, that Alexander may have learned from the higher level of Italian abbacus algebra, which he seems to have digested though with some approximation.[17]

Rudolff, beyond the "8 rules," took over and established the standard notation for the algebraic powers definitively; these notations were still standard when a disgusted Descartes had to learn them in the Jesuit school. Rudolff also borrowed the schemes for polynomial arithmetic familiar in Italy at least since 1400.

However, before saying more about Rudolff's discipline-defining work we should notice Heinrich Schreyber's *Ayn new kunstlich Buech, welches gar gewiß und behend lernet nach der gemainen regel Detre, welschen Practic, regel falsi unn etlichen regeln Cosse* from 1518 (published under Schreyber's Latinized name Grammateus in 1521). This is a general *Rechenbuch*, but written before norms crystallized as to what such a book should contain; beyond an extensive algebra, it also contains Boethian music theory and bookkeeping, otherwise strangers to the *Rechenbücher*. As Alexander's algebra, that of Schreyber describes the arithmetic of polynomials by means of schemes, and offers a restricted set of rules (seven only). Noteworthy, however, is the notation for the algebraic powers: instead of the already established standard symbols, Schreyber uses abbreviated ordinal numbers, corresponding to exponents: *N*, *pri*, *2a*, *3a*, *4a*, etc. It is obvious from the texts that both Alexander and Schreyber came from a university background; the latter studied at Vienne University from 1507 onward and taught there from 1517 to 1521 (Kaunzner 1970, 229; Vogel 1975, 589).

Rudolff was taught by Schreyber, as he tells (1525: 204); whether he frequented the university directly is unclear, we know almost nothing about his biography. There is no doubt, however, that he knew basic university mathematics—algorism as well as the Boethian naming of ratios, both of which he extends: the former (as "algorism of the *coß*) he uses as a framework also for the arithmetic of monomials and polynomials; the latter he refers to also when considering ratios between broken numbers.

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[17] That is, if we disregard the "historical" beginning of the *Initius algebras*, which is charmingly hilarious and quite different from anything Italian I know.

Beyond such extensions, Rudolff creates no new mathematical knowledge, but he provides order and structure. Beyond what was already said, he teaches the use of a second unknown (actually more than two unknowns, but used in a way where never more than two are in play at a time, so two names suffice—Pacioli [1494, 191^v] had done the same); while predecessors had simply done so without taking much notice, Rudolff states that this technique is "a completion of the *coß*, indeed in truth a completion without which it would not be worth much more than a trifle [*pfifferling*]."

Rudolff's book became and remained the defining basis for the *coß*. Schreyber's book was reprinted several times, but nobody took over his symbolism; in the rudimentary presentation of algebra in the *Deutsche Arithmetica* from 1545 Stifel suggested to use the names *sum*, *sum.sum* and *sum.sum.sum* instead of *radix*, *census,* and *cubus*, with no more effect. In 1553, when Rudolff's book, long of print and not to be found "even at triple or quadruple price" (an indication of its status), Stifel produced an "improved much augmented" new edition of the cherished work, from which this quotation is taken (fol. A 2^v).

Before that (namely in [1544]), Stifel had published the *Arithmetica integra*. Stifel there acknowledges the importance of Rudolff's *Coss*, but he goes far beyond it—for instance by dealing in depth with *Elements* X transformed into arithmetical theory. He further invents a letter-based notation for many variables that allows higher powers and products, without using it himself for anything spectacular.[18]

In his expositions of algebra from 1550 and 1551—the former printed in Basel, the latter in Paris—Scheubel does not advance on Rudolff and Stifel within algebra proper. His integration of algebra into *Elements* I–VI in the former is restricted to the insertion of numerical examples, going beyond advanced current practical geometries (and Heron's *Metrica*, unknown at the time) only by including radicals and binomials in the range of accepted numbers. The separately republished algebraic introduction from 1551 (reprinted in Paris in 1552, evidence that the book sold well) exhibits Humanist aspirations, firstly (fol. 2^r) by endorsing Regiomontanus's ascription of algebra to

[18] It had little immediate impact but may have inspired Jean Borrel (Buteo 1559), who like Stifel makes use of capital letters. Borrel's notation is borrowed with due reference by Guillaume Gosselin (1577, 80^r), and Gosselin *may* again have provided inspiration for Viète's letter symbolism.

Diophantos, secondly by including a number of Greek arithmetical diagrams provided with Latin translations and algebraic solutions.

Jacques Peletier tells us in *L'Algebre* from 1554 what was accessible in France. Peletier knows Pacioli's *Summa* and Cardano's *Ars magna*, and from Cardano he knows about Fibonacci. He also knows Stifel and Scheubel, and he has heard about Rudolff, Ries, and Nuñez but not seen their books (at the time, that of Nuñez was indeed an unpublished manuscript). Peletier himself takes Stifel as his basis, using also his symbolism and, even as classicizing condiments (fols 24^r, 76^r), an arithmetical riddle about Alexander the Great and the philosopher Calisthenes and the story about Archimedes and Hieron's crown—the latter going back to Rudolff.[19]

To judge from the technical terminology, Viète's primary reference for algebra was Gosselin, who knows Stifel, Cardano, and Peletier.[20] Descartes learned algebra in his Jesuit school, La Flèche, from Christophorus Clavius's *Algebra*. Even this is in the general style of Rudolff and Stifel. Clavius, a great pedagogue, makes his own formulations, but for instance his way to deal with negative numbers and negative powers (Clavius 1608, 28–29) leaves no doubt that he had the *Arithmetica integra* (here [Stifel 1544, 249^{r-v}]) on his desk while writing.

So, what Viète (1591, 2^v) experienced as "an old art defiled and befouled by barbarians" and what Descartes (1637, 19) described as "a confused and obscure art that puts the mind in difficulty instead of a science that cultivates it" was not Arabic algebra but *abbacus algebra transformed and put into order* as *coß*, and to some extent as unfolded by Cardano.

[19] (Stifel 1544, 234^r, 267^r) and (Rudolff 1525, 84^r). Peletier knows that the story comes from Vitruvius's *De architectura* IX, while Rudolff, even more precise, states that "I have read in Vitruvius, in the third chapter of the ninth book of his *Architecture*." Stifel has nothing. It appears that Peletier has looked up the details in Vitruvius's text.

[20] Thus, (Gosselin 1577, 47^v). Gosselin has also read (Nuñez 1567; thus fol. 67^r) but that is not a main inspiration. Gosselin's terminology for several unknowns comes from Borrel, as said in note 18; for a single unknown it *might* have been taken from (Ramus 1560), not least the term *latus* for the first power (for which Borrel used a Florentine ρ, while his second power is ♦). In principle, Viète's *latus* might thus come from Gosselin as well as Petrus Ramus, but there is so little substance in Ramus's primer (and only one unknown) that Viète could have learned nothing even if he should happen to have known this anonymous piece.

This putting into order was effectuated by writers like Alexander, Schreyber, Rudolff, Stifel, and Cardano, not university teachers all of them but all strongly influenced by the Boethian-Euclidean norms of the university tradition. What Viète, Descartes, and their ilk knew as interesting mathematics was already different, however—we might speak of it as "Humanist mathematics," rather perhaps as "post-Humanist."

Humanism had always been centered on the "civically useful" as understood in courtly culture. Around 1500 it had become clear that Latin letters might perhaps still be "a weapon more to be feared than a troop of horses," as claimed by the Chancellor of Florence in 1406 (Gragg 1927, x)—but Latin letters were definitely no match for the French artillery, nor did they help much when the Portuguese and Spanish courts engaged in transoceanic travel. It was also during the years around 1500 that Pacioli, putting into writing a century's experience of architects and military and hydraulic engineers, reinterpreted Aristotle's opinion that mathematics is the most certain of sciences as a claim that all the other sciences derive from mathematics.[21]

In consequence of such experience, some Humanists or court mathematicians with a Humanist bent engaged in publishing and translating the Greek mathematical classics. Bartolomeo Zamberti's problematic translation of the *Elements* was published in 1505, and Grynaeus's complete edition of the Greek Euclid with Proclos's commentary in 1533. The *editio princeps* of Pappos's *Collection* appeared in Basel in 1538—Commandino's Latin translation in 1588, after having circulated in manuscript. The *editio princeps* of Archimedes was printed in 1544; Memmo's Latin edition of books I–IV of Apollonios's *Conics* appeared in 1537 (that of Commandino in 1566); Xylander's Latin translation of Diophantos was published in 1575 (the Greek *editio princeps* only in 1621). When Viète reached mathematical maturity, a rather full range of the Greek mathematical classics was thus within his reach.

However, this acquisition of new material was relevant for the transformation of algebra only because the mathematical undertaking itself had changed. The medieval university taught theory in lectures, and disputations and their written emulation in *quaestiones*

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[21] (Pacioli 1509, 2^r) in the dedicatory letter to Ludovico Sforza—written in 1498, before Sforza lost his court and Pacioli his position as a court mathematician precisely to the French artillery.

invited metamathematical reflection about the status of the object and objects of the theory. That is also what we see in Scheubel's volumes: the algebra does not change the geometric theory, nor does the Humanist orientation expressed in the inclusion of Greek epigrams affect the algebra.

The metamorphosis of the mathematical undertaking is epitomized in the famous concluding line of Viète's *Isagoge* (1591, 9^r): *nullum non problema solvere*, "to leave no problem unsolved." The mathematics of Viète, his antagonist Adriaan van Roomen, and later Descartes, Fermat, etc. was centered on *problems* within an agonistic culture which made van Roomen the *antagonist* of Viète.[22]

The Italian abbacus culture, too, had been agonistic—the abbacus masters challenged each other with higher-degree algebraic problems and difficult versions of "the purchase of a horse," "the finding of a purse," and other recreational classics. This led to the invention of specious roots and made Benedetto da Firenze create a notation for first-degree algebra with up to five unknowns. But nobody appears ever to have noticed Benedetto's innovation—one reason at least being the exiguous number of practitioners who were at a level where they might have understood and appreciated it. The culture of the German *Rechenmeister* probably had a higher density; since it was a print culture, at least it had much more efficient communication. It was not intellectually agonistic, however; books were competing on the book market and were therefore almost invariably marketed on their title page as *new*. In this environment, the specious roots had no social role, and they never left their abbacus home.

The culture of Viète and his kind was agonistic once again; but now its problems were those inspired by the Greek geometers. *That* was what inspired Viète and Descartes to create their very different versions of the new algebra, which turned out to be the hoped-for tool for problem solving within the new mathematics, just as abbacus and *Rechenmeister* algebra had been an efficient tool for solving traditional problems.

[22] The details about the confrontation between van Roomen and Viète add an extra twist (Busard 1975, 533; 1976, 22). It was brought about by an ambassador from the Netherlands who boasted to Henri IV that France did not possess a geometer able to solve a problem suggested by Adriaan van Roomen. That is, mathematical prowess was now taking over the symbolic power of Latin letters. I shall not pursue the question *why* this agonistic culture arose; still, this episode illustrates that it was not just a mathematicians' fashion.

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Inheritance and Innovation: Casting Technology and Craft Production of Bronzes in the Shang Dynasty (16–11 c. BC), China

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Abstract

Piece-mold casting technology in China initially appeared in the Erlitou period (18–16th c. BC) and reached its first climax in the late Shang dynasty (13–11 c. BC), but the details of its evolutionary path have not yet been clarified. The formation of piece-mold casting technology tradition in ancient China, which differs greatly from bronze-making technologies such as the forging method and the lost-wax process in West Asia and Central Asia, is closely related to artisans' technological choices. Since bronze production is a complex process, research on casting technology, manufacturing sequence, and the organization of production would contribute to a better understanding of technology, economy, labor organization, social structures, and cultural interactions in ancient societies of China. Several foundry sites with numerous remains and bronze wares of the Shang dynasty were found in the Central Plains of China. Casting technology and the craft production of bronzes in the Central Plains during the Shang dynasty (16–11 c. BC) were studied based on the observation of these excavations and led to abundant further research, especially the analysis of the excavation of foundry sites with remains and bronze vessels. Discussions were also pursued on the inheritance and evolution of casting technology, the distribution of foundry workshops, and the extent of large-scale manufacturing, which exerted profound and lasting influences on the subsequent development of metal technologies.

Keywords: Casting Technology, Craft Production, Bronzes, Shang Dynasty, China

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Introduction

The most prominent characteristic of ancient Chinese bronze wares consists of the utilization of piece-mold casting technology to manufacture ritual bronzes, which is vastly different from the traditional use of the forging method and lost-wax process for weapons, tools, and decorative items in West Asia, Europe, and other regions. This understanding has only come into shape after a long course of research. Due to the complex forms and exquisite decorations of ancient Chinese bronze wares, many scholars tended to hold that their production employed the lost-wax process (*cire perdue*) similar to that used in Greece and Rome (Yetts 1929; Simpson 1948; Maryon & Plenderleith 1954; Aitchison 1960, 161). However, since the excavation of the Yinxu site in Anyang, Henan province, in 1929, a large quantity of bronze wares and foundry remains of the late Shang dynasty have been uncovered, leading scholars to believe that the bronze wares in Anyang were locally produced. Liu Yuxia (1933) pioneered in the preliminary research on the production of bronze wares in Anyang based on the archaeological findings from Yinxu. Karlbeck (1935) explored the materials and manufacturing technologies of the clay molds collected from Anyang and deemed them as the molds formerly used to make bronze wares. Shi Zhangru (1947; 1955) described the materials and structures of the molds, models, and other foundry remains and discussed the manufacturing procedure. The five monographs coauthored by Li Ji and Wan Jiabao (1964; 1966; 1968; 1970; 1972) systematically discussed the casting processes of bronze wares from Yinxu, connecting the techniques' evolution with the form changes of the bronzes. They also established the fundamental methods for the research on ancient Chinese bronze-making technologies. Gettens (1969) conducted analyses on the production technologies of the Shang and Zhou bronzes held in the Freer Art Gallery using various scientific analysis methods. Barnard's (Barnard 1961; Barnard and Satō 1975) research was concerned with the multiple aspects of the production technologies of Chinese bronzes and the origin of Chinese bronze-casting technologies. Guo Baojun (1981) delineated the Chinese Bronze Age into different stages; Bagley (1987; 1999) expounded on the relationship between the forms, decorations, and technology behind bronze wares; Tan Derui (1999) restored the materials and manufacturing processes of clay molds for the Shang and Zhou bronze wares; Su Rongyu et al. (1995), Hua Jueming (1999), and Han Rubin and Ke Jun (2007) provided systematic discussions on ancient

Chinese metal technologies. These endeavors jointly formed the understanding of the basic framework of ancient Chinese bronze-casting technology.

Piece-mold casting technology for bronzes was initially established during the Erlitou period (18–16 c. BC) and developed steadily throughout the early and middle Shang dynasty, reaching its peak in the late Shang dynasty (13–11 c. BC). The discovery of groups of bronze wares and foundry sites, as well as the analysis and research on bronze wares and foundry remains, rendered it possible to reconstruct the specific evolution of this technology.

The Initial Establishment of Piece-Mold Casting Technology

The Erlitou period witnessed the beginning of the large-scale production of bronze wares and heralded the function of valuable bronzes as ritual vessels. More than 250 bronze wares and some foundry remains were excavated from the Erlitou site in the city of Yanshi, Henan province. The alloy materials were of great complexity, involving copper with alloys such as copper-arsenic, copper-tin, copper-lead, copper-tin-arsenic, and copper-tin-lead, etc. The general trend of the decreasing use of copper and the increasing use of bronze indicates the progress of early metal technologies. In addition, a considerable number of furnace fragments, clay molds, and slags were unearthed from the Erlitou foundry site . There were also workshops for casting, kilns for firing clay molds, and constructions for preheating clay molds, suggesting the specialization of technologies and facilities for bronze casting (IA 1999; 2021).

During the Erlitou period, bronze wares were generally characterized by their thinness and simple decorations (mostly single-layer patterns). Wine vessels stood out among ritual vessels, which were exemplified by *jue* (an ancient Chinese vessel used to serve warm wine, Figure 1), *jia* (a cauldron for warming wine), and *he* (a wine vessel shaped like a tea pot with three legs), with the *jue* and *he* vessels often in collective use. There were also cooking and dining vessels such as *ding* (a cauldron for cooking and storing meat) and *li* (a boiling vessel); weapons already accounted for a significant proportion (Liu Xu 2021). From the Erlitou site were also excavated inlaid turquoise circular artifacts and animal face decoration plaques which exhibited masterful inlay technology (Figure 2).

Figure 1: The bronze *jue* vessel excavated at the Erlitou site (from *Anthology of Chinese Bronzes: Xia and Shang* 1, plate 7).

Figure 2: The turquoise inlaid bronze plaque unearthed at the Erlitou site (from *Anthology of Chinese Bronzes: Xia and Shang* 1, plate 20).

Despite the fact that Erlitou bronze wares were of relative simplicity, they generally borrowed their form from contemporary or slightly earlier clay and stone wares and still retained primitiveness to some extent; the basic framework of Chinese bronzes had already been put in place during this period, establishing the independent tradition of Chinese bronze wares: special forms and piece-mold casting technology (Su Rongyu et al. 1995). Bronze-making technology in this period was already typical of clay-mold casting technology: clay molds were made of sieved loess, with the microstructure of high silt content, low clay content, and high porosity, which was completely disparate from the high clay-content materials used in pottery making (Liu Yu 2018). The molds' assembly was divided vertically and horizontally; the referencing line technique was used on the clay models to make molds easier and on the clay molds during the assembling of mold sections (Chang Huaiying 2017); the piece-mold design of some bronzes of simple forms (such as tools and weapons) had reached maturity; the patterns were designed horizontally on the molds, that is, the so-called "mold-based patterns" (Li Ji Jiabao 1972). Pattern making constitutes one of the key features of the piece-mold casting technology for Chinese bronzes, in which, before the formation of bronze wares, the patterns had been designed and completed in advance during the course of mold making. This technological process and concept are distinctly different from the technological tradition represented by Mesopotamia and ancient Egypt, where the patterns were directly applied to the surfaces of bronze wares (Zhang Changping 2011).

The Continuous Development of Piece-Mold Casting Technology

From the early Shang dynasty (16–14 c. BC) onwards, the production of bronze wares increased significantly. Bronzes were uncovered in such sites as Zhengzhou Shang City, Yanshi Shang City, Wangchenggang in Dengfeng county, Yuanqu Shang City, and Dongxiafeng, while the early Shang bronze group was found at the Panlongcheng site in Huangpi county, Hubei province, in the middle reaches of the Yangtze River. Bronze wares dating to the middle Shang dynasty (14–13 c. BC) were discovered in more places in the Shang culture region, and an increasing number of tombs containing bronzes was found in the Zhengzhou Shang City and the surrounding areas. The middle Shang bronzes were also seen in Hebei, Shandong, Shanxi, Shaanxi, Anhui, and other places, and the Panlongcheng site remained a site of immense significance to the concentrated excavation of bronzes (IA 2003).

Multiple bronze foundry sites have been discovered, from the Yellow River Basin to the Yangtze River Basin. For example, remains of clay molds, furnace walls, slags, charcoal, tools, grindstones, and bronze fragments, as well as remains related to bronze casting processes, were unearthed from sites such as the Nanguanwai and Zijingshan bronze foundry sites in the Zhengzhou Shang City (Henan Provincial Institute of Cultural Heritage and Archaeology 2001), the bronze foundry site in the Huanbei Shang City (Anyang Archaeological Team 2020), the bronze foundry site at Taijiasi village, Funan County, Anhui province (School of History, Wuhan University et al.2018), the Xiaozui bronze foundry site at Panlongcheng (School of History, Wuhan University et al. 2019; 2020), and the Guoyuanzui bronze foundry site at Lutaishan, Huangpi District of Wuhan city (Hu Gang et.al 2021).

In the early and middle Shang dynasty, bronze wares generally carried forward the style of the Erlitou culture, featuring thin walls and strip-shaped and single-layered patterns, and rarely seen inscriptions. The ritual bronzes remained dominated by wine vessels. The most common wine wares were *gu* (tall wine cup), *jue*, *jia*, and *he*, while the vessels for cooking and dining involved *ding*, *li*, *yan* (a boiler), and *gui* (a bowl-shaped vessel used to hold offerings of food). There were also other vessels such as *zun* (a tall cylindrical wine cup), *lei* (a wine vessel with a round body), *pou* (a small jar), *pan* (plate), and *you* (a covered pot). The three-legged vessels were mostly conical and hollow-legged, and in terms of weapons, in addition to the most common *ge* (dagger-axe) and *zu* (arrowhead), there emerged such novel weapons as *mao* (spear) and curled-head knives, with *yue* (an ancient type of battle-axe) being the most prominent (Liu Xu 2021).

In the early and middle Shang dynasty, bronze alloy technology remained in the phase of exploration and gradual standardization. The alloy types were progressing towards the copper-tin-lead ternary alloy with medium tin content or low tin content. The bronzes in the capital city were primarily the copper-tin binary alloy and the ternary alloy with high lead content. At the Panlongcheng site and its surrounding areas, various ternary alloys with high lead content constituted a large proportion (Zhang Ying 2021).

In the early and middle Shang dynasty, most of the three-legged vessels were equipped with hollow conical legs (Figure 3), and the molds were basically in the form of 3fz (Y),[2] which was composed of three vertically and equally divided piece-molds from the top to the bottom, and the lines above converged to a point at the bottom, forming a Y-shaped mold mark (Figure 4). The three-legged vessels with ears, such as *ding*, *li*, and *yan*, still adopted the form of "four-point arrangement." This form overlaid one ear on one vertex of the triangle, and placed the other ear on the middle line outside the bottom, thereby revealing four points of the two ears and two legs from an overhead view.

During this period, some auxiliary processes, such as clay spacers and metal spacers (chaplets), were used for the quality control of bronze vessels. Such clay spacers were small blocks set on the cores, and in the early and middle Shang dynasty, were set directly beneath the vertical mold mark and could also keep the core in proper alignment; the perforations would appear after pouring (Figure 5, Liu Yu 2014). Metal spacers were usually placed between the molds and the core in order to maintain the wall thickness and hold the molds in position (Figure 6, Liu Yu 2018).

On the surfaces of such bronze wares as *zun* and *lei* in southern China (for example, the dragon-tiger *zun* in Funan county, Anhui province, Figure 7), the mid-relief patterns applied the "convex-concave casting method." This method produced the core by scraping the model, and the inner wall of the raised patterns was correspondingly

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[2] The quantity of divisions of molds is indicated by the number, and the position of the horizontal division of molds is denoted by the initial letter of where the division was made ("f" represents "*fu* [abdomen]," "z" means "*zu* [foot]," and "j" stands for "*jing* [neck]"). The shape of the mold mark at the bottom is represented in parentheses by "Y."

indented so that the vessels could maintain the same wall thickness and solidify at the same time, thus preventing casting defects such as cracks (Zhang Changping 2003).

Figure 3: The beast-faced *ding* excavated from Panlongcheng, Huangpi, Hubei (from *Anthology of Chinese Bronzes: Xia and Shang 1*, plate. 32).

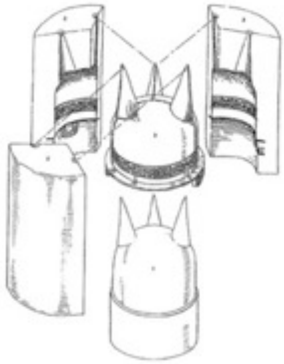

Figure 4: The mold assembly of the round *ding*, 3fz (Y) + 1.

Figure 5: The handled *you* (M1:9) from the Panlongcheng site, the square perforation on the ring-foot were formed by clay spacers.

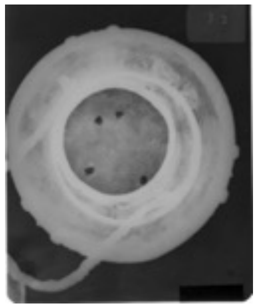

Figure 6: X-ray of the handled *you* (M1:9) from the Panlongcheng site; the black blocks were metal spacers.

Figure 7: For the dragon-tiger *zun* from Funan county, Anhui province, and its mid-relief patterns on the shoulder, the inner wall was correspondingly indented (from *Anthology of Chinese Bronzes: Xia and Shang 1*, plate 117).

Figure 8: The *gu* vessel with hollowed thunder patterns from the Panlongcheng site (from *Anthology of Chinese Bronzes: Xia and Shang 1*, plate 155).

During the early and middle Shang dynasty, most bronze wares had decorative patterns on their surfaces. The types and techniques of these patterns had attained significant advancement; there were thematic beast-faced patterns, *kui*-dragon patterns, whirl patterns, and triangular cloud patterns, which were often accompanied by complementary patterns such as circular and string patterns. There were two types of beast-faced patterns: thin-line patterns and broad-strip patterns. The former were made on the mold directly while the latter were probably made by copying the pattern from the model onto the mold. Considering that making patterns directly on molds was limited to expressing decorative layers, the pattern-making method shifted from being "mold-based" to "model-based." Traditions were established by some decorative technological principles for bronzes during this period, such as the correspondence between the grouping of patterns and the division of molds, and the symmetrical structure of pattern units, exerting far-reaching influences as late as the early Spring and Autumn period (Zhang Changping 2011). The practice of making special hollowed patterns at the Panlongcheng site should follow the method of deep incision on the model (Figure 8).

The setting of the pouring system places its evolution in the direction of the late Shang dynasty. Most three-legged vessels would install this system on the rim or on the mold mark of the abdomen, but in the late Shang dynasty, the system would have been transferred to the bottom of the legs, and the ring-foot wares usually set it on the ring-foot.

Much attention has been paid to the finishing work of bronzes during this period, such as the concealing of casting traces on the surfaces and the careful polishing of some mold mark. There also appeared flanges among the patterns, which might have originated from the overflow of pouring metal (Barnard 1961).

The prevalence of the repair method reveals that the artisans were not sufficiently skilled so there were frequent occurrences of mold deformation and inadequate pouring. Nonetheless, this led to the emergence of the new process of separate casting method. In fact, the earliest known instance of the separate casting method can be traced back to a *jiao* (a wine cup) of the Erlitou period (Figure 9). However, it was not until the early Shang dynasty that the method was in wider application. The separate casting method was adopted by the bronzes from the Panlongcheng site, including two *jia*s, a *gui*, a *zun*, and a handled *you*. Among them, the *gui* handle (PLZM1:5) was the earliest to employ the riveting cast-on method. It was performed to reserve three holes on the handle during the casting of the entire vessel, allowing the lock-on structure to be formed when the metal liquid flowed through the holes during the pouring of the *jia* handle (Figure 10). The separate casting method was applied to the square *ding*, round *ding*, handled *you*, and *yu* (water basin) with a central column unearthed at the storage pit in Zhengzhou, whose process varied from the one used for the bronzes at the Panlongcheng site (Li Jinghua 1999). The particularly special casting process used for the large square *ding* (Figure 11) was different from the separate casting method employed for the *gui* vessel (PLZM1:5) at the Panlongcheng site. Unlike the method that cast appendages onto the main body, the casting process of the square *ding* joined the cast with different parts (Li Jinghua 1999). In addition, the handles of the *you* vessel from Shimen in Hunan and from Taohuazhe, Shilou county in Shanxi province were connected using brazing technology, indicating that brazing technology was initially used for repairs (Su Rongyu et al. 2016; 2020).

Figure 9: *Jiao* excavated in Luoning county, Henan province, during the Erlitou period. The spout part was cast onto the abdomen (from *Anthology of Chinese Bronzes: Xia and Shang 1*, Plate 11).

Figure 10: A case of riveting cast-on method: the *gui* vessel (PLZM1:5) unearthed at the Panlongcheng site and the rivet-like bumps on its inner wall.

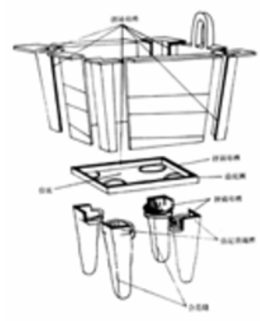

Figure 11: The square *ding* from the storage pit in Zhengzhou (upper H1:3) and its casting joint structure (left: from *The Bronzes in the Storage Pit in Zhengzhou*, plate 5; right: from *The Bronzes in the Storage Pit in Zhengzhou*, p.11, fig. 3).

During the middle Shang period, a special mold assembly process was invented. This process made separate and detachable mold sections for the patterns (Figure 12), and these individual molds with patterns were embedded into the posterior molds which were plate molds without patterns, thereby forming the complete casting area (Figure 13; Zhong Zhengquan 2021). At the bronze foundry site at Taijiasi, Funan county in Anhui province, there were also found posterior molds with reserved cavities, as well as patterned molds (embedded molds). On the bronzes of the middle and late Shang dynasty, the patterns were mostly distributed around the body in a strip shape, as seen on the bronze *ding* excavated at the Sanjiazhuang site, which has a strip of pattern on the abdomen. The bronze *jia* vessel was decorated with two strips of patterns, which adopted the technology of embedded molds consistent with the pattern's design. The use of embedded molds could produce finer patterned molds and reduce unnecessary waste and loss, but at the same time required the high accuracy of assembling mold sections.

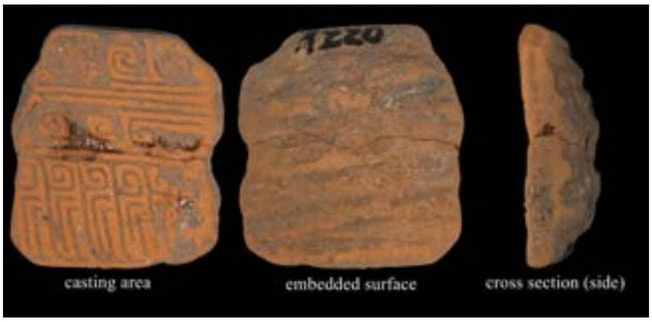

Figure 12: The embedded mold with arrays of flag patterns unearthed at the Huanbei Shang City bronze foundry site.

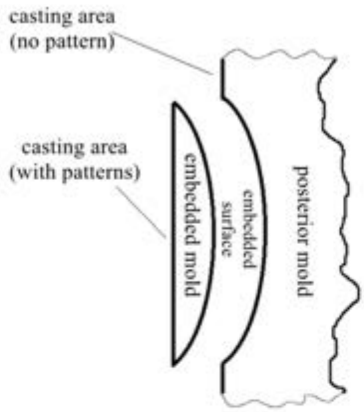

Figure 13: A diagram for the embedded mold process.

The Heyday of Piece-Mold Casting Technology

The late Shang dynasty marked the heyday of ancient Chinese bronze technology, which reached an unprecedented level in terms of the number, scale, type, and exquisiteness of bronze wares. So far, more than 2,000 containers have been uncovered among the ritual bronzes in Yinxu, as well as a large number of weapons, tools, and miscellaneous wares. The late Shang bronzes were characterized by thick walls and multi-layered patterns, some of which were almost the equivalent of low reliefs. These bronzes were usually decorated with patterns all over the body, with the most popular ones being beast-faced patterns, *kui*-dragon patterns, and animal patterns. Inscriptions of limited length could also be found on some bronze wares. Wine vessels continued to predominate among ritual vessels, and the square *ding* was the most prominent of all the vessels. Some new wares also appeared, such as complete sets of musical instruments and bronze carriages. Additionally, large-scale bronze foundries were disclosed in many places in Yinxu, such as Xiaomintun, North Miaopu, Northeast Xiaotun, and Xindian sites in Anyang, and quite a few bronze-foundry remains were found outside the scope of the Shang culture. Outside Yinxu, there was a plurality of Shang bronzes unearthed in such places as Laoniupo, Lingshi, Shilou of Shanxi, Subutun of Yidu, Qianzhangda, Dayangzhou of Xingan, and Sanxingdui of Guanghan. These ritual bronzes either bear a resemblance to or differ vastly from those in Yinxu in terms of their form. The early Western Zhou bronze wares inherited the forms and patterns of the late Shang bronzes. In addition to the Beiyao foundry site in Luoyang city, a vast quantity of bronze foundry sites in the western

Guanzhong region and the neighboring areas was found, such as Zhouyuan, Zhaojiatai, and Zhougongmiao in Shaanxi, as well as the Yaoheyuan site in Pengyang county, Ningxia province (Liu Xu 2021).

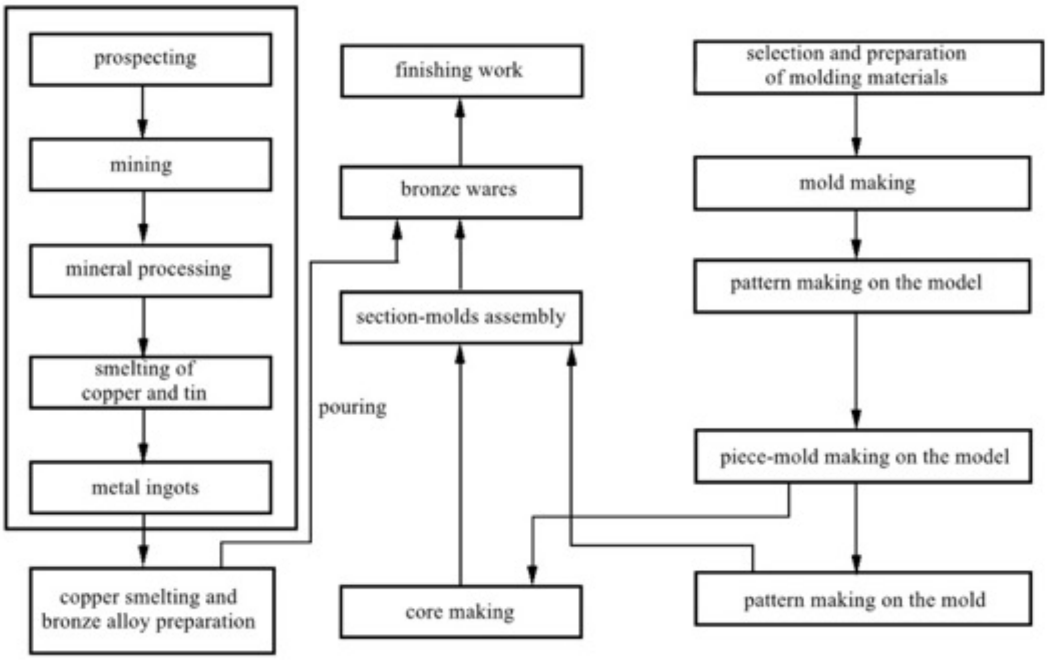

Figure 14: The casting processes of bronze wares.

Bronze ware production was marked by multi-craft or co-craft production (Costin 1998). The complex production process in the late Shang dynasty demonstrates that bronze casting during this period was not conducted by one individual industry as for pottery, bone, or jade wares, but rather a combination of two technological systems: mold making and metal casting, resulting from the mutual adaptation and adjustment of the two sides. This multi-craft characteristic was embodied throughout the production process. During the course of production, the metal-making step was inclusive of the prospecting and mining in mines, and the smelting of metals, which was carried out in the mining areas and smelting foundries beyond the capital cities spanning the Erlitou period to the late Shang and Western Zhou dynasties. In the bronze foundries within the scope of the capital cities, raw metal materials, which were smelted from other regions, were applied to metal smelting and alloy preparation. Another step was mold-making, which chose loess—the same raw material used for pottery wares—and required different technical processing. Specifically, many processes were necessary, from the selection and preparation of molding materials to the completion of the entire mold, as shown in Figure 14.

In the case of the late Shang ritual bronze wares, high-tin bronze and lead-tin bronze with high tin content were dominant in alloy preparation. In the early phases of the Yinxu period (phases 1 and 2), the alloy types were much simpler. For example, during phase 1, ternary alloy was more commonly used than binary alloy, with high-tin bronzes accounting for a low proportion. In phase 2, high-tin bronze and lead-tin bronze with high tin content were the mainstream types of alloys, and high-tin bronzes were the most strikingly used alloys in the high-level tombs. In the later phases of the Yinxu period (phases 3 and 4), the alloy types became much more abundant. From phase 3 onwards, high-tin bronzes were decreasing, while the lead content in alloys was rising, and the proportion of ternary alloys was also increasing substantially. This tendency was more evident in phase 4, with high-lead alloy types holding the dominant proportion.

In the late Shang dynasty, the molding process was becoming more standardized, and the mold-dividing technology was increasingly complicated. Besides the vertical division of molds, the horizontal division of molds was applied to a considerable number of bronze wares for the better production of complex patterns. The mold for the large round *lei* vessel, for instance, was divided into five layers horizontally (Figure 15) and six parts vertically, requiring at least thirty molds. At this point, the *ding* vessel shifted from the "four-point arrangement" (one ear matching one leg) to the "five-point arrangement" with two completely symmetrical ears. The advent of the blind core technology rendered the vessel bottom totally closed, indicating the transformation from hollow to solid legs with the blind core inside. There also occurred the 3fz (△) + 1 type using the triangular bottom mold (Figure 16). In the late phase of the Yinxu period, the "Y", "△"-shaped ridges or bands, or the crisscross lines on the bottom of vessels were used to suspend the foot core in the mold to permit molten bronze to flow between upper and lower cores and thus form the bottom of the vessel. The "convex-concave casting method" dating to the early and middle Shang dynasty remained in use during the early phase of the Yinxu period. The same method was applied to the *lei* vessel (M333), square *yi* sacrificial vessel (YM238), ox-shaped square *ding*, and deer-shaped square *ding* (M1004), which however had not been seen after phase 2 of the Yinxu period.

Figure 15: The round *lei* vessel and its 5-layer horizontal divisions of mold unearthed at Yinxu (left: from *Bronze Wares Newly Unearthed from Yinxu*, Plate 171).

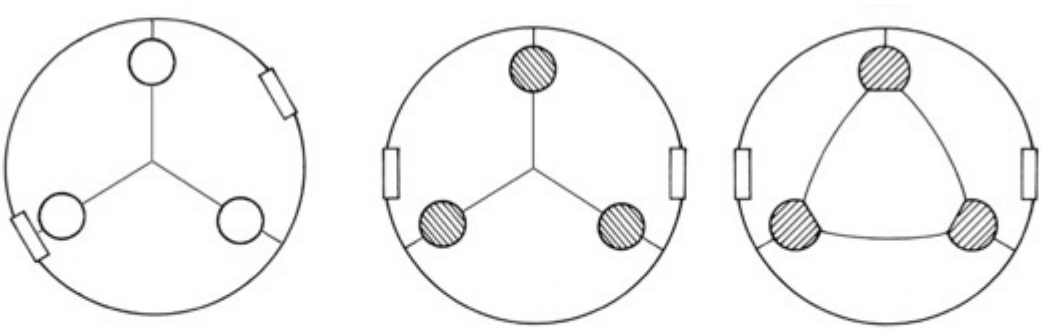

Figure 16: Changes in the form of the *ding* vessel (top view). A: the *ding* vessel with the four-point arrangement. B: the *ding* vessel with the five-point arrangement. C: 3fz (△) + 1, the *ding* vessel with the bottom mold.

In the late Shang dynasty, the proficient application of the separate casting method made it possible to produce a large number of exquisite and complex ritual vessels. Since phase 2 of the Yinxu period, a variety of separate casting methods—including pre-cast, cast-on, and multi-cast-joints—had been extensively employed, leading to larger vessels with more complex shapes. In order to connect the appendages to the main body of a vessel, the most commonly used separate casting method was "the tenon type" cast-on method, which was used to cast the sculptured appendages on the body (Figure 17). There were also other methods, such as the riveting cast-on method, the pre-cast method, and multi-cast-joints (Figure 18). Following phase 3 of the Yinxu period, the bronzes were becoming less complex and the number of those produced using the separate casting method was also dwindling. In phase 4 of the Yinxu period and during the early Western Zhou dynasty, there was an increasingly stronger tendency to use the method of casting with a single pouring.

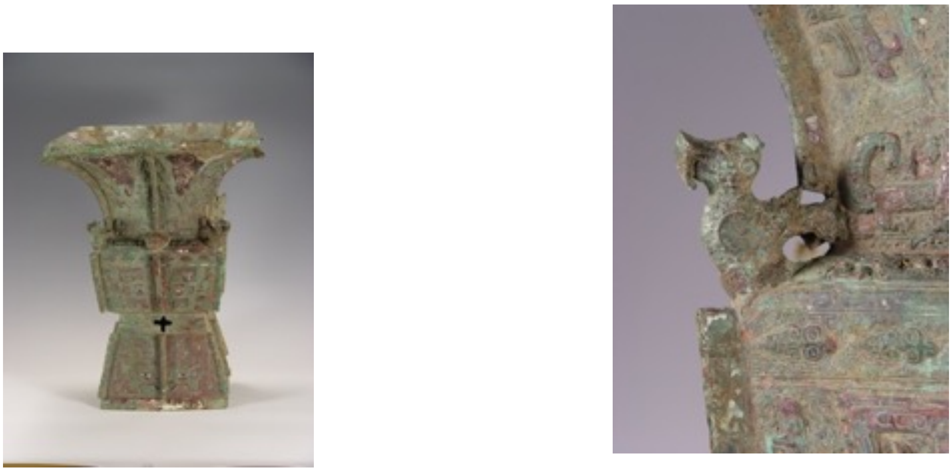

Figure 17: A case of "the tenon type" cast-on method: the square *zun* vessel from Yinxu (M54:84) and its sculptured appendages on the shoulder.

Figure 18: The handled *you* vessel (M5:765) from Yinxu (from *Anthology of Chinese Bronzes: Shang 2,* plate. 114).

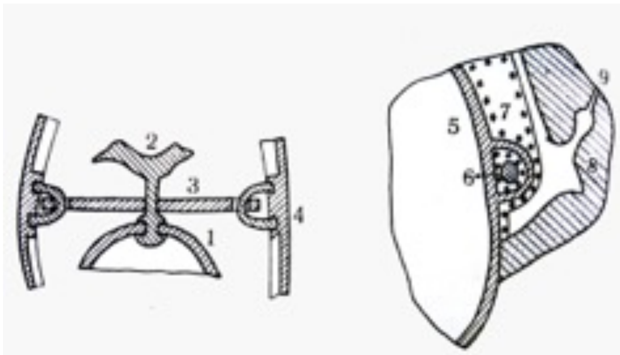

Figure 19: The multi-cast-joint structure. 1: the lid. 2: the knob of the lid. 3: the lantern ring. 4: the handle. 5: the body of *you.* 6: the pin. 7: the clay core of the handle. 8: the molds of the handle. 9: the sprue. (Hua Jueming 1999, 140; Figs. 4–79).

During the mid to late Shang dynasty, the brazing technology might have been invented based on the repair technology. The late Shang brazing technology principally used bronze as the solder, and the "tenon-type brazing method' was mostly used to join the appendages to the body. Tenon joints were placed on the outer wall of the main body of the vessel, and the appendages were linked to the body by pouring the soldering flux under their roots. The earliest case of the "tenon-type brazing method' can be traced back to the large bronze *yu* (M54) from the East Huayuanzhuang site in phase 2 of the Yinxu period (Figure 20, Zhang Changping 2018). In the early Western Zhou dynasty, the bronze *yan* adopted the riveting brazing method to connect the hooks, reserving holes on the main body of the vessel and connecting the attachments to the body by filling the soldering flux into the holes. In the late Western Zhou dynasty, the application of the brazing technology became much more prevalent and was commonly utilized to attach such ears and handles with complex decorations. The soft-soldering technology occurred no later than in the early Western Zhou dynasty. For the two cases of bronze *lei* from the Yejiashan Cemetery of the Zeng State in Suizhou City, Hubei province, and the bronze *zun* from the tomb of Changzikou in Taiqinggong of Luyi county, Henan province (Figure 21), the component testing proved the usage of low-melting-point solders. The use of the soft-soldering technology in the late Shang period could not be ruled out either (Liu Yu et al. 2022). The emergence of brazing and soft soldering technology in the Shang dynasty could be regarded as an objective supplement to the technology of casting with a single pour, thus meeting the practical and decorative needs.

In the late Shang and early Western Zhou dynasties, the decorative patterns on bronze wares still featured imaginative animals, such as beast faces, dragon patterns, *kui*-dragon patterns, and bird patterns. The pattern structures became more concrete, and there arose the “three-layer pattern,” consisting of the mid-relief major pattern and two layers of cloud and thunder patterns above and below. Such patterns all belong to the composite “model-mold design,” which copied the major pattern from the model to the mold to make modifications. The intricate and multi-layered patterns made it more inconvenient to transfer the complete patterns from the model to the mold. To address the difficulty in removing the clay mold from the model, the division of molds became much finer, which resulted in the horizontal division of molds, making it more technologically difficult to assemble the mold sections.

Figure 20: Brazing traces could be found on the handle *of the yu* (M54:157) unearthed at Yinxu.

Figure 21: For the bronze *zun* vessel (M1:125), unearthed from the tomb of Changzikou in Taiqinggong Town of Luyi County; the grey-white substance underneath the beast heads is the lead-rich solder.

The late Shang bronze wares beyond Yinxu—such as the bronze group excavated from the Dayangzhou cemetery, Xingan county, Jiangxi province and from the Sanxingdui site, Guanghan county, Sichuan province—also employed the piece-mold casting technology but displayed distinct technological characteristics, forming what Bagley (1987) referred to as "stylistic provincialism." Examples could be found in the *ding* with pre-cast flanges (Figure 22; Su Rongyu et al. 1997) or the brazing technology extensively applied to the bronze sacred tree (Figure 23, Xu Jay 2008).

Figure 22: Tiger-legged *ding* unearthed from the Large Shang Tomb of Dayangzhou, Xingan county in Jiangxi province. The flanges on the abdomen adopted the pre-cast method (from *Anthology of Chinese Bronzes: Shang 4*, plate 18).

Figure 23: Brazing technology was extensively applied to the sacred tree unearthed from Sanxingdui (from *Anthology of Chinese Bronzes: Bashu 13*, plate 44).

Conclusion

The aforementioned explorations could reveal the formation of piece-mold casting technology for ritual bronzes with characteristics of the Central Plains. In the course of its development, in which the tradition was carried on, the Erlitou period established its basic framework and pattern: that is, using loess containing high silt to make clay molds, using tin bronze or lead-tin bronze as the raw metal materials, and making bronzes with the use of piece-mold casting technology.

The early Shang period constructed the general rules and styles for bronze casting technology: the majority of bronze types in the Bronze Age appeared; the high tin-bronze or the ternary alloy with medium tin and lead content was applied; the casting process had been basically established; the basic molding process, which was characteristic of the division of molds, had been standardized, and various patterning methods on vessel surfaces matured; the separate casting method was adopted to join the main body and the appendages; and the auxiliary processes, which could keep mold sections in position and maintain wall thickness, for instance by using clay spacers and metal spacers, were utilized for quality control.

The late Shang dynasty saw the inheritance, deepening, and overall improvement of these techniques, producing a large quantity of bronze wares with highly complex forms and intricate patterns. The standardization of the molding process and the settled casting process laid the technological foundation for large-scale production. The application of the separate casting method reached an unprecedented level and became the most representative technical feature during this period, embodying the technical characteristic of “simplifying the complex.” In addition, the maturity of the high-tin bronze alloy technology and the phenomenon of “overuse of tin” in the second-phase high-grade burial ritual wares indicate the great national power and abundant resources of Yinxu, especially in the Wu Ding period. This could justify the textual records of King Wu Ding’s “unmatched military attainments.” In the late phases of the Yinxu period, the production scale kept on expanding. Technologically, there occurred a stronger tendency toward the method of casting integrally. The emergence of the method of brazing and the applications of movable sectional models and movable sectional molds were all technical measures undertaken to return to the method of casting integrally. The period from the late Shang to the early Western Zhou dynasties marked the heyday and technological maturity of the ancient Chinese clay-mold casting.

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Part II. External Representations of Knowledge

Means and Ends in the History of Science

Peter McLaughlin[1]

Two key figures, through their diametrically opposed philosophical positions, put their stamp on twentieth-century history of science. Both of them were Russians, born one year and 400 miles apart towards the end of the nineteenth century: They were Boris Mikhailovich Gessen and Alexandr Vladimirovich Koyra. Alexandre Koyré and Boris Hessen represent opposite poles in the philosophical historiography of science: internalism and externalism. I say *philosophical* historiography because *historical* historiography—or just call it the normal discipline of history—knows nothing of *externalism* or *internalism.* Historians, at least contemporary ones, contextualize: that's what they do; it is so to speak their *ergon*—what they do that makes them what they are. The quarrel between internalism and externalism is a *philosophical* issue injected into the discipline of history of science when philosophers hijacked the discipline in the 1930s. The institutionalization of history of science as a branch within history was interrupted by the externalist intervention of the Soviet delegation to a London conference and, more importantly, by the subsequent internalist reaction. Thus, when contemporary historians of science attempt to put the quarrel between internalism and externalism behind them, they are basically trying to return history of science to history—something that I as a philosopher need not approve of.

But first let me say a few things about means and ends:

> If you only have a hammer, every problem looks like a nail.

You have all encountered a slogan like this at some point. There is even a *Wikipedia* page "The law of the Instrument" devoted to and propagating this and similar misunderstandings of the relation of means and ends. Instrumental rationality—the one form of rationality that we all understand—can be formulated a bit like this:

> Someone acts rationally, if she seeks appropriate *means* to given *ends*.

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The joke made in the slogans can be formulated this way: Someone acts strangely, if he seeks appropriate *ends* to given *means*. I want to convince you that the person who seeks appropriate ends to his means is in fact acting rationally.

Let's try some trivial observations with similarly trivial generalizations:

- You can cut your hair with scissors or a razor.
- You can use a screwdriver to turn screws or to open a can of paint.

We can generalize:

- Purposes served by one tool can (often) be served by others.
- Tools invented for one purpose can (often) be used for other purposes.

Now, just as you can use a screwdriver to open a can of paint, so too, could Galileo use the rule for quantifying *caritas* to derive the law of fall.

Furthermore: Given that we have some rather unspecified ends, such as the wish to fly, what we must do to convert such a wish into a concrete plan of action depends on the means available. That is, even ends that we already have are determined by the means available as soon as they become concrete. Whether we use wax and feathers (like Daedalus) or a canvas balloon (like the Montgolfier Brothers) determines what it actually means to want to fly.

Let's go back to the history of science: After the "national editions" of Galileo's and Descartes' *Works* around 1900 and the founding of the disciplinary Journal *ISIS,* an international disciplinary organization was founded in 1928 as a special subarea of history, and biannual conferences were held in which historians, educators, and retired scientists as well as the half-dozen actual professional historians of science met and conferred. During the second such international conference, the historians of science opened themselves up to considerations of science and *technology,* even in the title of the congress: Second International Congress of the History of Science and Technology (1931). But at the congress itself things went off the rails. A large delegation of Soviet scientists and philosophers attended, took the newly announced connection to technology seriously, and effectively took over the discussion: prominent among them was Boris Hessen, who has come to stand for externalism in the history of science. But it was more the subsequent internalist reaction to this assault, led by Alexandre Koyré, that delivered history of science into the arms of philosophy—not institutionally but intellectually. And it stayed there until the historians wrested back intellectual control in the 1980s and 1990s.

So, what is externalism and what is internalism? The terminology is usually traced back to Robert Merton in the 1930s. Internalism was a reaction to the externalism of the Soviets or at least to what was perceived to be their position. The internalists were very much interested in the putative motivations of figures in the history of science. Two typical examples are G.N Clarke and Bartel van der Waerden. But instead of continuing with the opposition between Koyré and Hessen and their supporters, let me jump back to two positions that have been passed on since ancient Greece and which we can read as archetypes: Herodotus and Aristotle.

Our first recorded "externalist" is Herodotus. In his *Histories* from around 440 BC, Herodotus tells the story of how the Egyptians discovered or invented geometry: Taxes paid to the King—his name was *Sesostris* (Senusret III)—were based on the size of the plots of land that changed every year when the Nile flooded its banks. So every year after the flood the King would send his inspectors to remeasure the plots of land and so to determine the basis of the correct tax. Herodotus tells us that "from this geometry was discovered and made its way to Greece."

Here we have the classical source of *utilitarian* explanations of science: Economic necessity calls forth new scientific knowledge and technique. The weakness of this kind of explanation is easy to point out: Many needs go unfulfilled. We need a cure for cancer and a response to climate change, but that doesn't mean we get one. We could just as well make up a story that the King didn't remeasure the plots every year but built a big dam at Aswan to prevent the Nile from overflowing and then assert that his agents discovered hydrostatics, which then came to Greece. So an explanation of the origin or advance of science from the fact that is was needed—or just desired—is not going to work very well.

But before we dismiss Herodotus as too simplistic, let's take a look at the competition. An alternative story about the origins of geometry is told by Aristotle right at the beginning of his *Metaphysics*. He, too, attributes the discovery of geometry to the Pharaoh's inspectors and surveyors, whom he calls priests. Aristotle praises various useful and pleasing forms of knowledge but makes it clear that he thinks the most valuable forms of knowledge are those that do not aim at utility or pleasure, but at wisdom. In Egypt, he tells us, after all the useful and pleasant forms of knowledge and art were discovered, they then invented forms of knowledge that "related neither to pleasure nor yet to the necessities of life" like the "mathematical arts," that is, geometry. This development occurred in Egypt because there the caste of priests had *leisure*. Thus, Aristotle says—just the opposite of Herodotus—that science demands a step back from practice or technology: The priests were not engaged in manual labor nor bound to production schedules. It is the step away from utility that is said to mark the origin of real science. This is the classical internalist characterization.

But Aristotle's story is also much too simple. When the priests in the temple with their leisure were sitting around with nothing to do and no one wanted to write any more religious poetry, they discovered geometry. It is, however, hard to imagine that the priests in the temple would have discovered geometry at all if they didn't already know how to use a compass and rule, which they learned as surveyors while measuring the fields and documenting the results. Thus, we are dealing not with leisurely priests in the temple but with underemployed land surveyors, who have measuring instruments lying around idle; and they ask themselves: what else can we do with these things?

Now even the most famous representative of the utilitarian view of science, Francis Bacon, was not necessarily so utilitarian as he is often taken to be—nor so irrelevant to the origins of modern science as Koyré insisted he was. The most widely known slogan associated with Bacon's gram for the Advancement of Learning is the assertion that "Nature to be conquered must be obeyed" (*Natura enim non nisi parendo vincitur*)—which may or may not contain an allusion to the preface to Aristotle's *Mechanical Problem*s. The first and most obvious meaning of this slogan is that if you want to conquer nature in technology you must obey nature's laws, and to find these laws you might want to pursue science. Thus, Bacon recommends that we learn nature's laws so that we can apply them in technology; so to speak, we should learn geometry so that we can measure the fields in the Nile Delta. But you can also read Bacon in the other direction and take him to be saying something radically different: Since we are conquering Nature all the time in technology, we must have been obeying her laws all the time without knowing it. Thus, if we study and analyze technology we might just learn about the laws of Nature. Bacon—backwards—doesn't pursue science for the sake of technology; he analyzes technology for the sake of science. This kind of project of studying technology for the sake of science is what Galileo Galilei explicitly advocates in the opening lines of his *Discourse concerning Two New Sciences.* Galileo praises the Arsenal in Venice for providing "all kinds of instruments and machines" for speculative minds to theorize about, especially in mechanics. Galileo wasn't interested in building better cranes or assembly lines for ship-building; he was interested in studying nature through its conquest in technology. Thus, his interest in technology was not so much the *motivation* for pursuing physics but the *means* for achieving progress in one area of physics, mechanics. Technology's relation to science need not be reduced to a potential and disreputable final cause of science, as we find in Koyré.

With this in mind, let's return to Herodotus: If we look more closely, we see that he does not actually say that geometry was invented *in order to* measure the fields, but only that it was invented *while* measuring the fields or *after* measuring them or somehow *in the context of* measuring them. The Greek word is simply *entêuthen*, which means *hereby* or *thereupon* or something like that. As you see, Herodotus is a real historian, not an externalist, just a contextualizer. Thus, he is no more committed to a purely utilitarian perspective on science than is Galileo or even Bacon. Even the first cited externalist is a bit more subtle in his analysis of science than are some twentieth-century internalists.

If we now return to Aristotle as well with his leisure and his step back from practical application, we can see the Egyptian surveyor-priests using their surveying instruments for other purposes than surveying. Or, what is perhaps a more likely story, given Peter Damerow's research on Summerian "priests," the training of the surveyors used exaggerated examples and problems that had no necessary connection to actual field measurements, and thus *explored the limits* of the use of their instruments and practices – and thus explored the properties of space as such: which is what geometry was. This is clearly a leisure activity, but one done with resources and instruments developed on the job, which is why the leisure science also applies to the real world.

So what can we learn here from the Greek historians and the early modern heroes of natural science? Things developed for one purpose *can* be used for other purposes, purposes which the original inventor may not have envisioned. And if the tools that are used to develop new purposes are derived from technology, we have an example of the social determination of new internal goals of science.

To what extent do the means or objects of scientific research—whether experimental machinery or mathematical techniques of representation—determine the course of research and the aims of science? Whereas instrumental reason defines rational action as seeking appropriate means to given ends, its Hegelian inverse views scientific rationality as seeking appropriate (scientific) ends for given (technical) means.

This is something that Thomas Kuhn blurted out but didn't elaborate on in the *Structure of Scientific Revolutions*:

> …the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them.

The Egyptian surveyors, insulated from society in the temple, could ignore the external desires and wishes of the society at large, which are binding for practitioners like Kuhn's engineers, physicians, therapists, and counsellors. The external conditions allowed them to pursue a project of internally exploring the limits of various techniques and instruments—in the pursuit of goals made possible—or even first revealed by those tools that the society that had trained them was able to supply from outside.

The true internalist asks about the external determinants of the internal dynamics of scientific development.

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On Cognitive Artifacts[1]

Stephen C. Levinson[2]

> All this hints at the role of material culture as the backbone of an evolution of knowledge. … what if the game-changing role of material culture as a means of cognition also extends to the symbolic means of our thinking … ? (Renn 2020, 50–1)

Abstract

Wearing the hat of a cognitive anthropologist rather than an historian, I will try to amplify the ideas of Renn's cited above. I argue that a particular subclass of material objects, namely "cognitive artifacts," involves a close coupling of mind and artifact that acts like a brain prosthesis. Simple cognitive artifacts are external objects that act as aids to internal computation, and not all cultures have extended inventories of these. Cognitive artifacts in this sense (e.g., calculating or measuring devices) have clearly played a central role in the history of science. But the notion can be widened to take in less material externalizations of cognition, like writing and language itself. A critical question here is how and why this close coupling of internal computation and external device actually works, a rather neglected question to which I'll suggest some answers.

1. Introduction

I hope and believe that the ideas here are quite relevant to the history of science, although they are drawn from other domains of academic thinking, especially anthropology, philosophy, language science, and the cognitive sciences in general. The central idea in a nutshell is that the power of human thinking is owed very largely to its *externalization*, which allows us (often quite literally) to manipulate ideas represented in objects, devices, or external codes. This is of course not a new idea—Jerome Bruner already outlined it succinctly in (Bruner 1966), and it can be traced much further back still. But it has since been much further developed independently in a number of disciplines, and the ideas can now be stitched together and fruitfully recycled. My own contribution here is to try and sketch *exactly why externalizing ideas so empowers them.*

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[1] This preprint is adapted from a forthcoming book (Bennardo, Chrisomalis, and De Munck, forthcoming), but closely reflects the paper I gave at Trieste in honor of Jürgen Renn on July 2022.
[2] Max Planck Institute for Psycholinguistics.

The story I will outline has relevance for the history of science in the following ways. First, the hybrid nature of externalized thinking—part cognition, part external device or representation—explains why the manipulation of instruments and representations plays such an important role in the development of scientific and technological thought. It embeds the history of science and technology within a general theory of cultural evolution (although that is not developed here). By so doing, it equally explains how easily scientific progress can go into reverse: the loss of either idea or instrument destroys the hybrid advantage. In addition, by focusing closely on how exactly these externalizations of thought and process work, we may be better able to see why some such hybrids were winners and others withered on the vine.

I start from the position that culture is such an improbable biological outcome that it needs an evolutionary perspective to explain its emergence.[3] We could begin by asking: What is the function of culture? Given the many overheads (including the cognitive effort, the costs of behaving altruistically and giving away good tricks), what are the payoffs? The central function, I would suggest, is for sharing things that are useful (i.e., tricks for doing things), and the payoffs are that individuals get more out than they put in. What they get out, I suggest, is a whole new form of cognition, which has a curious ontological status and is not wholly in the head.

A way of thinking about the shared tricks that constitute culture is to think of culture as composed of multitudes of *tools*, some concrete and some abstract. The concrete tools are mostly devices that amplify the body: hammers, knives, levers, pulleys—even horses, jet planes, and jack hammers are essentially bodily prostheses. The kind of exoskeleton that deep sea divers use is a kind of prototype to bear in mind. In contrast to these bodily prostheses, the more abstract tools are devices that amplify the mind: they are what I will call *cognitive artifacts.* They may be concrete things like a slide-rule, an abacus, or a computer, and their job is not to amplify the body, but to extend our mental powers. The rest of this paper outlines these mind-expanding tools, and tries to show how our cultural worlds are pervaded and sustained by them, and indeed how they constitute the backbone of culture.

[3] In Levinson (2022), I suggest that the source of the human propensity to share lies in an early evolved use of alloparenting, requiring some kind of matching of empathy and communication.

2. Culture as Cognitive Technology[4]

2.1 The Idea of a Cognitive Artifact

Nearly half a century ago, Jack Goody (1977) introduced the idea of a "technology of the intellect." His key example was the invention of writing. When writing is first introduced in a society, he noted, it is used for very simple mundane uses, of which making lists is the most prominent. Such lists have two rather different uses: they can be forward-looking to-do lists (like shopping lists or recipes), or they can be backwards-looking lists (as in records of tribute received). The first has a directive or computational character, while the second performs a mnemonic function. In both cases, he noted, in externalizing thought, and freezing the evanescent linguistic signal, they make new modes of cognition available: you can inspect the signal, reorder it, count the elements, and edit it. It enables meta-cognition. He held that these cognitive effects were revolutionary, and that writing therefore "changes the type of data an individual is dealing with, and it changes the repertoire of programs he has available for treating this data. Whether or not it changes the hardware … is another matter, but on the analogy of language the possibility is there" (1977, 109–10).

That speculation turns out to be correct: literacy radically rewires the brain, colonizing the fusiform gyrus at the expense of our face-recognition and strengthening the white-matter connections between the hemispheres and elsewhere (Dehaene 2009; Carreiras et al. 2009). It is a revolutionary piece of cultural technology. Goody also pointed to the extraordinary sociocultural effects of literacy, which allows communications over space and time, with consequences for the very distinct histories of the literate civilizations of Eurasia and the non-literate civilizations of sub-Saharan Africa. Literacy after all allows the cumulative use of data and records which lie behind the growth of science and technology, the bureaucracies of vast empires, the rise of mass communications, and indeed, many aspects of the modern condition.

[4] An extended version of this argument can be found in (Levinson 2020). In turn, this draws on the work of Edwin Hutchins (1995), Jean Lave (1988), Lucy Suchman (2007), Andy Clark (Clark & Chalmers 1998), ethnoarchaeologists (e.g., Malafouris 2013; Overmann and Malafouris 2018), human-machine interaction (Norman 1993), and ultimately Vygotsky ([1936] 1986)—movements that have gone under the rubrics of Activity Theory, Distributed Cognition, Extended Mind, Situated Cognition, Material Engagement, and so forth.

Goody concentrated solely on literacy, but I believe the idea of technologies of the intellect has in fact far wider applicability (see Levinson 2020). A "cognitive artifact" is, in my favored sense to be developed, an external aid to internal computation (see also Norman 1991). There's no shortage of obvious examples, from the abacus to the slide rule, from the theodolite to the compass, from the map to the diagram, or from the electronic calculator to the computer. But there are much simpler examples, and they are all around us.[5]

Consider this simple example: a Tzeltal man, who is typically a subsistence farmer, carries with him a meter-long wooden staff. As well as a walking stick, a weapon of self-defense against dogs on a trail, and sometimes a rod of office, it is basically a dibber: when planting corn he pokes it three times into a little heap of soil and plants three corn kernels. Then he uses the full length of the stick to measure the place for the next little heap, and so along the row; then he goes up one stick's length and plants the parallel row. If he plants twenty heaps per row, and there are twenty rows, he knows he can expect 1200 (400 x 3) corn cobs (unless the variety yields two cobs per plant, in which case he can double it). So, he can calculate the likely harvest and decide whether to plant more. If he hires someone to do it, he will pay by the number of rows. If he is thinking of selling the field, or buying it, he knows how much of the family's needs he will lose or gain. That simple stick gives him a system of precise estimation.

Every system of weights and measures is a simple piece of cognitive technology, and most of them had their origin in just such calculations: an acre, for example, was the area that medieval peasants could plow in one day with a team of oxen. The enormous value of a simple measure of length was brought home to me by watching Rossel Islanders make houses and canoes from bush materials. Rossel Islanders, traditionally at least, use no measures. For example, looking for a ridge pole they trot off into the jungle and find the longest straight timber of the requisite diameter they can find. They carry these heavy timbers (sometimes weighing more than 300 kg) back down the mountain, haul them up on top of the house structure, and chop them off to fit the emerging shape. If they had a tape measure, they could save themselves a lot of sweat and a lot of wasted materials.

[5] Although they are sometimes so simple that they are not always easy to recognize. Consider the transparent window on a bottle of engine oil that allows you to see how much is left, or the depth gauge on a river crossing, or the LED that lets you know your device is plugged in, or any symbol like a road sign warning of rockfalls.

The measure would be a way to carry with them a precise estimation of what is needed. Rossel Island has no market economy and no pressing use for weights and measures, unlike traditional Mayan communities like those that speak Tzeltal.

A cognitive artifact then is an aid to solving a mental problem by means of an external instrument, which returns some value which can be re-internalized. Figure 1 represents this export of a mental problem out of the head *via* a query to an external device, which then returns a value which can be re-imported into mental computations. In the parlance of the philosopher Andy Clark (2011)[6], this is a "coupled system," a circuit that depends both on an inner and outer component. It is because the outer instrument is externalized that it can be shared, and indeed is subject to the processes of cultural evolution, which typically hones the instrument into an ever more efficient form. Those processes of cultural evolution are absolutely central to the efficacy of these ever-improving devices, and thus to the development of technology and scientific advance.

Thinking of culture as composed largely of such coupled systems (and I will enlarge the scope below) gives us a satisfying way of cashing out Durkheim's (1895) ontological insight that "social facts" are "things" that cannot be reduced to psychology.

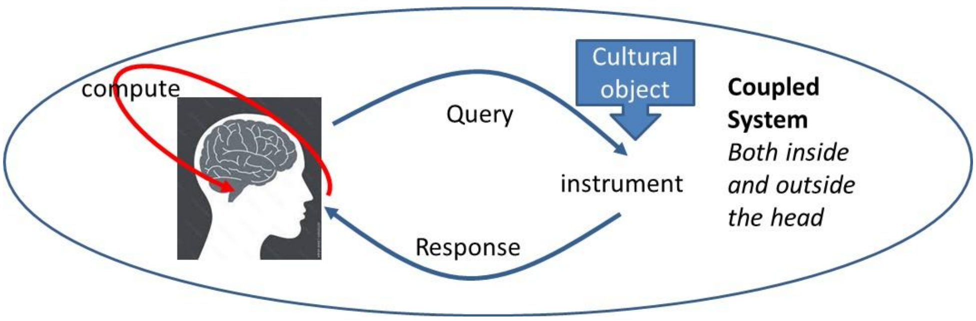

Fig. 1: Cognitive Artifacts as Coupled Systems Bridging Mind and Matter.

Let us turn to obvious examples of cognitive artifacts to get further clarity. There is no dearth of examples in the history of technology. Aids to spatial navigation—where the problem is to solve location and direction—have been much studied and include Micronesian star maps, medieval portolan charts, Roman strip maps, compass roses, astrolabes, sextants, and the modern GPS and radar systems. Traditional Polynesian navigation relied on master navigators memorizing star rising and setting points, and

[6] I should however distance myself from the metaphysical claims of Clark and others (Clark & Chalmers 1998; Clark 2011) that mind can then literally spill into the environment, and is no longer confined to the head; such ontological claims are not germane to the present argument.

internalizing rules of thumb about wave interference, orographic cloud, and the predominant settings of winds and currents (Gladwin 1970; Feinberg 1988; Lewis 1972). The Western tradition externalized these processes—measuring speed by a chip log, star azimuths with the astrolabe, direction by compass, and so on—using these measurements to plot locations on a chart (Hutchins 1995), so answering "Where am I?"

Another much studied domain is the measurement of time, both diurnal and calendrical. Simple systems include noting the location of celestial bodies and noting the fruiting or migration times of natural species. Thus, Rossel Islanders arrange to meet on the morrow by pointing to where the sun will be, while Tenejapan Mayans may arrange to meet before dawn by pointing to where the moon will be, which requires more careful observation—here, gestures act as externalized aids to computation (see also Floyd 2016). On Rossel Island, the seasons are estimated by changes in prevailing wind directions and the coastal running of fish, like sardines and mullet, triggering events in the agricultural calendar. So, the noting of external events can answer the problem of when to plant taro seeds.

From such humble beginnings, artifacts and machines for temporal reckoning slowly developed: for diurnal reckoning, water clocks, sundials, and clockwork clocks which counted divisions of the day for purposes as diverse as shared irrigation (Iran), allocating time for plaintiff and defense in a court of law (Rome), the opening of markets (Greece and Rome), or times of worship (Aveni 1989). For calendrical reckoning, complex celestial measurements were undertaken using instruments as massive as Stonehenge or the Caracol at Chichen Itzá. Huge investments in astronomical observation were made to bring the lunar and solar cycles into alignment with the solar year.

A final obvious example is geometry and mathematics. Innate number concepts are restricted to subitizing (recognizing between one and five objects at a glance) and estimating masses: it is counting that seems to bridge across the systems, giving us precise quantities (Dehaene 1996). Peoples who lack number words like the Pirahã are famously unable to match precise quantities (Gordon 2004; Frank et al. 2008). First steps involve matching external counters with the things to be enumerated, and the body provides a convenient set of digits for this purpose (Bender and Beller 2012). Other early methods of reckoning involve tallies—the 20,000-year-old Ishango bone may even have acted as an early slide rule (Ifrah 1998; Marshak 1972). Rossel Islanders keep track of thousands of shell "coins" by collecting the strings on which they were strung in tens, counting the strings and then multiplying in their decimal number system. Devices like the abacus, which go back to Sumerian times, speed up mental computations for seasoned number users (Wang 2020), while today most of us use calculator apps. The actual form of the externalization of number concepts crucially matters, as history shows: the Roman non-positional system hindered calculations, while the Arabic system favored them. More exactly, as Chrisomalis (2020) explains, although Roman number representations could easily be supplemented with calculations on the abacus, there was no easy way to show the workings of the calculations in the numerical notation—hence double-entry bookkeeping was instrumental in ushering in the Arabic notation. Geometry is another area where externalization makes available mental calculations that are otherwise not achievable—the Babylonians already had approximations to *pi*, ways of calculating the areas of various figures, and even calculus (Ossendrijver 2016).

A computational cognitive artifact has the following properties:

1. There is a recurrent cognitive problem, finding the value of f(x) = ?
   e.g., *How big is the gap? (measuring for a door)*
2. The artifact is externalized in a publicly accessible medium
   e.g., tape measure
3. The instantiated artifact is shared in type, and honed by cultural evolution
   e.g., standardized units on a compact retractable metal coil
4. There is a procedure for operating on the instantiation
   e.g., holding the tape against the gap
5. The process is economical: the cognitive advantage outweighs the costs of externalization
6. The output of the process must be re-internalizable (e.g., memorable)
   - if f(x) = y, y must be easily assimilated
     e.g., the number of centimeters

Fig. 2: Characterization of a Computational Cognitive Artifact.

I believe these examples are sufficient to illustrate the idea of a cognitive artifact. Let us now firm up the concept. Figure 2 provides a characterization, using the simple example of a measurement system (like a modern tape measure) to estimate how big a door I need to fill the door frame. As spelled out there, there is a mental problem to be solved, represented here as a function with an unknown value, and the use of an external device—the cognitive artifact—yields the value in a format easily returned to the mind.

The simple example makes clear that we are not talking about artificial intelligence here—the artifact itself can be as simple as the Tenejapan measuring stick.

This is, however, only one species of cognitive artifacts, namely one that aids active computation. Jack Goody (1977) drew attention in the case of literacy to its two functions: a list could be an inventory of what I need to do, or it could be a record of what I have already done. In the latter case, the writing serves the purpose of recall—perhaps by an actor other than the writer. All of our historical, bureaucratic, and scientific records are of this type, and they enable that amazing accumulation of knowledge that characterizes advanced literate societies. This is another type of cognitive artifact, which rather than enabling computation, enables the retrieval of an earlier solution.

Curiously, it seems that the very origin of Sumerian writing (and thus ours) lies in mnemonic clues to the contents carved in the top of clay envelopes which contained tokens (representing, e.g., a debt of five sheep; Schmandt-Besserat 1996)—after a while, the tokens became superfluous, and the mnemonics took over. Once again, though, we can generalize away from literacy and look at other ways in which simpler retrieval systems work. Consider a knotted handkerchief, for instance, to remind me to contact Jill in the morning. It works because by encountering the knotted handkerchief in my pocket in the morning, I am reminded of the intention to call Jill, which caused me to knot it. Or consider the use of notches in a bone used to keep a tally of the days passed in this location, or the successful kills in the hunt—such tallies seem to go back to the Upper Paleolithic (Bahn 2016, 324ff.).

Simple mnemonics may be private, as with the handkerchief, or they may be public, like road signs reminding one not to speed. On Rossel Island, people erect taboo signs to signal and remind people that a coconut plantation is no longer free for foraging, while many kinds of boundary markers in other societies serve a similar purpose (*tim* trees in the case of Tenejapa, planted at the boundaries of fields). Non-literate retrieval systems can be vastly more complex than this. Consider the ancient Peruvian *quipu*—knotted cords that served (amongst other things) to keep track of tribute paid in the Inca empire, using a positional system that kept track of vast numbers (Urton 2003)—a system still used a century ago by herders keeping tabs on their flocks.

Let us call this a mnemonic cognitive artifact and give the following characterization, as in Figure 3.

A mnemonic cognitive artifact has the following properties:

1. A cognitive artifact for recall has the function of encoding the thought *A* at time *t*, in such a way that it can be retrieved at a later time t_{+n}
   e.g., How much did I pay for the ticket?
2. There must be some external marker of *A*, call it *ά*, such that encountering *ά* brings *A* to mind
   e.g., a printed receipt
3. To be recoverable by random others, there must be a shared convention that *ά* stands for *A*
   e.g., the receipt has a standard form
4. The procedure for encoding *ά* is effective and simple enough to be economical
5. The encoded thought *A* must be recoverable, useful, and easily assimilated at time t_{+n}

Fig. 3: Characterization of a Mnemonic Cognitive Artifact.

2.2 How Cognitive Artifacts Work—Thought Transduction

It is not entirely self-evident why cognitive artifacts have the efficacy they have. Why does translating a mental problem into an external medium seem to automatically aid computation? One principal reason seems to be that the problem now has a double representation—one in an internal mental medium and the other in an external medium. Each kind of representation may offer different kinds of cognitive affordances. It has been noted, for example, that children faced with Piagetian problems gesture the correct answer before they learn to expound it verbally (Goldin-Meadow 2015), and if one gives route directions, one will find that one's gestures precede the verbal instructions as one literally "feels out" the solution.

Another feature is that the transfer from inside to outside involves some kind of *transduction*, something other than a direct 1:1 mapping (in engineering, a transducer is a device that converts energy from one form into another—e.g., sound waves into electrical current and back into sound waves). An abacus used by a speaker of a decimal system re-represents number in terms of units of fives and ones. Linear speech is converted into two-dimensional orthography stripped of many expressive features. A guess at the size of a timber is converted into a precise set of conventional units. Transduction typically involves the following transformations:

a) Transduction often involves a mapping into a higher or lower dimensional space—1D to 2D (musical notation, writing, written addition of a string of orthographic numbers), 1D to 3D (sundials, astrolabes), 3D to 2D (maps, blue prints, geometry of solids), 3D to 1D (oral recitations of, e.g., Aboriginal song lines, or a reduction of an itinerary to a list)
b) The transduction mostly involves a mapping into a more concrete medium—e.g., an abstract number into the tactile medium of the abacus, or an abstract direction into gesture—where it can literally be manipulated
c) At the same time, the external representation simplifies and strips away incidental distracting properties: consider a map, which only represents a few chosen features of the landscape, or a musical score that abstracts away from variable phrasing and performance
d) The transduction may involve a mapping from a weaker sense into a more dominant one, e.g., internal representations of sound into vision (writing, musical notation), abstract reasoning into visual spatial representations (graphs, Venn diagrams, numbers), or abstract order or number into tactile manipulations (prayer beads, abacus)
e) It may involve the transduction of a fleeting or changing signal into a static one where time is frozen, as in writing or musical notation, or a seismograph
f) The external device may physically constrain the solution space, so reducing errors, as with an abacus, or a pilot's checklist with tabs to be flipped one by one
g) Crucially, by externalizing a computation, parts of it can be put in an external memory buffer, so overcoming the highly limited buffer capacities of our working memory, as explained further below. The importance of this is evident when doing long division, or remembering a string of digits, or going through a checklist in preparation for a journey

These transductions of thought into different types of representation seem to capture Goody's (1977) insight that the early uses of literacy involve the exploitation of metacognitive rumination, which is less available for flashes of thought in the mind or the ephemeral signals of speech. In addition, they offer some account of what Goody, following Bruner (1966), called the *amplifying* function of externalization. From the point of view of the user, the cognitive artifact may substitute an entirely different and easier task for the target one, as when logarithms are added to multiply large numbers, a procedure partially automated in a slide-rule (Norman 1991).

Another prominent property of cognitive artifacts is that they involve both externalization and re-internalization, often repeated recursively, as when writing and re-drafting, or doing complex calculations. Such repeated re-ingestion will inevitably remake the internal representation into a format closer to the external representation. The oral performance of a literate person is not like the oral performance of an illiterate praise-singer (as noted in Goody 1987), nor is the mental arithmetic of a mathematician like the mental arithmetic of a peasant. Skilled and practiced users of these repetitive transductions may end up not actually needing the externalization, as with skilled abacus users who can use a mental representation of the 3D thing without resorting to the external aid (Frank and Barner 2012; Barner et al. 2016). In the same way, while I may need notes to keep my lecture on track, skilled orators have since Cicero (Yates 1966) used internal systems of mnemonics based on imagined externalization (for example, by traversing an image of a familiar room). What the example of literacy shows is that this recursive re-ingestion induces the mutual adaptation of brain to external device and external device to brain, in what Deheane and Cohen (2007) have called a "cultural recycling of cortical maps." It is this mutual adaptation that characterizes a "coupled system" between mind and device.

It is possible to invent private, secret cognitive artifacts, as when one uses external clues to remember software passwords—this is prominent especially in mnemonic cognitive artifacts. But a huge part of the efficacy of cognitive artifacts is that they are mostly shared, public representations. As a consequence, they have been honed, often by eons of cultural evolution (as with our alphabet or number systems) to maximize the metacognitive affordances of the external representation (e.g., making addition or multiplication easy) and to minimize the effort of transduction when externalizing and re-internalizing the representations. Sometimes there will be an arbitrary quality to the external representation—e.g., it could be in inches or centimeters—drawing attention to the fact that part of what makes it valuable is the very fact that it is a shared, standardized system even when the format is suboptimal (like the QWERTY keyboard).

Summary: How Cognitive Artifacts Work

- Double representations in different formats give extra "handles" for computations or retrieval
- Transductions into a different dimensional media afford different operations, allowing metacognitive rumination, inspection, and the "freezing" of temporal succession
- Stronger senses can be used to reinforce weaker ones, as with the visual representation of abstract properties
- Externalization offers mid-computation "memory buffers," thus overcoming the severe limitations of working memory
- Externalized representations can be honed by cultural evolution to maximize the metacognitive handles and minimize the efforts of transduction
- External representations tend to simplify, selecting a few dominant features of the underlying thought
- Recursive externalization and re-ingestion bring inner and outer formats closer in alignment, easing the difficulties of transduction

Fig. 4: The Efficacy of Cognitive Artifacts.

2.3 Language as a Cognitive Artifact

So far, the proponents of cultural models may retort, "Sure, those are interesting examples, but they hardly add up to that massive knowledge base that we call a culture." Yes, but we have not finished yet. Any theory of culture has to come to grips with language. One of the most striking things about the human species is that it is the only one on the planet which has a communication system that varies so fundamentally across social groups at every level of structure—from the sounds to the syntax, from the syntax to the semantics (Evans and Levinson 2009). Language is the foundation for cultural diversity, one of the prime modes of its transmission across generations.

Language of course is also a cognitive artifact, if an enormously complex one. It transduces a thought into sound waves. It solves the problem of making overt an intended meaning. It has an external form or representation which is shared—we learned our languages in an open forum. A language offers some of the same computational advantages we have already noted of dual representations—when I express a thought, I streamline, simplify, and clarify it to myself. Thus, I can re-internalize my now clarified thought. Writing a paper like this, the thought goes in and out a few times before stabilizing on the page. Now, just like the tape measure or the calculator, I have inherited the technology which has been honed by cultural evolution to serve a community of speakers. What I have inherited, amongst other glorious things, is a vast repository of ready-made concepts packed into words.

Most of these words encode notions that would take a lot of round-about description to convey by other means (e.g., ogee, logarithm, architrave, sonata, algorithm, *pi*, shaman, etc.). At the outset of cognitive science, George Miller (1956) pointed out that these perform a crucial computational function. We can only hold, he suggested, 7 ± 2 items at once in working memory (we now know the mode is much less than this, more like four; Cowan 2001). So, the only way we can compute complex things is by packing complex concepts into memorable chunks, as we do in maths when we let *x* stand for the output of another operation. Think about words as pointers to complex concepts in virtually unlimited long-term memory—but unless they themselves form a chunk (as in a sentence), you can only hold four or five such random pointers in short-term memory at a time. A culture, then, provides you with a huge stock of words and expressions, a bonanza of "zipped" thoughts, little nuggets of intensive meaning. These allow us to get complex multi-stranded thoughts through the bottleneck of working memory. And although linguists tend to think of the lexicon as an unstructured repository, it comes with immanent structure, as in the taxonomies, partonomies, "semplates" (Levinson and Burenhult 2009), and semantic fields which are familiar objects of anthropological enquiry.

In addition, every language provides us with its own recursive structure for building complex propositions. Studies have shown that those who have been deprived of full-blown language in early childhood have difficulty conceptualizing complex embedded propositions (Pyers and Senghas 2009). Besides the major constructions of the central syntax of a language, languages have additional mini-grammars like those found in their numeral systems or their kinship systems, which again can recursively construct specialized thoughts of arbitrary complexity.

The transduction of thought into speech is an amazing process involving upwards of a second and a half of intensive mental processing and the deployment of over a hundred muscles, and in conversation it often has to work in parallel to the processing of the incoming turn at talk (e.g., Levinson 2016). We ease the process by learning to regiment our thoughts into a form that fits the categories of the particular language (Slobin 1996).

It is because of this that our mental categories come to match language-specific grammatical and lexical categories, yielding many features of “linguistic relativity” (see Levinson 2012). The socially-shaped patterns of usage come to dominate the way we think: In one culture we think in terms of left vs. right, in another in terms of north vs. south. In one culture we think of relatives in terms of a Crow system, in another according to an Omaha system of reckoning. In one culture we think in terms of blue vs. green, while in another we think in terms of grue, and so on.

In sum, languages have all the hallmarks of cognitive artifacts. They transduce thoughts into a socially shared medium in order to solve myriad problems; they are tools for persuading, cajoling, encouraging, ordering, comforting, and exchanging information with our social others. We re-imbibe thoughts clothed in language, benefiting from the transduction into a streamlined and culturally shaped medium.

2.4 Generalizing the Idea: Other Kinds of Cognitive Artifact

I have outlined two main types of cognitive artifacts, the computational and the mnemonic. These two types are central exemplars, but there are almost certainly other kinds of cognitive artifacts. At the risk of over-widening the category, consider the following. Cultures use various means of inducing mood changes, that is, altering the emotional stance of participants. These may include mood-changing substances like alcohol or kava, or emotion-inducing cultural performances like music and dance. One might object that these changes are mechanical, natural causations, but it is noticeable that they do not necessarily work across cultures: kava and Chinese opera leave me cold, while alcohol often turns my Pacific friends into zombies rather than socialites. Ritual flagellation may induce trance in a Hindu devotee, but I doubt it would work on me. Nor will magic mushrooms automatically make a shaman out of you. Within a culture, these mood changing artifacts work in the requisite direction because there are already expectations of the state that should be reached. Presuming that emotional states are part of our cognition (and not in a separate compartment of the mind), then devices for changing those states might also be candidate cognitive artifacts (see Figure 5 for a rough characterization).

There is a goal state, an emotional tone *T*, to be obtained.

There is an artifact or performance that is publicly recognized to induce this tone *T*.

The artifact or performance is honed by cultural evolution to induce this tone *T*.

There is a specific procedure for using this artifact or performance.

There is a good probability that undertaking the procedure will induce *T* as desired.

Fig. 5: Characterization of an Emotion-Inducing Cognitive Artifact or Performance.

Music and dance might be the most powerful and most common devices of this sort, but more extreme events like human sacrifice or sacred mortal combat (as in the Mayan ball game) might be others, inducing mixtures of fear and wonder. These kinds of human activities are hard to explain in terms other than their intended emotion-inducing functions.

Representational art may also have this function—after all, it is historically deeply associated with ritual, religion, and multi-modal performances. Consider too the architecture intended to make those who enter feel small and insignificant or wonder at the crystallization of massive labor and expense—the emotions one feels on the thresholds of castles, cathedrals, palaces, or parliaments. Clearly, huge amounts of effort and resources are expended in mind-bending external devices, performances, and installations.

A more straightforward extension of the notion of cognitive artifact is through perception. The history of science is populated by devices that extend the range or acuity of human perception. If telescopes and microscopes allow us to see things somewhat beyond our normal vision, radar, infrared sensors, and X-rays make visible the invisible. The current direction of wearable technology is likely to make cyborgs of us all (Clark 2003). Perceptual prostheses, however, may not be technologically complex at all—as Bateson (1972, 359) famously pointed out, the blind man's stick makes an extended cognitive system. Barking guard dogs or the Capitoline geese also served as extra eyes and ears.

A completely different line of reasoning may lead in the direction of seeing social teams as computational devices. The argument here has been well rehearsed by Edwin Hutchins (1995) who showed how, for example, a team of sailors on the bridge of a ship may navigate the behemoth via a division of labor in which each sailor reports readings from different devices (sonar, radar, line of sight) to a central navigator plotting the course. The "distributed cognition" movement may itself have lost steam, but the insights are perfectly valid: we solve a lot of problems by outsourcing them to a team. In the simplest case, I can ask you when we went to Rome together, using your recall device to solve my failed one.

In more complex cases, a team of scientists or developers can jointly solve long-standing problems. Whole bureaucracies can operate merely to answer the questions posed by the state. In complex societies there are elaborate divisions of epistemic expertise and labor, and we employ accountants, lawyers, surveyors, web designers, and translators. The modern trend of course is to try and shrink these teams by substituting them with artificial intelligence, merely the latest step in a long line of experiments with cognitive artifacts. The insights of the "distributed cognition" movement nicely tie cognitive anthropology into the study of social interaction and social organization (see Enfield and Kochelman 2017).

2.5 The Payoffs of Viewing "Scientific Cultures" as Built out of Cognitive Artifacts

Notoriously, science at any one time and place is not a homogenous and harmonious whole, but a buzzing confusion of rival ideas, techniques, and instruments. The framework of cognitive artifacts can easily encompass variation (not everyone needs to use the same tools, or use the same mental tricks with identical tools), and therefore has no need to idealize away from variation, imperfect performance, and real-time processing. Above all, cognitive artifacts escape the head: they are partially in the environment and subject to all the normal processes of cultural evolution, with rival designs competing for use, often in an ever-upwards spiral of sophistication. Cognitive artifacts are tools in active use, not abstract data structures, and together with a theory of cultural evolution can encompass rapid technological and mental change. That external part of the hybrid at least can be empirically studied with comparative ease. At the same time, because we are dealing with a hybrid of internal and external representation, once the internal side is not transmitted, the key to the external device may be lost, as with the ancient Peruvian *quipu*. While the sophistication of the Antikythera mechanism betrays a history of development, the lack of parallels shows how fragile these composite idea-machines are.

From the fact that human cognition is so interdependent with these externalizations, it follows that our cognition itself has a curious ontology (Clark and Chalmers 1998; Overmann 2017). Is it my recollection or my digital diary's that I have an appointment on April 1, 2023? Is it my belief or my calculator's that the square root of 3 is 1.73205080757? Is it my estimation or that of the bubble in my spirit level that this door jamb is level? Is it my estimation or the map's that there is a distance of 430 km between Trieste and Rome? Clearly, human minds form coupled systems with their cognitive tools—we have partially outsourced our cognition, and in doing so hugely *amplified* it, as Goody (1977, 109ff.) was fond of pointing out.

A cognitive artifact's perspective also has the merit of tying scientific development—partially, obviously a story of mental triumphs, directly to external culture—to artifacts, to their use, to social interaction, to social transmission, and, by way of the cognitive division of labor, to the organization of society. In this way, it does not leave history of science marooned from all the other historical and cultural trends studied in other sciences. In addition, it offers some kind of account of how culture and technology enhances cognition, and thus an account of how culture endowed humans with an evolutionary advantage. It suggests that culture is not some outcome of oversized brains, but rather of the way in which cognition interacted with the social environment in a feedback relation over hundreds of thousands of years resulting in the gradual development of this expensive organ.

To conclude, I have offered here a "take" on the special nature of human cognition that may perhaps be congenial to historians of science. It is cobbled together from the previous efforts of many scholars in cultural anthropology (in the situated and distributed cognition traditions especially), archaeology, philosophy, psychology, linguistics, and other sciences. I view the mongrel nature of the ideas as a positive virtue, because they show how to connect the history of science more directly to a wide spectrum of the surrounding sciences, from psychology to anthropology. How we evolved this deep coupling of our cognition with our shared culturally-shaped environment is a topic of first importance for a range of disciplines.[7]

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[7] Niche construction theory offers a pan-species approach to feedback relations between constructed environment and organism (Odling-Smee et al. 2013, Laubichler & Renn 2015); the full human exploitation is going to require special explanation (one factor sketched in Levinson 2022).

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The Shape of the Hanging Chain: Exploring the Limits of Classical Conic Section Geometry

Tilman Sauer[1]

Introduction

The problem of determining the shape of a hanging chain that bends under its own weight and is perfectly flexible but not extendible is a famous problem in the history of calculus. In modern terms, the solution to the problem, the *linea catenaria*, as it came to be called, is a hyperbolic cosine, and hence a transcendental curve in the sense of Gottfried Wilhelm Leibniz (1646–1716). The problem of determining the shape of a hanging rope was explicitly posed by Jacob Bernoulli (1654–1705) at the end of a piece in the *Acta Eruditorum* of May 1690, in which he gave a solution of the problem of the isochrone. There he wrote:

> In turn, this problem is proposed:
> *To find which curve is made by a slack rope that is freely suspended between two fixed points.* I am assuming here that the line is perfectly flexible in all its parts.[2]

In the July issue of the same year, Leibniz took up this problem, sharpened it by stipulating that the rope would retain a fixed length like a chain and not be extendible, and further broadcast the challenge. In his *Ad es, quae J.B. publicavit, responsi* he repeated Bernoulli's question, and then continued:

> This proposed problem has been famous since Galileo's time and as far as I know still remains unsolved. Therefore, I could excuse myself from the obligation, especially since I am very busy with other tasks. However, the kindness of the excellent man persuaded me not to withdraw from his first challenge. So I tackled what until now I had not even attempted before and happily opened the locked doors with my key.[3]

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[2]"Problema vicissim proponendum hoc esto:
Invenire, quam curvam referat funis laxus & inter duo puncta fixa libere suspensus. Sumo autem, funem esse lineam in omnibus suis partibus facillieme flexilem." (Bernoulli, 1690, p. 219)

[3]"Propositum hoc problema inde a Galilaei temporibus celebre, nondum, quod sciam, solutionem accepit. Itaque ab onere imposito jure me possem excusare, praesertim aliis rebus distractissimus. Fecit tamen humanitas clarissimi Viri, ut primae ejus compellationi deesse noluerim. Aggressus itaque sum, quod hactenus ne tentaveram quidem, & occlusos aditus clave mea feliciter reseravi." (Leibniz, 1690, p. 360)

Within a year, three compatible—and correct—solutions were submitted in response to Leibniz's challenge: one by Leibniz himself, of course, another one by Jacob's younger brother John Bernoulli (1667–1748), and a third one by Leibniz's mentor Christiaan Huygens (1629–1695). All three solutions were published back to back in the June 1691 issue of the *Acta*.[4]

As Leibniz had indicated in his challenge, the problem had had an interesting prehistory before it was posed as a challenge in the early years of differential calculus. In their paper *Hunting the White Elephant. When and How did Galilei Discover the Law of Fall*, Renn et al. (2002) discuss extensively Galilei's investigations of the question of whether one can use a hanging chain to represent the shape of a parabola in his *Discorsi* of 1638. In fact, these authors claim that the unpublished fifth day of the *Discorsi* was devoted to the very question of whether the hanging chain is a faithful representation of the parabola. In their analysis they draw extensively on Galilei's unpublished manuscripts and on their careful reconstruction of those manuscripts.

A few years after Galileo's death, the teenaged Huygens, in response to a question raised by Marin Mersenne (1588–1648) but also probably stimulated by a reading of Simon Stevin (1548–1620), gave a geometric proof of the fact that the hanging chain cannot be represented by a parabola. In 1646, Huygens drafted a manuscript, in which he formulated the problem mathematically in terms of five axioms and proceeded to prove geometric lemmata which implied that the geometric shape of the catenary cannot be a parabola. Somewhat ironically, his proof that the catenary line is not a conic section relied itself heavily on classical Apollonian theory of conic sections. Huygens' proof was never published during his life time, and was only made available in Volume 1 of his *Oeuvres complètes* in 1888.

After Huygens' no go theorem, the problem lay dormant for several decades,[5] until it was picked up again, as mentioned, by the early pioneers of the differential calculus.

Galilei's conjecture that the hanging chain represent a parabola

The hanging chain is mentioned twice in the *Discorsi*. At the end of the second day, Salviati advertizes two methods for drawing parabolic lines in an easy and convenient way. The context here is a theorem that asserts that a wooden beam prism cut along a

[4]The history and chronology of the catenaria problem is, in fact, more complex, see Truesdell (1960), Hofmann (1966), Ohly (2004), Palomo (2017). Jacob Bernoulli's original challenge had posed the problem in a more general way, referred to as the "funicularia." It could really be specified in various different ways to include the problems of determining the shapes of either the elastica, the velaria, the lentaria, or the catenaria. For a detailed, recent discussion of Jacob Bernoulli's analyses of the Funicularia, see Alassi (2020a,b).

[5]Fabry (1669) and Pardies (1673) discussed the problem of the hanging chain, see Truesdell (1960) and Ohly (2004) for further discussion.

parabolic line on one of its sides has the same strength as the entire prism. Motivated by this context, Salviati announces

> There are many ways of tracing these curves; I will merely mention the two which are the quickest of all. (Galilei, 1914, p. [185])

The first method to produce a parabola, he continues, then is

> to take a perfectly round brass ball about the size of a walnut and project it along the surface of a metallic mirror held in a nearly upright position, so that the ball in its motion will press slightly upon the mirror and trace out a fine sharp parabolic line; this parabola will grow longer and narrower as the angle of elevation increases. The above experiment furnishes clear and tangible evidence that the path of a projectile is a parabola; [...] (Galilei, 1914, p. [185])

The other method uses a hanging chain:

> The other method of drawing the desired curve upon the face of the prism is the following: Drive two nails into a wall at a convenient height and at the same level; make the distance between these nails twice the width of the rectangle upon which it is desired to trace the semiparabola. Over these two nails hang a light chain of such a length that the depth of its sag is equal to the length of the prism. This chain will assume the form of a parabola, so that if this form be marked by points on the wall we shall have described a complete parabola which can be divided into two equal parts by drawing a vertical line through a point midway between the two nails. The transfer of this curve to the two opposing faces of the prism is a matter of no difficulty; any ordinary mechanic will know how to do it. (Galilei, 1914, p. [186])

Of course, one could always also construct a parabolic curve, using Galileo's sector, i.e., proportional compass:

> By use of the geometrical lines drawn upon our friend's compass, one may easily lay off those points which will locate this same curve upon the same face of the prism. (Galilei, 1914, p. [186])

The second discussion of the hanging chain in the *Discorsi* is found at the end of the fourth day. Here the general context is the parabolic trajectory of motion. An analogy is put forth between the claim that no strictly horizontal motion exists and that a rope can never be stretched to the point of exact straightness:

> (Sagredo:) This phenomenon is the impossibility of stretching a rope in such a way that it shall be at once straight and parallel to the horizon; the fact is that the cord always sags and bends and that no force is sufficient to stretch it perfectly straight.
> (Salviati:) In this case of the rope then, Sagredo, you cease to wonder at the phenomenon because you have its demonstration; but if we consider it with more care we may possibly discover some correspondence between the case of the gun and that of the string. The curvature of the path of the shot fired horizontally appears to result from two forces, one (that of the weapon) drives it horizontally and the other (its own weight) draws it vertically downward. So in stretching the rope you have the force which pulls it horizontally and its own weight which acts downwards. The circumstances in these two cases are, therefore, very *similar*. If then you attribute to the weight of the rope a power and energy [possanza ed energia] sufficient to oppose and overcome any stretching force, no matter how great, why deny this power to the bullet? (Galilei, 1914, p. [309], my emphasis)

So far it seems that Galilei rather uncritically assumed full equivalence between the shape of a hanging chain and a parabola, giving it here also a theoretical justification. But an observation that immediately follows indicates that Galilei was aware of some problem here. Salviati says:

> (Salviati:) Besides I must tell you something which will both surprise and please you, namely, that a cord stretched more or less tightly assumes a curve which *closely approximates* the parabola. This *similarity* is clearly seen if you draw a parabolic curve on a vertical plane and then invert it so that the apex will lie at the bottom and the base remain horizontal; for, on hanging a chain below the base, one end attached to each extremity of the base, you will observe that, on slackening the chain more or less, it bends and fits itself to the parabola; and the coincidence is *more exact in proportion as the parabola is drawn with less curvature* or, so to speak, more stretched; so that *using parabolas described with elevations less than* 45° the chain fits its parabola *almost* perfectly.
> (Sagredo:) Then with a fine chain one would be able to quickly draw many parabolic lines upon a plane surface.
> (Salviati:) Certainly and with no small advantage as I shall show you later. (Galilei, 1914, p. [309], my emphasis)

From these quotes, we can clearly see that Galilei saw the hanging chain as a convenient *tool* to create plane parabolic or near-parabolic curves. Based on dynamical analogies to both the elasticity of beams and the superposition of horizontal and vertical components of motion (or of impressed force), he also had good reason to believe that the tool worked as well as it did. Nevertheless, he clearly knew that the chain line did not *exactly* coincide with a parabola, and he even gave a criterion ("elevations less than 45°") as to when the approximation would visibly break down.

Parabolas and catenaries in Galilei's Ms 72

As it turns out, there is more material to go with in order to assess Galilei's understanding of the issue. In his famous Ms 72,[6] Galilei studied the problem of motion, including attempts to come to grips with parabolic trajectories,[7] and these manuscript pages also show some instances of catenaries.

Folios 42r and 113r show a number of different catenary lines which apparently were produced by copying to paper the shape of a hanging chain with different sags, see Figs. 1 and 2. Renn et al. (2002, pp. 39–40) indeed found out that the shapes of both manuscript folios 42r and 113r match exactly, thus showing "a preserved example of the application of the very technique of drawing supposedly parabolic curves by means of a hanging chain which Galileo describes in the *Discorsi*."

While folio 43r seems to support a reading according to which Galilei believed in the parabolic shape of the catenary, another interesting page of Ms 72 is 107r, see Fig. 3.[8] This page shows two parabola-shaped curves, one again is a fully symmetric curve, another curve only goes from the rightmost point to the base point. Both curves pass through the bottom and through a point at the upper right, but in between they both differ visably. Naylor (1980, p. 554) claimed that "[b]oth curves are catenaries." But Renn et al. (2002, pp. 95ff), who had access to the original folio in Florence, make it quite clear that this is wrong:

> Precise measurements have established beyond any doubt that the symmetrically completed curve is a parabola whereas the deviating curve is a catenary, that is the curve of a hanging chain. (Renn et al., 2002, p. 96)

We can confirm this claim already by looking at the facsimiles. In fact, a numerical analysis of the two curves shows quite clearly that the upper curve is, indeed, a true

[6]The manuscript is most easily accessible through the electronic edition prepared by P. Damerow, P. Galluzzi, J. Renn and coworkers. For the following I have made intensive use of the tools provided by their electronic edition of Ms72. For an in-depth study of the signficance of this manuscript for Galilei's theory of motion, see Büttner (2019).

[7]For a comprehensive exposition of Galilei's work, see Damerow et al. (2004) and further literature cited therein.

[8]For a discussion of this page, see Naylor (1980), Damerow et al. (1992, pp. 150–151), Renn et al. (2002, pp. 95ff).

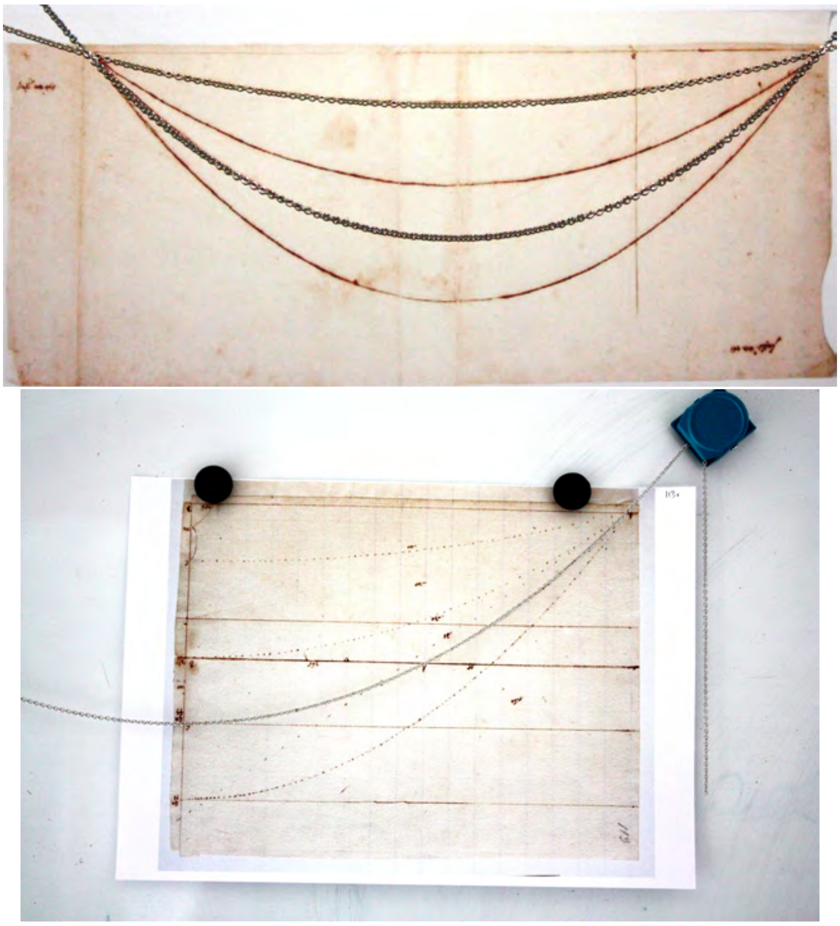

Figure 1: Galilei, Ms 72, f.42r and 113r (©Biblioteca Nazionale Centrale, Florence, by concession of the Ministry of Culture—any further reproduction is prohibited.). The curves on this folios appear to be catenaries as demonstrated by the chains overlayed on the facsimile.

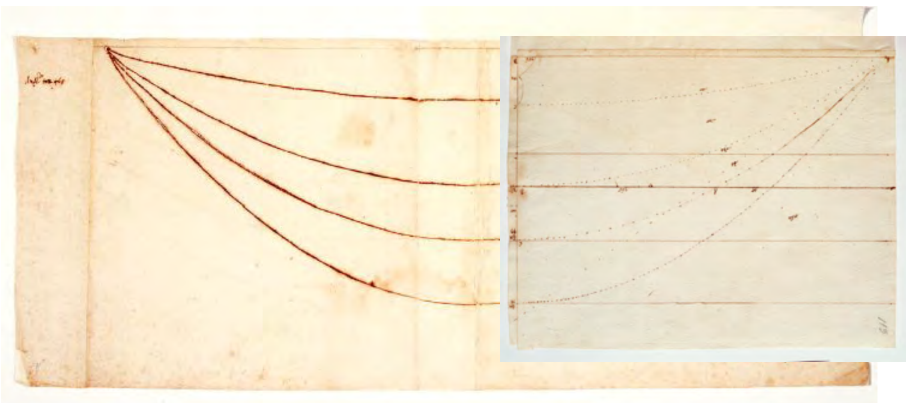

Figure 2: Galilei, Ms 72, f.42r and 113r (©Biblioteca Nazionale Centrale, Florence, by concession of the Ministry of Culture—any further reproduction is prohibited.). The curves generated by hanging chains on these two folios match perfectly.

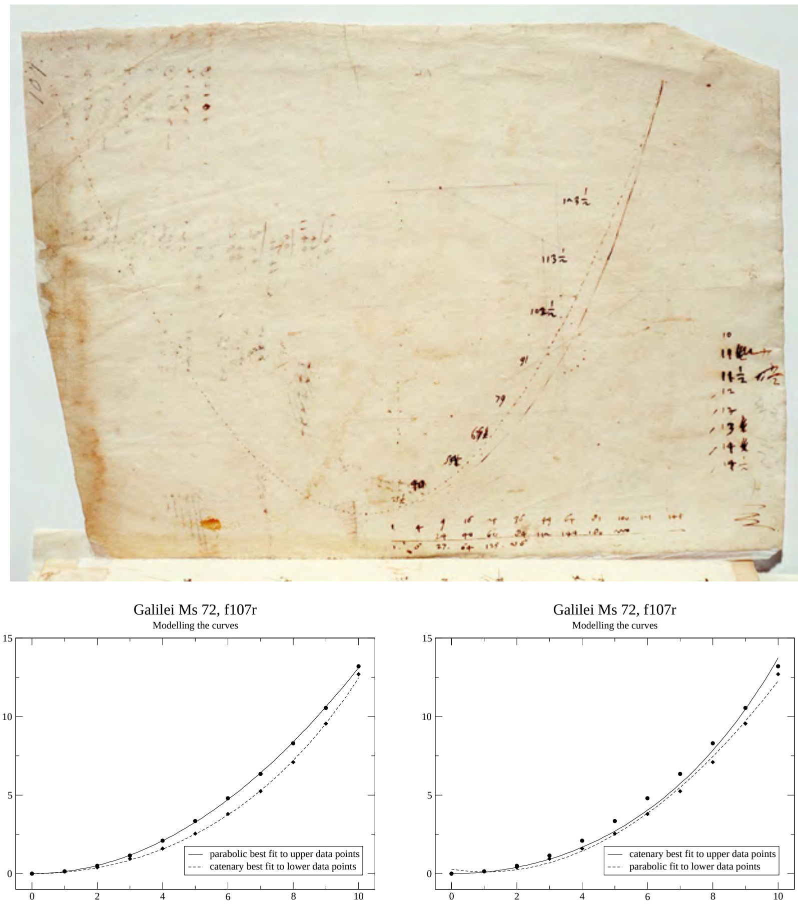

Figure 3: Galilei, Ms 72, f.107r (©Biblioteca Nazionale Centrale, Florence, by concession of the Ministry of Culture—any further reproduction is prohibited.). Facsimilie and fits to the curves. One clearly recognizes that the upper curve is compatible with a parabola, and the lower one with a catenary (left panel). On the other hand, a modelling the upper curve by a catenary and the lower curve by a parabola fits the curves rather poorly (right panel).

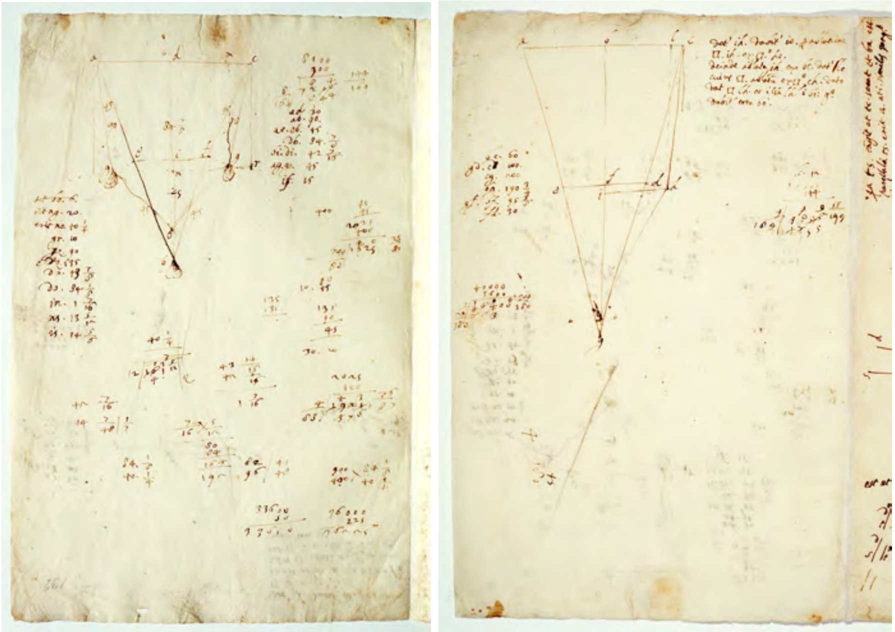

Figure 4: Galilei, Ms 72, f.132r and 132v. (©Biblioteca Nazionale Centrale, Florence, by concession of the Ministry of Culture—any further reproduction is prohibited.).

parabola, while the lower curve is, indeed, a catenary. This conclusion can be reached by modelling both curves with either a catenary

$$y(x) = a \cdot \cosh\left(\frac{x}{a}\right) - a, \tag{1}$$

or a parabola

$$y(x) = b_2 x^2 + b_1 x + b_0. \tag{2}$$

In order to fit Galilei's curves to our model functions, we need to digitize Galilei's data. For that purpose, we impose Cartesian coordinates on the paper and read off the function values as x, y pairs, which allows us to perform a least square non-linear fit to either model.[9]

Renn et al. (2002, pp. 104–113) have also given a reconstruction of the meaning of a sketch visible in the lower part of the verso 107v. For their reconstruction, they rely, again, on the discovery of uninked construction lines that are invisible on the facsimiles. They also established a direct link to another folio, f.132r and f.132v, see Fig. 4. They were able to give an exciting and fully convincing reconstruction of these pages.

The idea is that Galileo tried to analyze the dynamics of a hanging chain, by looking at three weights which are suspended by threads in a configuration displayed in Fig. 5. Let two points B_1 and B_2 at the same height be fixed with a horizontal distance of $2a$ and let

[9]Details will be presented elsewhere.

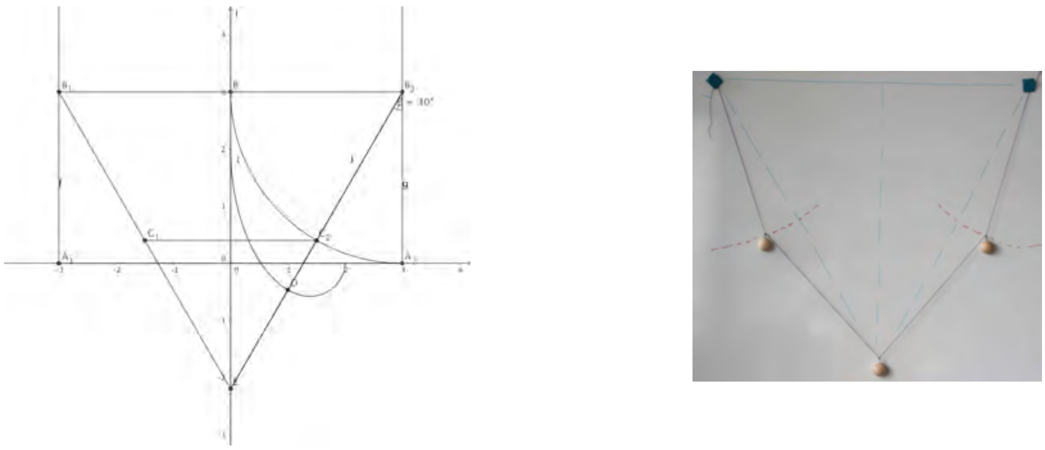

Figure 5: Galilei, Ms 72, reconstruction for the calculations of f.132r and 132v.

a thread hang between these two points with three (equal) weights attached to it, at points C_1, E, and C_2 that are again each a distance a apart from B_1, B_2, and from each other, respectively. The equilibrium configuration for the three weights is displayed on the right hand side of Fig. 5. If the weight E in the middle is pulled down as much as possible, the configuration will be that of a equilateral triangle as displayed on the left hand side of Fig. 5. The center of gravity of the three weights is at two-thirds of the height of the triangle $\Delta C_1 E C_2$.

If the two outer weights B_1, B_2 are moved by pulling them apart (but keeping the axial symmetry of the configuration), starting from the equilateral triangle configuration, the center of gravity will also move. More precisely, it will be located on the vertical central line at a height corresponding to two-thirds of the line connecting B_1 and E. In other words, this point D is on the same height as the center of gravity. This observation allows us to formulate a criterion for the configuration of mechanical equlibrium. It is that configuration where the point D is at a minimum.

Let us introduce orthogonal Cartesian coordinates, placing the origin at a distance a below the line connecting B_1 and B_2, i.e. along the line corresponding to the case where the two outer weights have been pulled to the outside as much as possible. Then, by elementary geometric construction, the coordinates x_D, y_D of the point D are found to be related by the relation

$$y_D = -\sqrt{3ax_D - \frac{9}{4}x_D^2} - \frac{1}{3}\sqrt{a^2 - \frac{9}{4}x_D^2}. \tag{3}$$

This is, as Renn et al. (2002, p. 112) emphasize, “an irreducible equation of fourth degree,” and a calculational determination of the equilibrium configuration was beyond Galilei’s means, at least along this line of attack. In any case, in order to find the equilibrium position one would need some means to determine the point of horizontal tangent to the curve $y_D = y_D(x_D)$. What Galileo does on f132 is to compute the change in the height of the center of gravity for two specific values of pulling the weights apart. But none of the two configurations can be determined to be the equilibrium configuration, even though the center of gravity is in both cases found to be lowered compared with the equilateral triangle configuration.

Clearly, if Galilei's calculations on f107v and f132 were part of an argument to check whether the hanging chain is a parabola or not, the result would have been inconclusive.

In their interpretation, Renn et al. (2002) conclude that Galileo did convince himself in the end that the hanging chain does indeed properly represent a parabolic shape. Their interpretation is supported by further arguments which we will not discuss here. Nevertheless, the claim has a few weaknesses. It seems odd, after all, that Galileo would ignore in the end all indications and empirical evidence which he had found and which clearly displayed a discrepancy between the hanging chain and a parabola. These indications were both of an empirical nature by looking at actual chain data and comparing them to parabolic data, as in f107r, and they were of a theoretical nature as implied by the model of the hanging weights. Finally, the wording of his comments in the *Discorsi* clearly seem to indicate that he was aware that the chain only *approximates* a parabola.

Perhaps a way to express the historical situation more adequately would be to emphasize Galilei's specific interest in looking at the hanging chain. It was for him a tool to create, in a both convenient and accurate way, *material* realizations of the *conceptually defined* curve of a parabola. The question as to what kind of curve the hanging chain represents, or even the question whether the shape of a hanging chain is parabolic or not, might after all not be the adequate expression of what Galilei had in mind. Those questions would take on their prominence in the context of the emergence of the differential calculus, when the *linea catenaria* would emerge as a paradigmatic case of a transcendental curve that could only be handled using the tools of calculus. What Galileo might have had in mind is the much more pragmatic question whether and to what extent a hanging chain can be used to create parabolic curves. Dynamical arguments may have suggested a similarity that would perhaps justify the observation that for only little sag the approximation works quite well. But he also knew, and explicitly cautioned about this, that the chain provides a bad approximation if the sag is getting too pronounced.

Be that as it may, the question whether the hanging chain does represent a parabola continued to be investigated. It turned out that Galileo's model of three hanging ball-shaped weights was very close to a situation that turned to be the key for Huygens' geometric proof that the hanging chain does not represent a parabola.

Huygens's proof that the hanging chain is not a parabola

Four years after Galilei's death, a young student at the University of Leiden took up the problem of investigating the shape of the hanging shape again. Instigated by Mersenne, 17-year old Christiaan Huygens set out to prove rigorously that the hanging chain does not have the shape of a parabola.[10]

The episode is interesting for the role that Huygens's excellent knowledge of the Apollonian theory of conic sections played in his thinking. Conic section theory here played a crucial role at various levels. First, Huygens tried to conceptualize, as did his contemporaries, the hanging chain as an instance of a parabolic shape. Second, he made ingenious use of geometric properties of the parabola in his proof of the incompatibility of the catenary with a parabola. Third, it turns out that the crucial assumption on which his proof his based is also justified, in a rather sophisticated way, by applying geometric knowledge of the ellipse to the mechanical problem of the equilibrium locus of a hanging weight.

Just as Galileo had done, Huygens assumed that he could analyze the shape of a hanging chain or rope by a systematic approximation: first, he would analyze a sequence of hanging weights, attached to a rope or thread at given intervals, then, in a scond step, he would approximate the chain or rope as a sequence of rigid rods of given mass density that are connected to each other by frictionless joints. This basic assumption underlying the reasoning of both Galileo and Huygens, however, was not made explicit.

What was stated explicitly, are four basic premises formulated by Huygens. Based on these premises, Huygens proceeded to prove a number of propositions in which he eventually demonstrated the incompatibility of the shape of the hanging chain with a parabolic conic section.

Huygens laid out his proof in a letter to Mersenne, written in French and published in Vol. 1 of his *Oeuvres complètes* as Nos. 20 and 21. This volume of Huygens' papers also contains another manuscript (No. 22) that pertains to the problem of proving the non-parabolic shape of the hanging chain. This manuscript item differs in several respect from the contents of Nos. 20 and 21. First of all, the entire manuscript of No. 22 was written in Latin, even though it also starts with the same four basic assumptions, called "axiomata" in this Latin version. It then differs from the other numbers in formulating a fifth premise or axiom, and then in proceeding to state and prove three lemmata which, in fact, are then used to prove what in the Latin version is *Propositio 1*, which is equivalent to the fifth proposition in the French version. In other words, the Latin version, presents a more complete account of Huygens's argument.[11]

[10]Bukowski (2008) is an insightful and useful account of Huygens's early work on the hanging chain and will be the starting point for our reconstruction.

[11]See also the version published as *De Catena pendente* in (Huygens, 88ff, Vol. 11, pp. 37–44).

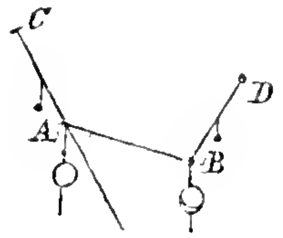

Figure 6: Huygens's figures illustrating his first axiom, asserting the existence of a vertical direction picked out by the gravitational field. (Huygens, 88ff, Vol. 1, pp. 34).

The first of these propositions formulates a crucial assumption for Huygens's subsequent proof, namely the assumption that the extended lines of neighboring elements to one given chain element meet in a point which falls on the "hanging diameter of the weights", i.e. they meet in a point directly underneath the center of gravity of the chain element in question. and, in particular, it gives a justification for Huygens's crucial assumption concerning the 'perpendiculars of the weight'.

Huygens's five axioms

Before going into the details of Huygens's proof let us briefly review the first four basic premises or 'axiomata'. In English translation they read as follows.[12] The first axiom establishes the existence of a homogeneous vertical gravitational field. Specifically, Huygens asserts that the lines of gravitational force do not point to a common center but are all parallel, see Fig. 6:

> 1. All chords, from which a weight is hanging freely are assumed to be parallel to each other; and by the same reasoning the weights are not tending towards the center of the earth but to a plane.[13]

The second axiom idealizes the chord as a massless string where masses have been attached at regular intervals. The situation is illustrated by a sketch given in Fig. 7. The axiom asserts the existence of a unique equilibrium position that satisfies the condition that the center of gravity is at its lowest possible point:

> 2. Two or more weights, such as A and B, attached to the chord $CABD$, which is being held at points C and D, can only come to rest at a unique location:

[12] I am giving the English translations of the Latin versions of (Huygens, 88ff, Vol.1, No.22) but will also cite the French version of (Huygens, 88ff, Vol. 1, No.20) in the notes.

[13] "1. Je suppose donc premierement que toutes les cordes dont quelque gravité depend librement, tendentes au centre de la terre, sont paralleles l'un á l'autre."
"Omnes chordae ex quibus gravitas libere pendet supponantur parallelae inter se; et eadem ratione pondera non ad centrum sed ad planum descendere conari."

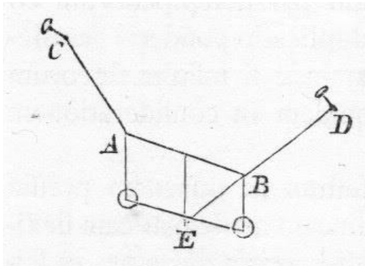

Figure 7: Huygens's figure illustrating his second axiom, asserting the uniqueness of an equilibrium position of hanging weights characterized by being the configuration of lowest center of gravity E. (Huygens, 88ff, Vol. 1, pp. 40).

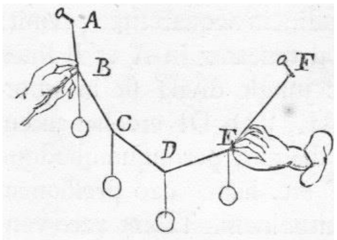

Figure 8: Huygens's figure illustrating his third axiom, asserting that the equilibirum position is stable even when the chain is shortened. (Huygens, 88ff, Vol. 1, pp. 35, 40).

> and this is such that their center of gravity, which here is denoted by E, can descend as much as possible and move towards the plane of the earth.[14]

The third and fourth axioms state that the chain in its equilibrium position can be shortened or extended without changing its equilibrium shape, see Figs. 8, 9.

> 3. If a certain number of weights $BCDE$ are hanging from some chord ABD in that place as required by nature, and if two arbitrary points such as B, E were being held fast at that point where they are located, the location of the remaining weights like C, D will not be changed.[15]
> 4. If certain weights B, C, D are fixed to a chord A, B, C, D, E, and a point D is held fast, and the weights B and C hang in their natural place; then an end point E can be placed in such a way, that after letting the point D go free,

[14]"2. Secondement que deux ou plusieurs gravitez A et B attachez à la corde $CABD$ qui est tenue en C et D, ne peuvent demeurer en repos que d'une seule façon."
"Duae vel plures gravitates, ut A et B, alligatae chordae $CABD$, quae tenetur in punctis C and D, non possunt nisi unico situ quiescere: idque tali ut centrum gravitatis earum, quod hic est E, quantum potest descendat et plano terrae admoveatur."
[15]"3. Troisiemement, que si d'une corde ADF dependent quelques gravitez, selon leur situation naturellement requise, et qu'on arreste quelque deux points B, E, dans leur situation, que cela ne changera point celle des poincts C et D, qui sont entre deux."
"Si ex chorda aliqua ADF pendeant gravitates quotlibet $BCDE$ in situ a natura requesito, et unum duo quaelibet puncta ut B, E, in eo quo sunt situ retineantur, caeterorum ut C, D, situs ideo non mutabitur."

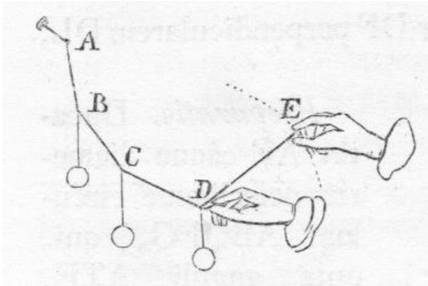

Figure 9: Huygens's figure illustrating his fourth axiom, asserting that the equilibirum position can uniquley be extended without changing its shape. (Huygens, 88ff, Vol. 1, pp. 35, 40).

> it will remain at its same place, and consequently also the other weights B and C.[16]

The French version of No.20 proceeds from here directly to formulate the first proposition. The Latin version, however, includes a fifth axiom. It asserts the equivalence of a straight line to a piece of a circle with infinite radius:

> 5. The finite part of the circumference of a circle of infinite magnitude is the same as a straight line. An example is to be added.[17]

The significance of this fifth axiom will become clear later on.

The theorem of the hanging prism

Then Huygens gives a French version of what we will call the *theorem of the hanging prism*, see Fig. 10:

> Proposition 5:
> If there are so many weights as one wants S, R, P, Q hanging from a chord $ABCD$, I say that MD and BC continued intersect at L on the hanging diameter of the weights P and Q. AB and DC [intersect] at K on the hanging diameter of the weights R and P, and in this way the rest. Because if one holds fast any two points A and D (leaving two others between them like B

[16]"4. Soyent suspendues de la corde $ABCDE$ quelques gravitez comme en B, C, et D, et que celles en B et C pendent selon leur situation naturellement requise quand la corde est tenue au poinct D ou est attachée la gravité G; je suppose qu'il est possible que la main Q tiene en quelque façon le bout E, que le poinct D, demeure en sa mesme place, apres que la main P l'aura quitté."
"Si quotlibet gravitates B, C, D, sint annexae chordae A, B, C, D, E, et retento puncto D. gravitates B et C pendeant in siut naturlai; potest extremitas E ito disponi, ut punctum D derelictum eodem tamen loco maneat, et per consequens etiam reliquae gravitates B et C."

[17]"5. Pars finita circumferentiae circuli infinitae magnitudinis aequipollet rectae lineae, exemplum addendum."

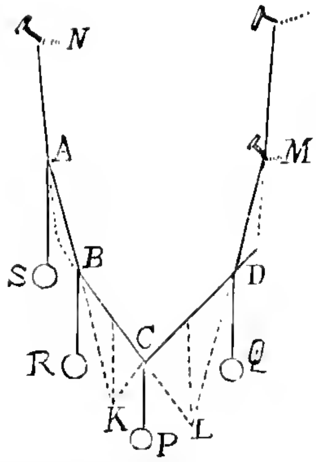

Figure 10: Huygens's figure illustrating his Proposition 5, the theorem of the hanging prism. (Huygens, 88ff, Vol. 1, p.35).

> and *C*), in the same location where they are, this will not change at all the location of the points *B* and *C*; but when the points *A* and *D* will be held fast, the intersection of their continuations *AB* and *DC* must be on the hanging diameter of *R* and *P*. C'est donc signe qu'aussi auparavant elle ij a esté: Et ainsi le prouvera ton des autres.[18]

In the French version Huygens does not give any more justification for this proposition. Instead he continues with further propositions which he continued to formulate in Latin. It turns out that Huygens must have felt the need to provide a justification for this proposition. Before discussing Huygens's own proof, we note that he probably took the proposition directly from Stevin.

Stevin's version of Huygens's proposition

Bukowski gives a source for Huygens's proposition, pointing to Simon Stevin's *L'art ponderaire*, as published in Vol. 4 of his *Oeuvres mathematiques*, which were posthumously edited in 1634 by Albert Girard (Stevin, 1634).[19] In fact, Stevin's works—among other works by Kepler, Vitello, Apollonius, Descartes, Tycho, Copernicus, Clavius, and Viète—had been recommended reading listed to the young Huygens by his first teacher Stampioen

[18]"Si il y a tant de gravitez qu'on veut comme *S*, *R*, *P*, *Q* pendues à une corde *ABCD*, je dis que *MD* et *BC* continuées s'entre-couppent en *L* au diametre pendule des gravitez *P* et *Q*. *AB* et *DC* en *K* au diametre pendule des gravitez *R* et *P* et ainsi du reste. Car si on arreste quelque deux poincts *A* et *D* (en laissent deux autres entre deux comme *B* et *C*), en la situation ou ils sont, cela ne changera poinct celle des poincts *B* et *C*; mais les poincts *A* et *D* estants arrestez, l'intersection des continuées *AB* et *DC* doit estre au diametre pendule des *R* et *P*. C'est donc signe qu'aussi auparavant elle ij a esté: Et ainsi le prouvera ton des autres."

[19]The Dutch original *de beghinselen der weeghconst* was published in Leiden in 1586.

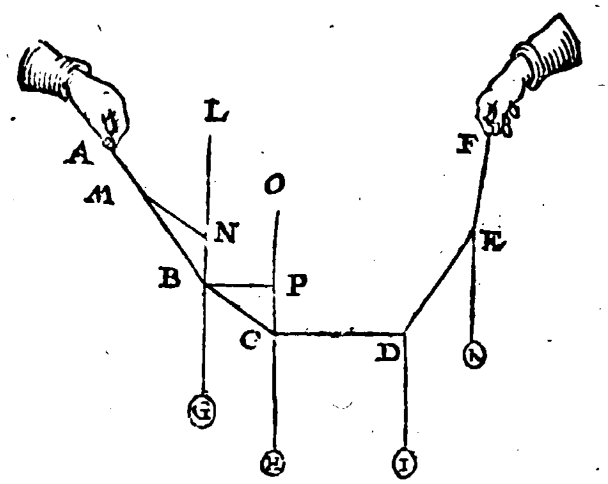

Figure 11: Stevin's figure illustrating his conjecture that the hanging chain displays a parabola. (Stevin, 1634, p. 505)

de Jonge (Huygens, 88ff, Vol. 1,p. 7). And in a letter to Leibniz, written many years later in 1691, Huygens refers to Stevin's works, and, in particular, to Girard's comments of the 1634 edition, saying

> [Girard], however, is wrong in his commentary to the *Statique par cordages* on the subject of the curvature of the line made by his weights, which curvature he pretends to be parabolic and that he would have a proof of it.[20]

In fact, as noted by Yoder (1988, p. 184, n.12), it has become traditional to claim that Mersenne set the problem of the catenary for Huygens after having read Galileo's asertion that it is a parabola" and he points to (Bell, 1947, p. 22) as an example. It seems, however, that Huygens's may first have encountered the problem in Stevin's works. On p. 505, we do find a figure that is strongly reminiscent of Huygens's illustration of his third and fourth axiom, see Fig. 11 The figure here is part of a *Corollaire* to the 27 Propositions of the Statics, which were put together under the title *Spartostatique, ou de l'art ponderaire par Cordages*. At the end of that section, the editor of Stevin's works, added the following note:

> But it must be known what Stevin says, about lines instead of strings, and that he has seen that they are not in straight lines being extended, except for the only case where the string is perpendicular to the horizon; for the other strings, loose or strongly extended, are parabolic lines, (as I had already demonstrated in the past, around the year 1617) and as I will also demonstrate below, at

[20]"[Girard] se trompe pourtant au commentaire sur la Statique par cordages au sujet de la courbure de la ligne qui plie par son poids, la quelle courbure il pretend estre parabolique, et qu'il en a la demonstration." (Huygens, 88ff, Vol. 10, p.188).

> the end of the following corollary, what will come here for the sake of the ornament of this Spartostatic.[21]

> To satisfy the promise that precedes the last corollary, not having the leisure, however, to put here the copy of my entire proof, I will give it again to the public, with my other works, with the help of God, when the search for science will be more commendable than it is now.[22]

It seems that Huygens, as he would recall many years later, may indeed have reacted to this claim by Girard which may thus have prompted him to look into the shape of the hanging chain.

In any case, we find in Stevin's text the definition of a "perpendicular of gravity" ("perpendiculaire de gravité"). In Definition V, Stevin wrote:

> The diameter of gravity of a body is an indefinitely long, straight line, passing through the center of gravity: and that diameter, which is perpendicular to the horizon, is called the perpendicular of gravity.[23]

After a number of such definitions, Stevin also lists a few "postulates" ("petitions"), the second of which will be used in the proof of our theorem. It demands:

> that to mathematical lines one can attach or put such weights as it may be, and they will not tear or break.[24]

With a view to Huyegens's first axiom, it is interesting to note that, in his fifth postulate, Stevin also explicitly takes all vertical lines of gravity to be parallel,

> All plumb lines, (which are perpendicular to the horizon) are to be taken as parallels.[25]

---

[21] "Mais il faut sçavoir que Stevin dit à propos, des lignes au lieu de cordes, & qu'il a bien veu qu'elles ne sont pas en lignes droites estant estenduës, sinon que la seule corde perpendiculaire à l'horizon; car les autres cordes lasches ou fort estenduës, sont lignes paraboliques, (comme j'ay autrefois demonstré, environ l'an 1617.) ainsique je demonstreray cy-apres, à la fin du corollaire suivant, ce qui viendra icy fort à propos pour l'ornement de ceste Spartostatique." (Stevin, 1634, p.508)

[22] "Pour satisfaire à ma promesse qui precede le dernier corollaire, & n'ayant pas le loisir toutefois de mettre icy la copie de ma demonstration entiere, je la donneray un autre fois au public, avec mes autres œuvres, moyennant l'aide de Dieu, lors que la recerche des sciences sera plus recommandable, qu'elle n'est à present." (Stevin, 1634, p.508)

[23] "Diametre de gravité d'un corps, est une ligne droite indefinie, passant de quel costé que ce puisse estre par le centre de gravité: Et tel diametre estant perpendiculaire à l'horizon, s'appelle perpendiculaire de gravité." (Stevin, 1634, p. 434).

[24] "Qu' aux lignes Mathematiques on puisse attacher ou poser telle pesanteur que ce soit, sans qu'elles puissent rompre, ou plier." (Stevin, 1634, p. 436)

[25] "Toutes les linges à plomb, (assavoir perpendiculaires à l'horizon) soyent prises pour paralleles." (Stevin, 1634, p.436).

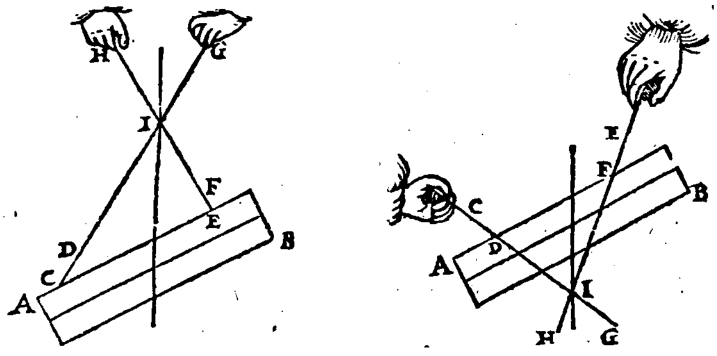

Figure 12: Diagrams illustrating the first two examples for Stevin's theorem. (Stevin, 1634, p. 454)

Later in the book then, in Theorem XVI, Proposition XXV, Stevin explicitly stated the following

> When a beam is suspended by two non-parallel lines, those lines produced will meet on the perpendicular of gravity of the beam.[26]

This theorem is exemplified by four situations, the second of which is almost identical to Huygens's proposition, see Fig. 12. In the first example, Stevin looks at a beam hanging suspended by two no-parallel lines as in the left hand image of Fig. 12. Here the two non-parallel lines converge toward each other and meet in a point above the beam. The second example then is what we are interested in. In this case, the two non-parallel lines diverge above the beam and the beam may either be held in place by holding the two lines at points C, E above the beam. Alternatively, they can also be held at points H, G, which are below the beam. In the latter case, one would have to assume that the "mathematical lines" are to be imagined materially as thin, rigid rods. Stevin wrote

> 2nd Example.
> *The given.* Let AB be a beam to which are attached non-parallel lines CD, EF, which if produced will meet in point I.
> *The required.* It is to be demonstrated that I is in the perpendicular of gravity of the beam AB.[27]

Here is the proof that Stevin provided for this second situation:

> *Proof.*
> Let us assume that DG, FH sustain the beam, then by the second postulate

[26]"Si une colomne est suspendue par deux lignes non paralleles, icelles produictes se rencontreront dans la perpendicle de gravité de colomne." (Stevin, 1634, p. 454).

[27]"2 Exemple. *Le donné*. Soit AB une colomne attachée aux lignes non paralleles CD, EF, lesquelles produites se rencontrent en I." *Le requis*. Il faut demonstrer que I est en la perpendicle de gravité de la colomne AB" (Stevin, 1634, p. 4354).

they will neither bend nor break, and they act on the beam in the same way as DC, EF: because they keep the beam in the same position in a way that is just as well as the other; and whichever points one may take on DG, FH to sustain the beam, this will always be the same; that it will be the common point I that will sustain the beam, it will be on the perpendicular of gravity of the beam; therefore it will be on the said perpendicular of gravity.[28]

We may conclude that the theorem was part of the shared mechanical knowledge, even if it is unclear whether Galilei or Huygens had read the 1586 Dutch original of Stevin's *beghinselen der weeghconst* or else the 1634 French edition of his *Oeuvres mathematiques*. Either publication, in principle, was available to them.

Huygens's proof of the hanging prism theorem

In the Latin version, the proof of this proposition, which is called *Propositio 1* there, proceeds via the following steps

1. a fifth axiom, stating that a finite piece of a circle of infinite radius is equal to a straight line,
2. a lemma 1, describing a way to create an elliptic conic section by moving a rigid rod within two guiding lines of a given angle,
3. an unnumbered lemma, which constructs the tangent to an ellipse in terms of its "applied ordinate",
4. a lemma 2, that constructs the normal and tangent to a point of an ellipse, which was created according to lemma 1, by constructing two perpendiculars at the end points of the moving rod to the legs of the angle.

The proposition is then proven by

5. arguing that the equilibrium position of a chain link under consideration can be found by assuming that the neighboring links are of infinite length such that their motion creates straight lines, so as to allow
6. application of the three lemmata, to show that the center of gravity of the moving chain link moves on an ellipse and is tangent, i.e. lowest for the point of the "perpendicular of the weights"

[28] "*Demonstration*. Prenons que DG, FH soustiennent la colomne, lesquelles par la 2 petition ne rompent ny plient, elles agiront egalement sur la colomne, comme DC, EF: car elles tiennent la colomne en la mesme disposition, en une façon aussi bien qu'en l'autre; et quelques poincts qu'on prenne dans DG, FH pour soustenir la colomne, ce sera toujours la mesme chose; que ce soit I poinct commun qui soustienne la colomne, il sera dans la perpendicle de gravité de la colomne: donc I sera dans ladite perpendicle de gravité." (Stevin, 1634, p. 454–455).

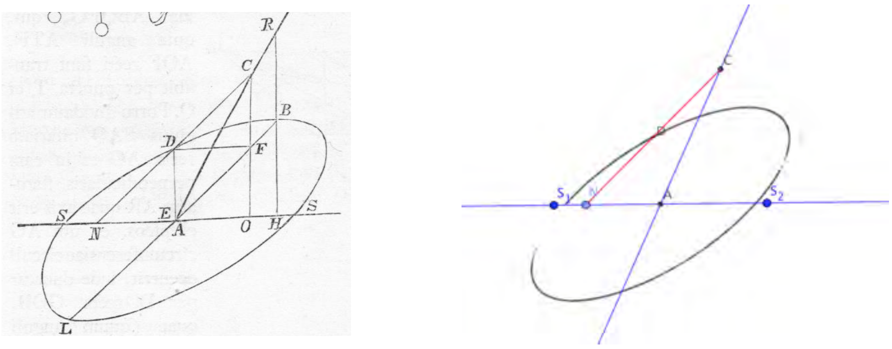

Figure 13: Figure for Huygens's Lemma 1: The mid-point D of a moving rod NDC, gliding with its end points between two lines describes an ellipse-like curve. Huygens sets out to prove that the curve is indeed an ellipse. (Huygens, 88ff, Vol. 1, pp. 41).

Huygens's first lemma: creating an ellipse by a moving rod

In his first lemma, Huygens establishes that the mid-point of a rod that slides with its end points along two straight lines which act as tracks will describe an ellipse. Specifically, with respect to Fig. 13, he claims that the midpoint D of the rod NDC, the end points N and C of which move along SA and AR, describes an ellipse $SDBSL$. If we visualize the moving rod and trace its mid-point, we see that it traces out an ellipse-like curve, see Fig. 13.

To see and to prove that it really is an ellipse, Huygens proceeds as follows. He identifies from the setup of the problem two conjugated diameters and draws an ellipse for those diameters. He then shows that the mid-point D for some arbitrary position of the rod is a point of that ellipse.[29]

If the rod slides between lines that are orthogonal to each other, Huygens's lemma is a case of a well-known ancient ellipsograph, known as the Archimedean trommel. Huygens may have taken his proof for the more general case from a contemporaneous publication on more sophisticated material devices for drawing conic sections by his Leyden mentor Frans van Schooten (1615–1660), entitled *de organica conicarum sectionum in plano descriptione tractus* and published in Leyden in the same year (Schooten, 1646).[30]

After having established that the moving rod describes an ellipse, Huygens needs another lemma about ellipses. This one does not get a number and is only called "Lemma". It gives a prescription to construct a tangent to a point on an ellipse, given its *latus transversum* and its *latus rectum*.

Consider an ellipse ACB, as in Fig. 14, with center point F and some point C to which we wish to construct its tangent line. The theorem tells us to draw the ordinate from C

[29]A more detailed reconstruction of this proof will be given elsewhere.

[30]For a discussion of the general problem of the so-called "wonky trommel", using modern techniques of polynomial Gröbner bases, see Sangwin (2009).

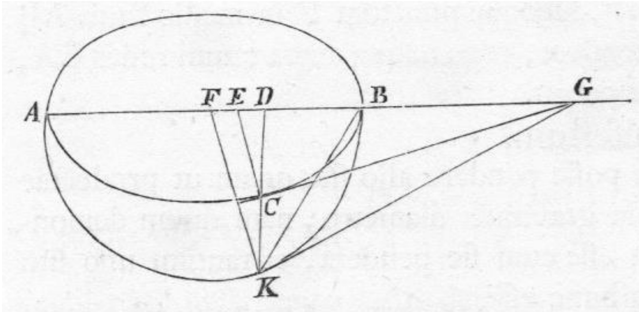

Figure 14: Figure for Huygens's Lemma about tangents to an ellipse, asserting that the line CG is tangent to the ellipse ABC if it is normal to EC and the ratio of FD to ED is the same as the ratio of the *latus transversum* to the *latus rectum*. (Huygens, 88ff, Vol. 1, pp. 43).

onto the diameter AB to obtain the point D.[31] Then one needs to divide the line FD by a point E such that FD to DE is in the same ratio as the *latus transversum* to the *latus rectum*. If one then draws EC, it will be normal to the ellipse, i.e., the perpendicular to it in C will be the required tangent line. It meets the extended diameter AB in a point G.

In order to prove this theorem, Huygens invokes prop. 37 of the first book of the *Conics*. This proposition asserts that if CG is a tangent to the ellipse, then the rectangle $GF \cdot DF$ equals the square of FB.

Huygens's second lemma: constructing the normal to the moving rod ellipse

Then, in his lemma 2, Huygens combines the previous two lemmata to a new one, that tells him how to construct the tangent, and hence also the normal, to an ellipse that is being created by the moving rod device. The figure illustrating his lemma (Fig. 15) shows a confusing variety of auxiliary lines which he needs for his proof. We will explain his proof step by step in the following.

The lemma again sets up an angle A_1AA_2 of guiding tracks which allow a moving rod TDQ to move with its end points along its tracks, see Fig. 16. The midpoint D of the rod TDQ is then moving on an ellipse, according to lemma 1. Huygens lemma 2 now states that one can obtain the normal and tangent to that ellipse by drawing perpendiculars to the tracks at the endpoints T and Q of the rod and connecting their intersection point F with the midpoint D. This line will be normal to the ellipse at D, and hence a perpendicular to it in D will be tangent to the ellipse.

In order to prove that this construction does provide the required tangent, Huygens

[31] In Fig. 14, where AB is drawn as the major axis, the ordinate is given by the perpendicular on AB through C. If AB were just a diameter, the ordinate would be a parallel to the conjugate diameter, which can be obtained by constructing the tangent to the ellipse at B or by joining the middle points of chords parallel to the first diameter. Huygens lemma only works in the case that the diameter is also the axis.

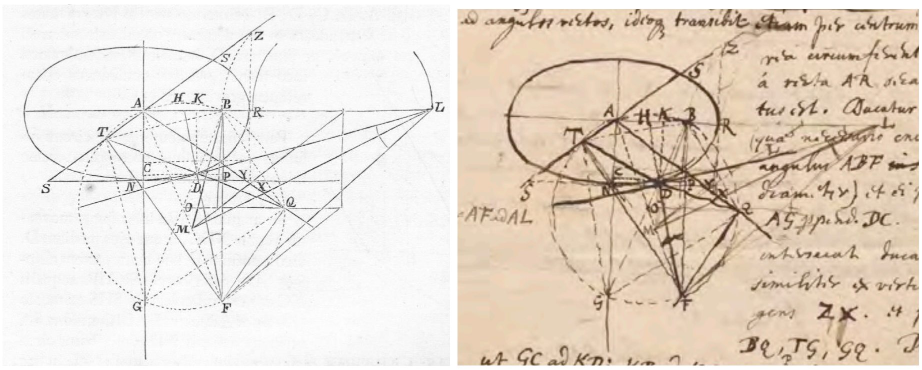

Figure 15: Figure for Huygens's Lemma 2. The ellipse created by the midpoint D of the moving rod TDQ has a tangent in the line DL which is normal to the line DF where F is the intersection of lines TF and TQ which are perpendicular to AT and AQ, respectively. (Huygens, 88ff, Vol. 1, pp. 41) and (© University of Leiden, Codices Hugeniani, Signatur: HUG26, f.131v.)

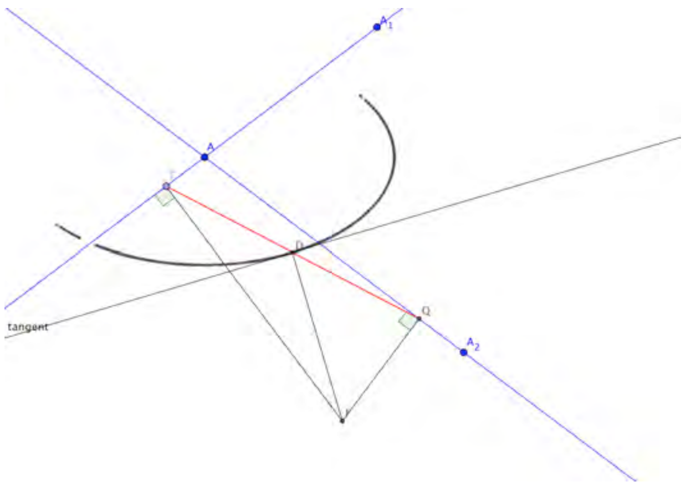

Figure 16: Figure for Huygens's Lemma 2: A rod TDQ is moving along guiding tracks AA_1 and AA_2, its midpoint D creating an ellipse. Perpendiculars to the guiding tracks meet in F, and the line FD is normal to the ellipse in D.

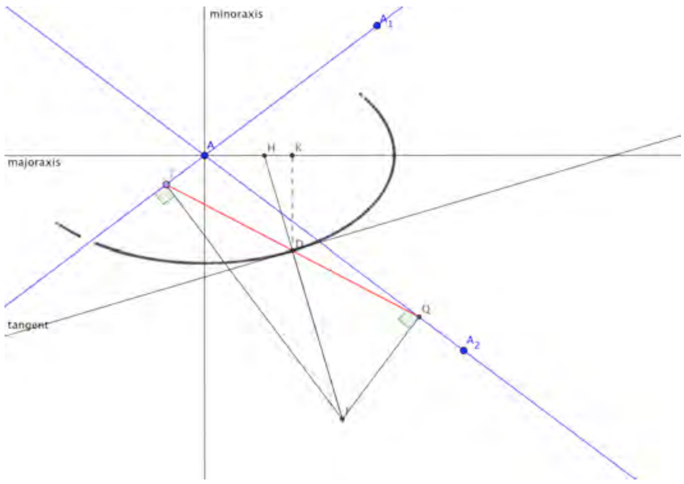

Figure 17: Figure for Huygens's Lemma 2: The principal axes of the ellipse are given by the angle bisectors of the guiding tracks. In order to show that DF is normal to the ellipse, Huygens needs to show that the ratio of AK to AH is equal to the ratio of *latus transversum* to *latus rectum*.

will invoke his lemma on tangents. For that he needs the principal axes of the ellipse, which he gets as angle bisectors of the guiding tracks, and he needs the ordinate DK, a perpendicular to the axis through D, as well as the intersection point H of the extended normal with the axis. According to his lemma, the line HDF will be normal to the ellipse at D if the ratio of AK to HK is as the ratio of the *latus transversum* to the *latus rectum*, i.e.,

$$\frac{AK}{KH} \stackrel{!}{=} \frac{\text{latus transversum}}{\text{latus rectum}}, \tag{4}$$

see Fig. 17.

In order to show that this relation holds for the moving rod construction, Huygens first draws a few auxiliary lines, see Fig. 18. First he connects points A and F and draws a circle over AF with AF as diameter. The circle intersects with the minor axis in point G and in the major axis in point B. Since, by construction ATF and AQF are right angles, the end points T and Q of the (momentary position of the) rod are also points on the circle. Then he connects GB and FB. Finally, he finds the points R and N where the ellipse intersects with the major and minor axis, respectively, and draws perpendiculars XRZ and SNY to the major and minor axes. Huygens looks at the triangles ΔABG and ΔFHB (see Fig. 19) to find

$$\frac{AK}{KB} = \frac{GC}{KD} \tag{5}$$

and

$$\frac{KB}{KH} = \frac{FP}{KD} = \frac{GC}{KD}, \tag{6}$$

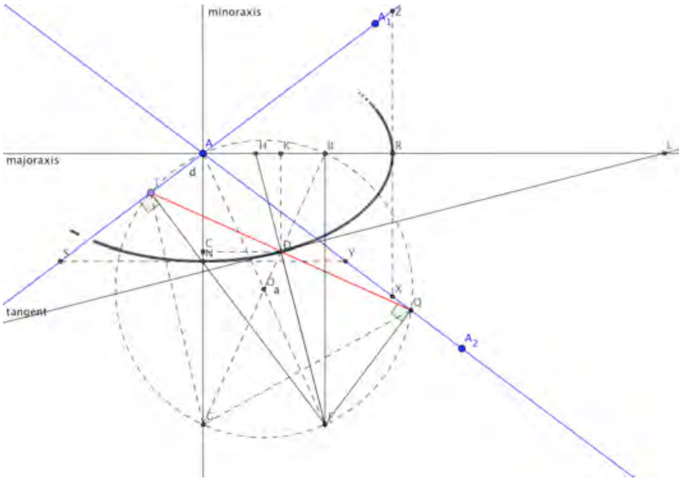

Figure 18: Figure for Huygens's Lemma 2: Auxiliary lines needed to prove the theorem.

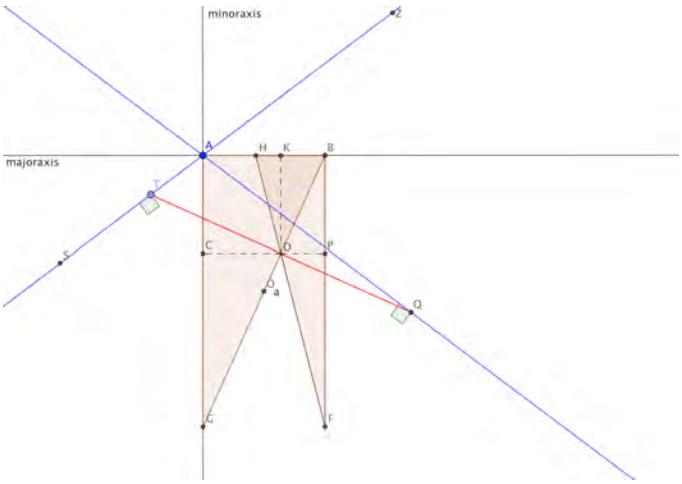

Figure 19: Figure for Huygens's Lemma 2: The triangles used to establish the relations (5)–(9).

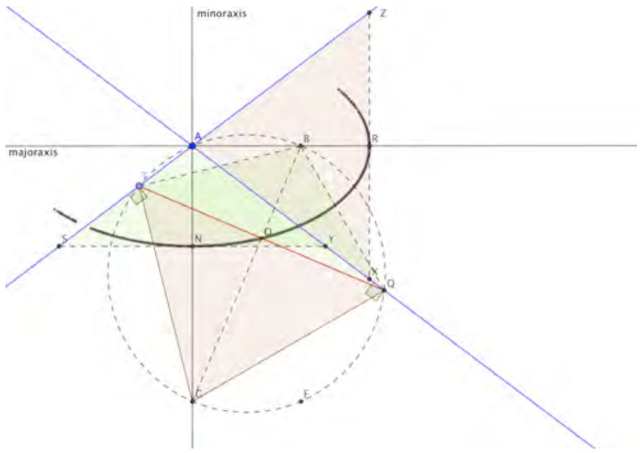

Figure 20: Figure for Huygens's Lemma 2: The triangles used to establish the final relation (11).

and comparing the two latter relations he can conclude that

$$\frac{AK}{KB} = \frac{KB}{KH}. \tag{7}$$

From triangle ΔABG, he also concludes that

$$\frac{AK}{KB} = \frac{GD}{DB}, \tag{8}$$

and therefore, also

$$\frac{AK^2}{KB^2} = \frac{GD^2}{DB^2}. \tag{9}$$

From (7) and (9), Huygens now gets

$$\frac{AK}{KH} = \frac{AK}{KB} \cdot \frac{KB}{KH} = \frac{AK^2}{KB^2} = \frac{GD^2}{DB^2}. \tag{10}$$

For the final step of his argument, Huygens needs to relate this ratio to the *latus transversum* and the *latus rectum*. In order to establish that link, he looks at triangles ΔXAZ and ΔTGQ and at triangles ΔSAY and ΔTBQ, arguing that these pairs of triangles are similar and equal to each other, respectively (see Fig. 20). To see why these triangles are similar and equal, look at ΔXAZ and ΔTGQ. The side XZ is equal to TQ because the point R is obtained by moving the rod TQ to the position XZ. Both triangles are isosceles, since the minor axis is the angle bisector of AT and AQ, and therefore the angles $\sphericalangle TAG$ and $\sphericalangle GAQ$ are the same, which implies that the arcs and chords over TG and GQ are the same. Finally, we have $\sphericalangle TFQ = \sphericalangle TGQ$ and since in the rectangle $TFQA$ the angles at T and Q are each right ones, we have $\sphericalangle TFQ = 180^o - \sphericalangle TAQ = \sphericalangle XAZ$. Therefore, $GD = AR$ and, similarly, $DB = AN$.

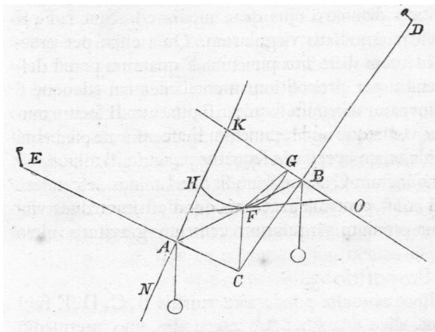

Figure 21: Figure for Huygens's Proposition 1: the theorem of the hanging diameter of weight. (Huygens, 88ff, Vol. 1, pp. 43).

Therefore, Huygens can continue to complete (10) to obtain,

$$\frac{AK}{KH} = \frac{GD^2}{DB^2} = \frac{AR^2}{AN^2} = \frac{\text{latus transversum}}{\text{latus rectum}}. \qquad \square \tag{11}$$

The hanging diameter of weight theorem

With his last lemma, Huygens is finally able to prove the hanging prism theorem, denoted as Proposition 1. For this he imagines a part of a chain where weights are attached at points A and B (see Fig. 21), while the end points E and D are held fixed. Since the equilibrium position of AB remains the same if the lines EA or DB are extended, he imagines those lines to be infinitely long, in any case as long as need be to invoke his fifth axiom, which would allow him to treat the circular segments KAN or KBO as straight lines. If indeed they were straight lines, then a motion of the segment AB which acts as the rod of his previous lemmata would create a part of an ellipse with its midpoint F. If one extends the lines EA and DB, which then meet in C, these lines are at right angles to the straight lines KAN and KBO, respectively. Hence, the line connecting the intersection point C with the midpoint F of the moving rod is orthogonal to the ellipse that F is part of, by invoking lemma 2. If FC is a vertical line, then FO is a horizontal line, and hence the point F cannot go down any further on any allowed motion of the rod AB.

The hanging prism theorem therefore is stated in Huygens's words like this

> Manifestum.
> [...] the two said weights cannot hang in any other place than when the extended lines EA, DB intersect each other in the hanging diameter of the weight, [...][32]

[32] "Manifestum. Hinc sequitur duo dicta pondera non posse pendere alio situ quam ut producta EA, DB invicem intersecent in pendula gravitatis diametro." (Huygens, 88ff, Vol. 1, p. 44)

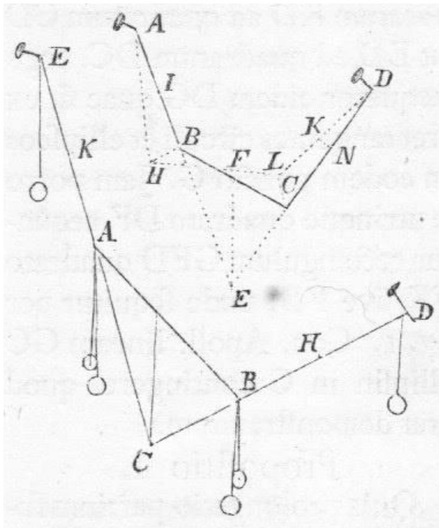

Figure 22: Figure for Huygens's Proposition 2: the proposition transfers the theorem of the hanging diameter of weight, derived as Proposition 1 for hanging weights, to the case of a chain of rigid heavy rods. The proposition refers to the upper sketch. The lower sketch has no corresponding text to it, that would comment on it. (Huygens, 88ff, Vol. 1, pp. 44).

The theorem was proven rigorously using only geometric means and Apollonian theory of conic sections. The only critical assumption that had to be made in order to apply it to the real case of hanging weights was the idealization that the suspending lines EB and DB can be extended at will without affecting the physical applicability of the theorem.

Based on his first proposition, Huygens takes the next steps towards his goal of proving that the hanging chain is not a parabola.

Proof that a parabola is inconsistent with the hanging diameter of weight theorem

In *Propositio 2*, he transfers the claim of the first proposition to the case where rigid rods ("virgulae") are linked rather than weights hanging from a thread (see Fig. 22). He assumes that AB, BC, CD are rigid, heavy rods, similar to each other and equal ("virgulae ponderantes similes invicem et aequales") and they are fixed at their end points A and D. In the equilibrium position, the extended lines AB and DC meet in point E which is on the "hanging diameter of the weight", in this case the center of gravity of the rod BC which is F. In other words, the line FE is a vertical. Imagine the rods are being moved away from this equilibrium position by moving, say, the rod AB to the position AH, which entails that CD moves to the position LD. In this case, the Proposition 1 asserts that the point F in any case goes up. Huygens now argues that the point B descends less than the point C and hence, the center of gravity I of AB descends less than the center of gravity N of CD. In sum, the center of gravity of the entire configuration also elevates on such motion away from the equilibrium.

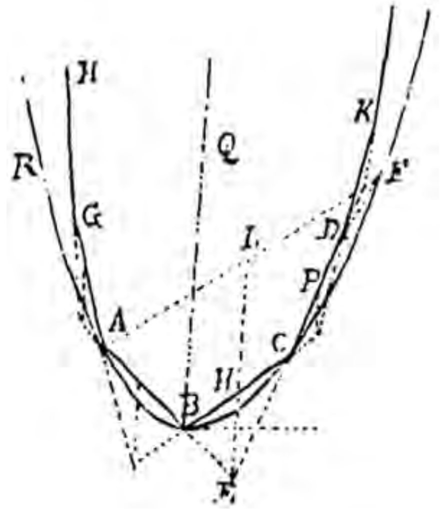

Figure 23: Figure for Huygens's Proposition 8. (Huygens, 88ff, Vol. 1, pp. 36).

The next step that remains to be done is to generalize the theorem inductively to the case of many hanging weights or linked rods. For the hanging weights this is done in *Proposition 3* of (Huygens, 88ff, Vol. 1, No. 22), which is the theorem that is finally identical to the French *Proposition 5* of (Huygens, 88ff, Vol. 1, No. 21), cited above.

In Proposition 8, Huygens finally gives the proof that joints of a hanging chain cannot be located on a parabola. He refers to Fig. 23. He assumes that $HGABCDK$ are joints of a hanging chain of equal length, i.e. $HG = GA = AB = \dots$. He wants to show that "the points $GABCDK$ cannot coincide with the same parabolic line."

The proof proceeds by assuming that there is a parabola $RABCF$ through points ABC that also goes through the as yet undetermined points R and F since the three points ABC already uniquely determine a parabola with a vertical axis. Huygens nowa wants to show that the point D of the chain $HGABCDK$ cannot lie on the parabola $RABCF$. He fixes the point F on the parabola by requiring that it falls on the extension of DE and that the proportion $FC : CE = AB : BE$ holds where E is the intersection of the extended straight line DE with the extended straight line AB. By construction, ΔEBC and ΔEAF are similar triangles and hence AF is parallel to BC. Now the point E is located on the hanging diameter, i.e. a vertical through E divides BC into two equal halves $BH = HC$, and since AF is another secant of the parabola parallel to BC the extended line EH, i.e. the hanging diameter also divides AF into two equal half lines $AL = LF$ since it is a diameter of the parabola. But unless BC is exactly horizontal, the segments BE and EC are unequal, $BC \neq EC$, and therefore also the segments $AB \neq CF$. Since $AB = CD$, it follows that $CD \neq CF$ and hence the points D and F do not coincide. By construction $ECDF$ are all on a straight line, and therefore CDF cannot be on the same convex parabola, since otherwise a straight line has to meet a parabola in three different points. It follows that the parabola $RABCF$ does not go through point D.

The main result has now been achieved: if the points ABC of the hanging chain are located on the uniquely determined parabola through these points, then the next joint D cannot be located on that same parabola. □

40 years later: Huygens, Leibniz, and Bernoulli

The result achieved by the young Huygens was a no go theorem. Applying Apollonian geometric concepts in a most creative way, he was able to prove rigorously, i.e. *more geometrico*, that the curve traced out by a hanging heavy, flexible, non-extendible chain cannot be described by a parabola.

But with this result, the mathematical toolbox of geometric conic section theory in the ancient tradition was exhausted. Note that both Galilei and Huygens specifically asked the question, whether the shape of the catenary might represent or be represented by a conic section. They did not ask, more generally, what kind of curve the *linea catenaria* did represent.

Some forty years later, the situation had changed considerably. When Jacob Bernoulli put the problem on the agenda in 1690, he explicitly asked "which curve" would be formed by a hanging chain. And Leibniz immediately sensed that this is a promising problem to apply his new tools of differential calculus. Among other feats, this calculus had substantially enlarged the spectrum of curves that could be given a detailed and precise mathematical description. Taking up the challenge himself, he eventually succeeded by looking at the chain not as a *discrete* and local mechanical problem. Instead, the breakthrough is provided by conceiving of the chain as a *continuous* line.

61-year old Huygens, confronted anew with the problem of his youth, recalled his earlier treatment of it and gave a description of properties of the curve, utilizing essentially the central theorem of the "hanging diameter of weight" that had been at the core of his earlier no go theorem (Huygens, 1691). Leibniz and the younger of the Bernoulli brothers were using the new differential calculus and were able to derive a differential equation that captured the essential property of the *linea catenaria* (Leibniz, 1691), (Bernoulli, 1691). They did not give the explicit solution in terms of a hyperbolic cosine, $\cosh(x) = (1/2)(e^x + e^{-x})$, which was only introduced as a special function later in the eighteenth century (Loria, 1911). But the differential equation allowed them to deduce the basic properties of the *catenaria*. In fact, the Leibnizian construction amounts essentially to constructing an exponential function, called *linea logarithmica* in Fig. 24, and then mirroring it at the axis in order to construct the arithmetical mean of the two mirrored exponentials as the *catenaria*.

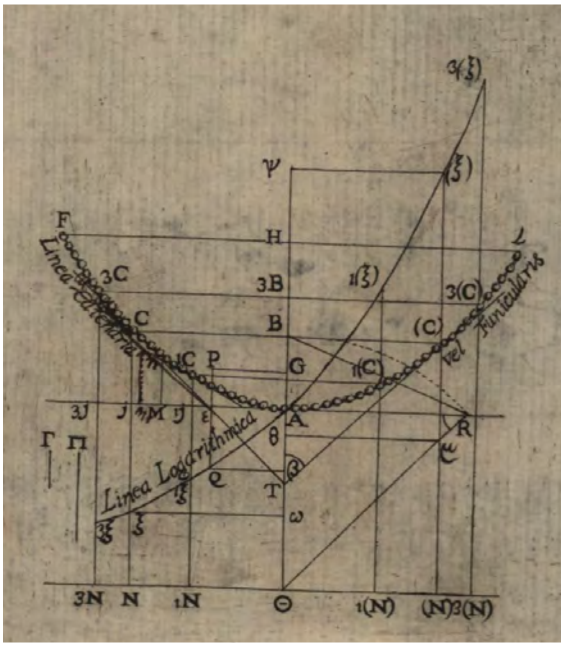

Figure 24: Leibniz's construction of the *Linea Catenaria vel Funicularis*: the *Logarithmica* is, in fact, a graph of the exponential function, with the ratio of the two lines Π and Γ representing Euler's number *e* (Raugh and Probst, 2019). A point ***C***, or (***C***), on the catenaria is then constructed as the arithmetic mean of the two lines ξN and $(\xi)(N)$, where $N\theta = \theta(N)$, by finding the midpoint ***B*** on the axis between ω and Ψ. (Leibniz Archiv, Niedersächsische Landesbibliothek, Hannover, Signatur: LH35,6, 7[27]-14r. Scan GWLB © Public Domain Marked.)

For Leibniz it was a major triumph of his *nova methodus* since his solution connected the *linea catenaria* with the *linea logarihmica*, or else with the exponential function. It demonstrated that his new method of calculus could treat non-algebraic curves, which he termed *transcendental* (Lockwood, 1961), (Blasjö, 2017). He was exceedingly proud of his achievement and published his construction of the *linea catenaria* three times and at different places, once in Latin in the *Acta* (Leibniz, 1691), once in French in the *Journal de Sçavans* (Leibniz, 1692a), and another Latin version for an Italian audience in the *Gionale de' letterati* (Leibniz, 1692b).[33]

The problem of the shape of the hanging chain had started out by Galilei's hope that he could to use chains as a material device to realize representations of a parabolic conic section in his search for a theory of motion. Problems of accuracy had turned the chain from a prospective tool for representing a theoretical geometrical object to becoming itself the object of theoretical investigation. Huygens had used his knowledge of geometric properties of conic sections to demonstrate that the hanging chain could not be itself a conic section. With Bernoulli's challenge the hanging chain had become explicitly an object of investigation which allowed testing the new analytical methods of calculus. Indeed, the new calculus could represent the new curve by means of a differential equation which gave rise to concrete geometric constructions of the precise shape of the curve.

When Leibniz set out to advertize his new insight into the specific character of the *linea catenaria*, he suggested to turn the hanging chain again into a material tool. Having shown that the *linea catenaria* is closely connected with the *linea logarithmica*, he claimed that a hanging chain could be used as a material means to compute numerically logarithms, if no logarithmic tables were at hand:

> This may be helpful since during long journeys one may lose ones table of logarithms or maps with logarithmic grid lines [...] In case of need the catenary can serve in its place.[34]

Clearly, the latter suggestion was mere propaganda, and it is hard to imagine that chains were ever used to compute logarithms in any practical context, not the least because Leibniz did not follow up to explicate how the method would work in practical detail (Blasjö, 2016).

With further elaboration of the differential calculus, its algebraization, and the further diversification and refinement of tools and methods, the catenary soon lost its status as a prominent hard-to-solve problem. In fact, another fifty years later, Euler took up the

[33]For German translations of Leibniz's mathematical papers including helpful commentary, see Heß and Babin (2011). For a discussion of Leibniz's solution of the catenary, see Sauer (2023).

[34]"Cela peut servir, parce que dans les grans voyages on peut perdre la table des logaritmes, ou la table logarithiquement graduée [...]. Mais la chainette y pourroit suppléer en cas de besoin." (Leibniz, 1692a, pp. 224f).

problem and solution of the catenary problem as only one example (of more than 100) for applications of his variational calculus (Caratheodory, 1952).

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Science and the Industrial Revolution

Wolfgang Lefèvre[1]

Abstract

The topic "Science and the Industrial Revolution" has been much discussed. This contribution addresses a few aspects of the topic that are of particular interest to the history of science and that, I believe, have not yet been sufficiently discussed. At the center of my short and often very pointed remarks lies the question: How did some sciences—here mechanics and chemistry—develop during the early modern period of the West in such a way that they became what they are today, namely, an indispensable factor of material production and thus of modern civilization?

Introduction

The topic of this session of our conference in honor of Jürgen Renn is "Modes of Evolution of Knowledge." In Jürgen's department at the Max Planck Institute for the History of Science, three principal forms of knowledge are distinguished: *intuitive knowledge*, *practical knowledge*, and *scientific knowledge*.[2]

It is clear that each of these basic types of knowledge has different modes of development and transmission as well as distinct cultural preconditions and contexts, and so on. And even if, as I believe,[3] the use of and dealing with the material means of work must be seen as an essential factor in the development of all of the three types of

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[2] To quote from Jürgen's recent book (Renn 2020, 430): "*intuitive knowledge*: The broadly shared knowledge that results from [the] interaction of humans with their natural and cultural environments in the process of ontogenesis. … Certain aspects of intuitive knowledge may have a universal character. … *practical knowledge*: The knowledge resulting from the experiences of specially trained practitioners. It is generated from the pursuit of specific tasks or the use of specific tools and is characteristic of all kinds of craftmanship (e.g., architecture, medicine, etc.). … When practical knowledge is habitualized, it becomes "automated" and then may resemble implicit knowledge. … scientific knowledge: Knowledge resulting from the exploration of the potentials inherent in the material or symbolic culture of a society within a knowledge economy specifically dedicated to the generation of such knowledge, … ."

[3] See (Damerow and Lefèvre 1981). English in (Damerow 1996, 395–404; Lefèvre 2005).

knowledge, an attempt at outlining general patterns or an overarching law of development common to all of these types of knowledge will not be pursued here.[4]

Rather, I will only address the development of *scientific knowledge*. Additionally, I do not intend to sketch a basic mode of its development thought to be applicable for all fields of science or for all epochs and cultures in which scientific knowledge was produced. Instead, under the title "Science and the Industrial Revolution," I want to pursue a much more specific but still daringly general question: how did some sciences develop during the early modern period of the West in such a way that they became what they are today, namely an indispensable factor of material production and thus of modern civilization?

I. A Turning Point in the History of Sciences

The topic "Science and the Industrial Revolution" has been much discussed in recent years.[5] In this short contribution, I will address a few aspects of the topic that are of interest to the history of science and which, I think, have not been sufficiently discussed so far. Given the time restriction, I will do this only very pointedly and often simply present theses.

First of all, science neither generated nor induced the Industrial Revolution, nor did the Industrial Revolution generate or induce sciences that were applicable in material production. This statement is, I think, uncontroversial among historians, whether economic historians or historians of science. Less discussed and perhaps not quite as undisputed may be the statement: The industrial mode of production stabilized science by making it a factor of the material production. Science, in turn, stabilized the Industrial Revolution and became, and still is, a major factor of the ensuing industrial mode of production.[6]

The fact that the sciences have played an important role for the sustainability and further development of the industrial mode of production since the first half of the nineteenth

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[4] See (Lefèvre 1984); (Damerow and Lefèvre 1998; Renn (2020).

[5] For the Industrial Revolution in Britain, see e.g., (Ashworth 2017). For debates on the relationship between science and the Industrial Revolution, see e.g., (Klein 2020, 9–10).

[6] The phrase "Science … industrial mode of production" goes back to Karl Marx, who noted in view of the industrial mode of production that was on the rise in his time: "science becomes a factor, a function so to speak, of the process of production." "*Wie der Productionsproceß zur Anwendung der Wissenschaft, wird umgekehrt die Wissenschaft zu einem Factor, so zu sagen zu einer Function des Productionsprozesses.*" [As the process of production becomes application of science, science, inversely, becomes a factor, a function so to speak, of the process of production.] (MEGA[2] II.3.6 1982, 2060).

century is obvious; think only of the chemical and the electrical industries that arose in the nineteenth century, or of the industries that made possible the electronic inventions and devices that shape our lives today.

But how far did and does the industrial mode of production stabilize the sciences? The principal answer is that the industrial mode of production not only gradually brought about a manifold world of scientific research in industrial enterprises, but it also induced new forms of economic policy adopted by the states which included the establishment of a new and manifold system of institutions of applied sciences—from Ècoles polytechniques in France, or academies like the Bauakademie in Prussia, up to the modern technical universities.[7]

The historical significance of this new situation can hardly be overestimated: For the first time in history, societies became essentially dependent on the sciences and had no choice but to promote them. Consequently, for the first time in their history, the sciences rested on a solid base of existence and development.[8] It must be emphasized that it was not their high quality nor the level achieved in the early modern sciences that guaranteed them either secure continuation at this level or further development. This can be seen in civilizations in history with similarly highly developed sciences (think of Hellenism or the so-called Golden Age of the Arabian culture), which experienced not only stagnation but even a decline of their sciences.[9] That the early modern sciences of the West did not suffer the same fate was essentially due to the fact that, because of the industrial mode of production, they could become an indispensable factor of material production and thus of the life of modern societies.

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[7] See e.g., (Belhoste 2003) and (Klein 2020).

[8] On this basis, the nineteenth century saw an unprecedented upswing and, above all, an unprecedented differentiation of scientific production that affected the social form of science. To indicate this with one example: At the end of the eighteenth century, a scholar like Immanuel Kant was still able to survey and follow the knowledge in contemporary natural sciences in a competent way and even become productive himself in some fields. Such a universal scholarship was unthinkable a few decades later due to the explosive increase of scientific knowledge. For Kant and the sciences, see e.g., (Lefèvre and Wunderlich 2000).

[9] The societies thus addressed—Classical Greece, Ptolemaic Egypt, and the Roman Empire on the one hand, and the societies of the medieval Islamic world (from Persia to Andalusia) on the other—were agrarian societies in which, in addition to the agricultural sector, some highly developed crafts were blooming but not in branches of production where science played an essential role. Therefore, this means that science did not play an essential role in the practical life of these societies but was rather a luxury activity that could be dispensed with. For antiquity, see, e.g., (Marrou 1957), and for the Golden Age, see, e.g., (Hassan 1996); (Chaney 2015; Djebbar 2016).

II. The Question

The initial statement: "Science neither generated nor induced the Industrial Revolution, nor did the Industrial Revolution generate or induce sciences that were applicable in material production," although undisputed among historians, contains a number of problems from the point of view of the history of science that concern the topic of this session—Modes of Evolution of Knowledge.

These problems can be indicated by the following questions:

- If we are right that the Industrial Revolution neither generated nor induced science that was applicable in material production, how were such employable sciences found during the Industrial Revolution just when they were needed?

And if such sciences had actually been available at the time, why did they exist and how had they been developed in such a way that they were applicable in material production or could be transformed into factors of production when the Industrial Revolution initiated the industrial mode of production?

In other words: Why and how had sciences developed in the early modern period before the Industrial Revolution, that is, between the fifteenth and eighteenth centuries, that could be used as or transformed into resources of material production when the Industrial Revolution (or more generally, the industrial mode of production) provided the prerequisite conditions for their deployment?[10]

The legitimacy and urgency of these questions arises when one considers the following:

- It goes without saying: not every science and all scientific knowledge is useful in practical terms; nor does every science have a practical value, let alone prove to be applicable in material production.

[10] The crucial reason why the industrial mode of production allows scientific knowledge to be used in both production and for its improvement is the replacement of manual work by machine work. (Hence the use of scientific theories in other production sectors in which manual work did not play a decisive role—e.g., navigation, chemistry, etc.) Only when the hands and physique of the worker are no longer the central agents of the production process can science be used for the optimal design of machines, their parts, and their combination into a machine system. However, the historical prerequisite for replacing workers' hands with machines was not a scientific but a practical analysis of the traditional production process, namely, the practical analysis that underlay the development of the manufactories in the early modern period. See (Marx and Engels 1962, chap. 12 and 13).

- It also goes without saying that a finalistic argumentation is out of the question, for example, the assumption that during the early modern period some sciences developed into sciences deployable in production for the reason that an industrial revolution would be in need of such sciences somewhat later.
- And, furthermore, the assumption that it had been the demands of material production that pushed the development of some sciences in this direction in the early modern period has been proven questionable, not for methodic but for factual reasons.[11]

Thus, again our question: What was the mode of development of those sciences that could be transformed into resources of material production by and in the wake of the Industrial Revolution? Is there something in and about this mode of development that was decisive in bringing forth sciences that could be transformed in this way? And how can we establish this?

To investigate our question, one could suggest a closer examination of the relationship between scientific and technological knowledge in the early modern period. Even if it is true that sectors of material production barely experienced a need for science-based solutions to problems before the eighteenth century, this does not necessarily mean that the practical knowledge gained and applied in the sphere of material production played no role in the development of some sciences. Thus, a systematic investigation of the relationship between scientific and technological literature may be one suitable way to uncover characteristic and perhaps surprising features of the development of the sciences that were later transformed into resources of production. Having recently undertaken such an investigation,[12] I shall proceed to briefly present some exemplary results for the fields of mechanics and chemistry.

[11] In the early modern period, sectors of production did not face problems that could only be solved with the help of scientific knowledge. This is clear for agriculture, at that time the dominant sector of production. The sector of artisanal production, too, did not experience such problems. In some exceptional sectors of advanced production—particularly mining or daring construction projects—the need for new methods and knowledge for problem-solving was felt, but that was hardly the case before the eighteenth century, i.e., before the beginning of the Industrial Revolution. See (Lefèvre 1978, section I.2).
[12] (Lefèvre 2021).

III. Mechanics

The characteristic pattern of interrelations between learned and practical knowledge observable in the field of mechanics and mechanical engineering in the early modern period can be summarized as follows, whereby I must ask you to be content with keywords and some assertions in this short presentation.[13]

Perhaps the most remarkable aspect of the relationship between theoretical and practical mechanics is that in the early modern period both developed largely independently of each other, with only very few mutual impulses between the two.

The medieval science of mechanics, if there was any, largely ignored the practical mechanics of the age. In turn, medieval practical mechanicians (or anachronistically, engineers) paid no attention to Latin manuscripts on mechanical issues circulating in the distinguished sphere of the universities. However, these engineers were familiar with the so-called mechanical powers (lever, wheel and axel, pulley, inclined plane, wedge, and screw) in whichever way of transmission they had become acquainted with results, not necessarily with the theory of ancient statics.[14]

The revival of studies in theoretical mechanics in the second half of the sixteenth century—Federico Commandino, Giuseppe Ceredi, Guidobaldo del Monte among others—was due not to questions and problems posed by contemporary technological literature on mechanical engineering but to the rediscovery of classical texts (Archimedes, Heron of Alexandria, and the Ps-Aristotelian *Quaestiones mechanicae*). These studies found some resonance among mechanical engineers (Ceredi, Simon Stevin, and others) but proved to be of little help, particularly because they initially excluded dynamical issues.[15]

In the case of gunnery, one encounters a real contact or a kind of commerce between learned and practical mechanics, namely a classical example of a practical problem—the range and trajectory of shots—that became a challenging object for learned mechanics. However, the ensuing theoretical ballistics developed largely in detachment from

[13] For the following, see (Lefèvre 2021, chap. 4 and 5).
[14] Vitruvius's short chapters about machinery, which some engineers already noticed in the fifteenth century, might have been a channel of tradition. See also (Drake and Drabkin 1969, 7).
[15] From the theories on dynamics going back to antiquity, i.e. Aristotle and the theory of impetus, the engineers were at least familiar with the latter, as their reflections on the well-known flywheel show.

practical ballistics and assumed the character of "shooting with ink," as Jochen Büttner put it.[16] It must be emphasized, however, that gunnery was an important impulse on the part of the practice from which the revision and further development of the traditional dynamics emanated. However, the further elaboration of this theory in the seventeenth century, which developed nothing less than the basic concepts of modern (Newtonian) dynamics (momentum, force, inertia, etc.), owed almost nothing to the rich and impressive developments of contemporary mechanical engineering—and *vice versa*.

Thus, the seventeenth-century elaborations of basic conceptions of modern (Newtonian) mechanics cannot be taken as resulting from reflections on the procedures, tools, or the insights and open questions of contemporary engineers or gunners. Rather, these elaborations were bound to follow a particular style of reasoning they inherited from ancient models: the style of deductive (Euclidean) reasoning by mathematical means. The prevailing fruitlessness of the interrelations and interactions between learned and practical mechanics that can be stated on the whole for the early modern period owed much to exactly this style of reasoning on the side of learned mechanics.

Deductive reasoning implied not only certain methodological constraints but also imposed an idealization of the subjects of inquiry: physical objects such as levers or projectiles then became geometrical lines or points. Idealization proved to be a serious obstacle for interactions between scientific and practical mechanics. It was a major reason why practical mechanics was unable to benefit from scientific mechanics for a long period. What is more, this science of mechanics failed when confronted with urgent practical problems such as friction, recoil, or aerodynamic drag. On the other hand, there was a latent and at times even open conflict between the deductive style of reasoning and the orientation of the incipient modern science of mechanics, which was empirical in principle. The solution of this conflict required special forms of access to the experiential world, namely access mediated by experiments, particularly those that procured exact measurements as an operational basis for mathematical reasoning. It was exactly this constellation in which the achievements of practical mechanics became a resource of scientific mechanics, namely in relation to the latter's experimental work.

[16] (Büttner 2017).

Eventually, in the eighteenth century, the new science of mechanics, along with the new tool of mathematical analysis, made it possible to tackle some of the urgent problems of mechanical engineering: measurement of the driving forces of machines, calculation of friction, finding optimal shapes for machine parts like cogs, and so on. This advanced science not only met up with competent mechanical engineers, but also with an advanced technology that allowed the appropriation and translation of useful scientific results. As a result, a new and close relationship between technological and scientific literature on mechanics arose and continued to develop.

In summary, one can say: On largely independent paths of development, both learned and practical mechanics brought about the conditions which together, in interaction with each other, made mechanics a resource of material production. By continuing, revising, and further developing traditional theories of mechanics, learned mechanics of the seventeenth and eighteenth centuries elaborated the theoretical and mathematical means that were suitable to develop solutions to practical problems in advanced sectors of production. On the other hand, advanced practical mechanics of the age produced exact measuring devices and machines – a prerequisite both for an experimental theory of mechanics[17] and for the application of found theories in production.

IV. Chemistry

Let's now turn to Chemistry. The characteristic pattern of interrelations between learned and practical knowledge in the field of chemistry can be briefly summarized by the following features.[18]

The learned counterpart of early modern chemical practices was not a unified body of chemical theories but consisted instead of a few diverse natural philosophical theories of the ultimate constitution of matter. The West had inherited, among a host of barely elaborated so-called alchemistical "theories" of various origins, a number of ancient Greek philosophical theories of the ultimate constitution of matter: an Aristotelian, a neo-Platonic, and a Stoic theory, alongside several atomistic theories. The reception of this

[17] In the case of gunnery, it was experimentation—devised and performed by practitioners—that gave rise to what is known as the Ballistic Revolution of the eighteenth century.
[18] For the following, see (Lefèvre 2021, chap. 3).

heritage by early modern chemists cannot be called anything but eclectic. This is particularly true for Paracelsus and his followers.

As regards specific chemical issues, no learned or theoretical literature existed separately from the technological literature before the seventeenth century. Rather, theoretical explanations and practical descriptions and instructions were, as a rule, arranged in one and the same booklet or manual. Before the eighteenth century, technological and natural philosophical writings were mostly indistinguishable in the field of chemistry. Casual statements about or deliberate and explicit expositions of theoretical assumptions held by an author were part and parcel of almost every significant technological work on chemistry since Brunschwig's *Liber del arte Destillandi* of 1500. In the seventeenth century, it had become the standard for chemical manuals to begin with a theoretical part before going on to the various technical descriptions and instructions.

The relationship between natural philosophical assumptions and chemical practices was one of associations and analogies. The two sides did not build upon one another. Rather, these two sides were largely autonomous, even though the theoretical and practical explanations could be linked with each other. Theoretical terms like extraction, sublimation, or distillation, for example, were referring to technical processes and at the same time were utilized to lend some empirical plausibility to assumptions in the framework of natural philosophical theories of matter, that is, to theories that had originated and developed completely independently of chemical technologies. This holds not only for the inherited ancient philosophical theories but also for early modern theories of the ultimate constitution of matter, the Paracelsian ones as well as the atomistic theories of matter of the seventeenth century. While the categories and key assumptions of these philosophical theories of matter owed nothing to developments of chemical procedures or the expansion of the multitude of substances they processed, these technological developments owed nothing to these theories either.

Around 1700, a new type of theory began to emerge. It was not yet again another philosophical theory of matter but a "chemical theory" in the modern sense of the term, that is, a theory of the laws that rule the behavior and interactions of chemical substances. This theory resulted from reflections on metallurgical processes and above all on new procedures of salt production—reflections based on a rich technological literature on these processes and procedures.

Apparently two different relationships occurred between learned and practical knowledge in the field of early modern chemistry: first, up to the seventeenth century, the relationship between practical knowledge and natural philosophical theories of matter and second, from the mid-seventeenth century on, the relationship between practical knowledge and an emerging theory of certain interactions of chemical substances.

The first relationship, between practical knowledge and natural philosophical theories, proved to be bewilderingly paradoxical. On the one hand, both sides used the other as a kind of resource: the technological literature used philosophical doctrines when interpreting natural philosophically technical processes such as distillations and, conversely, philosophical explanations used such processes as illustrations and examples of their statements. On the other hand, the two sides did not impact one another: technological developments owed nothing to the philosophical theories of matter and technological developments had no effect on the doctrines of these philosophical theories. This also holds for the attempts of certain seventeenth-century chemists to conceptualize chemical processes based on resurrected atomistic or newly developed corpuscular theories; these attempts were not a reaction to new technological developments and were of no consequence.[19]

As Ursula Klein has shown,[20] what eventually prompted a new theory of chemical processes was indeed certain technical developments, or more precisely, irritating occurrences brought about by these developments or challenging objects such as the reversibility of certain chemical processes, the preservation of substances in dissolutions, and so on. However, it was not the interpretations of these technical developments in terms of ultimate causes, as proposed by any of the philosophical theories of matter, that led to this new chemical theory, but rather reflections on intermediate and experimentally accessible causes. In its beginnings, this new theory profited less from experiments than from systematic analyses and evaluations of relevant technologies, as described in the rich technological chemical literature of the seventeenth century. The close relationship between technological and scientific literature is rarely as clear as in

[19] (Chalmers 2010).
[20] (Klein 1994).

this case, where a scientific theory evolved through studying and conceptualizing natural laws as utilized in technological processes and documented in technical manuals.

V. Conclusions

This recapitulation of investigations of the interrelations between the early modern technological and scientific literature in the fields of mechanics and chemistry displays not only the wide range but also the main features of these interrelations and, thus also characteristic patterns of the relationship between theoretical and practical knowledge in these two fields. In the case of mechanics (and ballistics), the most striking feature is that challenging technical issues first triggered *internal* developments of a purely theoretical nature on the scientific side before a real commerce between learned and practical mechanics became possible. In the case of chemistry, the salient feature is that for a long period, technological developments only had associative relations with learned knowledge, namely, natural philosophical theories of the ultimate constitution of matter, before some of these technological developments triggered the emergence of a new scientific theory.

These features indicate at the same time ways in which these sciences developed in the early modern period, that is, they indicate the paths or modes of development of scientific knowledge we are looking for.

The fact that the incipient *science of mechanics* developed until the eighteenth century, largely without significant exchange with practical mechanics, was ultimately due to a profound but often overlooked ambivalence regarding the theoretical constitution of this science in the early stage of its development. It started as a deductive (Euclidean style) enterprise with vague or at least barely substantiated claims to empirical validity. Engaged with theoretical revisions and reconstructions of traditional conceptions of dynamics, it initially gave rise to theorems such as the parabolic trajectory of projectiles that testified to achievements in dynamics but could not stand the test of empirical findings or practical applicability. No wonder practical mechanics could not utilize such theorems or enlist the support of this science of mechanics in overcoming its bottlenecks. And conversely, this early modern science of mechanics found little more in contemporary practical mechanics than thought-provoking problems, with no starting points for promising conceptualizations.

Thus, before the eighteenth century, theoretical and practical mechanics developed largely along separate paths. However, each of them on its own path generated achievements that eventually proved to be prerequisite conditions for productive relations in which each could profit from the developments of the other. Concerning theoretical mechanics, it was new, particularly powerful mathematical methods (above all mathematical analysis) that made it possible to tackle intricate empirical problems such as friction, aerodynamical drag, or recoil. And concerning practical mechanics,[21] it was standardized mechanical devices and precision instruments that facilitated the use of theoretical mechanics for practical purposes and opened up access to the empirical world for this science by means of experiments. The latter enabled the science of mechanics to become a real empirical theory that could be transformed into a resource of production.

In the case of the incipient *science of chemistry*, we encountered a completely different path of development. The unity of practical knowledge and theoretical reflections in the framework of traditional natural philosophical theories of the ultimate constitution of matter was an apparent unity only. There was no real interaction between the two sides, but only associative references to each other, which did not go beyond analogies. Practical knowledge and natural philosophical theory belonged to different worlds. A real development took place in the world of practice and practical knowledge, while in the theoretical world one finds re-combinations of traditional natural philosophical theories, but no elaborations that could be compared with the elaboration of theoretical dynamics in mechanics – and this also applies to the revived atomism of the seventeenth century.

A chemical theory in the modern meaning of the term (a theory based on knowledge of laws that rule the interactions of chemical substances) emerged in the decades around 1700 through comparative analyses and reflections on certain chemical operations in

[21] As regards the development of *technologies*, one can generally state that their admirable achievements owed little to traditional or newly-developing scholarly theories before the eighteenth century. One exception is the role played by the inherited theories of statics and mathematics, and particularly by geometry, in a considerable number of practical fields. However, results of these theories were mostly used in an eclectic and pragmatic way by practitioners in these fields, as we have found. Another exception that simultaneously includes a more reciprocal give and take is the development of mathematical instruments, which owed much to mathematics as well as to optics. The development of these instruments was decisive for improvements in practical astronomy, mathematical geography, navigation and cartography, as well as in lower and higher geodesy, that is, for practical fields on the borderline of theoretical disciplines. It is therefore not surprising that these developments frequently led to the invention of instruments for specific scientific purposes such as observation, measuring, and experimentation.

metallurgy and the brand new production of salt by dissolution and precipitation.[22] This science started as an empirical one, by induction rather than deduction, to put it in terms of philosophy of science. And comparably to the abandonment of ancient philosophical cosmology by the new astronomical theory, which was elaborated by Galileo, Kepler, and Newton in the seventeenth century, this new chemical theory eventually led to the abandonment of ancient as well as new philosophical theories of the ultimate constitution of matter as a frame of reference for reflections. In both cases, explanations shifted the focus from an intangible ontological sphere to accessible causes—a "key to understanding the scientific revolution" as Alan Chalmers put it.[23]

The development of the incipient *science of mechanics* is often taken as being paradigmatic for the genesis of modern sciences. Considering the variety of ways in which the modern physical sciences developed in the early modern period, it appears highly questionable for an historian to privilege one of these ways as epitomizing the Scientific Revolution. Only an integrated view or synopsis of this variety can do justice to both the richness and the contingencies of these developments.

As regards the developmental paths of the theories in the fields of mechanics and chemistry considered here, it is suggestive to take up Kuhn's distinction between two main types of early modern natural sciences, the distinction between "classical" or "mathematical" sciences (astronomy, mechanics, optics) on the one side, and "Baconian" or "experimental" sciences (chemistry, electricity, heat, magnetism), on the other.[24] The former type has its indeed classical model in Archimedes' theory of statics; that is, in a mathematical theory of a tool: the lever.[25] With regard to the second type, the chemical theory that arose around 1700 can claim to epitomize Bacon's "experimental

[22] (Klein 1994, sect. V).
[23] When hinting at a certain parallel between the evolutionary paths of astronomy and chemistry with regard to empirical orientation and in relation to inherited philosophical theories of nature (cosmology and philosophical matter theory, respectively), an important difference between the empirical investigations at the beginning of these new sciences must not be overlooked: The observational data studied by astronomy were already geometrically encoded within the celestial coordinate system and could therefore be analyzed by mathematical means. This was not an option in chemistry, where reaction patterns of chemical substances had to be compared, weighed, and ordered to discover the laws governing these patterns. In this respect, chemistry resembled the incipient sciences of magnetism or electricity more than astronomy (Chalmers 2017).
[24] For this distinction, see (Kuhn 1976).
[25] (Lefèvre 2001).

history" (*historia experimentalis*) as an investigation of production processes.[26] It is important to keep this in mind when it comes to understanding how some early modern sciences developed in such a way that they became sciences that could be transformed in resources of material production by and in the wake of the Industrial Revolution.

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[26] For early modern chemistry and "experimental history," see e.g., (Klein 1996) and (Klein and Lefèvre 2007, 22ff.); for early modern chemistry as technoscience, see (Klein 2005).

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Part III. Science and Society

Spread of Scientific Knowledge in Islamicate Societies

Sonja Brentjes[1]

Abstract

The main thesis of this paper is that the spread of scientific knowledge in Islamicate societies has been primarily studied so far in a conceptually very narrow sense by focusing on the study of end results, that is, on extant scientific texts in translation and instruments. The processes and mechanisms of the spread of scientific knowledge have not yet received the same degree of attention. As a result, we do not know in any depth how and why scientific knowledge spread between specific places, regions, or states in concrete historical times. We often only know that it spread. While much work has been done in the last sixty years on editing, translating, and analyzing scientific texts and instruments in specific parts of the Islamicate world and concrete historical periods, most often connected to the rule of a dynasty or an individual ruler, the fact that such texts and instruments spread across space and time, while often known through the extant copies, has been unevenly investigated on a disciplinary level and with regard to the geographical location of the preserved copies. In this paper, I survey the main questions asked in the past half century about the spread of knowledge in Islamicate societies, the shifts and changes that emerged during the last two decades, and the questions that need to be addressed if we wish to acquire a better understanding of this undeniable spread in terms of its how and why.

Core Questions about the Spread of Scientific Knowledge in the Second Half of the Twentieth Century

Research in the second half of the twentieth century about the spread of scientific knowledge focused on two main points, namely translation and reception. The interest in translations already had a very long history, reaching back at the least into the fifteenth

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century. The knowledge that many texts had been translated first from Greek into Arabic, and then from Arabic into Latin had crystalized into the well-known historiographical position that the main or even sole achievement of scholars in Islamicate societies had been their preservation of ancient Greek texts for the renewed scientific activities in (western and central) Europe in the twelfth and thirteenth centuries. Since the late nineteenth and early twentieth centuries, two more languages into which and from which scientific texts (and to a lesser extent instruments) had been translated were included in historical investigations – Syriac and Hebrew. These five languages and the regional centers of their respective translation activities were the focus of intensive philological and scientific analysis in the second half of the twentieth century. Research interests were mainly directed towards ancient Greek texts that had been translated into Arabic (either directly or via a Syriac intermediary translation) and towards Arabic texts that had been translated outside of the Islamicate world into either Latin or Hebrew. Other possible combinations, for instance Arabic or Syriac into Greek, or Latin into Hebrew remained largely outside of such efforts, except for a few special cases. The main questions asked concerned which texts by which author had been translated, who had done the translation and from which language – and with which methods and results, and also who had polished or corrected the translations.

The study of reception concentrated on the question of who else had been involved in the various translations and which impact the translations had had on the later scientific activities. Of special interest were issues such as patronage or cross-religious cooperation.

Five main shortcomings characterize this engagement with the results of the spread of scientific knowledge, although their extent and impact vary with regard to the studied regions and historical periods. The study of translation was limited to the sciences (including of course philosophy) in explicit separation from translation in other fields of knowledge such as history, diplomacy, or religion. Within the range of the sciences, the attention focused mainly on philosophy, medicine, and the mathematical sciences, while most of the occult disciplines were largely neglected. When studying translations of texts in the named disciplinary fields, philological and scientific issues were considered of primary relevance, while other points of analysis, in particular the veracity of claims made in historical sources about the translators, the patrons, the translations, and their properties, remained mostly outside the investigation. The occasionally present references to activities spreading knowledge such as teaching, cooperation with other scholars, intended audiences, or forms of textual organization were not emphasized as important information. From my current view on the history of translation within the Islamicate world, the greatest problem of these past studies of translation and reception as the results of the spread of knowledge is the creation of the historiographical concept of the translation movement. Due to its weak theoretical underpinning and the lack of any engagement with the social sciences, which have been working with the concept of social movements since the early nineteenth century, the rise in quantity of translation activities in comparison to previous periods was taken to signify a social movement. Adhering myself for a long time to the idea of a translation movement in Abbasid Baghdad, I today believe that it is a myth. A survey on its development and recent changes has just been published by Miri Shefer-Mossensohn (2022).

Beyond translation and reception in the regions and languages indicated above, some other themes were pursued to a limited extent. Mostly outside of history of science and more within the field of Persian studies, the relationship between translation and newly composed scientific texts in New Persian was occasionally investigated. The role of Middle Persian in the astrological translations of the eighth century was strongly promoted at the end of the twentieth century in Dimitri Gutas's book *Greek Thought, Arabic Culture*. This endorsement caused severe criticism because some historians of science felt that the relevance of the Greek texts was unduly diminished by Gutas's position. While at first I was deeply impressed by the novelty of the argument and approach, I am now much more skeptical because of the methodological flaws in the evaluation and analysis of the sources.

A second minor theme studied during the twentieth century was the translation of a few Arabic and Persian astronomical and geometrical works derived from Arabic translations of Greek ancestors (Euclid, Ptolemy) into Sanskrit in the early eighteenth century in Mughal India. A third, more continuously pursued theme concerns translations of Latin and vernacular texts, in particular French, German, and English, into Ottoman Turkish from the seventeenth to the late nineteenth century. As in the case of the two main topics covered by the term "translation movement," in these minor fields of study the focus was also on the linguistic and technical aspects of the translations and their subsequent impact. Some attention was paid to contextual aspects such as patronage.

Shifts of Emphasis and Orientation

Altogether, beginning in the late twentieth century, I have observed the unfolding of eight shifts of emphasis and orientation with regard to the study of the spread of scientific knowledge within and among Islamicate societies and between them and some of their neighbors. This process has several roots. Some of these sprouted within the history of science and philosophy in Islamicate societies such as ‘Abd al-Hamid Sabra’s historiographical papers, the above-mentioned book by Gutas, the introduction of the concept of “Ottoman science” by Ekmeleddin Ihsanoğlu, or the work of Julio Samsó and his colleagues on scientific interactions between scholars in North Africa and al-Andalus. Others resulted from developments in neighboring fields such as intellectual history and history of science and philosophy in medieval Latin Europe and its Hebrew communities as well as in Byzantium. A third group of reasons for these shifts grew out of new approaches to the intellectual and cultural histories of religious minorities and cross-communal interactions in China and Mughal India. The most recent shift is still in the making; it concerns the transfer of mathematical and astral knowledge between Muslim groups in North and West Africa.

The term “reception” was increasingly replaced by the concept of “appropriation,” first proposed by Sabra in 1987, indicating a shift from relative passivity to agency (Sabra 1987). For the early Abbasid period, two major conceptual shifts were proposed for the evaluation of the causes or inspirations of the translations and their impact on subsequent developments. The first, coming from Gutas in 1998, saw an Abbasid cultural policy at play, introduced by the second Abbasid caliph who adopted a “royal Sasanian ideology of translation” that allegedly existed several centuries before the rise of the Abbasid caliphate in the Sasanian empire (Gutas 1998). While there is ample narrative evidence above all in Arabic historical sources for a later belief in such an ideology of translation at the Sasanian court, the extant Middle Persian sources are less than clear and show traces of debates among Zoroastrian priests in relationship to intellectual debates at the court of the sixth Abbasid caliph in the second and third decade of the ninth century. A concerted policy of translation as part of legitimation strategies used by the rulers of the newly established Abbasid dynasty from about 762 to 833 is thus probably too strong a shift in historiography. This does not deny, however, the support of individual rulers for specific translation projects or translators until about the end of the ninth century. The second proposal for a conceptual shift was formulated in 2014 by Kevin van Bladel (2014). He argued that Indian astrologers had a dominant position at the Tang court in China, which impressed the Abbasid ambassadors who repeatedly visited the Chinese court in second half of the eighth century and informed Caliph al-Mansur about the impact of those astrologers on Tang politics and culture. These reports motivated the caliph to turn to the astral sciences and their experts as support for his politics. This proposal needs to be taken with caution as well. Not only do Indian names not necessarily signify that the named person or their ancestors had indeed come from a region in South Asia, the impact of Indian astrology at the Tang court was weaker (in comparison to the home-grown variety) than Van Bladel assumes, and ideas of Indian provenance had been transmitted, at least in part, via Buddhist Central Asia. Moreover, the identity of the Abbasid ambassadors is not well known. Van Bladel assumes they were military leaders of Abbasid frontier regions in the East. Whether and what they may have understood (and hence reported) about the practitioners of astrology at the Tang court, their type of knowledge, and its impact on the emperor remains unclear since no textual evidence seems to have been preserved.

Nonetheless, some direct impact from South Asia is known from Arabic historical sources and remnants of translations of Sanskrit medical and astral texts into Arabic from the time after Caliph al-Mansur. To what degree these translations and the Indian scholars in Baghdad in conjunction with the activities of the Barmakid viziers of Caliph Harun al-Rashid and Caliph al-Ma'mun stimulated a caliphal cultural policy of translation is difficult to say. A linear sequence of translations first from Middle Persian, then from Greek and Syriac, and then from Sanskrit certainly is an unreasonable construct. A mixed parallelism with some chronological differences seems to be closer to the events of the second half of the eighth century, whereas in the course of the ninth-century translations from Greek and to a lesser degree from Syriac dominated the scene (Martelli 2022; Shefer-Mossenson 2022).

With regard to the previous focus of translations from Arabic into Latin or Hebrew, in the last two decades new research has highlighted the reciprocal translational activities between Latin and Hebrew. While Gad Freudenthal argued that medieval Jews were not very interested in the knowledge of their Christian neighbors and thus rarely translated Latin works into their language, Fidora, Hames, and Schwartz, Zonta and others, have discovered more such translations than previously known and expected (Freudenthal 2011a, 2011b; Fidora, Hames, and Schwartz 2013; Zonta 2011). In particular, Fidora situated such translations in their intellectual, political, and religious contexts and thus engaged in studies of the processes of knowledge spread (Fidora 2021).

The already established studies of results of the spread of scientific knowledge to and within the Ottoman Empire experienced a particularly strong expansion, crossing the boundaries between history of science and medicine as well as Ottoman studies and history of mapmaking. Ottoman scientific culture as a whole has become a research field both within and beyond history of science. Translation studies now also include translations from Arabic and Persian into Ottoman Turkish, while translations from and into Greek and Slavic languages still seem to be a desideratum. The study of institutions and different political actors has also broadened the components that are investigated with regard to the spread of scientific knowledge across the Ottoman empire and its provinces as well as with its Islamic and Christian neighbors. Important subfields of studies concern Ottoman translators and interpreters, the flow of knowledge with Muslim and Jewish migrants from Europe in the fifteenth and sixteenth centuries, and the flow of knowledge about Islamicate societies, in particular the Ottoman Empire, with the migration of Christian subjects from the Ottoman Empire to Western and Central Europe. In those contexts, aspects of the processes of knowledge spread have been studied by incorporating the roles of cultural brokers and trans-imperial subjects or the various types of non-linguistic translations (Rothman).

Increased attention to the spread of knowledge between Iran and China during Mongol dynasties in both regions, and among Muslim minority scholars and mostly Confucian scholars during the Ming and Qin dynasties, opened new avenues for studying the movements of people across large parts of Asia: the translation of Arabic and Persian astral, medical, and other texts into Chinese, or the emergence of textual practices that combine Islamic and Chinese traditions (Weil 2018, 2020; Isahaya 2013, 2019, 2020). The interest in Mughal translations from Sanskrit into Persian and vice versa led to the creation of a substantive collaborative project in which scientific, historical, occult, and religious texts along with the impact of their translated concepts, methods, doctrines, techniques, and visual representations were investigated in an overarching manner. Consequently, this project rejuvenated philological and technical textual studies by emphasizing cross-linguistic and cross-denominational cooperation together with the integration of production techniques and types of remedies, for instance, in the medical practices of Greco-Islamic medicine on the one hand and Ayurveda on the other (Speziale 2018).[2] Other aspects of the spread of culturally different kinds of scientific knowledge taken into consideration more recently by historians concern the political and social relevance of astrology, patronage for and modernization of the sciences by Muslim rulers in different Indian states before and during the British Raj, the role of libraries, and the spread of mathematical knowledge and metrology in South Indian linguistic cultures (Orthmann 2017; Orthmann and Schmidl 2017; Raina 2022; Habib and Raina 2023; Senthil Babu 2022; IFP 2019).[3]

As stated above, the latest newcomer to the study of the spread of scientific knowledge between Islamicate societies is the study of mathematical and astral texts in manuscript collections in West Africa and their textual and scholarly affiliations to people and schools in North Africa, including Egypt (Djebbar and Moyon 2011; Moyon 2023). Here, the roles of Sub-Saharan trade and of Sufi orders are highlighted as the two most important factors that enabled and structured the travel of people, knowledge, texts, and instruments.

Several of the more recent studies have been undertaken outside of history of science, enriching the understanding of contextual aspects still under-researched within history of science in Islamicate societies. The concrete features of the processes and mechanisms through which scientific knowledge spread over a millennium between societies in Eurasia and Africa also remain understudied. Two specific projects (one by Elaheh Kheirandish, the other by Robert G. Morrison) address this particular topic from two different angles. Kheirandish traces the distribution of optical texts by locating the manuscripts and preserving them in collections across the globe (Kheirandish 1998: this is an ongoing research project). Morrison follows the travels of a single Jewish scholar between the Ottoman capital and its court and the Veneto and its scholars, literati, or merchants to clarify as much as possible the spread of knowledge of non-Ptolemaic

[2] See also http://www.perso-indica.net
[3] See also https://echoes.cooponscitech.in/atlas

models of planetary movements from the Ottoman Empire to Christian Europe, in particular to Padua (Morrison 2014). The third approach was chosen by Langermann and Morrison in a collected volume on texts transiting the eastern Mediterranean (Langermann and Morrison 2016). The contributors to this volume discuss different forms through and in which texts of different cultural origins transited around and across the Mediterranean and how they changed due to the specificities of those forms and their contexts. The forms include, for instance, the oral transmission of texts, the inclusion of fragments of older texts in younger texts, discussions of theories and opinions in letters, or the encryption of ideas or entire texts. The analysis of fragments embedded in younger texts did not only reveal alternative versions of a text canonized by a later commentator, but also revealed traces of so far unknown direct translations of Greek medical material into Hebrew before the ninth century. The papers of this volume document that the study of processes of spread demands painstaking philological analysis and attention to detail. They also highlight that the study of texts can only offer a limited contribution the spread of knowledge or, as Langermann and Morrison called their agenda, the transit of texts. Other contexts need to be investigated too.

Institutions and Niches

In his book *Evolution of Knowledge*, Jürgen Renn highlights, among other factors, the importance of institutions and even niches for the spread of knowledge. At least one contribution to the above-mentioned book by Langermann and Morrison makes the point about niches for religious groups adhering to non-mainstream beliefs (Asatryan 2016). For scientific knowledge that I am familiar with, that of mathematical sciences and mapmaking, I cannot offer an example that would be appropriately described as having survived in a niche. Institutions, however, that stabilized scientific knowledge and enabled its spread existed in Islamicate societies in several forms and numbers. What needs to be stressed though is that not every Islamicate society possessed those institutions and that not every institution always participated in the stabilization and spread of scientific knowledge.

Islamicate societies possessed a variety of institutions that were involved with knowledge production, reproduction, and spread. Some served all those kinds of knowledge that were considered *ʻulūm*, often translated as "sciences" and less often as "fields of knowledge." The most prominent and enduring of them was patronage, primarily exercised by members of the ruling family and their political and administrative elite as well as by libraries. Schools, shrines as recipients of donations, educational travels, discussion circles, and networks dominantly served the spread of religious knowledge. Travel for the search of knowledge is well known from about the ninth to the fifteenth century for scholars from al-Andalus visiting major centers in Egypt, Syria, and Iraq, often in combination with a pilgrimage to Mecca and Medina. With changing political constellations and new power centers, the direction of educational travel shifted repeatedly. The conquest of Anatolia by Turkic groups beginning in the mid-eleventh century resulted in travels to Iran and about two centuries later to Cairo. Similar changes in patterns of educational travels occurred with the Mongol conquest of Central Asia and Iran and with the subsequent dynasties in those regions, like the Timurids and the Safavids. With the rise to power of the Ottomans in the western parts of the Islamicate world, educational travelers flooded the new capital of Constantinople (Istanbul). Others moved to seek better paying patrons or a paid position at a school. In addition to the voluntary travel of scholars, be it for education, positions, or pilgrimage, enforced mobility occurred time and again. Scholars migrated to flee war and religious persecution or were forced to move to the main location of a new ruler. Schools for advanced religious education developed from about the eleventh century as important carriers of scientific knowledge among Muslims.

In addition to the institutions serving primarily religious education, institutions such as observatories and hospitals were specialized carriers of astral, medical, and related knowledge, in particular geometry, arithmetic, philosophy, and alchemy. The work at such institutions included as a rule the exchange of knowledge with colleagues, teaching newcomers and younger practitioners, and voluntary and involuntary travels. Often libraries were built after the opening of such an institution, which sometimes received parts of the princely library as a kind of starting grant. An important type of knowledge spread, already known from early Abbasid Baghdad, was the cooperation between scholars and instrument-makers, or the evolution of the scholarly instrument-maker, who in addition to making astrolabes, globes, and other instruments, wrote texts about instruments and other astral themes. In later centuries, the profile of such men was shaped by school and in-family education in the mathematical sciences at large and could also include architecture. Education provided by family members was a widespread form of knowledge preservation and continuation in all disciplines.

Members of other faith groups did not have access to all of those Islamic institutions, in particular, the schools and shrines and their associated libraries or networks. Their access to patronage changed over time and differed from region to region. Medicine was the field of knowledge for which members of other faith groups had the greatest opportunities of being hired by the ruler, one of his family members, or other wealthy patrons. In the Muslim ruled societies of South Asia, Hindu and Jain practitioners of the astral sciences also profited from princely patronage. Personal interactions between practitioners of medicine, philosophy, the astral disciplines, and perhaps also alchemy are known not only from Baghdad in the eighth, ninth, and tenth centuries, but also from later centuries in Damascus, Cairo, Maragha, or Tabriz. Vollandt and Gibson have discussed such relations across different faith groups for a number of disciplines mainly on the basis of Ibn Abi Usaybi'a's book on the classes of physicians from the thirteenth century (Vollandt and Gibson 2023).

The increasing acceptance and appreciation of parts of scientific knowledge as components of the education of male members of the ruling elites and other social groups is reflected in many different forms. Textual or visual references to such knowledge in historical and biographical literature point to this cultural and social spread as does the production of luxuriously illuminated scientific manuscripts for court libraries and for gift-giving, or the flourishing of instrument workshops producing both simple and affordable astronomical instruments and luxury objects for collection and display. The binding together of texts on scientific and other themes in manuscripts and the formation of personal libraries, including also scientific works, speak of the spread of scientific knowledge among less affluent groups, in particular in educational contexts. Such materials also document the spread of scientific knowledge across all law schools whether Sunni or Shi'i, including those like the Hanbalis (who are usually considered as the most hostile towards this kind of knowledge) as well as among Sufi orders. Since there is no systematic study of the involvement of different religious groups in the (re)production, use, and transfer of scientific knowledge available, such observations should be considered impressionistic, not representative, of all of these groups in their entirety everywhere and in all periods.

These institutions that promoted and structured important parts of the spread of scientific knowledge within and between Islamicate societies operated both synchronically and diachronically in different and differently configurated intellectual, artisanal, and infrastructural frameworks. Spatial and climatic conditions were further factors in the spread of knowledge, or lack thereof. Caravans, guides, resting places for merchants and other travelers, paved roads, and armed protection were some of the infrastructural necessities for overland travel. As David Reisman (2002) has exemplarily shown, Ibn Sina's participation in a philosophical discussion between his students in a different town crucially depended on his speedy access to a courier and a riding animal. Other stories report about many camels transporting thousands of manuscripts through a desert or of armies attacking and looting cities, stealing precious items, including books. The latter took place, for instance, when the Ghaznavid ruler Mahmud attacked Rayy and burnt down the library of the Dar al-ʻilm (The House of Knowledge), founded by princes of the Buyid dynasty (Carey 2001, 29–30). Such destruction also happened when inimical neighborhoods in a town fought with each other. An example is the destruction in 1059 of the well reputed library Dar al-ʻilm bayn al-surayn (The House of Knowledge between the Two Walls), which the Abbasid vizier Abu Nasr b. Ardashir had founded in 993. In addition to religious writings, it contained donated copies of texts on astronomy, geometry, and medicine, among them ʻAbd al-Rahman al-Sufi's *Book on the Constellations* or an Arabic translation of Dioscurides' *De Materia medica* (Carey 2001, 29–30). As a result of the Seljuq attack against Baghdad, Sunni and Shi'i civilians living in the quarter of Karkh took up arms to fight against each other, destroying the library in the process. Some of its books were saved by the Seljuq vizier ʻAmid al-Mulk al-Kunduri (Carey 2001, 30). Before its destruction, a scholar had received permission to read and copy al-Sufi's work, as the colophons in later copies report or suggest (Carey 2001, 31; Bohloul and Brentjes 2022). Al-Sufi's work was also copied in other famous locations such as the Nizamiyya Madrasa in Baghdad, founded by al-Kunduri's successor Nizam al-Mulk. The permission to do so assured the reputation and spread of the copied text, its author, and the codified knowledge (Savage-Smith 2013, 135). Moreover, it is remarkable that the copyists of such manuscripts were mostly scholars whose names are unknown to us today. This suggests a fairly liberal accession policy of the named libraries and their elite owners. Besides libraries, book producing workshops and book markets provided important economic infrastructures for the spread of scientific knowledge. In particular in large cities, they had their own commercial quarter, which in a number of cases was situated in the vicinity of a major mosque to entice the believers to stop, browse one or the other manuscript, and possibly buy one.

Differently configurated intellectual, artisanal, and infrastructural frameworks were for instance the role of book art in specific societies and the types of themes depicted in books. In North Africa, for instance, ornamental art dominated, while in many regions to the East, the artistic themes also included the depiction of architecture, landscapes, humans, animals, plants, celestial bodies, the crafts and instruments, social events, and religious narratives. This broad range of illumination and illustration used in the central and eastern lands of the Islamicate world was also applied in the production of luxury copies of medical, astral, philosophical and mathematical texts, although the quality of the art work and of the used materials differed between regions and workshops. Less richly embellished works were often, albeit not always, produced in provincial towns and thus are witness for the spread of texts on scientific themes. Whether and how they were used in remote areas has, however, not been studied yet. Biographical and bibliographical literature as well as historical works reporting on scholars engaged in the scientific disciplines, their teachers or students, the texts they read or wrote, and the institutions they served as teacher, physician, timekeeper, or in a religious function, differed significantly across the wide expanse of the Islamicate world. In Baghdad, Cordoba, Toledo, Damascus, Cairo, or Istanbul, to name just a few, literati, physicians, religious scholars, or the occasional vizier or prince composed books either encompassing all fields of respected knowledge (including the sciences and their practitioners) or focusing on those scholars who primarily worked on scientific matters. Interest in the latter books can also be found in Iran, where this kind of literature was copied or translated into Persian but not compiled for the Persianate world. Information about scholars of the sciences is thus less available in the eastern parts, where Persian became the main language of culture. When one looks for it, one needs to check works on history or geography. Sometimes, biographical dictionaries of poets also provide one or the other snippet about an astrologer, a physician, or an instrument.

The differences between the types of literature reporting about the sciences and their scholars, texts, instruments, and institutions point to another important frame of the spread and recognition: the narrative forms chosen to present and valorize such information and thus highlight social status and geographical distribution. An oft used story, which could be connected with positive values of admiration or at least recognition, or with negative values of rejection or condemnation, is the one or other form of report about the measurements of 1° meridian sponsored by the Abbasid Caliph al-Ma'mun between 819 and 833. The longevity of this story and its wide spread from the Mediterranean to Central and South Asia are witnesses to the creation of a historical memory reflecting the importance of those scientific activities and their embeddedness in the translation and appropriation of knowledge from pre-Islamic cultures. They also confirm the spread of texts that repeat those stories over thousands of kilometers and across different intellectual circles, because they were also told by religious scholars, historians, or authors of geographical works.

What is Missing and What Should/Could be Done?

In my view, despite the bits and pieces I briefly surveyed so far, the most important issue concerning the spread of knowledge in Islamicate societies is its reduction to the presence of texts and instruments in different places. While this is an important point to be established for any study of the spread of scientific knowledge—and admittedly, we know far too little about it—we need to pay attention to many other issues, above all, the processes that led to knowledge being spread and their properties and preconditions, that is, the mechanisms that encouraged, enabled, or hindered the unfolding of such processes. Often, we do not even know how a text had moved from point A to point B. Digitized manuscripts and catalogs from Turkey seem to suggest that texts moved from some starting point to the intellectual center (mostly the capital) before arriving in towns in the vicinity of the starting point or in those along the road the scholar had traveled. As in many other cases, this impression needs substantive research before it can be declared a reliable characteristic of the process of transportation. Furthermore, the preference for researching intellectual centers, inspired by what has been preserved in major manuscript libraries, prevents the investigation of smaller circuits of spreading knowledge and smaller networks of scholars. There are, however, numerous small holdings available, not only in Europe, but above all in Muslim countries. Their composition and process of coming into being should also be studied and would offer fruitful insights. Extending the net to regions so far widely neglected in the study of the sciences in Islamicate societies such as West and East Africa, the post-Mongol Muslim societies in the steppes north of the Caspian and the Black Seas, post-Timurid Central Asia, many regions of South Asia, but also parts of East and Southeast Asia will contribute in particular to the topic of how, where, and with whom scientific knowledge spread among Islamicate societies in different historical periods.

The probably most profitable ways to come to a better understanding of the spread of scientific knowledge, both in terms of geographical and historical extension and conceptual depth, are the study of such large and eminent manuscript holdings as those in Astan-i Quds, the shrine of Imam Riza, in Mashhad, and a large-scale DH empowered analysis of thousands of manuscripts with scientific manuscripts across the globe. Both approaches demand academic cooperation across disciplines and nations. The first type of project would analyze the provenance of its scientific manuscripts and the histories inscribed both in the institution and in the individual manuscripts, either via the texts bound together in them, or via the paratexts entered into the manuscripts in various phases of their life cycle. The study of the current global distribution of manuscripts with scientific content would also aim at uncovering distribution patterns of this scientific content over time and space and content types, as well as cataloguing and analyzing paratextual information starting from titles, tables of content, foliation, catch words and colophons to marginal comments, beneficial notes, magic formulae, ownership statement, seals, prices, or erasures.

The data we would acquire in either case would be tremendously rich, including names of people linked through family connections or teacher-student relationships, seller-buyer contacts, names of institutions, relations between religious and scientific interests of teachers, student, or owners, the spread across social and linguistic/ethnic groups and the material changes over time that accompanied traveling manuscripts through many hands. We would also obtain more substantial insights into the changes that the scientific knowledge itself underwent. This would include transitions from drawing and painting to the usage of photography or from verbally formulated mathematical knowledge to formulae, the impact that newly translated scientific texts from different parts in Europe had on the existing scientific cultures, the conflicts or forms of coexistence or cooperation with the older and newer forms of knowledge, the coexistence between manuscript and print cultures, the rise of agriculture as a science transformed through economic practice, the political use of ancient Greek, Indian or Mesopotamian forms of physiognomy, or the rise of new forms of occult sciences, and most likely many other things that remain unknown to us today.

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The Theatre of Science in Jesuit Colleges

Rivka Feldhay[1]

My lecture, this afternoon, will be dedicated to the Jesuits, and I am quite sure that everyone sitting in this room has some kind of an image of Jesuits in his or her mind. Over the years, I have learned the extent to which Jesuits have been perceived, in popular imagination, as being hostile to modernity in general, and to the newly emerging science of the seventeenth century in particular. Images, however, sometime succeed in occluding very important historical features of science in the seventeenth century by creating sharp dichotomies between "ancients" and "moderns," as well as between science and religion. Such dichotomies constrain research rather than promote it. In these circumstances, <u>it is often overlooked</u> that between the sixteenth and eighteenth centuries, for more than 200 years, most of the Catholic elite, and Catholic scientists in particular, were educated and socialized in Jesuit schools, where Torricelli, Descartes, Mersenne, Fontenelle, Laplace, Volta, and many others studied (Feingold 2003, 38).

My aim today will be to tell a different story about the Jesuits. I will argue that the Jesuits' religious mission was not anti-science. On the contrary, it was precisely the religious mission that pushed them to explore, teach, criticize, and challenge the ideas of those "savants" identified alongside the "moderns." Thus, what we now call "science" and the Jesuits named "physico-mathematics" (the exploration of physical phenomena through mathematical methods) played a central role in Jesuit education, alongside other innovations such as baroque theatre, ballet, physical education, and sometimes riding. It is this aspect that I will accentuate while speaking about some strategies of doing and transmitting physico-mathematics in a form which I name a "theatre of science."

This paper will consist of two parts:

a) I shall first present a case for solving a mathematical problem in front of large audiences and will analyze its contents and context.

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b) In the second part, I shall analyze the "performative" dimension of the problems and show how it turned into a teaching strategy and a peculiar cultural form. Such analysis, I hope, exposes the social function of the problems within the college as well as between the college and the political environment of the Jesuits.

The Contents and Contexts of one Problem

My example of problem-solving as a public event took place at the provincial congregation of the Jesuits, which convened in Parma on November 5, 1614.

At the time, Parma was a flourishing university town and capital of the rich and successful state of Ranuccio I Farnese. In 1600, the Jesuits were invited by the Farnese family to assist in building up a system of higher education in their state. Eventually, they controlled three interrelated prestigious institutions in the city of Parma: the old university, the Jesuit college, and a *collegio dei nobili*. There, sons of noble families from all over Europe used to spend a few years at a kind of boarding school, where their formation as the future serving elite of many Catholic states was meticulously taken care of.

Let me now quote from a manuscript I found at the Parma archive, where a vivid description of the afternoon routine of the congregation is included:

> After lunch, they took part in the disputations that were four in theology and one in philosophy … Also two most curious problems in mathematics [*due Problemi curiosissimi nella mathematica*] were demonstrated.

As I do not have time to analyze both, I shall only quote the description of the first problem:

> Why, when a ray of sunlight passes through a hole of whatever shape, does it nevertheless show the image of the sun in a circular (form) on the *terminus ad quem*, namely the place where it arrives? (Relazione Parma 1614, 2)[2]

From the point of view of its contents, the problem—in contradistinction to the commentary, the textbook, or the treatise—offered an ideal textual space in which the old Aristotelian conceptual framework was somehow accommodated to allow for teaching, reflecting on, and criticizing the results of the new science.

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[2] All translations from the *Relazione* have been done by the author.

In this case, the problem concerned the image of the sun reflected through a hole of some finite shape and size. It was rooted in the tradition of the Aristotelian *Problemata* and extensively discussed by all great figures of the medieval optical tradition. It was both very ancient and at the same time had acquired new solutions and new meanings in the decade preceding 1614. Thus, our problem had been discussed by Alhacen, Witelo, Bacon, Pecham, as well as by Leonardo da Vinci and the great Renaissance mathematician Francesco Maurolico. However, it was not until Kepler—the first to publish the correct geometrical solution—that the problem was given its fully fledged epistemological meaning.

In his 1604 *Paralipomena ad Vitellionem*—written as a commentary to Witelo, but meant to serve as an introduction to the science of astronomy—Kepler completely dismissed the whole medieval discourse on visual rays and on "species" emanating from sensible objects. Instead, he showed how an image of an external object is first deconstructed and then reconstructed in an inverse form on a screen, but also, passing through the eye's lens, on the human retina (Gal and Chen-Morris 2010). Kepler's solution implied much more than a new use of geometry, as he developed a theory of vision and an epistemology of geometrical entities that justified his solution epistemically. This kind of epistemology had implications for understanding the way images of distant objects were formed not only on a screen but also through the telescope's lenses, as is testified by Kepler's *Dioptrice* from 1611. This problem of the image of an object on a screen or on the retina was of high interest to Jesuit mathematicians in general. Certainly, they were able to construct their own telescope just a few months after the publication of the *Sidereus nuncius* (1611), which they used to confirm most of Galileo's celestial observations. Moreover, the Jesuits soon developed an insatiable appetite for optical instruments, which they used not only for research and teaching purposes, but also for missionary propaganda outside Europe, and for buttressing their status as an intellectual elite (Vermeir 2005; Gorman 2007). In diffusing a variety of practices, then, the Jesuits became defenders of the telescope and other optical instruments against attacks on their epistemic credibility. The decision to perform the problem of a sunray passing through a pinhole could function as an emblem, reminiscent of traditional respectability but simultaneously invoking all the connotations that accompanied the most recent scientific and epistemological debates and controversies of the age.

Now, in spite of the fact that I have not been able to find the original texts of the "curious mathematical problems" presented during the provincial congregation in Parma in November 1614, I believe that I do have rather good knowledge of their contents.

In 1615, the Jesuit mathematician Josephus Blancanus (Giuseppe Bianchani, 1566–1624) published a book named *Aristotelis loca mathematica ex universes ipsius operibus collecta et explicate* (Mathematical topoi [loci] of Aristotle, collected and explicated out of his [own] entire oeuvre). In that book, he collected references to mathematics from the whole Aristotelian corpus: the books of *Physics*, *On the Heavens,* and *Metaphysics*; the biological writings; the works on ethics and politics; and also the questions on mechanics and physical problems attributed to Aristotle.

Blancanus was born in Bologna and joined the Society in 1592. He studied mathematics at the Collegio Romano—the Jesuit university in Rome—with the mathematician Christopher Grienberger, and was then sent to Parma where he spent most of his life teaching mathematics at the Jesuit college there. Among the references he collected in his book on mathematical topoi, there is one taken from Aristotle's *Problemata physica*, which Blancanus presents in the following words:

> Why is the light of the sun, while entering through quadratic or triangular openings, or even through small pinholes, after being received on a plane which is [positioned] far enough from the opening – such as a wall, or a floor – not received in the same shape through which it enters. In spite of the angular entrance, the image of light (*illuminatio*) made/formed on the opposite plane is always round, if the plane is distant enough and the ray of sun is opposite – or perpendicular – to the object. If it is not perpendicular, but oblique, such images will not appear entirely round, but elliptic (Blancanus 1615, 231–2, translation from the Latin by the author).

This problem, Blancanus maintains (quoting now from his book):

> … is rather difficult, for which, as far as I know, not one of the ancients arrived at a perfect solution … in truth, neither Aristotle nor Witelo satisfied the mathematical intellect. Maurolico in his *Photismi* [published only in 1611] and Kepler in his *Paralipomena* [published in 1604] offered probable arguments on this issue in the best way (Blancanus 1615, 231).

It turns out (from the quotations above) that Blancanus divided his exposition into two main parts: In the first, he unfolds two non-satisfying explanations presented as "Aristotelian." Only then, in the second part, does he deal with the modern and correct solution, based on the work of the "*moderni*" Maurolico and Kepler.

Blancanus starts the first part with a geometrical explanation taken from Aristotle's discussion of the sun projecting its crescent shape onto the earth during an eclipse (and not from his original explanation of the pinhole image problem). Here he invokes Aristotle's two cones joined at their apex: one between the sun and the opening, the second between the opening and the earth. He develops an analogy between rays of vision and sunrays, in accordance with the tradition of the medieval perspectivists. To this he adds the element of the distance of the image from the opening that will eventually play a predominant role in his own modern solution. The second explanation he offers is more faithful to Aristotle's original problem. It invokes again the thesis that an angular form seen from a distance looks circular, but this is related to Aristotle's qualitative contention according to which the angular form of the opening is not seen because sunrays falling at sharp angles are weak. My conclusion from the first part is that implicitly—though not explicitly—Blancanus actually points out a challenge that characterized the entire Aristotelian-medieval tradition: namely, the attempt to combine qualitative assumptions about the physical nature of weak or strong sunrays, judged by their angle of fall, with geometrical explanations based on the concept of visual cones.

The second part of the problem opens with a chapter on the "figure of light" (*De Lucis Figuratione*). Here Blancanus formulates four principles from which he would then derive his own solution to the problem. Hence, the very structure of the argument is meant to emphasize its demonstrative, geometrical nature. First, Blancanus restates Ibn al-Haytham's principle—accepted by all medieval perspectivists—that light is emitted in straight lines in all directions, from every point of the body to every point in the medium. Second, he contends that images of light sources tend to fuse in a certain distance from the screen, on the basis of a theorem taken from Maurolico's *Photismi de lumine et umbra (Napoli 1611)*. Third, he explains the principle of intersection on the basis of which both Maurolico and Kepler proved that the image is projected onto the screen in an inverse form. Last, Blancanus stresses that the cause of the circular image on the screen is the circular form of the sun that is transferred through each single point of the opening to the opposite screen.

Blancanus then repeats Maurolico's geometrical proof, which I shall not repeat here. However, two additional remarks at the end of the text testify to his understanding of the meaning of the solution suggested by Maurolico and Kepler. The first remark concerns the inclusion of Kepler's "experiment," described in the second chapter of his Ad Vitellionem *Paralipomena* (Frankfurt 1604) that led him to wipe out Witelo's solution to the ancient problem. Kepler accompanied this description with a harsh critique of his predecessors. Blancanus chose to ignore the critique, but quoted the experiment verbatim:

> I placed a book on a high point to take the place of the shining body. Between it and the floor I set a table having a many-cornered aperture. Next, a thread was sent down from one corner of the book through the aperture and onto the floor; it fell on the floor in such a way that it grazed the edges of the aperture. I traced the path produced and by this method created a figure on the floor similar to the aperture. Likewise, by means of a thread attached to a third and fourth corner of the book, and finally to an indefinite number [of points] along the edge, these resulted on the ground as an indefinite number of thread figures [each having the shape] of the aperture which together produced a great and four cornered [figure having] the shape of the book. (Blancanus 1615, 236, translation from Latin by the author)

In Kepler's experiments, the movements of light are literally described in terms of real entities (geometrical lines) having physical meaning in the world. Blancanus needed such a perception in order to substantiate his claim that the straight lines by means of which mathematicians explained optical phenomena such as the image of the sun on a screen were mathematical entities that "existed as the archetypes of all things both in the mind of the Author of Nature as well as in the human mind." This could justify his additional claim that his explanation is universal and causal, as befitted true "*scientia*." He then hasted to add that such an explanation is relevant not only for solving the ancient problem of the crescent image of the sun seen through a pinhole during eclipse, but that it is also applicable to the very recently discovered phenomenon of sunspots, which appear on the sun's disk not where they are truly located, but on their inverse spot (Blancanus 1615, 236).

The *Mathematical loci*—Blancanus' book from 1615—contained more than the *camera obscura* problem. In another chapter from the same book, the following title appears: "On things that sink in water, along with a new demonstration of that problem of Archimedes, where he investigated a mixture of metals by an insoluble crown." No doubt the chapter refers to Hiero's crown problem mentioned in the Parma manuscript as the second "curious mathematical problem" recited in front of the provincial congregation. However, instead of a text, there is one single printed sentence:

> In this place a commentary is required in the last chapter of *De caelo*, meanwhile, in its place the reader should approach Galileo's Italian discourse on bodies that move in water. (Blancanus 1615, 88)

Blancanus signed the dedicatory letter to his *Loca* in 1614 in Parma. The catalogue of Jesuit teachers of mathematics in Italy shows that he held the chair of mathematics in Parma that year. It is very likely, then, that he wrote the two "curious" problems performed in front of the Congregation and may have even presented them there. Be that as it may, when Blancanus finally attempted to work this problem through into a short treatise and insert it in his book, he was not allowed to do so. In my opinion, this fact indicates a kind of Jesuit sensitivity to the problem of leaving the secure space of the college in an attempt to become an ordinary part of society and its lay culture. Nevertheless, his book saw the light of day in 1615 with the name of the omitted treatise on the index, while the curious reference to Galileo's discourse on things that float or move in water remained in the body of his text.

I would like to draw an interim conclusion from my discussion up to now concerning the dissemination and diffusion of the problems. Blancanus's text containing the *camera obscura* problem, like other Jesuit texts into which I cannot delve now, points out that under the most traditional disguise, new and modern scientific contents were discussed and delivered to students and wider audiences. The problems chosen dealt with topics currently in high demand (like *camera obscura* and floating bodies) at exactly the time Galileo was conducting his campaign against the Aristotelians in Tuscany. Similarly, a student named Johan George Locher—studying with the Jesuit mathematician Christopher Scheiner at the University of Ingolstadt—defended a series of theses in September 1614, among them one on the intersection of light rays caused by a pinhole. His text was immediately printed under the title: *Disquisitiones mathematicae, de controversiis et novitatibus astronomicis.* The reaction of the Jesuit establishment was similar to that experienced by Blancanus: Scheiner got a letter of admonition from the Jesuit General Acquaviva, reminding him to stick to tradition and not meddle with novelties.

The Performative Dimension of Problems

Recitation of problems in the halls of Jesuit colleges and at special public events was one activity, among similar ceremonious practices, that colored Jesuit educational routines with elements of drama and ritual. "Problems," such as the inaugural lectures at the beginning of the academic year, constituted a discursive space that mediated between the closed world of students and boarders and the wider circles of the city, its dignitaries, and rulers. In a text by Christopher Clavius, written in preparation of the first *Ratio studiorum* of 1586, the architect of Jesuit mathematical studies pointed out the cultural thirst for hearing public lectures on mathematics in many cities. By this he meant to convince his colleagues and superiors that the Society would acquire much prestige by training its scholars to develop their talents in speaking about mathematics in public:

> … an effort must be made so that, just like the other disciplines (*facultates*), mathematics also may flourish in our schools (*gymansiis*), so that from this also Ours will become more suited for serving the various interests of the Church; especially since it is not a little unseemly that we lack professors who are capable of presenting a lecture about mathematical topics, *longed for in so many, such famous, cities*.

In addition, an "academy of mathematical topics," namely, an advanced seminar for Jesuit graduates with a talent for mathematics, was established in some of the colleges and universities run by the Jesuits. The academicians were expected to fulfill a public role by speaking about mathematical subjects whenever a special occasion came about:

> Every month or every other month an academician – before a large gathering of students of philosophy and theology – has some famous mathematical problem to work out and afterwards … to defend his solution. (Smolarski 2002, 464)

The recitation of problems was a regular event in Jesuit colleges. It found its pedagogical justification in the necessity to stimulate not only intellectual capacity but also the capacity for presentation and interaction. Jesuit education placed enormous emphasis on the development of the rhetorical skills that accompanied many of the common activities in the college such as repetitions, disputations, public defense of theses, and theatrical productions. The Society also hoped to gain some advantages in the public sphere from such activities. In addition to the goal of training students in presenting their work to their fellow students, as well as to wider circles of dignitaries and intellectuals, the recitation of problems was meant to attract visitors to the College and to contribute to the cultural life of the city. The fusion of educational purposes with more general cultural-political goals found its ultimate expression in the *Ratio*'s rules for the professor of rhetoric, which captures the dramatic, baroque spirit of Jesuit education with the following words:

> Nothing, in fact, so develops resourcefulness of talent as frequent individual practice in speaking from the platform in the lecture hall, in church, and in school … as well as in the refectory. (Farrell 1970, 79)

The lecture hall, the Church, the school tribune, and the spaces allocated for public disputations, mathematical problems, and defense of theses emerge in the text of the *Ratio studiorum* itself as meeting places between novices and externs. Simultaneously, they should be seen as "trading zones" that were used in a process of mediation between the controlled spaces of the Jesuits enclosed behind the gates of the college and the less regulated, sometimes chaotic space of the city bursting with so many different rhythms and a plurality of cultural forms.

The Parma manuscript—a major source for me while writing this contribution—provides substantial evidence for the cultural and social function of reciting problems in public.

The opening day of the Congregation, November 5, 1614, was carefully chosen by the Jesuits after consultation with the Duke. All of them believed that the city would be full of scholars and citizens at this time, namely, at the beginning of the academic year. In preparation, the Jesuit Church was decorated "from top to bottom with its rich drapery." The Duke ordered from his personal "*guardaroba*" beds, tables, chairs, and other paraphernalia necessary for the reception of a large number of guests from the Province. He endowed the Jesuits with a large sum of money (*buona soma di danaro*) for buying generous quantities of food and other useful equipment. On the opening day, His Highness, *Sua altezza Serenissma*, came to the Jesuit church to attend Mass. As Mass was over, he called the Fathers and gave his speech: "speaking with a lot of affection, showing how much respect he held for the Society, and the love with which he cared for them." When the Father Provincial expressed his wish to pay tribute to the Duke before the Congregation began, the Duke sent two coaches and received a small group of Jesuits in his palace. He welcomed them with the same kind of majesty that he paid to great personalities, with torches and candles in the halls and rooms that "equaled the clarity of day" (Relazione Parma 1614). The congregation started in the evening, in presence of the Duke, who took part in almost all of the sessions. It lasted for ten days, during which some of the Jesuits were invited to a ceremonious meal in the palace.

The message for us, historians, is to be found in the details. Being educators, teachers, and intellectuals did not prevent the Jesuits from playing a major social, cultural, and even political role. Presenting themselves as mediators between the religious establishment—the Pope for whom they performed a special vow—and the political authority of the king—the Duke of Parma, Ranuccio Farnese—they well recognized their own power in carrying and representing a scientific message.

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Galileo and His "Six Long Meetings" with Urban VIII in 1624[1]

Kostas Gavroglu[2]

When you have eliminated all which is impossible,
then whatever remains, however improbable, must be the truth.

Arthur Conan Doyle, *The Case-Book of Sherlock Holmes*, 1927

..solutions to the problem of knowledge are embedded within practical solutions to the problem of social order, and that different practical solutions to the problem of social order encapsulate contrasting practical solutions to the problem of knowledge… .We argue that the problem of generating and protecting knowledge is a problem in politics, and, conversely, that the problem of political order always involves solutions to the problem of knowledge

Steven Shapin, Simon Schaffer
Leviathan and the Air Pump, Hobbes, Boyle and the Experimental Life, 1985

Our contemporary awareness of narrative techniques and narrative structures points out the inherent constraints imposed on historians who strive to tell a true story…[and though] we may never know "how it really happened", we should abide by duty to understand the kind of thing that could have happened.

Rivka Feldhay
Galileo and the Church, Political Inquisition or Critical Dialogue?, 1995

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Abstract

In August 1623, Cardinal Maffeo Barberini became Pope Urban VIII, and in April 1624, Galileo went to Rome to pay his respects to his old friend. Before his departure to Florence at the beginning of June 1624, Galileo, in his own words, had "six long meetings" with Urban. There is no record about what they discussed. Galileo scholars, though they mention these "six long meetings," avoid any (detailed) discussion of them. It is assumed that in these audiences with the pope, Galileo most probably raised the question of repealing the 1616 decision of the Congregation, and the pope expressed the view that God's omnipotence implied that Copernican ideas could not be treated as necessary truths, but only as hypotheses.

In this paper, I shall try to test the viability of another hypothesis as to what may have happened in these meetings, and test it against the evidence of later developments. The hypothesis is that Urban asked Galileo to write a book about astronomy in which an Aristotelian philosopher would be in conversation with a mathematically trained natural philosopher, and as the narrative in the book unfolded, the Aristotelian would progressively embrace and promote the ideas that the mathematician had initially articulated. Though at the beginning of the book it was the mathematician who would provide the evidence and the arguments in support of Copernicus' ideas while the philosopher is rather reserved in fully accepting them, as the book progressed, the philosopher would be more and more convinced by the mathematician's evidence and arguments and would continuously show signs of appropriating the new ways of studying and perceiving natural phenomena. By the end of the book, there would emerge a philosopher with a different profile than the one he had at the beginning.

Urban's suggestion aimed at changing the thrust of what was in *The Assayer* and bringing conciliation between the Aristotelians and the newly emerging natural philosophers. This would be a new social consensus concerning the jurisdictions of different socio-professional groups. There were, of course, serious epistemological issues to be settled, and Urban may have felt that these would in the long run be resolved, especially since Grassi's *Libra*, Clavius' program, and Grienberger's works and discussions in the Universities showed signs that at least some Aristotelians were not as inflexible as others.

The plausibility of our hypothesis is further enhanced since it satisfies the following seven criteria: It is compatible with the overall context of other events taking place; it is consistent with the protagonists' direct or indirect involvement in what was happening "out there" and the stake each had in how these intellectual, social and, in effect, political processes will unfold; it can be read as part of an established epistemological "tradition"; it is commensurate with traits that have been persistently revealed in the protagonists' social and political pursuits and activities; it does not stem from the eccentric traits or extreme behaviors of Urban; it is in agreement with respect to all subsequent developments and does not come into any direct conflict with any of the sources; and it enriches existing interpretations and provides explanations to issues that have been overlooked by other scholars.

The Assayer, ready by October 1622, was dedicated to the pope. The dedicatory note expressed in no ambiguous terms what the Linceans wanted to convey to Urban— that this was their collective undertaking. How justifiable is it to "read" the *Assayer* as a text that is exclusively about epistemological interventions and devoid of social and political undertones, as some Galileo scholars do? *The Assayer* had all the characteristics of a manifesto by the Linceans and expressed what was in store for the Aristotelians. And, as is the case with almost all manifestos, this one too articulated a social and political agenda. It was not difficult for Urban to realize that if what was in *The Assayer* was dynamically pursued by the mathematicians, then the ensuing problems within the Church would be very serious indeed. Urban—as any good politician would wish—through his suggestion to Galileo took upon himself the role of harmonizing relations and re-conciliating the warring groups through the formation of a new social alliance.

Our hypothesis helps the further clarification of five issues that are to be addressed in detail: the rush of Galileo to leave Rome in June 1624; the *Reply to Ingoli* of September 1624; why it took six years to complete the *Dialogue*; why issuing the imprimatur for the *Dialogue* took such a long time; and the cryptic "confessions" by the pope to Niccolini in September 1632.

Introduction

Galileo studies—and especially the analyses of the trial of 1633—are haunted by a few words, all thought to have been uttered within a few days of each other at the end of February and beginning of March of 1616: (not to) *Hold, teach, defend ... in any way ... verbally ... or in writing*. They conveyed the decision of the Inquisition[3] on how Galileo should, in the future, deal with the Copernican view that the sun was motionless at the center of the universe and the Earth revolved around the sun and around its own axis. Galileo scholars have scrutinized the various consequences resulting from the different emphasis put on each and all of these fateful words in 1616, putting forth a host of different arguments and nuanced interpretations of one of the most dramatic instances in the history of science.

Historical scholarship has dug deep into the different aspects of Galileo's life and, most significantly, into the events surrounding the 1633 trial. It appears that the crucial year was 1616. In February 1616, the eleven theological consultants appointed to assess the status of the two sentences about the sun being motionless at the center of the universe and the Earth being in motion around the sun and around its own axis had unanimously come to the conclusion that the two basic theses of Copernicus were "absurd in philosophy" and "formally heretical [and] ... in regard to theological truth ... erroneous in faith" (Finocchiaro 1989, 146).[4] The pope ordered Cardinal Bellarmine (1542–1621), chief theologian of the Holy Office who had also presided over the trial of Giordano Bruno, to convey to Galileo the assessments of the consultants and to "warn him to abandon these opinions; and if he should refuse to obey … to issue him an injunction to abstain completely from teaching or defending this doctrine and opinion or from discussing it; and further, if he should not acquiesce, he is to be imprisoned" (Finocchiaro 1989, 147). Bellarmine duly informed Galileo, who acquiesced, as it is witnessed from what the Cardinal reported to the Congregation during its next regular meeting (Finocchiaro 1989, 146–47).

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[3] Finocchiaro (1989) contains a great number of the relevant documents concerning the "Galileo Affair."
[4] The two sentences were: "The sun is at the center of the world and completely devoid of local motion" and "the earth is not at the center of the world, nor motionless, but it moves as a whole and also with diurnal motion."

Nevertheless, in Rome the rumors that Galileo had been found guilty by the Inquisition were intensifying and by the end of May 1616, Galileo convinced Bellarmine to give him a letter where it was stated that the Cardinal had heard that Galileo was "being slandered" but that this was not true, and Galileo "had not received any penances, salutary or otherwise. On the contrary, he has only been notified that … the doctrine attributed to Copernicus… cannot be defended or held" (Finocchiaro 1989, 153). By the time Galileo left Rome for Florence in June 1616, the authorities thought that he had been duly warned; his enemies saw a wounded Galileo and his friends thought that although Galileo was informed of the constraints he was facing, he could, nevertheless, continue his studies; and he himself was in deep thought as to what his next steps would be. All in all, however, his relations with Church officials had taken a jolt and no one could claim that Galileo left Rome for Florence with the same confidence—and arrogance—that he had when he left Florence for Rome a few months earlier.

Surely 1616 was no ordinary year for Galileo and the Church. No one among the scholars doubts that Galileo in 1616 was warned by Church officials to be careful—how careful is, of course, something that is still debated. And most scholars agree that the Church was justified in warning Galileo—how justified is still open to debate (Finocchiario 2005; 2009).

The assessment of the 1616 events has been further complicated by an unsigned document, "discovered" sometime in late 1632 in the file of the Inquisition related to the 1616 events and presented as incriminating evidence against Galileo in the 1633 trial. The document describes what happened on February 25, 1616 during the meeting between Galileo, Bellarmine, and Michelangelo Seghizzi (1585–1625), Commissary General of the Holy Office during 1615–1616 who was also one of the Consultants in 1616. In this document, it is explicitly stated that Galileo was told that he could not hold or discuss the Copernican theory *in any way*. This document has also been minutely dissected and every piece ingeniously analyzed by many historians from a variety of viewpoints; the end result is that scholarship has moved away from the conspiratorial interpretations concerning the unsigned document, with most historians conceding some kind of legitimacy to the document, and even if it was by itself not sufficient to sentence Galileo, it had ample (legal) weight to counterbalance Bellarmine's letter given to Galileo in May 1616.

Progressively, scholars' attention concerning the trial has focused on the analysis of important contextual matters: the criteria according to which one is to assess whether Galileo did or did not break the 1616 accord; the risks Galileo had taken by meddling in theological matters especially after the Council of Trent's decisions that dramatically curbed the possibility of laypeople to deal with theological matters; the intense antagonism that *The Assayer* expressed against the Jesuits; the motives for writing the *Reply to Ingoli* and its intended audiences; the legality of the judicial procedures, the bureaucratic anomalies in the procedures that were followed before the trial; the hypothetical status of the Copernican ideas in the *Dialogue*; the caricature of Simplicius in the *Dialogue*; the reasons why he wrote the book when it was clear in many ways that even he himself was not sure at all that the tides *prove* the motion of the earth; the ways in which the conditions leading to the trial were affected by the serious problems Urban VIII was facing with regards to the Thirty Year War while his political alliances turned against him; the ever more frequent and organized attacks against him by his adversaries; the challenge to the pope's authority in Europe and in the very Curia itself; the ways in which the Jesuits whom Galileo so ably (and, perhaps, incomprehensibly) managed to antagonize were (re-)asserting their presence; the role of the Dominicans who continued to be very active in the affairs of the Inquisition; the complicated ways for securing the imprimatur for Galileo's book; the involvement of Niccolo Riccardi (1585–1639) who from 1629 was Master of the Sacred Palace (i.e., the chief censor in Rome), of Giovanni Ciampolo (1590–1643) who was Urban VIII's correspondence secretary, and of Clemente Edigi (1571–1639), Inquisitor General of Florence, in providing the imprimatur for the *Dialogue*; the possibility that the trial was, in effect, brought about because of Galileo's views about atomism expressed in *The Assayer* and the extremely serious clash that such views had with the Christian view of the Eucharist; the court culture, etiquette, and practices in Florence and, especially, in Rome; the repercussions of the trial in the following centuries and many other issues comprising the historiographical richness of one of the most incisively studied incidents in the history of science. These analyses have produced a remarkable scholarship and have swept away the popular myth of the good and saintly Galileo who had been persecuted by the bad, evil, backward, and prejudicial Church. Historians have actually *proved* that such a view is a convenient ideological position adopted by scientists of the later centuries concerning the troubled beginnings of modern science. Though no one would question the significance of studying *what really happened and why it happened,* it is rather unfortunate (but in no way something unique) that the prodigious scholarship around the trial and the study of its complicated context has had almost no effect in changing the deeply entrenched public perception (including that of the overwhelming majority of the scientists) about the trial.

The Friendly Pope

On August 6, 1623, Galileo's old friend and admirer Cardinal Maffeo Barberini (1568–1644) was elected Pope Urban VIII. In 1616, Cardinal Maffeo Barberini was a member of the Congregation of the Index, the institution responsible for the censure of books. Bellarmine was a member of the Congregation of the Holy Office, the tribunal of the Inquisition. Formally, a member of the Congregation of the Index would be informed of the deliberations of the Congregation of the Holy Office when decisions were taken concerning books. Earlier, in 1611, Barberini had defended Galileo against the Aristotelians about his theory on floating bodies, and in 1620 he wrote a laudatory poem in Latin, *Adulato Perniciosa (Dangerous Adulation)*, praising Galileo. On June 24, 1623 he thanked him warmly for helping his nephew Francesco Barberini (1597–1679) obtain a doctorate in law from the University of Pisa and he "look[ed] forward to the opportunity of reciprocating."[5] For Galileo, Urban's election was a "miraculous conjuncture."[6]

In the spring of 1624, Galileo decided that it was the "right time" (Shea and Artigas 2004, 104)[7] to go to Rome and pay his respects to the new pope. Prince Federico Cesi (1585–1630), founder, funder, and the motive force of the *Academia de Lincei*, invited Galileo to stop over at his villa in Acquasparta, 110 km northeast of Rome. Galileo accepted the invitation. Cesi had already informed Galileo that *The Assayer* had been presented to the pope in the presence of many church dignitaries at the end of October 1623 and that the pope was eagerly expecting Galileo to pay him a visit.[8] Obviously, Cesi and Galileo had a lot to discuss and a lot to plan before the latter's meeting with the pope. After staying a

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[5] Maffeo Barberini to Galileo, on June 24, 1623 (Favaro 1906, vol. XIII, 118).
[6] Galileo to Federico Cesi on October 9, 1623 (Favaro 1906, vol. XIII, 134). Although the election of the Pope was on August 6, 1623, his investiture was on September 29, 1623 due to Barberini's illness (Gattei 2019, 285).
[7] Also Galileo to Cesi on October 9, 1623.
[8] Cesi to Galileo on October 21, 1623 (Favaro 1906, vol. XIII, 140) saying that he told the Pope that Galileo would soon come to Rome and bring a copy of *The Assayer* (Shea and Artigas 2004, 105). Yet on October 27, Cesi himself presents the Pope with a copy of *The Assayer* (Redondi 1989, 49).

fortnight in the villa at Acquasparta,[9] Galileo Galilei reached Rome late on April 23, 1624.[10] The next day, Galileo had an hour-long meeting with the pope and, by the middle of June when he left Rome, he had had, in his own words, "six long meetings" with Urban.[11] Galileo never disclosed what went on during these meetings even though six meetings with the pope in six weeks is not a minor incident in a person's life, even if that person happens to be Galileo; there is no record by the pope about what they discussed; there is absolutely no documentary evidence about what went on during these meetings; and there is no mention of them during the proceedings of the 1633 trial.[12]

What we know about these meetings amounts to almost nothing: Cardinal Antonio Barberini (1569–1646), the pope's brother, was present during the first meeting, and before returning to Florence on June 1624, Galileo had been given by the pope a painting (no further details), two medals (one gold and the other silver), and a number of Agnus Dei (cakes of wax stamped with the figure of a lamb bearing a cross or a flag). Furthermore, he was promised a pension for his son, the appointment of a priest to the Convent of the Poor Clares at San Mateo d'Arcetri near Florence—where his daughters Virginia (Suor Maria Celeste, 1600–1634) and Livia (Suor Arcangela, 1601–1659) were nuns—and he was given two letters for his patrons full of praise about Galileo.

These "six long meetings" from the end of April 1624 to the beginning of June are either side-stepped or altogether avoided by Galileo scholars (Santillana 1961, 171–181).[13] Though there is no documentary evidence as to what happened during these meetings, historians assume that, in these audiences with the pope, Galileo raised the question of repealing the 1616 decision of the Congregation—something refused by the pope, who nevertheless consented that Galileo write a book about the motions of the sun and earth as long as he considered the Copernican ideas as hypotheses and not as necessary truths. Even if we accept these quite plausible assumptions and accept as a fact that, as we know

[9] We also do not have much detailed information on what was discussed between Galileo and Cesi during the former's stay at Acquasparta for a fortnight. See (Galluzzi 2017, 301–302) and (Finocchiaro 1989, 147).
[10] "Late in the night of the next day after 21 April" (Shea and Artigas 2004, 109; Galluzzi 2017, 310).
[11] Galileo to Cesi on June 8, 1624 (Favaro 1906, vol. XIII, 182). Interestingly, Galluzzi (2017) does not mention anything about these meetings.
[12] See (Gebler 1876) for the sad yet fascinating "adventures" of the documents by someone closely involved in their partial retrieval.
[13] See also (Finocchiaro 1989, 311) about the Buonamici Memorandum (Favaro 1904, vol. XV, 343).

from various sources (von Ranke 1901, 340–380),[14] Urban VIII especially liked to hear himself talk, one wonders whether during these meetings all Galileo and the pope did was exchange niceties, discuss the repeal of the 1616 decisions of the Congregation, and exchange views about the hypothetical character of the Copernican ideas. Perhaps that is all they discussed. Perhaps they also discussed a whole lot of other things. In any case, these meetings surely deserve some further attention.

In this paper, I shall try to test the viability of another hypothesis as to what else may have happened in these meetings and test it against the evidence related to later developments.

What Could Have Been Discussed During the Six Meetings

Maffeo Barberini and Galileo undoubtedly held each other in high esteem. But did they need so many long meetings for Galileo to congratulate Urban and for the pope to reaffirm his admiration for Galileo? Might it have been the case that the pope instigated so many meetings in such a short period *because he was intent upon resolving a major issue*? This could have been the negotiation of a new social and political agenda that he wished to advance in order to hold back the Linceans' and Galileo's agenda of marginalizing the Aristotelians so clearly and aggressively expressed in *The Assayer.* This is the core of what Urban may have discussed with Galileo.

What if Urban asked Galileo to write a book about astronomy in which an Aristotelian philosopher would be in conversation with a mathematician, and the Aristotelian, as the narrative in the book unfolded, would progressively embrace and promote the ideas that the mathematician had initially articulated? The philosopher in the proposed work would be neither a passive listener nor a straw man to be bashed about nor someone whose (counter)arguments would be too weak to merit serious discussion. Though in the beginning of the book it would be the mathematically trained natural philosopher who will provide the evidence and the arguments in support of Copernicus' ideas while the philosopher remains rather reserved in fully accepting them, as the book progressed, the philosopher would be more and more convinced by the evidence and arguments

[14] von Ranke provides intriguing details about Urban VIII mainly through the reports of envoys and ambassadors of Venice to the Vatican as well as of other dignitaries. See especially p. 346 about Sorbonne, p. 375 about assassination attempts, and p. 381 about humors.

presented to him and would continuously show signs of appropriating the new ways of studying and perceiving the cosmos. By the end of the book, there would emerge a philosopher with a different profile than the one he had at the beginning of the book.

The hypothesis put forth does not come into conflict with other assumptions about what was discussed during these meetings nor does it aspire to replace them.

Galileo's overall dealings with the Church must have impressed on him that even so friendly a pope as Urban VIII would have been ill-disposed to repeal the 1616 edict of the Congregation. Apart from strict legal reasons which made such a repeal almost impossible, by 1624 the situation surrounding the issues that led to the 1616 events had calmed down; there were no extenuating circumstances to repeal such a decision and any revocation of the proceedings would rekindle problems that would not have been to the benefit of anyone involved.[15] Furthermore, it is not even clear that Galileo would have wanted to push for such a decision by the pope, knowing that any reversal would have created a new furor among the conservatives. The 1616 decision of the Congregation did not personally accuse him of anything, and he also had Bellarmine's letter; thus, it was not clear at all whether, in his discussions with the pope, it would have been to his advantage to bring up the issues of February 1616 on his own initiative. It is also unlikely that the pope would have encouraged so many meetings to discuss something that he was not willing to revoke. Moreover, in 1624 surely neither the pope nor Galileo could have known about the unsigned document "discovered" in 1632 in the files of the 1616 events. Thus, it cannot be the case that there were so many meetings in order to decide how to deal with this particular document.

The other issue—that of the hypothetical character of the Copernican ideas—did not need much discussion either, and Urban's approach to the Copernican ideas was also well-known to Galileo.[16] So far as the treatment of the Copernican ideas, Cardinal Maffeo

[15] Urban's view concerning the 1616 decree was well known and was further confirmed—though by a rather indirect way—in 1630: "if it had been up to us that decree would not have been issued." Castelli to Galileo on March 16, 1630 (Favaro 1904, vol. XIV, 87). Castelli quotes Campanella who claims he was told this by Pope Urban VIII.

[16] Ciampoli to Galileo 28 February 1615 (Favaro 1902, vol. XII, 145), after Ciampoli had met Cardinal Maffeo Barberini. In addition, Dini to Galileo 15 March 1615 (Favaro 1902, vol. XII, 155). In this letter, it is clearly stated that Galileo and Barberini had met, and the latter expressed his views to Galileo. Furthermore, these views also echoed Bellarmine's reservations in his letter to Foscarini. See Bellarmine to Foscarini, 12 April 1615 (Finocchiaro 1989, 67). These views are also noted in Agiostino Oreggi's book published in 1629 *De Deo uno*, p. 194–95. The passage is translated in (Finocchiaro 1980, 10). There is a consensus that Oreggi refers

Barberini adopted the same attitude as that of Cardinal Bellarmine in his letter to Foscarini: deal with them as hypotheses and everything will be fine! Considering them as necessary truths would compromise God's omnipotence—God could have created the universe in so many different ways, which would still be compatible with the observations and the explanations about the structure of the universe. This argument had a rather long history and had been widely used ever since the end of the thirteenth century.[17] If one wished to prove that the motion of the earth is a necessary truth, then one would have to show that its opposite implied a contradiction, otherwise God can do anything as long as it is logically possible.[18] Even if Galileo disagreed with such a position, it is highly unlikely that he would challenge the pope on something that was so frequently and in such a clear way expressed by the latter and which, in addition, created a framework that allowed freedom of research. If (and it is a big if) Galileo challenged the validity of such a view, it is quite reasonable to assume that the pope would have repeated his belief about God's omnipotence in a way that would not leave much space for further discussion. Thus, neither the discussion of the second issue concerning the hypothetical character of the Copernican ideas warranted many meetings and long discussions.

Most likely, these issues—the repeal of the 1616 decisions and the hypothetical character of the Copernican ideas—were indeed discussed between the pope and Galileo. But did the discussions of these questions require "six long meetings"? Perhaps they did. But it is also not unlikely that other issues may have been raised as well. It is also quite sensible to assume that it was the pope who asked Galileo to continue their meetings after the first one, which for obvious reasons, was requested by Galileo. It would thus be instructive and interesting to examine the consequences of the assumption that there may have been some kind of agenda on the part of the pope that had to be systematically discussed and concluded during these meetings. Otherwise, how is one to comprehend that a pope (or anyone in such a high position) who is under extreme pressure

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to a meeting between Galileo and Cardinal Maffeo Barberini in 1615 or 1616 (Heilbron 2010; Shea and Artigas 2004; Finocchiaro 2005). Biagioli (1993, 310), in footnote 134, claims that it is likely the meeting took place in 1624.

[17] See for example (Lindberg 2008). See also (Finocchiaro 2009, 144), footnote 33.

[18] It is interesting to note Duhem's version of this argument: If, say, the hypotheses of Copernicus were able to explain all known appearances, what can be concluded is that they may be true, not that they are necessarily true, for in order to legitimate this conclusion it would have to be proved that no other system of hypotheses could possibly be imagined that could explain the appearances just as well (Santillana 1962, 113).

concerning Church affairs wishes to meet someone (even of Galileo' s standing) for so many times in such a short period, *unless the pope was intent upon resolving a major issue*? The six lengthy meetings could be much better understood if there were some kind of dramatic new development. A development that would also be in tandem with Urban's high-spirited mood in the first months of his reign, about the kind of pope he wanted to become: "still dreaming spectacular successes….[in] the morning of his reign, a morning full of magnificence and confidence" (Santillana 1962, 171).

Hence, our assumption that the pope asked Galileo to play a pivotal role in reconciling the Aristotelians with the mathematicians for the sake of the Church and the future stability of its institutions does not appear to contradict, at least, the other assumptions as to what Galileo and the pope discussed during these meetings.

To discuss Aristotelian philosophers and their encounters with mathematicians or to talk about their respective dominions over questions about the cosmos in terms of dyads is certainly convenient but at the same time conceals the nuances brought about by the various subgroups in these two social groups. Perhaps the best excuse is found in C. P. Snow's *Two Cultures:*

> The number 2 is a very dangerous number: that is why the dialectic is a dangerous process. Attempts to divide anything into two ought to be regarded with much suspicion. I have thought a long time about going in for further refinements: but in the end I have decided against. I was searching for something a little more than a dashing metaphor, a good deal less than a cultural map: and for those purposes the two cultures is about right, and subtilising any more would bring more disadvantages than it's worth (Snow 1961, 9–10).

Undeniably, even within the Catholic Church, the commitment to Aristotelianism did not mean that there was unanimity on all the issues related to the Aristotelian tradition. In other words, there existed many subtle or not so subtle variations on the Aristotelian understanding of a number of themes among the Dominicans, the Jesuits, and many other scholars (Feldhay 1995).[19] But these differences would not have discouraged Urban from pursuing his agenda.

In Urban's view, what was at stake was not the dominance of one particular view of Aristotelianism over others, but the danger he foresaw of the dominance of the

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[19] Feldhay, apart from the differences in the philosophical approaches of Dominicans and Jesuits, discusses the limitations brought about by the historiographical category of "conflict" for a comprehensive understanding of the Galileo affair.

mathematicians in matters of the cosmos over all others. Hence his strategy: as far as specifically the Copernican ideas were concerned, Urban's view about God's omnipotence provided a framework that could accommodate all *Aristotelian*s *as well as* the mathematicians. The hypothetical treatment of the Copernican ideas created a common framework. There were of course serious epistemological hurdles that were clearly articulated in *The Assayer*. Nevertheless, Urban must have been informed that there were peripatetics who were quite flexible in their commitment to Aristotelianism. Even Grassi in his *Libra* showed signs that he was willing to seek common ground with the mathematicians. Furthermore, it was well known that another renowned mathematician—the successor of Christopher Clavius (1537–1612) at the Collegio Romano and teacher of Grassi—Christoph Grienberger, was an ardent supporter for a reform of traditional natural philosophy and cosmology[20]; were it not for the "deference which by the direction of his superiors he was obliged to show toward Aristotle, he would have spoken his mind clearly on the matter of floating bodies, in which Galileo was perfectly right."[21]

Urban's proposal aimed at changing the thrust of what was in *The Assayer.* Conciliation between the Aristotelians and the mathematicians could be achieved if a new social consensus emerged—a consensus about the jurisdictions of different socio-professional groups. Both Urban and Galileo were aware of the fact that socio-professional identities are *also* constructed by procedures for determining truth shared by the members of each social group.[22] There were, of course, serious epistemological issues to be settled, and Urban may have felt that these would, in the long run, be resolved, especially since Grassi's *Libra* and Christoph Grienberger's (1561–1636) works and discussions in the Universities showed signs that at least some Aristotelians were not as inflexible as others. Urban's suggestion to Galileo was a political response to what appeared as an epistemological *cul-de-sac* if the agenda of *The Assayer* was to be realized. His proposal to Galileo meant that he was willing to mediate the strife and to seek peaceful coexistence between the peripatetics and the mathematicians. Under one condition, however: any legitimation of the mathematicians' aims as aspired in *The Assayer* could not come about

[20] For a systematic discussion of this specific particular issue see (Feingold 2003).
[21] Giovanni Bardi to Galileo, 20 June 1614 (Favaro 1902, vol. XII, 76). In this letter, as well as in Bardi to Galileo 4 July 1624 (Favaro 1902, vol. XII, 76), Grienberger was also sending greetings to Galileo.
[22] A particularly useful discussion of these issues for the sixteenth century is in (Westman 1981).

at the expense of the Aristotelians. His earlier dealings with Galileo may have convinced him that his agenda could transpire only if Galileo was willing to cooperate. It was an agenda that would help the Church as well as the plans of Galileo, who also wished to reform philosophy.

Articulating such an agenda may, indeed, have needed many meetings.

In order for Urban to bring about his agenda, what was needed was not only an epistemological consensus, but a social consensus, a consensus about the jurisdictions of different socio-professional groups. Both Urban and Galileo were aware of the fact that socio-professional identities are *also* constructed by procedures for determining truth shared by the members of each social group. This is what *The Assayer* set out to do. But when individuals perceive themselves as part of a group whose members share similar socio-professional characteristics, then they collectively vie for ideological and social hegemony. This is what the Linceans envisioned. And this is what Urban wished to prevent. In order to prevent such a development, Urban would have to perform a balancing act that involved the multitude of *Aristotelianisms* expressed, on the one hand, by scholars of the different religious orders and their roles in the power structure of the Church and, on the other hand, by the aggressive mathematicians. Through such a balancing act, Urban strove for a political solution: to mediate in the epistemological *qua* social strife in order to bring about peaceful coexistence between the Aristotelians and the mathematicians. And he realized that this could be achieved only if Galileo was convinced and willing to cooperate.

In fact, what Urban was proposing to Galileo had the possibility of falling on receptive ears; Galileo was not opposed to philosophy. His insistence to have the title of "Mathematician and Philosopher" at the Court of Cosimo is indicative of his strategy to "reform" philosophy as well. So, Urban was proposing a plan to someone whose agenda was also to realign social alliances. Of course, the intensity of the conflict with the Aristotelians was such that, objectively, it was the mathematicians who appeared to be claiming center stage all for themselves. And such a claim came in by projecting a different way of philosophizing. Urban's and Galileo's agendas were, indeed, in conflict with each other. As it is often the case, negotiations for grand schemes start with strong disagreements between the different parties and, sometimes, they draw to a close by meeting on common ground. This is perhaps what Urban hoped for, and this is perhaps what Galileo was unwilling to take responsibility for.

Discussing such an agenda may, indeed, have needed many meetings between Urban and Galileo.

Is our Hypothesis Realistic?

How confident are we about a hypothesis or a claim we make concerning a particular historical event? This is not a question which can be answered unambiguously in historiographical terms, even though it has interesting ramifications in a philosophical debate. The plausibility of a historical claim is strongly dependent on the reliability as well as the quality and the quantity of the direct and indirect evidence related to a particular event. And almost all the arguments that lead to what "was, in fact, the case," admit a margin of uncertainty. Thus, historians prefer to talk in terms of what "may have been" the case. Uncertainty, comprehensibly expressed, gives further credence to the sensibility of a historical claim.

But is our hypothesis about what happened during the six meetings between Galileo and Urban in 1624 a tenable hypothesis? Surely its plausibility is enhanced if it can offer explanations about some subsequent events which have not been sufficiently clarified by other approaches. But there are additional aspects to our hypothesis that strengthen its implications, and its content is enriched by satisfying the following seven criteria:

1. Our hypothesis *is compatible with the overall context of other events taking place*. The ever-present politics of knowledge took a decisive turn during the last decades of the sixteenth century and the beginnings of the seventeenth. It is the period during which the research agendas of the mathematicians as well as their social status were redefined. There were intensive discussions about the epistemological status of observations and about the certitude of mathematics. Clarifying such seemingly theoretical issues had social and cultural repercussions. For example, Galileo and like-minded mathematicians were part of "*a newly reconstructed community of mathematicians whose field of research was in the process of being defined*" (Feldhay 1998, 81–2). But it was not only the new breed of mathematicians who were contesting a different status in the academic hierarchy. Philosophers in many universities in Europe were attempting to rethink different aspects of Aristotelianism, and Clavius was hard at work in overcoming the perceived incompatibility of mathematics and Aristotelian philosophy.
   Starting from the last quarter of the sixteenth century, the Jesuits' mathematical training was becoming progressively more and more sophisticated, and their mathematical presence was strongly asserting itself. Yet, their distinctive role in astronomical as well as cosmological issues was

seriously challenged by the emerging new epistemology and anti-Aristotelian agenda. Those who were progressively adopting the new natural philosophy were asserting their emerging socioprofessional identities. The Aristotelians were threatened since they felt that they were being "robbed" of the jurisdiction they had enjoyed over a series of matters. Different groups of scholars were acquiring different socioprofessional characteristics and the continuous appropriation and reappropriation of these characteristics brought about strong ties among individuals who shared similar outlooks. New social groups were forged—each vying for cultural and professional hegemony. Ideological differences became accentuated; antagonisms abounded and were rarely contained. And as each group attempted to carve for itself a social space, disputes were intensifying. In such situations, the members of each side rally around the core ideas defining their identities, leaving aside ambiguities and debatable issues and becoming inflexible in their public discourses. The members of each group were under increasing pressure to close ranks: the directive issued to the Jesuits to "keep away from anything that might weaken the Aristotelian position"[23] and Cesi's insistence that "the common endeavor of the Linceans was to fight Aristotelianism all the way" (Santillana 1962, 21).
It was within this general context that Urban decided to ask Galileo to help him change the course of events that were signaling serious problems for the Church.

2. Our hypothesis is consistent with the protagonists' *direct or indirect involvement in what was happening "out there" and the stake each had in how these intellectual, social, and, in effect, political processes would unfold.* Neither Urban nor Galileo could afford to be indifferent about the ways a new social equilibrium among the social groups would be reached. Obviously Urban did not want to alienate the Aristotelians and, especially, the Jesuits. Galileo's aspiration was to become the reformer of natural philosophy. The success of Galileo's enterprise depended on maneuvering his actions in such a manner as to have the Church's tolerance and, at the same time, secure further assertiveness by his followers. One cannot ignore the possibility that *The Assayer* may have also unrailed the attempts of the Jesuits to "modernize" Aristotelianism and, in fact, this text became a catalyst in forming two uncompromising groups, each with its own manifesto and leader. The two different groups gave different meanings to philosophy, mathematics, and natural science. Jesuits rallied around a strict Aristotelianism as it became evident from the militant speeches during the opening of the academic years of 1623–1624 and 1624–1625. Linceans rallied around the emerging natural philosophy with its strong anti-Aristotelian agenda. Urban understood that what was simmering for a long time between the Aristotelians and the new breed of natural philosophers could take explosive dimensions. This is exactly what he wanted to forestall. He had to find ways to moderate the situation, hence his suggestion to Galileo to adopt a style and argumentation that would not antagonize the Aristotelians and would attempt to unite the warring groups around a unified agenda.
3. *Our hypothesis gains further credibility since it can be read as part of an established "tradition."* Urban's suggestion to Galileo was not far from what Clavius had attempted to do earlier: articulate an agenda where mathematics was necessary for understanding Aristotle. Clavius was surely the key figure in

[23] In 1611, the general of the Society of Jesus, Claudio Aquaviva, would order Jesuits to defend Aristotle in all matters (Lattis 1994, 6). See also Giovanni Bardi to Galileo on 20 June 1614 (Favari 1902, vol. XII, 76).

bringing about a new way of philosophizing, and he certainly did not feel he was in a disadvantaged state when he defended the Ptolemaic system, discussing at the same time its weaknesses as a result of the new developments in astronomy. This is indicative of Clavius' openness to new ideas, something that is strongly supported by his other initiatives: the use of the telescope to observe what Galileo observed; the public acknowledgement that he and other Jesuits did, in fact, see the same things as Galileo; the invitation of Galileo to Collegio Romano and the amazingly warm welcome given by Odo van Maelcote during the *festa Galileana;* and Clavius' cordial relations with Galileo himself since before the telescopic discoveries. That our hypothesis is within this tradition is further reinforced by Bellarmine's inquiries in his letter of April 19, 1611 to "the Mathematicians of the Collegio Romano." Bellarmine stated in the letter that he himself had seen "some very marvelous things" through the telescope and asked the mathematicians to "tell me if these discoveries are well-founded or if they are rather appearances and not real" (Lattis 1994, 190). The firm confirmation of the observed phenomena in the answer to Bellarmine (without any commitment as to their reality) did not create any unsurpassed worries. The accommodating framework for "σώζειν τα φαινομενα" was already there and God's omnipotence demanded the hypothetical treatment of the implied cosmological views. Thus, well before Barberini became pope, Clavius had already created a framework where even the unexpected observed phenomena did not create any outstanding problems (Lattis 1994; Dear 1995; Feldhay 1998; Remmert 2006).[24] And this framework had the blessing of Bellarmine as it is also found in his letter to Foscarini sometime later. Hence, our hypothesis about Urban's suggestion to Galileo could be considered as a continuation of what Clavius commenced and had Bellarmine's blessing. It was as if Urban was asking Galileo to help him "complete" Clauvius' agenda.

4. Our hypothesis is *commensurate with traits that have been persistently revealed in the protagonists' social and political pursuits and activities*. The likelihood of our hypothesis is further strengthened if we extract the political nature of a number of issues. The papacy is a political office—this cannot be doubted. And as each political office is defined by a number of specific attributes, so is the papacy. Urban's views about the 1616 proceedings were also the views of someone who did not seek confrontation. Furthermore, his suggestion to Galileo involved a scheme that Urban expected to fall on receptive ears. As we mentioned above, Galileo needed a harmonious relationship with the Church; hence, it was rather reasonable for Urban to expect that Galileo would be well disposed to such a suggestion. Such negotiations demanded a degree of political acumen, and both Urban and Galileo had plenty of that. Neither of our protagonists was politically naive nor inexperienced about political intrigues and manipulations. In an old institution like the papacy, becoming a pope entails highly sophisticated negotiations and skillful machinations. Becoming a pope in such complicated times as those Europe was experiencing in 1623 required a talented candidate to bring together various factions among the cardinals. On the other hand, Galileo had already shown to have been a virtuoso in court politics. The way he became the First Philosopher and Mathematician in the Medici court, the suave style of the *Letter to Christina*, his sojourn in Rome in 1615–16, his letters to the Tuscan Secretary of State in March 1616 giving his own version of the events of the fateful proceedings, and the way he convinced Bellarmine to

[24] For the possible influence of Jesuit science on Galileo's physics, see (Wallace 2006).

give him the letter, all these bear witness to a behavior well-versed in subtlety, judiciousness, and prudence. Political astuteness goes hand in hand with the awareness of potentially explosive social tensions before they fully come into the open. It is within such a context that Urban asked Galileo to help divert the likelihood of the unsettling crises that would have had serious political repercussions in the unpredictable climate of the war being waged in Europe.

5. Our hypothesis does not stem from the *eccentric traits or extreme behaviors of Urban*. Urban was a megalomaniac; he did talk too much and did not like to listen to others; he was stubborn and it was difficult to change his mind; he deeply believed in astrology; he took nepotism to new heights and because of his long papacy, his practices shadowed what other popes did with equal fervor; he was a spendthrift and initiated extravagant projects associated with bolstering up his image; and he thought of himself as a particularly accomplished poet. Nevertheless, his suggestion to Galileo did not follow from any of these traits. Of course, any hypothesis can be based on these traits, but such a choice would not give the hypothesis the needed reliability within a much larger framework and it would reduce the meetings between the two to something that lacks a strategic significance.
6. Our hypothesis is *compatible with respect to all subsequent developments and does not come into any direct conflict with any of the sources*. This is indeed the case since there does not appear to be such an incompatibility with the sources.
7. Our hypothesis *enriches existing interpretations and provides explanations to issues that have been overlooked by other scholars*. Our hypothesis provides a better understanding for the disappointment of Galileo about his stay in Rome and his strong desire to leave Rome by the middle of May 1624, after he already had some meetings with the pope; it clarifies his rather mysterious initiative to reply to Ingoli eight years after the latter wrote a leaflet; it makes more tenable the mess concerning the delays for the imprimatur for his *Dialogue*; it provides judicious explanations about some passages in Ambassador Niccolini's correspondence with the Tuscan Secretary of State that have not been given any attention.

Finally, our approach has to be distinguished from counterfactual history. Strictly speaking, counterfactual history presents a narrative after an event supported by incontrovertible evidence is negated. What would have been the course of events if Darwin had not been born? Or would it have been possible to witness social Darwinist trends without the works written by Darwin? What would have been the course of a war if instead of a specific decision A, another decision B was taken? This is not what we attempt to do in this paper. Alternatively, we propose a hypothesis about what could have happened during a series of meetings about which we know nothing and, hence, we do not propose a hypothesis that negates something quite unambiguously recorded. Of course, in both cases there is an "if A, then B" structure to the arguments. However, in the case of counterfactual history, there is a rather strong causal relationship between A and B. In our case, the attempt is to test whether our hypothesis contributes to the cohesion of the narratives about various aspects of Galileo's dealings.

The Assayer or Copernicus' Ghost

At the end of August 1618, a comet appeared, visible in European skies. In October 1618, two brighter comets appeared, and they were visible until January 1619.[25] In March 1619, Orazio Grassi—the distinguished mathematics professor at the Collegio Romano—published the *Controversy on Comets*, where he argued that their paths were in accordance with Tycho Brahe's interpretation of the comet of 1577 (Redondi 1989, 29, 41–2, fn. 22). The reference of a number of Jesuits to the system of Tycho implies an attempt on the part of the Jesuits to circumvent the difficulties they had with Ptolemy's system. Such references do not undermine our hypothesis. The Tychonian system was surely providing a particularly convenient framework, and the openness to the Tychonian system was also an indirect way to free oneself from Aristotelian cosmology. Tycho in no uncertain terms had "proven" that there were no crystal spheres and that the sky above the moon was fluid—two important aspects of the Aristotelian cosmology. But if the Copernican system needed a new theory of motion to replace that of Aristotle's, so did Tycho's as well. Thus, the varying degrees of "faith" in Tycho were more within the overall culture of σωζειν τα φαινόμενα and a way of acknowledging that Ptolemaic astronomy had a lot of problems. Tycho provided a particularly effective safety blanket against the perils of both Aristotle and Copernicus. For many Jesuits, this state of affairs continued well after Galileo's telescopic discoveries.

The events following Grassi's publication have been exhaustively studied by many scholars. Galileo's reaction to Grassi was his *Discourse on the Comets*, published in June 1619, a text prepared for Mario Guiducci for his inaugural lecture at the Academy of Florence and sent to Archduke Leopold of Austria. There, it was argued that the comets were, in effect, optical illusions, the result of reflections of the sun by the vapors in the

[25] Almost everyone agrees that because Galileo was ill, he himself did not make any observations of the comets. But he was informed by others' observations. Stelluti Galileo on 25 December 1618 (Favaro 1902, vol. II, 430) informs him that he and Cesi made observations on the comets and that they did not see any appreciable differences compared to how they appeared with the naked eye. Johannes Remus—mathematician to the Archduke of Austria, Leopold—sent a detailed report of his own observations, as found on Remus to Galileo on 12 January 1619 (Favaro 1902, vol. II, 434). And Galileo had already informed Cesi that he had asked Guiducci to follow and record the course of the comets. See footnote 21 in (Galluzzi 2017, 248). Thus, despite his illness, Galileo was well-informed of observations made by his friends.

atmosphere. Grassi responded with his *Libra* in December 1619. And, then, there was a lull for some months, before Galileo started preparing his answer: *The Assayer*.[26]

When Galileo sent his *Discourse* to his fellow Linceans, their responses were not unconditionally encouraging. One Lincean, Giovanni Ciampoli (1590–1643), who was also echoing Virginio Cesarini's (1596–1624)[27] views, wrote to Galileo expressing his serious reservations of "taking issue with the Collegio Romano" (Galluzzi 2017, 253).[28] Even more alarming were the objections expressed by Johannes Remus, the personal mathematician of Leopold of Austria, to whom the *Discourse* was dedicated (Galluzzi 2017, 254). But the Linceans became increasingly distressed when they read Grassi's *Libra*. The author had no qualms to involve the Academy in his dispute with Galileo. Francesco Stelluti (1577–1646) wrote to Faber conveying Cesi's worries about what Galileo was planning to do (Galluzzi 2017, 256; see footnote 49). Cesi thought that the attack against Galileo and the Academy in the *Libra* could not go unanswered, but he was very much worried that Galileo would be rather aggressive and there was a serious danger of jeopardizing the relations between the Academy and the Jesuits. Though he wished to encourage Galileo to answer, he also wished Galileo would not be quarrelsome and aggressive. Cesi, Ciampoli, Cesarini, and Stelluti were all of the opinion that Galileo should answer through Mario Guiducci (1585–1646) and not target directly Grassi and the Collegio Romano. However, at the same time, they knew Galileo would be unwilling to "put into effect [Cesi's] advice on the reply" (Galluzzi 2017, 262).[29] After a quasi-official meeting of Cesi with fellow Linceans, Ciampoli, and Cesarini at Acquasparta, the message sent to Galileo was rather clear: "We have given full consideration to [your] reply," go ahead and prepare your answer but avoid the caustic and bitter tones (Galluzzi 2017, 264).[30] The rules of the Academy demanded that the manuscript be read by all.

What may have brought such a turnaround by those very people who had expressed their reservations in no uncertain terms during the past year? Might it be the case that the

[26] This work has been translated and extensively analyzed by Drake and O'Malley (1960), Redondi (1989), and Biagioli(1993), each through a different perspective.
[27] Ciampoli, in 1621, was promoted to Secretary of Briefs under Pope Gregory XV, and, in 1623, he became chamberlain to Pope Urban VIII. Cesarini served as chamberlain to Pope Gregory XV and became chief chamberlain to Urban VIII.
[28] See also letter from Ciampoli to Galileo on 12 July 1619 (Favaro 1902, vol. II, 466).
[29] See also Cesi to Faber 23 February 1620 (Favaro 1903, vol. XIII, 24).
[30] See also Cesi to Galileo 18 May 1620 (Favaro 1903, vol. XIII, 17).

news about the specific corrections of Copernicus' *De Revolutionibus* dispelled any skepticism entertained by some of the Linceans about the ways Galileo should go answering Grassi? The corrections appeared to complement the arguments in the *Libra* and serve as thinly veiled attack against the mathematicians. The Linceans may have been reserved and avoidant of confrontations, but they were not naive. To deal in as mild a manner as possible with an established scholar like Grassi was one thing; to have the Church cooperate with Grassi for the censure of Copernicus' book and attack the mathematicians was a different thing altogether. Surely, Grassi was establishing some kind of hegemony over the silent adversary and his friends. No wonder that in a less than a year the Linceans' approach shifted from extreme caution to impatient encouragement. After May 1620, moderation appeared rather useless to the Linceans. Even if Galileo was in doubt as to the way he would structure his answer, the involvement of these two Jesuits in the procedures for censuring *De Revolutionibus* was sufficient to make him take the aggressive position. If the Church, with the help of the Jesuits, wished to undermine Copernicus, Galileo would not be the Christian who would show humility and turn the other cheek. Those, Galileo could have reasoned, who chose to use all the institutions to "neutralize" Copernicus could not be let go of lightly.

Galileo was not a person predisposed to avoiding a (good) fight if the opportunity arose, and we do not have any letters of Galileo responding to such mindful advice by his colleagues. In fact, Galileo must have been busy composing his answer, since in a letter of Ciampoli to Galileo on July 17, 1620 (Favaro 1903, vol. XIII, 43), we are informed that the project was well under way! In fact, Galileo asked Ciampoli to publish the text under the auspices of the Academy. Cesi, Ciampoli, Cesarini, and Stellutti were honestly encouraging Galileo to answer while at the same time warning him to avoid a confrontation with Grassi and the fathers of the Collegio Romano. Galileo, ignoring the warnings of his fellow Linceans, went ahead with a deadly attack against Grassi and the Aristotelians.

The Assayer was planned as a long letter to Virginio Cesarini—a fellow Lincean and a distinguished member of the Roman nobility with strong ties to the Jesuits![31] The text

[31] Galluzzi (2017, 253) poses the rhetorical question: "Had Galileo forgotten Cesarini's close ties with the Society of Jesus and his endeavors to prevent the 1616 decree from compromising relations with those fathers who like Grienberger had shown their inclination towards a gradual reform of traditional natural philosophy and cosmology?"

was ready by October 1622; it was sent to Cesarini, who circulated it among the Linceans, who in turn carefully read it and made various emendations to the text, suggesting topics to be included and rewriting passages—all this with Galileo's consent. For five months, "it passed from hand to hand among the members of the Academia" (Gebler 1876, 106).

The election of the new pope in August 1623, however, appears to have swept away the Linceans' reservations. Federico Cesi and the Linceans decided to hasten publication and dedicate the book to the newly elected pope. It is also quite likely that the election of the new pope prompted Galileo to rework some parts, and perhaps he delayed his visit to Rome until the "right moment" (Shea and Artigas 2004, 104–5; Favaro 1903, vol. XIII, 133), waiting for the responses to the text.[32] Niccolo Ridolfi (1578–1650), who had been appointed Master of the Sacred Palace in 1622, signed the imprimatur for the book, which was printed in October 1623[33] with many typographical errors.

The *Assayer* is considered by all scholars as a great work of rhetoric and polemic (Redondi 1989, 14; footnote 10). Gebler (1876, 111) writes that it was a "masterpiece of ingenuity … model of dialectic skill." Giorgio de Santillana (1962, 166) remarks that the book "was promptly and unanimously recognized for what it was—the masterpiece of Italian polemic prose … nailing the enemy to the post … sparkling wit and destructive irony." Stillman Drake (1957, 227) considers it as "the greatest polemic ever written in the history of science." Shea and Artigas (2004, 100) argue that *The Assayer* is a masterpiece of style, a "model of devastating irony," and that it had been acclaimed by poets and writers. Pietro Redondi (1989, 28) notes that it made a remarkable impact because of "its iron, its murderous wordplay, the poetry of it allegories and its boundless intellectual passion … it delighted the humanists and the rhetoricians in the literary academies … [and the book became] a literary and intellectual sensation." Without disputing such characterizations of the text, Mario Biagioli (1993, 10; 267) comments extensively on it but notes that "it puzzled many Galileo scholars and occupies an uncomfortable place in Galilean historiography." Heilbron (2010, 245) regards it "a protracted tale of right against wrong,

[32] The controversy about the comets continued with Grassi's *Ratio ponderum librae et simbellae*, published in 1626, but Galileo agreed with his colleagues who urged him not to respond.
[33] The imprimatur for *The Assayer* was issued on February 2, 1623 by Nicolo Ridolfi. The opinion was written by Riccardi. See (Favaro 1896, vol. VI, 200; Gebler 1876, 295).

good against evil, innocence against deceit … a mocking or a mockery of a scholastic analysis of an Aristotelian text."

The polemic which everyone refers to was the devastating polemic against the Aristotelians. Sarsi, a thinly veiled anagram of Grassi, received the blows for all the sins of the peripatetics. The work was, of course, sprinkled with Galileo's use of Copernican ideas. The following captures succinctly the gist of what *The Assayer* was:

> The rejection of dogmatic submission to the principle of authority in the field of philosophy; the vindication of a new language; the rights of research and free intellectual discussion against the prevarication of institutional culture ---these were the contents that made The Assayer the manifesto of the new philosophy in Rome (Redondi 1989, 51).

The *Assayer*, in a richly bound copy, was ceremoniously presented to Urban on October 27, 1623. Federico Cesi, prelates from the Curia, and Cardinal Francesco Barberini were all present. It was three months after Urban's election and a month after he had officially taken office, since he was recovering from malaria (Pattenden 2017, 74). To the progressive intelligentsia of Rome and, especially, to the Linceans, Urban still carried Maffeo Barberini's aura. It is on record that Urban expressed his enthusiasm about the book when it was read to him by Ciampoli during meals (Redondi 1989, 49; footnote 52).[34] The latter—as a member of the Academia de Lincei—knew the text well and, perhaps, chose passages to elicit such a reaction from the pope, who enjoyed Galileo's enthusiasm in relentlessly devastating his adversary.

Of course, it was somewhat unconventional to present a book without the presence of its author. The dedication to the pope was signed as "At Rome, the 20th of October, 1623. From Your Holiness' most humble most devoted servants, the Lincean Academicians." The author's name is, of course, mentioned in laudatory terms in the dedicatory note. The dedication closes by an appeal: "we come to appear before you … as evidence of our devotion and as a tribute from our true fealty, we bring you *The Assayer* of our Galileo …

[34] See also Cesarini to Galileo on October 28, 1623 (Favaro 1903, vol. XIII, 141) and Ciampoli to Galileo on November 4, 1623 (Favaro 1903, vol. XIII, 146). Though no one doubts Urban's expressions of approval when passages of *The Assayer* were read to him by Ciampoli, this information comes to us in a letter by Cesarini to Galileo one day after the book was presented to Urban. But the fact that the Pope received the bound copy on October 27 and Galileo was informed on October 28 after the pope had time to listen to passages of the book during one of his meals may perhaps need further elucidation. It is not obviously impossible for events to have taken place in this way. Cesarini was the Chamberlain to the pope and, hence, surely knew what was going on in the pope's immediate circle.

this we dedicate and present to Your Holiness … humbly inclining ourselves at your feet, we supplicate you to continue favoring our studies with the gracious rays and vigorous warmth of your most benign protection" (Drake 1978, 153; Gattei 2019, 286). Let us note that if Urban had a chance to read the dedication and not only listen to the passages read to him by Ciampoli, he could have been somewhat alarmed by two points: there were too many "we's" and "ours", as if to make sure what the Linceans wanted to convey to Urban *that this was their collective undertaking*. The second point is the last sentence: the Linceans appealed to the pope to be their protector in their subsequent studies. As it is argued below, by the time Urban held his audiences with Galileo six months after he had been presented with *The Assayer*, the implications of such a dedication by the Linceans could have precipitated his worries. The whole ceremony and the form of *The Assayer* must have brought to Urban echoes of what Cesi kept on repeating that "the common endeavor of the Linceans was to fight Aristotelianism all the way" (Santillana 1962, 21). Urban must have regretted the enthusiasm he showed towards *The Assayer* when passages were read to him by Ciampoli. It was one thing to have a text in Italian with brilliant metaphors and sparkling attacks against Grassi, and it was a different thing altogether to realize that the Linceans were appealing to him to be their accomplice against the Aristotelians, through a remarkable text, "the great Trojan horse of a Roman cultural change" (Redondi 1989, 325). It is not far-fetched to assume that despite Urban's "broad-mindedness, he must have soon wondered about the wisdom of having his papacy associated with theories that threatened the foundations of the Church itself" (Freedberg 2002, 175).

There is, however, an additional issue of paramount importance that has, perhaps, not been given the attention it deserves.

All the maneuvering that went into the composition and publishing of *The Assayer* may perhaps not be independent of what was concurrently happening with the corrections to the *De Revolutionibus.* After the announcements of the Congregation of the Index in February and March 1616, Cardinal Bonifazio Caetani (1567–1617) was given the task to revise *De Revolutionibus.* He died on June 24, 1617, and his assistant—Francesco Ingoli (1578–1649)—completed the report and presented it at a meeting of the Congregation on April 2, 1618.[35] The Cardinals of the Congregation decided to inquire about the opinion of the mathematicians of the Collegio Romano concerning the suggested corrections. Interestingly—and this is not something usually commented on—these scholars were Grienberger and Grassi, who agreed with the corrections proposed by Ingoli![36] The deliberations concerning the corrections of *De Revolutionibus* show at least two things: First, the involvement of Grassi and Grienberger, who were known to be continuing Clavius' enterprise, was surely approved by the Jesuit leadership, independently of whether it was asked to send two scholars or Grassi and Grienberger were personally invited. Secondly, Cardinal Maffeo Barberini's continuous presence in these very crucial sittings of the Congregation of the Index provided him with an opportunity to get to know these two scholars before he became pope.

Thus, when Grassi started writing *Controversy on Comets* (published on March 1619), which prompted Galileo's *Discourse on the Comets* (published in June 1619), Grassi was already aware of the censures. Ingoli's report was accepted by the Congregation on July 3, 1618. "Nonetheless matters dragged on for another two years" (Shea and Artigas 2004, 87), and it was only on May 1, 1620 that the Congregation of the Index decided that *De Revolutionibus* could be printed with Ingoli's corrections.[37] On May 15, 1620, the changes to Copernicus' *De Revolutionibus* were officially announced.

Among the changes, the most radical one is that the paragraph where the emblematic phrase "mathematics is for the mathematicians" is censored! It is, in fact, instructive to quote the censored paragraph in the short text dedicating the book to Pope Paul III:

> There may be triflers who though wholly ignorant of mathematics nevertheless abrogate the right to make judgements about it because of some passage in Scripture wrongly twisted to their purpose and will dare to criticize and censure this undertaking of mine. I waste no time on them, and indeed I despise their judgement as thoughtless. For it is well known that Lactantius, a distinguished writer in other ways but no mathematician, speaks vey childishly about the shape of the earth when

[35] On January 6, 1622 Ingoli was appointed by Pope Gregory XV as secretary of the Congregation for Propagation of Faith.
[36] Details about the crucial meetings of the Congregation of the Index are in (Mayaud 1997, 70). On p. 77 there is a table comparing the suggestions by Ingoli and the final corrections agreed upon by the Congregation. For a systematic discussion about Ingoli's suggested corrections see (Lerner 2004).
[37] Maffeo Barberini was present only in the last session on February 28, 1619, when the Congregation decided to ban Kepler's *Epitome*. He was absent in all the other sittings (Mayaud 1997, 59). He was present in all the deliberations involving the corrections to *De Revolutionibus.*

> he makes fun of those who reported that it has the shape of a globe. Mathematics is written for mathematicians.[38]

Galileo had used this exact passage in his L*etter to Christina* in 1615 to strengthen his argument that *De Revolutionibus* did not come into conflict with the Scriptures (Finocchiaro 1989, 91). However, the Congregation in 1616 had decided to censure the books of Copernicus and Diego de Zuniga, and those passages which stated or implied that "the sun's rest at the center of the world and the earth's motion is consonant with the truth and does not contradict Holy Scripture" (Finocchiaro 1989, 149). As he mentioned in his letter to the Tuscan Secretary of State, Curzio Picchena (1553–1626), on March 6, 1616, a day after the announcement of the Congregation's decision, Galileo expected sentences of this passage to be censored but gives no indication that the last sentence would be censored as well (Finocchiaro 1989, 150).

The rationale behind this correction was to condemn the view that the Scriptures lack authority over the explanation of natural phenomena. In this passage, Copernicus was "implicitly saying that Scripture is irrelevant to astronomy, physics and natural philosophy" (Finocchiaro 2005, 22). Copernicus did this in a rather indirect way: if Lactantius claimed that the earth was not round, it may also be the case that the objections raised by many against the motion of the earth on biblical grounds may perhaps also be the result of misinterpretation. The phrase, then, "mathematics is written for mathematicians," inserted where it was, could be interpreted in two ways. Firstly, in an instrumentalist way, which would acknowledge the tradition of "saving the phenomena" and which had become the dominant culture for astronomers in the centuries after Ptolemy. Secondly, the specific phrase could mean that Copernicus did not wish for theologians (and philosophers with no training in mathematics) to meddle in issues of astronomy. The Congregation's censure by omitting these words resolved any ambiguity that may have been conveyed by this phrase.[39]

Nevertheless, in Ingoli's Report the usefulness of the book is a significant consideration for the changes to be introduced, and the censors give us a very clear answer on the way they view *De Revolutionibus.* Of the eleven emendations, except the one we discuss in the

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[38] Rosen (1978, 9) translates the last phrase as "astronomy is written for astronomers." For the purposes of this paper, this difference in emphasis does not play any role.

[39] For an equally strong claim about this correction see footnote 54 in (Westman 1990, 199–200).

Preface, there is another one which displays the Congregation's significant commitment to their overall view concerning astronomy. It is the second point to be corrected, in Book 1, chapter 5 (the crossed out and underlined segments are what the Congregation erased and what the Congregation added, respectively):

> To be sure, there is a general agreement among authorities that the earth is at rest in the middle of the Universe. They hold the contrary view to be inconceivable and downright silly. Nevertheless, if we examine the matter more carefully, ~~we shall see that this problem has not yet been solved, and is therefore by no meant to be disregarded~~. We think it is immaterial whether the earth is placed at the center of the world or away from the center, so long as one saves the appearances of celestial motions (Finocchiaro 1989, 200).[40]

The Congregation, thus, made its position absolutely clear: By removing the phrase "mathematics is for mathematicians," it removed any possible ambiguity and misunderstanding of what Copernicus may have implied *vis-a-vis* the Aristotelian philosophers. By including the phrase "save the appearances" in the second correction, it affirmed in no uncertain terms its own position: mathematicians would continue to be useful in saving appearances, a role they had for many centuries and, thus, philosophers would continue to cherish their sole jurisdiction on determining the reality of the cosmos.

Thus, with these *two* corrections the Congregation takes an active part in the ongoing controversy between the Aristotelians and the mathematicians. In effect, it takes an active role in what was brewing during the last decades and was very much in the general mood concerning the antagonisms between the Aristotelians and the mathematicians. In "cleansing" *De Revolutionibus* from such an explicit expression of alliance with the natural philosophers, the Congregation of the Index wished to state that it was perturbed not only by the epistemological status of the Copernican ideas and the danger in misinterpreting the Scriptures, but also by Copernicus' stand concerning the Peripatetics. The second correction settles the matter on the part of the Congregation in no uncertain terms.[41] Galileo and the Linceans must have had no second thoughts about what the Church's intentions were.

[40] See especially footnotes 4 to 15, p. 350–351.

[41] (Finocchiaro 1989, 23) argues convincingly about the ambiguities in the proposed corrections, in the implications of "hypothesis" and "assertion" –ambiguities that have to do with the Congregation's intention whether to adopt an instrumentalist interpretation or to exclude a realistic one.

Galileo and the Linceans were certainly aware of the changes in *De Revolutionibus,* and the intensely polemical style of *The Assayer* could have also framed a response to these corrections. But the polemics were something much more than a convenient style for the occasion. They signified an unambiguous stand against the Aristotelians. By exposing the Aristotelians' inherent inability to deal with astronomical issues, *The Assayer* was planned as the rallying banner of the new natural philosophy. The *Assayer* may have also been an ideal opportunity for a comeback of the victims. Galileo and fellow mathematicians would not have failed to realize that the corrections to *De Revolutionibus* had very little to do with the epistemological status of Copernicus' ideas, and they may have felt that the Congregation showed its unequivocal determination to silence the mathematicians by censuring the phrase "mathematics is written for mathematicians." The opportunity to strike back was there, since Galileo had not yet responded to Grassi's *Libra.* And that is what he did for two and a half years. Is it, in fact, an insignificant coincidence that three days after the official announcement of the censorship of *De Revolutionibus* on May 18, 1620 Cesi, Cesarini, and Ciampoli wrote to Galileo urging him to compose an answer to *Libra*? (Favaro 1903; vol. XIII, 37–8). This was preceded by a letter from Faber on February 15, 1620 that urged him to bring down the "pride of the Jesuits" (Favaro 1902; vol. XII, 23). Galileo decided that *The Assayer* should be as aggressive against the Aristotelians as the Congregation was dismissive towards the mathematicians.

Galileo's devotion to Copernicus was unquestionable. He felt that it fell on him *now* to fulfill *both* of Copernicus' aims: to prove the earth's motions *and* to exclude the Aristotelian philosophers from having jurisdiction over such matters. A close reading of Copernicus' dedicatory note to Pope Paul III shows in no uncertain terms that the future success of his idea about the moving earth is totally dependent on the philosophers not having any say on this matter. He says so explicitly in more than one place. In fact, he asks for the pope's help in this. And when Copernicus famously writes that "mathematics is written for mathematicians" *at the same time,* he means that "mathematics is *not* written for philosophers who are ignorant of mathematics." Thus, Galileo during the six meetings with the pope in 1624, 80 years after the publication of *De Revolutionibus*, could have realized that what his old friend Maffeo Barberini was asking of him, in effect, was to break away from the Copernican heritage. The cultured and shrewd Urban had fully understood what was at stake: it was not so much about the reality of the earth's motion—his deep knowledge about these matters must have convinced him that the evidence from Galileo and Kepler was already shaking the foundations of an earth-centered universe. Urban's aim was to avert the mathematical astronomers' having *sole* jurisdiction over these matters.

As if to counteract the censured phrase in *De Revolutionibus*, *The Assayer* includes a passage that has been ever since considered as the foundational stone of the new natural philosophy:

> Philosophy is written in that great book which ever lies before our eyes –I mean the universe– but we cannot understand it if we not first learn the language and grasp the symbols in which it is written. This book is written in mathematical language, and the symbols are triangles, circles and other geometrical figures without whose help it is humanly impossible to comprehend a single word of it, and without which one wonders in vain through a dark labyrinth (Shea and Artigas 2004, 100–1; Favaro 1896, vol. VI, 232).

It is an assertive and vigorous statement aimed at overcoming the distinction between Galileo's own philosophy and Aristotelian philosophy. The same intention and the same plan appeared in *The Assayer* as in the uncensored *De Revolutionibus*—but with a vengeance. Little does it matter if *The Assayer* is an indirect adoption of Platonism or not. It is certainly a proclamation that excludes Aristotelians from any participation in the new science.

Robert Westman (1990, 174) argues forcefully that often Copernican studies "underwrite an image of science that privileges conceptual and technical performances while ignoring social and political practices" and suggests that Copernicus has to be put "into the politics of his own church" (Westman 1990, 175). Copernicus himself was emphatic about who could follow his theories: it was the mathematically trained astronomers outside the philosophical schools. And, thus, he divided the church into two groups: those that were enlightened by mathematical training and those that were not—the idle theologians.

> Copernicus creates for mathematicians the disciplinary authority to violate received boundaries in their textual productions … those within the church where the mathematically skilled will not only understand and approve of his theory but will also accept the new standards for judging it. Those without such disciplinary credentials will misunderstand and deplore them. In short, mathematics is for mathematicians (Westman 1990, 183).

This is exactly what makes Galileo such a true and devoted ally of Copernicus.

Negotiating a Sociopolitical Agenda

As the impressions from the lavish ceremony of presenting *The Assayer* to the pope started to wane, the enjoyment of hearing Ciampoli read passages while Urban had his meals started to fade, and the Jesuits' defense of Aristotelians showed no signs of receding; it is highly likely that Urban reassessed *The Assayer* and its sociopolitical implications started to worry him. Is the apparent emphasis on epistemological issues a sufficient explanation of the intense and, especially, the uncompromising rhetoric of *The Assayer* against the Aristotelian philosophy, when it was absolutely clear to anyone who had a role in its preparation and, of course, to Galileo himself that the text would estrange and offend in irrevocable ways the Jesuits and the Aristotelians, whose role for the stability of the Church was so crucial? Certainly, Galileo's abrasive and confrontational character played a role in the style of the text. Further, the encouragement he received by the Linceans gave him the feeling that he was not alone and sharpened the tongue of the text even more. Surely Urban's election was a welcome occasion for all the Linceans to be more combative. But how justifiable is it to "read" *The Assayer* as a text that is exclusively about epistemological interventions and devoid of social and political undertones as some Galileo scholars do?

The Assayer had all the characteristics of a manifesto of the Linceans and expressed what was in store for the Aristotelians. And, as is the case with almost all manifestos, this one too was articulating a social and political agenda. The epistemological discourse had such an underlying agenda. It was expressing and, in fact, asserting and proclaiming that the mathematically trained natural philosophers rather than the Aristotelians should have the (sole) jurisdiction in assessing the structure of the cosmos and providing explanations about the phenomena of nature. This meant not only a change of the dominant views about nature and its phenomena, but a *receding social role* for the peripatetics. What appeared as a clash of ideas concerning epistemological issues in astronomy and physics was at the same time the articulation by the author(s) of *The Assayer* of a new social realignment between the Aristotelians and the mathematicians. New ways of viewing nature implied new power structures and, thus, it also meant the ascendance of a social group which would precipitate the dominance of the new ways of viewing nature. The former could not be achieved without the latter and that was something that could not be condoned by Urban, hence his suggestion to Galileo.

Manifestos have a rather complicated social function. They are not simply texts expressing the specific ideas of an individual or the objectives shared by the group of people who wrote the text. They are declaratory and, often, polemical texts; they become texts of reference for all those whose social (and in our case socioprofessional) identity is signified and defended through such texts. The self-perception of becoming a member of the collectivity that the manifesto intends to establish gives the manifestos a much-needed dynamic.

Undoubtedly, *The Assayer* did not adopt the position that Aristotelians and mathematicians formed two well-delineated that were homogeneous and inflexible so far as the epistemological values of each of its members were concerned. Furthermore, as has been shown by many scholars, Grassi's text, though polemical as well, was a text which was not as inflexible towards its adversaries as was Galileo's answer. But when a political and social agenda is at stake, such "soft spots" in a text are a liability. The nuances, the fine points of argumentation, and the mild expressions are left for the analysis of the experts of the future.

It is the fate of almost all polemical texts to leave their trace as texts which convey well-defined and uncompromising sets of argumentations. Polemical texts aim at rallying individuals and constructing the audiences for which they are written. The aims of those who write them is to "convince" the prospective members of their audience to fully identify themselves with what is being presented in the manifestos. And when political and social agendas are articulated in such polemical texts, these agendas are almost always about negotiating power relations.

It is too often the case that a host of political issues are seldomly explicated as what they really are. The discourse through which they are discussed may appear to the uninitiated that it includes all sorts of disjointed issues. As Biagioli argues, such is the ethos of court culture. In the discussions between Urban and Galileo, some of these issues were Copernicanism, God's omnipotence, necessary truths, and the role of philosophers and mathematicians. These topics *were* the means for discussing the serious political issue of the future social status of the Aristotelians and their relations with the mathematicians. J. G. A. Pocock's observations concerning political discourses is quite relevant to our case as well:

> when we speak of [political] languages, we mean for most part sub-languages: idioms, rhetoric, ways of talking about politics, distinguishable language games of which each may have its own vocabulary, rules, preconditions, and implications, tone and style (Lindberg and Westman 1990, 188; footnote 89).

It is one thing to be a mathematician and it is another to share common views with other mathematicians on how to oppose Aristotelians. It is one thing to be an astronomer and it is another to vie for the (exclusive) jurisdiction to investigate and assess the reality of celestial phenomena. Manifestos are important for those they manage to rally around the aims expressed in them and to whom they accord a collective consciousness.

They are also important because they explicitly state who they exclude. *The Assayer* drew socioprofessional boundaries. And boundaries create antagonisms, since those who are excluded become aware of what the "others" intend to deprive them of. Each social group is not characterized solely by the ideas its members carry. These ideas do, indeed, provide elements for the identity of the people comprising the group. The political significance of any social group is, in effect, their position in the power structures of the society they live in and the ways in which each group continuously stipulates, demarcates, and reasserts its identity. When Urban asked Galileo what we assumed he asked, he was very much worried that the changes he feared in the power structures themselves would be seriously detrimental to the Church and to him personally.

The agenda Urban may have proposed to Galileo during their meetings was the only way out of such a quandary, and it was also very much in agreement with the Congregation's decision about *De Revolutionibus*—but with a twist: Urban, as any good politician would do, took upon himself the role of harmonizing relations and reconciliating the warring groups. His was the third way: neither the Linceans' efforts to marginalize the Aristotelians nor the Congregation's effort to keep the mathematicians at bay. Urban was quite well-versed in these matters and he could not bear the responsibility of alienating the philosophers and the theologians. His proposal to Galileo aimed at dispelling this danger.

It was, of course, on record that the pope liked *The Assayer*. Strictly speaking, Urban enjoyed the passages read to him by Ciampoli, who was quite familiar with the text since it took months of gestation and negotiation of Galileo with the other Linceans concerning the final contents. The hypothesis we put forward may appear rather weak, since the pope's initial enthusiasm of *The Assayer* does not appear to be compatible with the agenda he might have proposed to Galileo which, in effect, aimed at reversing what was so clearly articulated in *The Assayer.* Might it, however, be the case that, between the end of October 1623 and the end of April 1624, the pope became aware of the full scope of *The Assayer*'s aims and its social aspirations? It is quite reasonable that such a realization may have forced Urban to change the feelings he expressed when passages of *The Assayer* were first read to him.[42] Perhaps as the months passed, more sober thoughts started to dominate Urban's mind. Was such a show of enthusiasm for *The Assayer* in October 1623 and for the bashing (and all that it implied) of the Aristotelians, an appropriate reaction for a pope? *Was it politically expedient?* Independent of how convincing Galileo was about the character of the comets, it was dawning on Urban that the real danger lay in those very artful polemics he liked so much. The hypothetical treatment of Copernicanism became something secondary, and the pope zoomed in to reverse the well-orchestrated attack against the Aristotelians. And, as is often the case in politics, thoughtless, frivolous, and spontaneous reactions have a way of coming back to haunt one.

Moreover, Urban was surely well informed of what was happening at the Collegio Romano at the beginning of the new academic year of 1623–1624, the first academic year after the publication of *The Assayer* and before his meetings with Galileo. As Cesarini reported it, every inaugural lecture stressed the importance of Aristotle and there were strong attacks against "the discoverers of the novelties in the sciences" (Redondi 1989, 130). That was the first warning that, perhaps, Urban's endorsement of *The Assayer* may not be too prudent a gesture. At the opening ceremonies for the next academic year, the Jesuits became even more assertive and left no doubt whatsoever that they would declare war on anyone who questioned the absolute authority of Aristotle.[43]

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[42] Almost everyone who refers to the pope's positive reaction to *The Assayer* refers to the letter by Cesarini to Galileo on October 27, 1623 (Favaro 1903, vol. XIII, 141). This letter, however, was composed the very next day after *The Assayer* was publicly presented to the pope. Of course, Cesarini would be informed about such a reaction through the close ties he had with Ciampoli.

[43] As Giovanni Bardi mentioned in his letter to Galileo on June 14, 1614, the Jesuits had a directive issued to them by their General to keep away from anything that might weaken the Aristotelian position. "The Society of Jesus was the guarantor that the Counter Reformation. A wedge between the papacy and the Jesuits would estrange the committed fighters of the Church … ." Urban undoubtedly was fully aware of the Jesuits' unbridled zeal to serve the Church. "Populations in territories that had been taken back from the protestants were reconverted to Catholicism, Jesuits raided churches where protestant rituals were practiced … and that no second thoughts when they were informed about corruption in monasteries. The decisions of the Council of Trent were to be followed unswervingly and the Jesuits had become the willing soldiers of such a crusade for the Church they served" (Redondi 1989, 47). See also (Galluzzi 2017, 275–276).

Even the slightest reaction against *The Assayer* by anyone in the Church or among the Jesuits, or even by well-meaning friends of Urban's, would have made Urban seriously rethink the overall aims of the text and, thus, of the Linceans. Two of his closest collaborators were Linceans, and the cardinal nephew was also a member of the Academy. The wording of the dedication of *The Assayer*, the text itself, and his public shows of admiration towards Galileo, if continued, could be sufficient motivation to be accused of siding with those who were no friends of the Aristotelians. Aristotelians were the backbone of the Collegio Romano and, hence, of the Jesuits, of the officials and the bureaucracy of the Church, of the various religious orders, of the teachers in schools, of the theologians. Of course, everyone who adopted such a socioprofessional identity was not a scholar of any distinction, and many would know only the mere rudiments of the Aristotelian philosophy. Nevertheless, what mattered was their concurring views and values and the associations and ties with the other members of a particular social group and its leaders, as well as their place in the power structure of Rome at the time. And *The Assayer* as a manifesto aimed at undermining this structure. Furthermore, Aristotelianism was the philosophy at the core of the interpretations of the Scriptures by the Fathers of the Church and of the theological elaborations of the Council of Trent. The Linceans, on the other hand, had a very outspoken spokesman in the person of Galileo, and *The Assayer* was a particularly articulate text laying down the directions of what the future had in store for the Aristotelians. Urban was becoming increasingly concerned, and he knew that the only effective way to bring about some kind of appeasement would be if the Linceans showed signs of agreement with his proposed agenda of reconciliation. Galileo was the key figure in such a plan.

Urban was asking Galileo to become a willing accomplice in creating a new consensus for a totally different relationship between the mathematicians and the Aristotelians than the one Galileo (and the Linceans) expressed in *The Assayer*. Furthermore, by enthusiastically endorsing a book by Galileo written along his suggestions, the politically embarrassing reaction of Urban when *The Assayer* was first read to him would fade away. The Jesuits would be happy, and a new social alliance between the philosophers and the mathematicians would be in the making. In the half year between the presentation of *The Assayer* to the pope and the latter's meetings with Galileo, Urban's priorities changed.

Urban was moving away from the intellectual agenda of Cardinal Maffeo Barberini and was slowly fenced in the political exigencies of the office he served.

Most importantly, as the months passed, Urban was realizing that the intellectual wanderings of Cardinal Maffeo Barberini *by themselves* could not meet the exigencies of the *political office* he now held. *As it happens in such historical institutions, in due course, the (newly) elected office holder becomes more and more enmeshed with the historical and political stipulations of the office itself.* This is an iron rule of, especially, old institutions. Sooner or later, the office holder becomes more and more aware of the nature of the office itself and of its constraints—and, of course, the opportunities it provides. The constraints of political offices such as the papacy do not dictate solely what the office holder cannot do, but also under what conditions he can do what he plans to do. So, the constraints of a political office define the range of possibilities of the intended policies of the office holder. And within such a framework, it was not a proper thing to let it be known that the pope enjoyed *The Assayer.* It was not difficult for Urban to realize that if what was in *The Assayer* was dynamically pursued by the mathematicians, then the ensuing problems within the Church would be very serious indeed. There was a dire need for appeasement. He knew that the only effective way would be if the Linceans showed signs of agreeing with his proposed agenda of reconciliation, and Galileo was the key figure to achieve this. Hence his suggestions to Galileo to (partially) preserve the *status quo* through the formation of a new social alliance: to guarantee that the Aristotelian philosophers and theologians would not be pushed off-center over matters of the cosmos, to stave off their marginalization, and at the same time allow the mathematicians to have the role Galileo was aspiring for them.

To reiterate, the constraints of political offices such as the papacy do not solely dictate what the office holder cannot do, but also under what conditions he can do what he plans to do. So, the constraints of a political office define the range of possibilities of the intended policies. Of course, in an institution such as the papacy, the dialectic between the office and the office holder is perpetually at work: the office, in effect, inflicts gradually the historically formed constraints on the office holder. At the same time, the person who has donned the office attempts to expand these boundaries in order to implement their own agendas. The history of the institution dictates its own constraints to the office holder, and the way an office holder gets enveloped in the political clout of the office is like an inverse phenomenon of the morning dew: as the months pass, the historical becoming of the office descends upon and envelops the office holder.[44] Thus, the pope's enjoyment of *The Assayer* the next day it was presented to him and a few weeks after he had officially taken office can be understood as more of a reaction in tune with Cardinal Barberini's personality which, as the months passed, would be progressively transformed into that of Urban.

The outcome of this dialectic depends on the active role of the office holder: either he surrenders to what the office dictates or attempts to bring about changes to the office itself, which will be progressively tagged on its historical becoming. But none of this negates the political essence of the office itself and the necessity of the office holder to adapt. And Urban VIII was "well versed in the business of the world and the interests of princes that it might be thought he passed his whole time in the school of politics" (von Ranke 1901, 340–2).

The spiritual character of the papacy can by no means supplant its profound political nature. Urban VIII was no longer Cardinal Maffeo Barberini, and his proposal to Galileo was a well-articulated political and social agenda favoring the (partial) preservation of the *status quo* through the formation of a new social alliance: to guarantee the continuation of the jurisdiction of Aristotelian philosophers and theologians over matters of the cosmos, to stave off their marginalization, and to allow mathematicians to have a role but not the one Galileo was aspiring for them. Alienating the philosophers and the theologians would be detrimental to the papacy. Urban did not want the age-long hierarchy between philosophers and mathematicians to be reversed. His proposal aimed at striking a balance where philosophers and mathematicians would be on a par regarding their socioprofessional status—in contrast to what Galileo advocated. Urban wished to reverse what his intellectual and theological dealings prepared him to see clearly: he was becoming increasingly alarmed that in the emerging new world the philosophers and the theologians would not have the role that the Church would want them to have. The signs were all too evident. And, most importantly, Galileo had repeatedly shown that he would be championing such a crusade against the Aristotelians. Barberini's suave intellectual wanderings, interests, and interventions over the years

[44] From the end of the sixteenth century, there was a bureaucracy that limited what a pope could do, putting constraints to the pope's absolutism (Pattenden 2017).

were translated by Urban into a solid political agenda. The lethal attacks against Grassi in *The Assayer*, apart from their playful and attractive ingenuity which he had liked so much a few months earlier, were now casting their long and menacing shadow. Seen from the point of view of the papacy, the humiliation of the philosophers became a chilling prospect. If things developed along the lines of *The Assayer*, and if Galileo and the Linceans were allowed to continue targeting the philosophers in such unforgiving ways, there would not be too much of a future for the Peripatetics and, perhaps, for their support for Urban's papacy. This is what Urban wanted to stop.

What Urban thought and asked Galileo for his cooperation with was not anything greatly removed from what was happening in the European universities—quite the opposite. Urban's agenda had much in common with what was happening among the Aristotelians in the universities all over Europe, who were attempting to come to terms with Cartesianism, Baconianism, and, to a certain extent, Copernicanism, later also including Locke's and Newton's ideas in the seventeenth century.[45] Not all attempts by the philosophers were successful, there was a lot of resistance in their ranks, but what was definitely the dominant trend among the professors of philosophy was their attempts to adapt to the emerging new circumstances. Urban must have also been a direct witness to such debates. While in Paris from 1601 to 1604, appointed by Pope Clement VIII as papal legate to the court of Henry IV of France, the then Cardinal Maffeo Barberini's intellectual interests must have brought him into contact with the philosophers of the University of Paris—the university which was *par excellence* the university of the Peripatetics. It is impossible for Barberini not to have witnessed at least some of the heated discussions about the ways the Aristotelian philosophers were discussing their own future and role. Urban was, indeed, informed of discussions and disputes at the Sorbonne, as it is noted in the instructions given by Urban to the Archbishop of Damiata—clerk of the chamber, nuncio in ordinary to the King of France—where the pope expressed his dissatisfaction with those members of the Sorbonne "by whom the doctrine of independence of the temporal power and the divine right of bishops was put forward and defended" (von Ranke 1901, 346). Furthermore, Urban's agenda was almost similar in the work of Marin Mersenne (1588–1648), who was a "salient example of a churchman intensively engaged with modernizing developments within the framework of traditionalist concerns"

[45] See for example (Gascoigne 1990). See also articles in (Osler 2000; Feingold 2002).

(Westman 2011, 496). This he did in his voluminous work on commentary of *Genesis* published in 1623.

Similar activities were also taking place in Rome. Giulio Cesare Lagalla, one of the people who had encouraged Galileo to react to Grassi's *Libra,* died in 1624. He was a rather eccentric figure at Rome's Sapienza University—nominally an Aristotelian, but with many borrowings from Galileo. Lagalla, Benedetto Castelli (1578–1643; appointed in 1616 as the chair of mathematics), Mascardi (who was appointed in the chair of rhetoric in 1628), and the support for the *The Assayer* by the Dominicans give ample signs that what we could assume Urban's agenda to be a feasible prospect (Redondi 1989, 99–101).

Furthermore, though the pope was surrounded by so many Linceans, he did not appear to wish for the Academia to continue its activities. If he did, in all probability, he would have encouraged his nephew Cardinal Francesco Barberini to become its head when it was offered to him by the Linceans after Cesi's death in 1630. The Academia's dire finances were known to all. Both the pope and Cardinal Francesco Barberini were well aware that with Barberini as its head, the Academia would have a chance to survive. Since it is highly improbable that Francesco Barberini did not consult Urban when he was asked to take Cesi's position after the latter's death, and since both the pope and the cardinal knew that without Barberini at its helm the Academia was doomed, Francesco's refusal of the offer could be understood as the decision of the pope to put an end to an institution that was causing such a headache to the Jesuits and the Aristotelians—and, thus, to the papacy. Francesco's refusal to take over was, of course, also the result of financial considerations. But the pope would be more convincing for the Aristotelians if he, in effect, put an end to the Academy, instead of having his nephew at its helm in order to "reform" it. The ensuing conflicts would have been so messy, and refusing to save it by having Francesco accept its direction was the best solution.

Urban's aim was to *redefine* the role of the Aristotelians and the theologians concerning a host of issues whose jurisdiction was slowly being deprived from them. In Urban's mind, it was an agenda for which Galileo was the perfect person. In the book, Urban urged Galileo to write the Peripatetics would no longer be those who challenge and resist the new ideas, but those who would, together with the astronomers, establish the hegemony of the (hypothetical) Copernican ideas. Serious epistemological readjustments were, of course, necessary, and it was a hopeful prospect that there were Peripatetics who had shown signs of their willingness to discuss these issues. If only Galileo and the Linceans were not so unyielding and resolute. The pope wanted to use Galileo in order to stabilize power structures over who commands the sovereignty for a whole system of ideas about the structure of the cosmos. The pope's friend of many years, who spared no blow at the expense of the Aristotelians, with the pope's endorsement could become the person who would help coalesce the new social order Urban envisaged. Urban knew Galileo well. His "social skills, extraordinary literary capacity, brilliant repartee, eloquence and charm" (Santillana 1962, 121) were well known to all. Urban was convinced that Galileo's vanity, his political shrewdness, his wish to be in good terms with the pope and the Church, the strength of his prose, and his lack of solid proof about the earth's motion with the tides may have prompted him to be sympathetic to such an agenda.

Urban wanted to uphold the social role of the philosophers and, thus, of the theologians; he did not want them to be stripped off a role they had enjoyed for centuries. This is what the stability of the Church demanded. Of course, the pope's agenda had been nourishing on Maffeo Barberini's ideas: if the ideas about the cosmos were expressed in accordance with the dogma of God's omnipotence and not projected as necessary truths but as hypotheses, why should the philosophers not have the same jurisdiction as the astronomers over these ideas? What good would come out of Galileo's bashing them? Old ways and old practices had to be reinstated, defended, and, most importantly, *redefined*.

Galileo, though unwilling to immediately accept Urban's agenda, may have wanted to think over his options, since such a request by the pope could possibly have a rather beneficial effect in the way he would treat Copernican ideas: if the pope was insisting on such an agenda, the issue of *how* hypothetically the Copernican ideas should be treated could, perhaps, take a back seat. In other words, if Galileo could find a way to deal with the pope's agenda, then it may have been easier for Galileo to push his own epistemological agenda. But what about his social agenda? The discussions during the six meetings did not only involve issues in epistemology and theology, but also topics regarding the identity of social groups, their social role and the dangerous consequences of the antagonism among these groups. These discussions unveiled the underlying incommensurability of the social and political agendas of the two protagonists. And as each was trying to elbow his way into the other's dominion, they needed many meetings to reach some mutual understanding of what the consequences of the different agendas to each one of the protagonists would be.

Negotiating such issues could have surely taken "six long meetings."

The aim of this paper is not to bring anything new to the elaborate analyses concerning the trial of 1633. The aim is to test whether the hypothesis we put forward as to what could have happened during the meetings of Galileo with Urban VIII in 1624 can help us elucidate a number of subsequent events that were either ignored by historians or have not been closely studied.

We will now discuss these five such topics.

The Rush to Leave Rome

How is one to explain the feeling of frustration and deep disappointment in the two letters Galileo sent from Rome to Cesi—one in the middle of his stay and one just before he left for Florence? How can one understand the lack of indication as to what went on in his discussions with the pope and his wish to leave Rome as soon as possible? How convincing is it that Galileo waited for Zollern's meeting with the pope in order to be informed of the latter's views about Copernican ideas, as if he had not had any opportunity to discuss these issues in all his meetings with the pope? How is one to understand that even though Galileo and Cesi had together organized Galileo's strategy while he would be in Rome, there is no mention whatsoever of the meetings with the pope in any of the subsequent letters of Galileo to Cesi? Might it be the case that the pope's suggestion strongly disoriented him since it was so unexpected, and both Cesi and himself did not anticipate such developments in their discussions? Might it be the case that this was the reason for turning down Cesi's invitation to stop over at Acquasparta on his way back to Florence? Might it be the case that Galileo did not want to share with anyone what happened in his meetings with the Pope because he was faced with such a dramatic dilemma and wanted to decide how to deal with the situation all by himself?

While in Rome during 1624, Galileo wrote three letters to Cesi. As for the first one right after his arrival, we do not have the original letter but only Cesi's answer. "The court my dear sir is a source of infinite trouble and beyond the official visits there are innumerable calls of courtesy to be made."[46]

On May 15, 1624 after possibly three meetings with the pope, Galileo wrote to Cesi (Favaro 1903, vol. XIII, 178–80) and complained that his obligations in Rome were taking their toll and he wished to go back to Florence to his "quiet and idle freedom" so that he could finish the work they had planned with Cesi. He tells Cesi he could make some progress concerning "some of the issues we discussed together" if he had enough time and patience but does not spell out what these issues are. "Life is short and to play the courtier one must be young and strong," and he could not sit around forever waiting for officials pressed by business to lend him an ear. He also informed Cesi that Cardinal Frederick Eitel von Zollern (1582–1625) would meet the pope in the next days and would raise the issues about Copernicanism: "although [he is] not very profound in these studies of ours. I shall gladly hear what he finds out, but there are so many issues that are considered infinitely more important than those I mentioned and these absorb all the time available so that our questions are neglected." How much more cryptic could Galileo be? What were these "infinitely more important" matters? Were they related to the political developments due to the war? Were they matters having to do with the demands of the office? Or was it the case that Galileo was hinting at the issues raised by Urban in their meetings? In any case, the frustration expressed by Galileo is evident. Cesi's advice is that "time and patience will help!"[47]

As it has been remarked, this letter is "studiously circumspect" (Shea and Artigas 2004, 110). If we adopt the received view that, in his meetings with the pope: Galileo asked for the repeal of the 1616 injunction and the pope refused, and that the pope was rather adamant that the Copernican ideas should be treated as hypotheses, are the sentiments emanating from this letter sentiments of a person who was so elated when Maffeo Barberini became pope that he went to Rome to congratulate him? Was Galileo so naive as to expect the repeal of the 1616 injunction and was now disappointed, so that he wanted to go back to the peacefulness of Florence? Surely Galileo was not that naive. But why such an urge to get away from Rome? Does this letter not express a state of distress

[46] The letters Galileo sent while he was in Rome are: Galileo to Curzio Pinchena 27 April 1624; a letter to Cesi, but we only have Cesi's answer from 30 April 1624; Galileo to Cesi 15 May 1624; Galileo to Cesi 8 June 1624 (Favaro 1903, vol. XIII, 175; 177–8; 182).
[47] Cesi to Galileo 18 May 1624 (Favaro 1903, vol. XIII, 180).

and consternation? One gets the feeling from the letter that, contrary to his wishes, Galileo is being dragged along in Rome. Does this letter express the feelings of someone who was looking forward to the rest of his stay, of a person who is rejoicing at the privilege he had of meeting the pope a few times? Furthermore, he tells Cesi that he is longing to go back and start working on their common project, as if he were forced to stay in Rome where he is wasting his time. It is somehow difficult to understand Galileo's mood if most of what they planned with Cesi had, at least partially, found a sympathetic papal ear. Might he have expected a more enthusiastic pope toward his and the Linceans' agenda since he had been informed that the pope liked *The Assayer* very much? Perhaps. But might it be the case that in these first meetings Urban might have presented him with his plan, at odds with Galileo's and, worse, he wanted Galileo to put it to practice. And might it be the case that these "infinitely more important matters" took precedence over everything else? One gets the impression that Galileo in this letter conveys to Cesi a sense of a dead end regarding his meetings in Rome and longs to go back to Florence.

On June 8, 1624 Galileo wrote again to Cesi (Favaro 1903, vol. XIII, 182–3).[48] He was still in Rome "although against my will." He had wanted to depart fifteen days before, in order to be in Florence in time "to be able to do a little purging, of which I feel in need." He informed Cesi that he would leave in a few days. How is one to understand Galileo's wish to leave Rome so that he can "purge" himself ("*essere a Firenze in tempo di poter fare un poco di purga*")? Purge himself of what exactly? The excesses of Roman life? But this very word used by someone so very familiar with Dante's work about which he lectured in his youth may carry some suggestive connotations: Might it be the case that these are the feelings of a remorseful Galileo who, after meeting the pope, thought that the all out attacks against the Aristotelians may have been a bit too much? Might Galileo feel that he wants to be purged from what the pope was asking him to do—something which was so much at odds with what he had planned? Whatever the answer is, the Galileo of this letter is a disturbed, confused, and shaken Galileo.

Six meetings with the pope, and this is what they came to? And why did Galileo, in his letter a few days before he left for Florence and after the six meetings he had with the

[48] In this letter Galileo also informs Cesi of his meetings with Cardinals Franscesco Barberini, Tadeo Barberini, Scipione Corbelluzzi, and Francesco Boncompagni, as well as of the presents he received from the Pope.

pope, not disclose anything substantial about the meetings? The sparkling and full-of-energy Galileo who had arrived in Rome on April 23 was six weeks later worn out and exhausted. And surely disappointed.

If, as it is widely accepted, Urban during these meetings asserted—again—his views about Copernicanism, why does Galileo mention to Cesi that he is delaying his departure? Because Zollern had promised to bring up the issue of Copernicanism with the pope during their meeting? Cardinal Zollern had been appointed bishop of Olmutz in Bohemia in 1621, and he planned to pay a courtesy call to Urban before leaving for his post. He was also given a microscope by Galileo as a present for the Duke of Bohemia. In a dinner with cardinals Scipione Cobelluzzi (1564–1626),[49] Francesco Buoncomagni (1592–1641), and Zollern, the latter expressed his willingness to ask the pope about his views on Copernicanism. But since the pope's views about the hypothetical treatment of Copernican ideas were so widely known, how reasonable is it to assume that Galileo delayed his departure from Rome in order to hear from Cardinal Zollern what the pope had to say about the way Copernicanism should be treated? Furthermore, Galileo had six meetings with the pope; therefore, are we to believe that the issue about the hypothetical character of the Copernican ideas was not brought up? After meeting the pope, Zollern, according to Galileo, remarked that the pope said that Copernican ideas were not heretic if they were treated hypothetically! But surely Galileo, after the six meetings he had had with the pope, did not need to wait for Zollern's meeting with the pope to be informed of what Urban thought about Copernicanism. It is thus more reasonable to assume that Galileo did not expect to hear anything new from Zollern and delayed his departure from Rome out of courtesy towards the cardinal who out of his own initiative volunteered to talk to the pope about Copernicanism.

There may, however, be a complementary explanation: if our hypothesis concerning what was discussed between Urban and Galileo is viable, then Galileo may have wanted to wait for the meeting of Urban with Zollern to see whether Urban would disclose anything to Zollern concerning what he had discussed with Galileo. Though it was not expected for Urban to disclose to Zollern the whole range of what he had discussed with Galileo, it was conceivable that he could part with some of what had been discussed. In this case, it

[49] He was chief archivist of the Vatican Secret Archives from 1618 until his death.

would be rather reasonable for Galileo to wait for the outcome of the meeting between Zollern and Urban.

The impression one gets from these letters to Cesi and from Cesi's answers can perhaps be understood if Galileo did not want to disclose to Cesi something so absolutely unexpected like Urban's proposal. Surely Galileo and Cesi had not anticipated such a development throughout their own lengthy discussions in Acquasparta before Galileo's arrival at Rome. Furthermore, Galileo must have felt trapped if he was asked to carry out Urban's agenda and put the Aristotelians on the same par as the mathematicians. Galileo was at a loss and perhaps faced with a moral dilemma: was he betraying his own (and the Linceans') aspirations and agenda? While in Rome, it was difficult to make up his mind, and, thus, he did not wish to share the confusion brought about during his meetings with the pope, not even with Cesi. And how could he, since the pope's suggestion was such a strong blow to the Linceans' agenda? The uneasiness and sense of disappointment in the letters, together with Galileo's desire to go back to Florence, could perhaps be more compatible with the shock he felt after Urban's proposal of the kind of book he was asked to write.

The sentiments conveyed by Galileo's two letters to Cesi, one in the middle of his stay in Rome and the second a few days before he left for Florence, are very far from the optimism and ebullience that were so compelling in his letters before the trip to Rome and during his stay at Acquasparta. In fact, the contrast between his feelings before and after the visit to Rome was so apparent that it could only be explained either by a deep disappointment of Galileo with respect to what he was expecting to happen but did not come to fruition, or by something else that may have happened and was so unexpected that he did not dare disclose it even to Cesi. It is quite possible that during these exhaustive encounters, the theological, intellectual, and even judicial issues were negotiated within the framework of long-term political agendas. And what emerged was the dramatic incompatibility between Galileo's expectations before meeting the pope and Urban's proposals during their meetings. Galileo aimed, of course, to reform philosophy, but in such a reform the peripatetics had no place. In these meetings, he was asked to give them a rather prominent place. The shocked and confused Galileo needed to go back to Florence and do some serious thinking. Perhaps in such a context Galileo's decline of Cesi's invitation to stop in Acquasparta could be better understood (Galluzzi 2017, 314).

The *Reply to Ingoli*

Why did Galileo decide to write an answer to what Ingoli had written eight years earlier after an oral dispute between them? Most historians believe that he did it in order to have a trial run of the ways he would deal with the Copernican ideas in the book he was planning to write and see what the reactions would be. Might it be the case, though, that he also wrote the reply in order to show to the pope his willingness to treat the philosophers in a much more civil way than he had done in The Assayer *and, more importantly, in ways that invite compromises—just as Urban suggested? Furthermore, Ingoli—after their dispute in 1615—was a key figure in the corrections of* De Revolutionibus*, and Galileo may have wished to have the last word on the matter.*

In 1615/1616, after Galileo and Ingoli had an oral dispute about Copernicanism, they decided to put their arguments in writing. Ingoli in 1622 was appointed secretary of the newly founded Congregation Propagation of Faith by Pope Gregory XV (1554–1623), and he was very close to the Ludovici family, who were among the benefactors of the Jesuits (Heilbron 2010, 261). Ingoli got involved in the corrections to *De Revolutionibus* in 1616 as secretary of Cardinal Bonifazio Caetani, who, like Maffeo Barberini, was a member of the Congregation of the Index but not of the Holy Office. The possible "continuity" between the corrections of *De Revolutionibus* and *The Assayer* that we discussed above might provide a little more insight as to why Galileo wrote in such a haste the *Reply to Ingoli*. It was a text written right after Galileo's meetings with the pope in 1624 and a response to a work written eight years earlier by Francesco Ingoli. Galileo was discouraged by the Linceans to publish it, but it circulated widely and was read to the pope who, as Ciampoli reported,[50] liked it. Sometime in 1625 there was an anonymous complaint against *The Assayer,* and Cardinal Barberini asked his personal theologian, Giovanni di Guevara, to investigate the complaint. However, he did not find any serious problems with the text.[51]

[50] Ciampoli to Galileo 28 December 1624 (Favaro 1903, vol. XIII, 295): "The Pope read the Tides and the Reply to Ingoli and the Pope liked it."
[51] Galluzzi (2017, 293) says that Guevara's appointment was made after Cesi's intervention (Shea and Artigas 2004, 119). According to Drake (1978, 300-1), this complaint about which Galileo was informed by Guiducci on April 18, 1625 (Favaro 1903, vol. XIII, 265) made Galileo stop work on the *Dialogue*. See also an analytical

Might it be the case that the tone against the Aristotelians in the *Reply to Ingoli*, which was already circulating in Rome and had been sent to the pope earlier, may have prompted the cardinal nephew to appoint Guevara?

Most probably in January 1616, Ingoli had written his essay *On the Location and Rest of the Earth, Against the Copernican System* and sent it to Galileo. It was one "of the most complete, intellectually ambitious and historically significant" (Finocchiaro 2005, 72) essays containing all the arguments against Copernicanism in a systematic way. Due to the 1616 developments of the Inquisition, Ingoli did not publish his essay (which was not published until 1891), but it circulated among many people. Most probably it was the basis for the Consultants' report of February 1616. By his own reckoning, in this essay he presents 22 arguments against Copernicus' claims: 13 are mathematical (i.e., astronomical), 5 physical, and 4 theological. The text is divided into sections related to astronomical arguments for geocentrism; physical arguments for geocentrism; physical arguments against the earth's diurnal motion; astronomical arguments against the earth's annual motion; comments on Copernicus' third motion of the earth; and physical arguments against the earth's motion in general.[52] In addition, there were four arguments based on theological considerations, to which for obvious reasons, Galileo did not respond. As previously mentioned, Ingoli was also the person whose report on what to censure in *De Revolutionibus* was approved by the Congregation in 1620—and the Congregation was also based on his report to put Kepler's *Epitome of Copernican Astronomy* on the Index of forbidden books on February 28, 1619 (Mayaud 1997, 59).

In his *Reply to Ingoli*, Galileo responded to all the arguments, except the theological ones. In addition, Galileo informed Ingoli that one of the reasons why he was responding after so many years is that no one had tried to show that it was not out of ignorance—as some Protestants thought—that Catholics proceeded with the prohibitions of 1616.

> I hear that the most influential of the heretics accept Copernicus' opinion, and I want to show that we Catholics continue to be certain of the old truth taught by the sacred authors, not for lack of scientific understanding (Finocchiaro 1989, 156).

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discussion by Redondi (1989), especially p. 142–3, 168–172. For a different interpretation of the G3 document discovered by Redondi, see (Galluzzi 2017, 286–299).

[52] This classification is can be found in (Finocchiaro 1989, 347). His extensive analysis of the *Reply to Ingoli* is in (Finocchiaro 2005, 154–197).

The author who makes such an assertion in a text written in the vernacular as an answer to a Church dignitary surely implies that he is also protecting the Catholics from the embarrassing accusations of the Lutherans.

Most historians are of the opinion that the *Reply to Ingoli* was a way for Galileo to test the limits of tolerance of the Church in view of his forthcoming *Dialogue*. Such an opinion may be defensible, even though there is almost nothing about the hypothetical character of the Copernican ideas in this text. Our suggestion of what went on during the meetings of Galileo with the pope in 1624 perhaps provides a complementary explanation of why Galileo decided to respond to something written so many years earlier. If Urban pushed for an agenda of reconciliation between the philosophers and the mathematicians, it was only natural for Galileo to ask the pope that such a reconciliation would not be achieved if the Church continued limiting any jurisdiction of mathematicians in cosmological matters, as it happened with the censures of *De Revolutionibus.* The pope was fully aware that if his suggested agenda had any chance to succeed, then there should be a way for the Church to amend what happened in the censure of *De Revolutionibus.* This, of course, could not have been done by revoking even partially the censure. Galileo (or even the pope) may have suggested that if Galileo composed a text as an answer to Ingoli and the pope subsequently approved it, then this would be a way for the pope to acknowledge his unhappiness with Ingoli—something that would not go unnoticed among the theologians and, of course, the Aristotelians.[53] By answering all of Ingoli's objections and being silent about the theological issues raised by the latter's text and with a much milder treatment of the Aristotelians—as a kind of teaser of what is to come in the *Dialogue*—Galileo would secure the pope's consent for the upcoming book. It was a plan favoring Galileo and indicating how he would deal with the pope's suggestion.

In the *Reply to Ingoli*, neither the intensity nor the dismissiveness of the attacks were anywhere close to those in *The Assayer*. So far as polemics go, Galileo, in this text, adopted a different treatment for the Aristotelians: while he channeled the ghastly roar of a forceful lynx in *The Assayer* against everything Aristotelian, in the *Reply to Ingoli* Galileo sounded more like the purring of a domesticated cat. The *Reply to Ingoli* was "a limpidly clear work brief, vivacious and yet serene. The debate with his adversary had

[53] Geymonat (1962) thinks that since the reply was not sent to Ingoli, it was addressed to the pope.

nothing of the sharp polemic character of *The Assayer*" (Geymonat 1962, 117). Was it because he wanted to show that he would go along with Urban's suggested agenda? Was it because he wanted to test the way in which he would deal with Copernican ideas? Was it the case that such seemingly unnecessary and untimely answer was to show to the culprit Ingoli, who was so critical of Copernicus, that he should be exposed for what he was—an ignoramus? Was it the case that he would accuse Ingoli that his views gave the impression to the Lutherans that Italians reacted against Copernicus because they were not aware of the details of astronomy? Though all these were part of the Reply *to Ingoli*, the gentlemanly closing of his text had nothing in common with the extreme polemic against Grassi:

> I beg you to receive these answers of mine graciously, and I hope you do that both because of your innate courtesy and also because it is the appropriate thing to do for every lover of truth. For, if I have correctly resolved your objections, your gain will not be small, since you will be exchanging false hoods for truth; on the contrary, if I am wrong, then your point of view will show itself all the more clearly (Finocchiaro 1989, 197).

Interestingly, in these exchanges there was a person-to-person confrontation, unlike what had happened with the disputes about the comets where the adversaries spoke through others.

Could there be a connection between the censures to Copernicus' *De Revolutionibus*, the decision of Galileo to write *The Assayer*, and the *Reply* to Ingoli? It must have been clear to Galileo that Kepler's ellipses had dramatically transcended the Copernican arrangements. Galileo, however, insisted on the double agenda of Copernicus that was absent in Kepler. Copernicus, in his dedicatory note to Pope Paul III, was planting the seeds of a mathematical natural philosophy and pleaded with the pope to exclude the Aristotelians from discussing these issues. Copernicus hesitantly but clearly heralded a program for a new power structure concerning the sciences and, especially, astronomy. To "protect" his ideas against slanderous attacks, he strongly underlined the unbridgeable cultural differences between the mathematicians and the Aristotelians. And that is exactly what Galileo did: more aggressively than Copernicus, more focused as far as the attacks against the Aristotelians were concerned, epistemologically more complete. One gets the feeling that he alone was the worthy heir of Copernicus. For somebody like Galileo, who was absolutely convinced about the earth's motions, Kepler's involved calculations, his mysticism, his Lutheranism, and his lack of combativeness against the Aristotelians rendered Kepler unworthy of Copernicus' legacy. To Galileo (and the Linceans) it became clear that unless one adopted a specific social agenda, the theoretical arguments about explanations of natural phenomena would bring about new counterarguments in a cycle of never-ending and self-duplicating discussions. The emphasis could not be exclusively placed on articulating convincing methodological rules, but on the intense attack against "the other." And, thus, the hegemony of one set of ideas can be achieved only if the social standing of one social group is realized at the expense of the other.

The *Reply to Ingoli* was not only a dress rehearsal for the *Dialogue* vis-a-vis the way Copernicanism would be dealt with. It was also a clear message that Aristotelians would be treated in a civilized way and, more importantly, in ways that invite compromises—just as Urban may have suggested. Independent of some jabs under the belt, the tone was definitely different than the one in *The Assayer*. To the Romans, who were well trained in the subtleties of the political discourse of the times, the signs were clear, so was the timid change of course in his attacks against the Aristotelians. Urban must have been among the first who could understand such subtle messages. He liked the text. And he may have thought that when the proper book came out everything would be fine. Furthermore, the *Reply to Ingoli* included for the first time a public acknowledgment by Galileo of what Cardinal Zollern had told him after meeting the pope—that it was not out of ignorance that Italians did what they did in 1616, as it was circulated by many Lutherans.

The *Reply to Ingoli* conveyed a different message to different audiences. To Urban, it conveyed Galileo's dutifulness that he may be moving along the instructions he received from him by toning down the vicious attacks against the Aristotelians. To the philosophers, that they could hope for a more "humane" treatment. To the mathematicians, that, despite *De Revolutionibus'* censure, they still had a forceful spokesman. To the Congregation, that the mathematicians did not budge. To the Italians, that they had a voice to defend them. And to Ingoli, that he was way off in all his objections and, hence, totally incapable of defending the Church and the Italians against the Lutherans. Of course, to accuse an important person in the Church's administration of his work not being sufficiently convincing to discourage the heretics from accusing the

Catholics of ignorance needed a bit of nerve. But we know that the maestro had plenty of that!

The Linceans' social and political agenda was well shielded by what appeared to be a radical epistemological agenda. The dedication of *The Assayer* to the pope, the information they received that Urban liked the passages read to him while he was having his meals, and Galileo's meticulous preparation at Cesi's villa as to how Galileo would push *his own* (and, of course, the Linceans') political and social agenda when meeting the Pope were all defused by Urban's suggestion to Galileo. What Galileo and the Linceans had so cunningly orchestrated turned into a boomerang. Urban's agenda was now casting aside the Linceans' agenda. All of a sudden, Galileo realized that he was outsmarted by Urban and was faced with an unexpected metamorphosis: Urban the Pope was no longer Maffeo Barberini the Cardinal. Urban the Pope was articulating a political agenda, one that like Galileo's would have long term social effects. But the two agendas had totally different aims. During these meetings, two conflicting political agendas emerged. Galileo may have thought that his own political agenda would not be discerned by the pope if it was shrewdly hidden behind an apparently astronomical enterprise concerning the earth's motions. The unanticipated turn of events undermined all other negotiations of purely intellectual matters. Surely, it is not unreasonable to assume that the discussions of these issues needed many meetings. This is especially so, since the pope's agenda did not seriously undermine Galileo's plans about the ways in which he could deal with the earth's motion and, thus, it was not something Galileo would in his own mind refuse right away. "An interesting bargain" he may have thought. "I will prove to the world that the earth moves, and will be more tolerant with the Aristotelians." It is our contention that Galileo attempted to carry out such an agenda in his *Reply to Ingoli*, but eventually it appears that he could not forgo his commitment to his own social agenda which was identical to Copernicus'. And though his social agenda was historically vindicated, Galileo in 1633 suffered, among other things, a humiliating political defeat.

A Delayed Publication of a Book That Was Already Completed

Why did Galileo take six years to complete the Dialogue *while the book was, in effect, ready by the end of 1624? The explanation that he had health problems cannot provide a fully convincing answer since during this time he did do work in engineering and physics. Was it* *not rather odd for a sixty-year-old person with serious health problems not to finish a book that was almost ready, soon after he returned from Rome? How are we to understand the situation whereby he announced the completion of the book at the same time that he received information that his prediction of the periodicity of the tides was not confirmed? Might it be the case that he delayed announcing the completion of the book because he was buying time and waiting for the right time to present the manuscript in Rome at a time when Urban would be faced with serious problems and what they may have discussed in 1624 would not be on top of the pope's mind? If this was his plan, then "completing" the book in 1630 and starting the procedures for getting the imprimatur was, indeed, the right time.*

Almost everyone agrees that the main arguments that supported Copernican ideas and subsequently found in the *Dialogue* were worked out by the end of 1624. The *Discourse on Tides*[54] (1616) and the *Reply to Ingoli* (1624) which he wrote immediately after leaving Rome contained almost everything substantial of what is in the *Dialogue* and much of what is in these texts is found verbatim in the *Dialogue.* Why, then, did it take Galileo almost six years to complete the *Dialogue* while at the same time carrying on with his other work (Drake 1978, 289–330)? It was, indeed, the case that he had health problems (something too common with him), but despite them he was able to work on other things such as engineering and physics. In fact, there is a consensus among Galileo scholars that the *Reply to Ingoli* could be considered as a first draft to the *Dialogue* (Finocchiaro 2009, 72; Galluzzi 2017, 320). It is characteristic that in the very last paragraph in his *Reply to Ingoli* Galileo tells Ingoli that the issues dealt with in his text would be treated "at much greater length if I shall have the time and strength to finish my *Discourse on the Tides* which takes the motions attributed to the earth a hypotheses and consequently gives me great leeway to examine in length all that has been written on this subject" (Finocchiaro 1989, 197). Here we notice that he already had in mind even the title of his forthcoming book, and he also talks about *finishing* the book.

So, the book was, in effect, ready; it did not need six more years to be completed. Why, then, did Galileo make such a detour and not finish the book right away, but busy himself with other pursuits? After all, any delay favored his enemies to regroup against him.

[54] In early 1616, he had written about the tides and sent it to Cardinal Orsini. But see also (Drake 1978, 274) about tides and misinformation.

Through a letter to Diodati, we are informed that by the end of 1629 he "took up the *Dialogue* again" (Favaro 1904, vol. XIV, 49), and on Christmas Eve of 1629 he was informing Cesi that he had finished the text "except for the ceremonial introduction … but these are matters more rhetorical and poetical than scientific, though I do want it to have some spirit and charm" (Favaro 1904, vol. XIV, 60). At the end of December 1629, all except the preface and the epilogue appear to have been completed, and in January 1630 the book was read in Pisa at the house of Canon Niccolo Cini (Drake 1978, 311).

He also did not have any outstanding astronomical issues to resolve—except, perhaps, one: the periodicity of the tides. In fact, we witness a rather peculiar behavior. In November 1629, Galileo had asked Gionfrancesco Buonamici, the Tuscan ambassador to Spain, for information about the periodicity of the tides at the Spanish coasts (Favaro 1904, vol. XIV, 52–55). Buonamici wrote to inform Galileo that the tides had a 12-hour cycle.[55] Galileo's theory predicted a 24-hour cycle. Though this was a rather serious blow to his theory of tides, which was so very critical for proving the motion of the earth, the disheartening news did not prevent Galileo to complete the book—the discrepancies being accounted for by the anomalies at the sea floor! Is it not a bit paradoxical that Galileo dragged his feet for six years to finish a book which was almost ready, and did not delay it at all when he received news that his theory did not fit the observations? By April the book was ready and Galileo reached Rome on May 3, 1630 with the complete manuscript ready to get the necessary permits.

A plausible explanation of why it took Galileo six years to complete the *Dialogue* (or to announce that he finished it) is that Galileo was stalling; he was buying time. The reason may be that he had second and third thoughts about writing a book like the one suggested to him by Urban during their meetings of 1624. As we argued above, such a book would infringe upon his commitment to fulfill the Copernican agenda, an agenda that he himself had been projecting as his flagship. Furthermore, it was the views that were so explicitly expressed in *The Assayer* about how to study nature and the cornering of the peripatetics that had been so decisive in enlarging and strengthening the ties of the network of Galileo's friends and allies. To proceed with his agenda about the earth's motion—even if treated hypothetically—would be incomplete if, at the same time, he did

[55] Buonamici to Galileo on February 1, 1630 (Favaro 1904, vol. XIV, 73–75). Galileo, most probably in 1619, was also warned by Richard White as to the periodicity of the tides in Lisbon (Drake 1978, 273–74).

not make sure that the mathematicians and not the peripatetics would have the exclusive jurisdiction over these issues. Otherwise, he could not hope that Copernican ideas or any topic from his other studies would be treated "properly." It is, thus, not unreasonable to assume that Galileo already had the book almost ready and he was waiting for the right time to make his move in Rome—after all, he had also waited for eight months for the "right time" to go to Rome and pay his respects to the newly elected pope in 1624.[56] And, in this case, the right time would be when Urban found himself in troublesome circumstances and thus, Galileo thought, the book would not be preoccupying him as it did in 1624. Of course, there was a possibility such circumstances might not have risen—Galileo hoped for the best! It was a gamble that Galileo won.

Galileo was, of course, fully informed of the developments in Rome and was, thus, fully informed of the complications brought about by the shifting balances in the war. Starting with the death of the Mantuan Duke on Christmas Day of 1627, Urban's political troubles started to pile up. A series of events led to the loss of the passive support of the Habsburgs on whose multiple roles Urban was so dependent during his papacy. In order to further limit their power, Urban sided with the French, and this brought about a strong resentment by the Spaniards who did not take such a prospect lightly. In the meantime, relations between the Vatican's and Tuscany's leaders deteriorated, since the latter had supported the Habsburgs' claim to Mantua.[57] France's alliance with the Swede Gustavus Alphonsus (1594–1632), who became actively involved in the war as a result of Richelieu's machinations and his subsequent victories in the field, created a whole new situation in Rome and dramatically weakened its role in the war. Urban, abandoned by the French, was in a truly tight situation. Furthermore, from 1628 onwards the tensions between Cardinal Borgia, who in 1631 had been appointed as the representative of the Spaniards, and Urban were intensifying (Mayer 2014, 11–46), and they led to the dramatic events in the Curia in 1632. Galileo was also informed that the pope was becoming increasingly paranoid about his health and afraid of plots to kill him. By the time Galileo reached Rome in May 1630, Urban was already in the comforting security of Castel Gandolfo—about 25 km southeast of Rome—in panic that his life was threatened by agents in Rome and

[56] Galileo to Cesi on October 9, 1623 (Favaro 1903, vol. XIII, 134–35).
[57] A rather detailed account of the complicated relations of Urban with the various factions in the war is given by Weech (1905).

under duress because of rumors that horoscopes were predicting his imminent death. Perhaps, he thought, the pope's troubles and problems were so pressing that the exact contents of the book would not be something very high on Urban's agenda. Waiting appeared to be in the advantage of Galileo. Encouraged by Castelli's contacts with Riccardi[58]—who on June 6, 1629 took the oath for the office of the Master of the Sacred Palace and became the person who would be responsible for issuing the imprimaturs—Galileo started preparations to go to Rome. The trip may have also been hastened after being informed that if the then Cardinal Maffeo Barberini would have been in charge in 1616, "the decree would not have been issued."[59] It has, however, to be noted that this is what Castelli had been told by Campanella—a character whose judgement was not always very sober and who conveyed the pope's words during a meeting they had.[60] If Galileo delayed the completion of the book for so many years in order to find the "right time" to go to Rome to secure imprimatur, the year 1630 was surely such a "right time."

Galileo reached Rome on May 3, 1630 seeking to receive the imprimatur for the book which would be printed at the expense of the Academia. Ciampoli and Riccardi were in ideal key positions, so it would not have been difficult to obtain an imprimatur. By mid-May the pope had returned to Rome from Castel Gandolfo (Shea and Artigas 2004, 139–40).[61] From their meeting on May 18, 1630, the only information we have is that the pope asked to change the title from *The Discourse on the Tides* to *The Dialogue on the Two Chief World Systems.* Did the pope remind Galileo of these meetings? Did the pope ask him whether he honored their agreement? We do not know, but even if he did, what exactly was *their* agreement? It is not unreasonable to think that when the pope and Galileo met in 1630, Urban's agenda of 1624 may not have been something that was preoccupying him. He had just returned from Castel Gandolfo. The problems he was facing on the warfront were compounding. His authority was questioned in Rome among the Cardinals. Galileo reached Rome a fortnight before the pope returned to Rome. Did Galileo know that Urban would be returning to Rome while he himself was in Rome? Did Galileo wish to meet the pope during his visit to Rome in 1630? It is not possible to convincingly answer

[58] Castelli to Galileo on February 28, 1630 (Favaro 1904, vol. XIV, 78).
[59] Castelli to Galileo March 16, 1630 (Favaro 1904, vol. XIV, 87–88).
[60] Which is identical to what Galileo had learned from Zollern in 1624, who had been told this by the pope!
[61] It is to be noted that we do not know whether Galileo decided to go to Rome at that particular period because he had some information about as to when the pope was planning to leave Castel Gandolfo and return to Rome.

these questions. Nevertheless, during the only meeting they had, it is likely that Galileo mumbled his way through and brought the discussion wisely to issues pertaining to God's omnipotence.

Two weeks after Galileo left Rome on June 16, 1630, perhaps one of the most serious incidents to shake the Roman scene took explosive dimensions: the Santa Prassede library was raided and Orazio Morandi (1570–1630)—abbot of Santa Prassede in Rome, one-time general of the Vallombrose Order, and a practitioner of the Hermetic arts and horoscopes—was arrested and imprisoned on July 13, 1630.[62] The library was a treasure of books of the occult, astrology, medical treatises, and theological texts; it was frequented by many members of the Roman elite, and outstanding members of the most powerful families were either borrowing books or attending soirées. Ridolfi, who by 1629 had become the Dominican General, was rather eager to learn about Urban's fate and had also been one of those consulting Morandi. Everyone, including the ecclesiastical authorities, knew of the activities of this quasi-clandestine library. As it happens in similar situations, such activities cannot be contained after a certain point. The horoscope Morandi had cast about Urban showed that the pope would probably meet a serious difficulty in 1630—perhaps even death. The so-called Lyons letter of February 1630 was rather ominous. It predicted the death of Urban. Galileo left Rome for Florence on June 26, and a few weeks later he was informed by his friend Vincenzio Langieri that Morandi was arrested and imprisoned because of his astrological forecasts.[63] The subsequent trial came to be known as the "Big Trial." The proceedings were kept secret because so many people were involved and it signaled a strong clampdown for all those who made horoscopes, especially about the pope. Though Visconti's name was mentioned as the author of a book titled *Astrological Discourse on the Life of Urban VIII*, he was cleared of any accusations and was banished from Rome. Everyone (including Galileo) rushed to take distance from what was going on in the Library of Santa Prassede.[64] Galileo, worried about the rumors, asked Michelangelo Buanarroti (1568–1646) to convene with

[62] For an analytical discussion of the Morandi affair, see (Dooley 2002).
[63] Langieri to Galileo on August 17, 1630 (Favaro 1904, vol. XIV, 134).
[64] As soon as it was known that Galileo reached Rome, the gossip column *Avvisi,* on May 18, 1630, had insinuated that Galileo was involved in casting horoscopes that predicted the pope's death (Shea and Artigas 2004, 139).

Francesco Barberini, who later said that he did not believe the rumors and that Galileo "had no better friend than the Pope himself."[65]

Galileo was not someone unknown to Morandi. During the first decades of the seventeenth century, anyone who was involved in the discussion of astronomical problems was bound to have come into some contact with astrologers and, in all probability, with those practicing black magic. Astrology was not a clandestine activity, and many mathematicians and astronomers either practiced it, knew the people who practiced it, or actively pursued it and sought the astrologers' advice. The framework and functioning of the Santa Prassede Library and the people borrowing the books are witness to the fact that a substantial section of the Roman elite did not feel that having access to astrological and Hermetic texts was something to be avoided at all costs. Caution was, of course, necessary to avoid confrontation with the officials who neither discouraged nor, of course, encouraged the activities at Santa Prassede. The activities and the visits to borrow books were, evidently, a gray area. No one doubted this fact. But gray areas have gradations of hues, and many people did not feel that their dealings with the Library was a cause for alarm.

The Morandi case showed that there was a very wide network of people involved in one way or another with the activities going on in Santa Prassede, and it was found that Galileo—no stranger, especially in his youth, to astrology and horoscopes—had attended soirées organized there and followed the developments of the Morandi trial. Among Morandi's papers, there was a horoscope for Galileo and Visconti during Morandi's trial reporting that Morandi and Galileo had spoken about horoscopes and astrology in one of their recent meetings (Rutkin 2005, 178). The Morandi case was rather crucial since thereafter Urban responded with his bull to forbid horoscopes for popes and up to their third-degree relatives. Assessing the overall picture, it was all too evident for Urban that what emerged from the trial was an increasingly messy picture. And although Galileo had taken no active part in machinations against the pope, his name kept on popping up.

Urban's increasing consternation with such a state of affairs was fueled by rumors about his own future. It was one thing to leave room for such practices, it was another when such activities explored the prospects of the pope's fate. The Morandi affair brought to

[65] Buanarroti to Galileo on June 3, 1630 (Favaro 1904, vol. XIV, 111).

surface activities indicating that there were people who were actually studying the pope's horoscope and predicting his imminent death. Things were worse with the Giacinto Centini case, which revealed he had conjured up all kinds of "tricks," involving murders of innocent women, so that Urban would die and be replaced by his uncle cardinal Felice Centini! The trial itself produced too much debris; it was discovered that there were people who were actively planning ways to assassinate the pope. It was, of course, an extreme case, and it was dealt with "properly." The trial, the defrocking at St. Peter's, the much publicized execution in 1635, and the consequent Bull all helped to terrify those who still contemplated similar actions (Rietbergen 2006, 336–75; von Ranke 1901, 407; van Gastel 2014). The Morandi case was different. He was imprisoned in July 1630; the interrogations started immediately, and Morandi was found dead on November 7, 1630. Compared to the Centini trial, a much more civilized trial took place, and the death of Morandi—certified by a doctor that he was not poisoned—was a handy end to a case that involved many people whose normalcy could not be jeopardized by such revelations. It was an all too familiar process: Someone who caters for the whims and wishes of the elite had gone too far. The arrest and subsequent interrogations brought out too many embarrassing things. The culprit was conveniently "found" dead.

Certainly after 1629 things did not look good for Urban: war, the Spanish faction among the cardinals, the horoscopes heralding an early death, the attempts to assassinate him when the planets would be in favorable positions, and the almost routine visitations of many people from the Roman establishment to Santa Prassede. Such polluted air could not be cleared even with Campanella's techniques—which had intrigued Urban—to annihilate bad omens (Rietbergen 2006, 361–65). But even more importantly, all these activities in Rome could not be contained under a lid. Such things have a way of spreading. It does not matter what exactly Galileo knew about the activities at Santa Prassede, but it does matter that he was someone who was surely on the receiving end of the rumors. Some people may have even wished to convey him the news—everything need not be in writing. In fact, since he was—again marginally or not, it does not matter— also involved with Morandi, Galileo may have even actively sought out news about the developments. Hence, the rush to finish the book since, despite such a change in Urban's behavior, the specific pope was still providing the best guarantee that Galileo would go unscathed.

Since 1626 astrologers predicted Urban's death and by 1628 "rumors of it became loud and widespread" (Walker 2000, 205; Amabile 1887, 280). In fact, the "dangerous" years were 1628 (eclipses of the moon in January and the sun in December) and 1630 (eclipse of the sun in June). Thus, it is not unreasonable that by 1629 Galileo may have decided that the right time he was waiting for had come. Urban's problems were piling up. There was no need for further stalling to announce the completion of the book. The rumors about what was happening in Rome painted a rather dim picture. Now was the opportune time when Urban was increasingly preoccupied by the distressing signals he was getting from the horoscopes and the discoveries of attempts to assassinate him while at the same time his political and spiritual authority were strongly challenged. Galileo wanted to present the finished manuscript when Urban would be weak but not dead. Was it a coincidence that Galileo finished the manuscript and said so to others in the very month of the Lyons letter, where the death of the pope in February 1630 was predicted? Galileo knew that he disobeyed the strict adherence to the hypothetical character of the Copernican ideas and ignored Urban's proposal to go easy on the Aristotelians. He hoped, though, that Urban would still be an ally and Ciampoli and Riccardi would still be on his side. Galileo, despite waiting for Urban's problems to pile up before finishing the book, had no doubts of the prospect of the book were Urban not around anymore. So, he rushed to Rome on May 1630.

If, as many scholars argue, Galileo in his meetings with the Pope received the go-ahead to write a book in which Copernicanism could be treated in a hypothetical manner, then all rules of psychology tell us that Galileo would have rushed to write and publish the book as soon as he could, if for no other reason that a delay would have given time to his adversaries to get organized against him after the privilege of meeting the pope multiple times had been bestowed upon him. To finish such a book may have been even more pressing for someone over sixty years old with perennial health problems. What happened in reality was the exact opposite. A book whose components were almost completed by 1625 took six years to be completed. On October 29, 1629, Galileo wrote to Elia Diodati that he had "taken up work on the *Dialogue* which was left on the side for three years … I have found the right line which ought to allow me to terminate it within the winter." In two months (long before winter was over), on December 24, 1629 Galileo announced to Cesi that the *Dialogue* was completed (Favaro 1904, vol. XIV, 60)! And, if

Galileo's "confessions" to Diodati are to be believed, a 900-hundred-page manuscript was completed in about two months, despite the fact that while finishing his writing he had received empirical evidence about the tides that contradicted his own predictions! He did, indeed, have some health problems, but these did not hold him back from working on a number of problems in engineering and physics, nor from refraining from writing his *Reply to Ingoli*. It is our contention that apart from waiting for Urban to be in a tight situation, these years were also a period of serious internal struggle related to the suggestion made to him by Urban in 1624. The man who never shied away from a good fight did not want to betray the role he had chosen for himself as the aggressive spokesman of the practitioners of mathematical natural philosophy. The timid indications he gave with his *Reply to Ingoli* that he would be accommodating towards the Aristotelians were soon to be brushed aside by his earnest commitment to an agenda set together with the Linceans. What he had to be careful about was the "right time" to publicize the completion of the book and ask for the imprimatur.

Cesi's death changed much of what Galileo planned. Perhaps, among his plans was something that has not been commented upon: Galileo was fully aware that despite the reservations of Cesi, Cesarini, and Cimapoli to go easy on Grassi before writing *The Assayer,* it was due to their determination to stand behind him that the text took its final form. Barberini's election had just accelerated its publication and changed the dedication. Galileo may have been planning a similar course of action for the *Dialogue.* Now that Galileo thought it was the right time to strike, he could have dragged along the other remaining Linceans. Cesi, despite the downturn in their correspondence, was among the first to be informed that the book had been completed, and Galileo may have hoped that he would be a significant player to secure the imprimatur and print the book—not only by bearing the funding of the printing.

The Difficulty for Securing the Imprimatur for the *Dialogue*

The utter confusion, the repeated setbacks, the stalling, and the delays in securing an imprimatur for his book are often considered to be the result of incompetence and indecisiveness on the part primarily of Riccardi, but also of Ciampolo. Cesi's death and the plague that ensued upon Florence further complicated matters. Might it be the case, though, that Riccardi and Ciampoli had received mixed and contradictory instructions from *the pope, who was unwilling to fully disclose his conversations with Galileo in 1624 but did wish for Riccardi and Ciampoli to be aware of at least some aspects of what was discussed before they proceeded to give the necessary permits? And might it be the case that Riccardi could not make sense of what exactly the instructions he received were and, thus, could not decide how to go about issuing the imprimatur?*

It may appear to be a superfluous question, yet if one follows the events closely, one would realize that we cannot be certain as to who actually read the fateful book *thoroughly, critically, and carefully* before it was printed.

How convincing is the evidence that Ciampoli and Riccardi *actually* read the book? Or, might it be the case that they were informed by others' understanding about the book's contents? Thus, how much did they know about the details of the book if and when Urban asked them about its contents? This becomes even more crucial if Urban had not disclosed to Ciampoli and Riccardi in a clear manner what he was expecting from Galileo as a result of the meetings they had in 1624. Any disclosure would have been a difficult decision, especially since Urban could not afford to be humiliated in the eyes of his close advisors if Galileo did not deliver. Furthermore, six whole years had passed from the 1624 meetings, and perhaps both Urban's suggestions and Galileo's non-committal responses had become rather opaque with the passing years.

But who actually *read* the book?

After receiving the manuscript from Galileo, Riccardi sent it off to Father Raffaello Visconti, who was an assistant to the Master of the Sacred Palace and whose interests lay in astronomy, astrology, and the occult. He was also a friend of Orazio Morandi. On May 26, 1630, Galileo was invited for dinner by Morandi. Two more people were also present: Visconti, whom Galileo had met through Morandi in 1624, and Ludovico Corbuse, who was an Inquisitor in Florence before he became a consultant to the Holy Office. Visconti was also approached by Filippo Niccolini, brother of the ambassador and advisor to the Duke's brother, and asked to expedite matters concerning the book (Heilbron 2010, 298). On June 16, 1630 Galileo received a letter from Visconti informing him that Riccardi was satisfied with "the small things such as those we adjusted together" and that he would discuss the front piece with the pope (Shea and Artigas 2004, 143; Favaro 1904, vol. XIV, 120).

The "small things" were not named. As of the end of June 1630, the book in all probability had been read by Visconti (how carefully, we cannot say) who then discussed it with Riccardi. Riccardi was eager to please everyone involved but hesitated to issue the imprimatur. His excuse for the delay was the front piece, which had to be discussed with the pope. The death of Federico Cesi in August 1630 brought about a series of events that proved fateful to Galileo. He planned to publish the book in Rome since the Academia would have paid the expenses. Now, this could not be done. Cesi was the undisputed head and exclusive financial benefactor of the Academia. Galileo asked Riccardi through Castelli to have the book published in Florence. Castelli replied (on September 21, 1630) that Riccardi told him that Galileo had agreed to return to Rome to fix small things in the preface and the body of the book, but because of the plague he could send a copy "so that it can be fixed with Ciampoli, as need be." Once this had happened, he could then publish it in Florence or any other place. Galileo had the only copy of the manuscript, and because of the plague he did not wish to send it to Rome. Castelli urged ("absolutely necessary") Galileo to send a copy. Galileo was not pleased; it was time-consuming and expensive to make a copy—but in all probability it was because he felt that he would not be in control of the procedures. Through the arbitration of Caterina Niccolini, wife of the Ambassador and cousin of Riccardi, it was decided that if Galileo sent Riccardi the preface and ending of the book, things would be OK. It could then be read by a censor in Florence. Riccardi suggested a Dominican father, Ignazio Del Nente, or anyone Galileo wished as long as he was of the Dominican order. Del Nente declined, stating that he would be short of time due to his commitments in some forthcoming religious ceremonies (Shea and Artigas 2004, 148). Galileo suggested that father Giacinto Stefani, a consultant of the Florentine Inquisition and former court preacher to Christina of Lorraine, do the job.[66] On November 17, 1630, Caterina Niccolini informed Galileo that Riccardi unenthusiastically agreed that Galileo send the preface and the ending before Riccardi appointed Stefani and gave him "a few instructions." This was also verified on November 30 by Castelli, who had talked with Riccardi and was told that things would progress with Giacinto Stefani (1577–1643). In a long letter to Andrea Cioli (1573–1641), the secretary of state of the duke, Galileo writes about the events around the manuscript,

[66] In his letter to the Tuscan Secretary of State on March 7, 1631, Galileo mentions that he had received the permission (appearing in the *Dialogue* dated September 12, 1630) of the Inquisitor Niccolo Antella, the ducal reviewer and censor of books to be printed in Florence (Favaro 1904, vol. XIV, 215–18).

claiming that Riccardi had "read, signed and authorized" (Shea and Artigas 2004, 150) the book, something that is not substantiated by any other document—not even by Riccardi's testimonial in the report of the Special Commission of September 1632.

Thus, nothing attests to Riccardi having read the book by the end of 1630. As for Stefani, Galileo writes to Cioli that he "shed tears more than once" when he realized "how much humility and reverent submission I offer to my superiors."[67] And he protests the procrastination. Riccardi had not sent back the preface and the ending, and Galileo, through Cioli, asked for the intervention of the duke. Riccardi was still dragging his feet and now insisted that Del Nente read the book—obviously having realized that Stefani was too close to Galileo to read it objectively. After much pressure from Niccolini, who had received express orders from the duke, Riccardi agreed to give a license to print the book but with "a statement to release him of any liability" (Shea and Artigas 2004, 152).

On April 25, 1631 Riccardi informed the duke that Stefani, despite his good will, did not know what the pope had in mind. "Stefani does not know the intentions of His Holiness and so he cannot give the endorsement which only I can give" (Finocchiaro 1989, 209; Favaro 1904, vol. XIV, 254). Riccardi suggests that Galileo sent the preface and ending and Riccardi checked whether it conformed with the pope's wishes. He would write to the Florentine inquisitor to tell him what to look for in the book, and then the inquisitor may allow it to be published acting on his own authority. When he was shown the letter, Galileo protested by writing that Riccardi for a whole year was delaying his decision. Galileo on May 3, 1631 suggested a meeting between the duke, the Florence inquisitors, Stefani, and whoever else the duke wished participated in order to decide about the book. Was Galileo so naive as to expect the duke would agree to such a meeting? Perhaps he proposed such a meeting to show that he had nothing to be afraid of in such a high-level gathering. Obviously, the duke declined to organize such a gathering—it would have been perceived in Rome as an aggressive act. The Tuscan secretary of state wrote to Niccolini to pressure Riccardi and have him write to the Florentine inquisitor Clemente Egidi (1571–1639) as Riccardi had suggested.

On May 24, Riccardi sent the letter to Egidi asking him to use his own authority and telling him what the pope wanted: another title, no inclusion of anything that had to do with the

[67] Galileo to Cioli 13 April 1631 (Favaro 1907, vol. XIX, 254; Shea and Artigas 2004, 151).

Scriptures, no claims of the physical truth of Copernicanism, the addition of an explicit statement that Copernican ideas were treated hypothetically, that Rome was kept aware of all the arguments related to these matters, and that Riccardi would return the preface and the ending—all this indicating that by the end of May 1631 Riccardi had not even read the few pages of the preface and ending.[68] Riccardi (and, perhaps, his assistants) could not deal with the text scientifically. Perhaps the pope may have given some instructions to Riccardi which could not (because of the character of the enterprise discussed with Galileo in 1624) have been explicit—and thus the mess with what Riccardi was instructing Galileo to do. Rewriting the preface and the ending was the easy way out.

In his reply to Riccardi, Egidi says that he will follow Riccardi's orders and "I have given the work for review to Father Stefani, of your Order … ." So, we are back to square one. It appears that only Stefani actually read the book. After pressure from Niccolini, Riccardi sent the preface and ending on July 19, 1631. He asked Egidi to make the wording of the ending conform to that of the preface. Does that mean that Riccardi had not even read the ending? Stefani required only a few small changes.[69] The *Dialogue* went to press on June 1, 1631. It had three imprimaturs: one undated from Riccardi (a provisional approval obtained in Rome as a license to print), the others dated September 11, 1631 issued by Pietro Niccolini (?–1651), Archbishop of Florence, and by Clemente Egidi, chief inquisitor of Florence. The printing of the book took 9 months and on February 1632, the printer Giovanni Battista Landini informed a friend of his that the book would be presented the next day to the duke. In procrastinating the whole business, "Riccardi was only seeking an excuse for the mental paralysis that had seized him in the face of the text. One explanation might be this: that Galileo had told him that this was exactly how the Pope had wanted it; and it is indeed very possible that the Pope who disliked pedantry may have given him once the gist of the argument in those few words we find in the text" (Santillana 1962, 197; footnote 9).

There are two issues that may be worth paying attention to. On September 11, 1632, Niccolini informed the secretary of state that he had seen Riccardi, who told him that he was "reviewing the work and is trying to fix it in certain places so that it is acceptable; and when this is completed he intends to bring it to the Pope and tell him that he is sure that

[68] Riccardi to Egidi on May 24, 1631 and July 19, 1631 (Favaro 1904, vol. XIV, 327; 330).
[69] Niccolini to Cioli on May 17 and 19, 1631 (Favaro 1904, vol. XIV, 251; 261).

it can be allowed to circulate." But this is *after* the book is published, yet Riccardi thinks there can be changes that will fix any problems. Does such an approach show a serious engagement with the book on the part of Riccardi?

The second issue has to do with Riccardi's own version of the events. Both Reports of the Special Commission on the Dialogue, are, in effect, texts exonerating Riccardi and clearing him of any responsibility (Finocchiaro 1989, 218–22). Riccardi, notes the Report, gave the manuscript to Visconti, "who after several emendations was ready to give his approval as usual, if the book were to be printed in Rome. We have written to the said Father to send the send certificate and we are now waiting for it." In other words, all hell broke loose, there is the Special Commission and they do not even have the Report of Visconti and are waiting for it? And what about the commitment of Riccardi? "The Master of the Sacred Palace wanted to review the book himself; but, in order to shorten the time and facilitate negotiations with printers, he stipulated that it be shown him page by page and gave it the imprimatur for Rome." First, there is not one word that Riccardi gave the imprimatur for Rome. Second, there is no mention that he did it without having seen any page whatsoever. Neither of these is mentioned in the Report.

More importantly, there is this almost irrational suggestion to read every page of the book before sending it to the printer. Let us not discuss the impracticality as well as the academic impropriety of such a suggestion: if say, one finds "something" on page 100 that necessitates the correction on page 59 which was not noticed when reading page 59 but became rather pronounced when put in context once page 100 had been read, what exactly did Riccardi's commitment mean, since page 59 would have been already printed? Riccardi, it is written in the Reports of the Special Commission, "in order to shorten the time and facilitate negotiations with printers, he stipulated that it be shown him page by page and gave it imprimatur in Rome" (Finocchiaro 1989, 219) and "to facilitate the process it was decided that before sending it to the press the Master would see it page by page … " (Finocchiaro 1989, 220). Interestingly, such commitment of Riccardi has been differently perceived by different scholars. Meyer (2015, 136) says that the *printed* sheets would go one by one to Riccardi, so do Shea and Artigas (2004, 152): "Riccardi had agreed to see it page by page as it came off the press."[70] So does Heilbron (2010, 108): "Riccardi

[70] See also Niccolini to Cioli on April 13, 1631 (Favaro 1904, vol. XIV, 248).

had given a provisional imprimatur for printing in Rome on the understanding that he would review the sheets as they came from the press." Gebler (1876), Santillana (1962, 195), and Galluzzi (2017, 412) follow Riccardi's claim.

Furthermore, both Riccardi as well Ciampoli give "hints" that they were acting under orders from the pope. Was this to be believed? Did they actually talk with the pope and receive very strict directions? It does not seem so. If the exclusive order they received was about the hypothetical status of Copernican ideas, that would be rather straightforward to check. Both Visconti's and Stefani's reports should have been sufficient. On the other hand, if the pope was not absolutely clear because he remembered the deal with Galileo but may not have wanted to disclose its entirety to Riccardi and Ciampoli, then the confusion would be serious and more in the direction of what we sense was happening. All this in addition to the messy situation the Pope was facing in 1631.

Confusion piled upon confusion and uncertainty reigned supreme on how to deal with the contents of the book, which, as it appears, was read by no one carefully—except perhaps Stefani in Florence, who was Galileo's friend! A routine procedure turned into a chaotic set of events. Was it Riccardi's and Ciampolo's inefficient management of an otherwise straightforward procedure? Both were extremely experienced in these matters. Might it have been that the conflicts and confusions were the result of mixed messages from Urban who had also not made up his mind whether to disclose to Ciampoli and Riccardi what he had discussed with Galileo in 1624?

Obtaining the imprimatur turns out to be a performance of musical chairs by actors who were impressively ill-prepared for the occasion. Riccardi, Visconti, Ciampolo, Stefani, Edigi, Catarina Niccolini (1598–1676), and Francesco Niccolini in one way or another are all taking part in the performance. Riccardi keeps on changing the rules of the game.

The pope knows little of what is happening, and the actors taking part in the performance know even less of what exactly the pope's wishes are. The preface was received by Riccardi and supposedly sent to the Pope on July 31, 1630 (Drake 1978, 320), but on August 21, 1632 Galileo learns "that the pope was still not informed though Riccardi professed to speak for him" (Drake 1978, 338).

It was like asking for some kind of an abridged form of the book, and Riccardi may have thought that his idea of reading every page before printing it was a rather clever way for dealing with the book. Ciampoli, so very faithful to the Linceans' agenda, may have thought that Galileo did everything right and may have said so to Urban, who in the state he was in may have also given confused messages of what he was expecting from the book.

Did the pope say anything about the 1624 meetings to Ciampoli? Did Ciampoli disclose any of the conversations he may have had with the pope? Were Riccardi and Ciampoli properly prepared to inform the pope in a comprehensive manner about the book's content? None of these questions can be answered affirmatively. If Ciampoli and Riccardi formed their opinion by reading the preface and the ending alone, they may have felt rather assured that things were fine. Everyone was acting according to what they thought Urban wished (the hypothetical treatment); thus, when Urban became aware of what really was included in the book, all hell broke loose—as it is also expressed in the letters of Niccolini to the Tuscan secretary of state after the former met the pope for a number of times.

The rather peculiar situation created by the efforts to receive the imprimatur was, as it has been assumed, the result of the pressing obligations of the church officials due to the dangerous developments in the war, of the open challenges to the pope's authority, of what was circulating in Rome against the pope (including the arrest of people who planned to assassinate him), of the pressures Riccardi had from Niccolini through his wife, and of the length and the difficulty of the book for the uninitiated. We argue that, in addition, the already chaotic situation was further intensified by the confusing directions that Ciampoli and Riccardi may have received from the pope, who for obvious reasons did not want to disclose all the details of what he had suggested to Galileo in 1624 but may have still imparted some information. If Riccardi perceived what he was told as something incoherent, that explains his "mental paralysis." If Ciampoli understood the gist of what Urban told him, then on the one hand he may have tried to convince the pope that everything was fine and, on the other, to disassociate himself from the process.

It is thus not implausible that apart from all the pressure brought upon the pope and his very close collaborators as a result of the developments in the war, the undermining of the pope's authority by the Spanish faction, the almost incessant activities of the astrologers to predict his date of death, and the practice of black magic to organize complicated schemes to assassinate Urban, the utter confusion concerning the issuance of the imprimatur may have also been due to the unwillingness of Urban to reveal what exactly he was expecting from such a book. If the pope decided to disclose what he was expecting from Galileo without being fully frank about it since he was afraid that Galileo might not deliver, then Ciampoli and Riccardi would be at a loss about how to deal with the manuscript.

Amid all this confusion, Riccardi's suggestion that a clearly written preface and ending may have taken care of all this confusion appears rather natural! How is one to understand the unusual situation of the *Dialogue* receiving three imprimaturs, none of which appears to be properly given? How can one explain a routine procedure, such as getting the imprimatur in Rome, turning into such an utterly confusing process? Our emphasis is not on what the *Dialogue* contains and how competent the censors were, but on the confusion around the granting of the imprimatur. It does not appear that anyone read the book carefully (except, perhaps, Stefani) during the crucial two years between the completion of the manuscript and its printing. From the pope's perspective, it appears that Ciampoli messed things up, but if Urban was not forthcoming on what exactly he was expecting from the book, how much is—the admittedly conniving—Ciampoli to blame? Riccardi had asked Galileo to mention in the ending the argument about Urban's version of God's omnipotence. Might it have been the case that Galileo put the argument in Simplicio's mouth, not in order to trivialize the hypothetical character of the Copernican ideas, but because he may have wished to "upgrade" the philosopher—especially if Ciampoli and Riccardi may have interpreted Urban's muddled directions in such a simplistic way? Most of the received scholarship concentrates on what Simplicio says at the end of the *Dialogue* to explain the rage of Urban. Is this really convincing? Is it not "somewhat unfair of historians to expand upon this Simplicio incident as the single motivation of the Pope's actions" (Santillana 1962, 208)?[71]

[71] Feldhay (1998, 128) argues for a similar point.

Cryptic "Confessions" by the Pope to the Ambassador

Though the correspondence between the Tuscan ambassador to Rome, Francesco Niccolini, and the Tuscan secretary of state has been systematically studied, there are two passages in their correspondence of September 18, 1632 that do not appear to have been commented upon. These are the pope's strict demand for absolute secrecy about something he told Niccolini and the incident about Cardinal Algioti. Might it be the case that these two passages become comprehensible in the context of our hypothesis of what may been discussed between the pope and Galileo in 1624?

There are a number of passages in some letters of Niccolini to the Tuscan Secretary of State from 1632 that do not make sense within the current view. They make more sense if we put them in the context of our assumption about what went on during the six meetings in 1624. There are, of course, many passages in these letters that support the prevailing view that the pope had considered the *Dialogue* as a book in which Copernican ideas were considered as necessarily true and, also, that Galileo's views on atomism were undermining the views on the Eucharist. But scholars who have used these passages in order to strengthen their arguments about the trial have ignored some other passages—which, as it will be shown, make sense within our hypothesis. Francesco Niccolini (1584–1650), the Tuscan Ambassador to the Vatican from 1621 to 1643, was a person very favorably inclined towards Galileo—unlike his predecessor, Giovan Francesco Buonamici (1592–1669), who was serving during Galileo's trip to Rome in 1615 and 1616. The correspondence during September 1632 between Niccolini and Cioli, who became Ferdinand de'Medici II's secretary of state in 1623, is particularly revealing.

In September 1632, about a month after Urban prohibited the circulation of the *Dialogue*, there were persistent rumors that there would be formal proceedings against Galileo. Niccolini met the pope a number of times, and in their first encounter Urban told him twice that he was being deceived by "Galileo and Ciampoli." Niccolini continues in his report to Cioli: "His Holiness gets something in his head, that is the end of the matter, especially if one is opposing, threatening or defying him, since then he hardens and shows no respect to anyone." The pope was especially hard on Galileo: "he knows very well where the difficulties lie, if he wants to know them, since we have discussed them with him and he has heard them from ourselves."[72] Surely the pope could have revealed what it was that they discussed, if what he meant was merely the hypothetical treatment of Copernican ideas.

Furthermore, in the same letter Niccolini informs the Tuscan secretary that he had met Riccardi, who told him "that the book would not be prohibited and it would only be corrected and emended in some points which are really bad." In a letter on September 11, 1632 Niccolini writes to Cioli that Riccardi "is reviewing the work and is trying to fix it in certain places so that it is acceptable; when this is completed he intends to bring it to the Pope and tell him that he is sure it can be allowed to circulate and that his Holiness now has the opportunity of using his customary mercy on Galileo" (Finocchiaro 1989, 232–34). How reliable are Riccardi's views when the book's circulation was already prohibited and he had for a long time, before the book's circulation, all the recommendations of Visconti and Stefani about the changes they had proposed? Did Riccardi have any further suggestions for changes? There is no evidence for that. Was such a reassurance by Riccardi a conscious lie to ease the worries of his cousin's husband? And while he was expressing these thoughts to Niccolini, those very same days Riccardi was trying to clear himself of any responsibility in the Special Commission, blaming Galileo for the mess he created and telling the Commission that he was waiting for Visconti (who was already sent to Viterbo about eighty kilometers north of Rome, because of his ties with the Santa Prassede activities) to send the report he had prepared in May 1630 about the changes to be included in the *Dialogue*! This is surely not the behavior of someone who wished Galileo well and would accordingly advise Niccolini on how to proceed.

On September 18, 1632, Pietro Benessi (1580–1642), who was head of the papal secretariat of state, visited Niccolini and, among other things, informed him that the pope's view was that "we are dealing with dangerous dogmas, His Highness [the Duke of Tuscany] should put aside all respect and affection towards his Mathematician and be glad to contribute himself to shielding Catholicism from any danger." That same morning, upset by what he heard from Benessi, Niccolini asked for an audience with the pope. In

[72] Niccolini to the Tuscan secretary of state on September 5, 1632. The Monday before September 5, 1632 Niccolini met with Riccardi (Finocchiaro 1989, 231). In the letter of September 11, 1632 to the Tuscan secretary of state, Niccolini writes that he agrees with Riccardi that it was not wise to publish the Dialogue in Florence and he, alas, informs the secretary that Riccardi told him for the first time about the document found in the files and that Galileo had not mentioned this to the duke.

this meeting the pope told him that Galileo's opinions were condemned about sixteen years ago, and Galileo got himself into a fix which he could have avoided. "These subjects are troublesome and dangerous, this work of his is indeed pernicious and the matter is more serious than his Highness thinks." Then he started telling Niccolini:

> about this matter and these opinions, but with the explicit order not to reveal these things even to His Highness under penalty of censure. Although I begged to be able to report them at least to His Highness only, he answered that I should be glad to have known them from him in confidence and as a friend, and not as a minister … (Finocchiaro 1989, 234–37).[73]

What could have been so mysterious that Niccolini was sworn not even to report it to the duke? If it was about the hypothetical treatment of Copernican ideas, it was not exactly a secret not to be divulged. If it was about Galileo disobeying the 1616 injunction, then why ask Niccolini not to share it with others, since Urban was so keen to impress upon the duke that his philosopher and mathematician had created so many problems for the Church? If it was about the recently discovered unsigned document, Niccolini was already informed by Riccardi the previous week, and such secretiveness about this document was also unwarranted since a number of people already knew about it. Furthermore, if the secret was about the unsigned document, why should the pope not want Niccolini to convey to the duke what the document was all about, since throughout his meeting with Niccolini, Urban was telling the ambassador that the duke should be careful and not harbor someone who was dangerous to the Church? Might it have been the case that the pope told Niccolini that *The Assayer* contained views that were coming into serious conflict with the Church's views about the Eucharist? Such a possibility cannot be ruled out, but, even then, why should the pope not want the duke to be informed he had in his court such a troublemaker—even a heretic—against the Church?

It is thus possible to entertain the assumption that the pope disclosed to Niccolini what he had asked Galileo to do in 1624 and how Galileo blatantly disobeyed him. This was, indeed, something that had to be kept a secret. At a time when Urban's authority was so intensely challenged, if Galileo's disobedience became known, it would have been too embarrassing for the pope if many people knew that he had asked Galileo to do

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[73] Galileo scholars do not appear to have attached any importance to this passage, except Redondi (1989, 257), who refers to it as some further corroborating evidence to his argument about the Eucharist and Galileo. The story, however, about Cardinal Alciato (see next paragraph) has not been discussed.

something and Galileo refused to deliver. A few months earlier, in March, the pope's authority had already been severely wounded in an ominously dramatic way by Cardinal Borgia in the Curia. Such recalcitrance by Galileo, if it became known outside the circle of a few confidants sworn not to disclose it and was leaked to a public so eager for gossip, would have further eroded the pope's standing. If what they discussed in 1624 became known, then the pope might have even triggered the Aristotelians' wrath, whose status he had tried to compromise and failed! It was obviously the case that the pope could not have disclosed his proposal to many people until he was sure that Galileo would deliver. By September 1632 Urban, from what he gathered from Ciampoli and Riccardi, must have been certain that Galileo did not keep his promise. And this was something too embarrassing for the pope. If the pope confided to Niccolini what went on during the meetings of 1624, then it is rather reasonable to have asked him in a pressing manner not to disclose it to anyone and "listen to him as a friend." Furthermore, the pope, so very isolated and deprived of allies, may have wanted Niccolini to know what went on during the six meetings in 1624 so that at least Niccolini, who was making such an effort to protect Galileo, would not think that Urban was unnecessarily strict with Galileo.

In the same letter, there is also a rather cryptic phrase by Niccolini, who mentions that the pope said "one must be careful not to let Galileo spread troublesome and dangerous opinions under the pretext of running a certain school for young people, because he had heard something." Niccolini does not appear to have a clue about what the pope refers to. It could not have been the Linceans—who could not be characterized as a school "of young people." Cesi had died two years ago, the cardinal nephew declined the offer to preside, and the Academy was defunct. Might it have been something "revealed" during the Morandi trial? Nothing could be excluded in the Roman context of the period. Perhaps what the pope might have heard was Galileo's support by young(er) mathematicians, something that would have further increased his concern that Galileo was also plotting against him.

During this meeting, Niccolini suggested to the pope that if the pope thought that Galileo did not follow the instructions he had received, then the pope could ask him to bring about the changes according to his instructions and not prohibit the circulation of the book. Having heard him, Urban "in an amicable way" told Niccolini the following story: a virtuoso once sent one of his works to Cardinal Alciato to have it reviewed; since it was beautifully written, in order not to have the sheets of paper messed up, he asked that corrections be marked with some wax. When the cardinal returned the book to the virtuoso without any marks, the latter then went to thank him and to rejoice with him at the fact that he had not marked anything, but the cardinal answered that he had not used wax because he would have had to go to a store, ask for one of those vases where liquefied wax is kept, and throw the whole book there in order to censure it![74] Why this particular story? This is another passage that is not discussed by historians. It could, of course, have been the case that the pope may have told the story just for its own sake. But if he disclosed to Niccolini what went on between himself and Galileo in the meetings of 1624, then there could not have been a better way to say that the *whole* book—the *Dialogue* in its entirety—had a misplaced emphasis; the *whole* book was wrong as far as what they had agreed; the *whole* book was of no use to him. This is rather reminiscent of what happened with Copernicus' book. During the deliberations for its corrections when, as we mentioned, Cardinal Maffeo Barberini was also present, Ingoli had noted that "the emendation of Copernicus would not be a correction, but rather the total destruction of the system" (Galluzzi 2017, 269–70 fn.95; Gingerich 1981, 52). Ingoli, a very able theologian and also very well versed in astronomy, had realized that there were limits to what could be done to Copernicus' book without fully changing its character. This shows, of course, the legal context within which Ingoli wanted to move after the 1616 decision of the Inquisition to prohibit the book until corrected. Maffeo Barberini went along with the minimal corrections with which both Grienberger and Grassi agreed. Interestingly, Bellarmine was presiding over these meetings and, of course, he agreed with what Ingoli wrote in the Report. In fact, many years before, in 1615, Galileo was also of the same opinion.[75]

These thoughts and events could not be disclosed to many people; they were far too embarrassing for the pope. Niccolini—a person not in the Church hierarchy, with no past dealings with the Academia, and whose sole preoccupation was to defend Galileo and

[74] Visconti's and Stefani's reports concluded that the book could be corrected. This appears to have been the view of Riccardi as well. But if Urban's story about Alciati had anything to do with what was being discussed at the moment, then Urban was definitely not of the opinion that the book could be corrected. Concerning the symbolism of wax, see (van Gastel 2014).

[75] Galileo to Pietro Dini on March 23, 1615 (Favaro 1895, vol. V, 299): "As far as Copernicus is concerned in my opinion he cannot be toned down, as the key point and the foundation of his whole doctrine is the mobility of the Earth and the stability of the sun: his doctrine has to be condemned as a whole or left in its entirety … ."

serve the duke—perhaps was the only person to whom all these feelings could be told in confidence. The story about the wax, appearing to come from nowhere, could have been an outburst of the pope, who may have thought that it would have been much better if Copernicus was banned altogether.[76] And the same for the *Dialogue*, since what he had suggested to Galileo in 1624 was doomed from the very beginning: Neither *De Revolutionibus* could be made into a text in which the hypothetical character of the earth's motions could be convincingly portrayed, nor was Galileo willing to stop pursuing the strategy against the peripatetics expressed in *The Assayer*.

There does not seem to be any evidence to suggest that things could *not* have developed in this particular manner. In a way, Urban may have been a victim of his own pacifying attitudes: not siding with the extreme faction to forbid the circulation of *De Revolutionibus* altogether, agreeing with the minimum interventions to the book in 1620, writing the poem to Galileo after 1620, putting Guevara in charge of the report to exonerate Galileo, being so sympathetic towards *The Assayer* in 1623, making sure that it was known that he liked the *Reply to Ingoli*, and having kept Ciampoli as a confidant for so many years. What did he wish in exchange? A book by Galileo as a sign to stop the further escalation of the conflicts between the peripatetics and the mathematicians. Instead, in 1632, perhaps in the worst situation he ever found himself during his long papacy—the longest in the last 324 years of the papacy—his standing was further deteriorated when he realized that he had to deal with a book which neither fulfilled what he thought Galileo may have agreed to in 1624 nor dealt with Copernican ideas in a hypothetical manner. In addition, his two trusted collaborators—Ciampoli and Riccardi—had led him astray.

Concluding Remarks

It should be emphasized that this paper does not intend to bring anything new to the much-studied trial of Galileo, nor does it discuss any unknown document. This paper

[76] Urban was involved in the Copernicus controversy since 1616. He was a member of the Congregation of the Index. All sources converge that he had tried to conciliate things. There were people who wanted to ban Copernicus altogether. See (Heilbron 2010, 219; footnote 73) and the reference to Lerner (2005), arguing that there was no way to make corrections in *De Revolutionibus*. Both Mayaud and Lerner pinpoint to passages that Ingoli could not correct. Was it because of oversight? Was it because of the then Maffeo Barberini's efforts? Might it be the case that Urban expressed his frustration at the fact that on the one hand Copernicus' work could not be "properly" corrected and, on the other, the *Dialogue* made things so much worse? But even this could not be considered as being such a secret not to be divulged to anyone.

attempted to explore the interpretative possibilities of a specific hypothesis about a neglected episode in Galileo studies: what was discussed during the six long meetings of Pope Urban VIII and Galileo in Rome between the end of April 1624 and the beginning of June 1624.

Our hypothesis that the pope urged Galileo to write a book which would proclaim a conciliation between the Aristotelians and the mathematicians so that neither would have the sole and exclusive jurisdiction on how to examine and determine the reality of the cosmos helps us elaborate on a number of issues that have been, on the whole, bypassed by Galileo scholars. By April 1624, the pope was convinced that the strong backing of *The Assayer* by the Academia dei Lincei and the deleterious attacks in the book against the Aristotelians aimed at their marginalization and, at the same time, expressed the determination of the Linceans to further strengthen the socioprofessional identity of the new breed of natural philosophers. Such a state of affairs was a harbinger of unwelcome developments in the Church. He thus proposed to Galileo to write a book in which the philosopher would become convinced by the astronomer's arguments and they would together herald the new astronomy. Such a suggestion expressed Urban's primary aim to guarantee a peaceful coexistence between the different social groups of Aristotelians and mathematically trained natural philosophers.

Through such a hypothesis, we are able to further clarify a number of issues in relation to Galileo: Galileo's disappointment with his stay in Rome and his rush to return to Florence in 1624; the reason for composing the *Reply to Ingoli* soon after he left Rome for Florence; the delay in completing the *Dialogue*; the chaotic state of affairs for securing the imprimatur for the *Dialogue*; and the two passages in Niccolini's correspondence where he relates what the pope told him. Except for the last issue which has not been commented upon in the literature, our hypothesis provides some complementary details to other interpretations.

Much of our argumentation hinges upon *The Assayer*. The reading of *The Assayer* as a political text, as a manifesto, brings to the fore its social aims camouflaged in an epistemological discourse. Manifestos are texts aiming at rallying and binding together their followers, asking them to close ranks. By the end of April 1624, Urban had realized that he was now occupying a political office—in fact he was occupying *the* political office. *The Assayer* conveyed perilous messages. It set the tone for what was potentially a subversive social agenda against which the Church had to be protected. What Urban attempted to articulate in the six meetings was an agenda of peaceful coexistence of philosophers and mathematicians, an agenda, of course, that would bring about a new balance of social forces—something not altogether foreign to his humanist origins. And, in contrast to *The Assayer*, the *Reply to Ingoli* was written in a different tone, as if Galileo wished to convey a message that was not altogether against what Urban had asked him to do in 1624. Both Urban and Galileo were "political animals" but with widely differing aims. No wonder it took them so many meetings, at least, to understand where the other stood.[77]

Urban's views about God's omnipotence did not come into any conflict with the decisions of the Congregation in 1616; on the contrary, it guaranteed ample epistemological maneuvering necessary for the conciliation he sought. What, however, remained an open question was the political and social prospect of such an agenda. The book he urged Galileo to write could, surely, be faithful to the ecclesiastical decisions and avoid mingling with scriptural issues, with the projected epistemology being negotiable. But the crucial question was: would the likelihood of such an agenda be acceptable to the Aristotelians and the mathematicians within the context of the power relations between these two groups as well as in their relations with the Church, the various religious orders, and the universities? Social and political realignments obey totally different rules than the implications of the arguments about God's omnipotence or the relative significance of observation in deciding the structure of nature. This is why Urban may have thought that the only person who could help him in such an undertaking was Galileo, who could talk both to the philosophers as well as to the natural philosophers and mathematicians.

The pope discussed all these issues with Galileo, who avoided any commitment concerning Urban's specific proposal. The pope insisted and kept on inviting him to all those audiences in order to iron out an agreement. Urban, who was not inclined to hearing others, talked a lot. Similarly, Galileo's talent to divert discussions from their core by "opening new parentheses" for many issues was also well known, and he discussed with the pope the latter's proposal for many hours. The pope did not wish for the

[77] "He was not given much to discussion with others and he prized his own opinion and his alone...He was prone to sudden changes of mood, and liked to be treated in delicate address and even if this does not make him change his views him, holds him from breaking off in anger" (von Ranke 1901, 373).

philosophers to be alienated; Galileo wished for the mathematicians to have full and exclusive jurisdiction over matters that were the Aristotelians' prerogative. These agendas had diverging social aims and favored different social groups. Perhaps Urban thought that Galileo would consider writing such a book and, independently of the difficulties involved in realizing the actual social and political realignments, the book would be a significant push towards the realization of Urban's political agenda. The pope hoped that if such a book was written and if the pope and Galileo (and hence the Linceans) backed the implied social agenda, then antagonisms could be contained and the Church would emerge more united. During these meetings, Galileo could not afford a showdown with the pope. Everything discussed with Cesi at Acquasparta hinged on the goodwill of Urban. The pope, though aware of Galileo's reluctance to directly endorse such a suggestion, may have been left with the impression that Galileo would not dismiss his role in such an undertaking. Urban was aware not only of Galileo's ambitions, but also of the latter's conviction that he could not advance his own agenda without some kind of approval by the Church. Galileo, it was well known to all involved, sought consistently to strengthen his ties with high-ranking Church officials. Urban sought a symbiotic state with Galileo in order to strengthen his own agenda.

Galileo could not come to terms with Urban's schema since he was convinced that to give voice to those who could properly understand his ideas was a necessary prerequisite for the further establishment of the ideas themselves. He was absolutely convinced that an appeal to authority, especially if this authority was Aristotle, was a dead end so far as the new natural philosophy was concerned. Mathematical natural philosophy, liberated from the authority of Aristotle, was the epistemological as well as the cultural prerequisite to properly understand nature and leave the Scriptures intact. And this cultural change needed a political agenda that was at odds with what Urban asked Galileo to do in 1624. Galileo refused to be drawn into a situation in which he would be compromised since he felt that it was upon him to create the conditions for the hegemony of the natural philosophers.

Of course, social identities are not inalienable. Such fluidity cannot be neglected if one is to understand the intentions of powerful institutions and their head—as it is the case with the papacy—as well as their relations with the spokesmen of emerging social groups which contest social spaces. In other words, the relationship between Urban and Galileo should also be assessed as a relationship homologous to what was happening between Aristotelians and mathematicians, and between Jesuits and Linceans.

> Rome was [not] a place in which factions and tensions did not exist and everything was solved by compromise and glossing over differences. On the contrary, the pattern of differences and tensions was impressive, but it was *continually shifting*. It is not just that people had specific identities, stances, views that were then rhetorically masked. Rather, Rome was a place where, because, of the dynamics inherent in the court structure, one's identity was continuously negotiated (Biagioli 1993, 262).

Strife for hegemony among different social groups lie at the heart of politics. Mediating such strife is the principal role of those who hold political offices. Politics is also about social peace. Mediation implies gauging the consequences of each possible course of action and understanding the contingencies and the constraints at every particular juncture so that ideological as well as political aims are served. Such political management implies that whoever is at the helm of the decision-making processes will have to deal with "their own lot" both consensually as well as confrontationally. This politically shrewd pope realized that administrative decisions by themselves could not turn the Copernican tide back: Aristotelianism was having too many cracks; Kepler's laws had overthrown another inalienable truth of Aristotelianism, namely that of circular motions; and the mathematicians had a very able leader and were becoming more and more confident.

For Urban, what was truly menacing was the prospect of the emerging new social power structures and not the possible dead end in the negotiations about a common epistemology between the Aristotelians and the mathematicians. Aristotelians and mathematicians being at each other's throats was an alarming prospect. Urban's view was that the struggle between peripatetics and mathematicians should not continue at the expense of either side, since both sides were honest and good Catholics and what was at stake was the Church's unity. Of course, the guidelines of the Council of Trent were to be absolutely respected, and Urban was convinced that Galileo's letter to Christina showed that the Scriptures were not in any danger at the hands of an able mathematician—even though such a show of interpreting the Scriptures was out of bounds. Furthermore, neither the Jesuits nor the Linceans constituted a tightly homogeneous group; there were many signs of some philosophers extending an olive branch to the mathematicians—just as there were many signs of intransigence and hounding.

Urban formed a unified strategy of conciliation based on five pillars: a theological, a legal, an epistemological, a political, and a personal pillar. Theologically it was clear to everyone involved that the rules of the Council of Trent would be upheld. Legally, the injunction of 1616 had to be obeyed. Epistemologically, the commanding principle would be God's omnipotence, which provided ample space to articulate arguments and put forth new concepts. Politically, he would use the powerful office of the papacy to initiate and control procedures and proceedings and bring pressure onto a number of people who might otherwise create problems for this strategy.

The personal aspect proved the most difficult to harness. During the six meetings in 1624, Urban proposed to Galileo to accept a common strategy for the benefit of the Church. The latter may have been shaken by Urban's suggestion, and though he may have shown some signs of playing along, he could not consent to the adoption of Urban's agenda in its entirety. Galileo had taken upon himself to vindicate Copernicus and complete the latter's social agenda. His new epistemology made him not simply the founder of a new natural philosophy, but a torchbearer for the mathematicians who aggressively sought a new socioprofessional identity at the expense of the Aristotelians. Urban's appeal to their friendship and to the mutual trust they had built over the years did not persuade Galileo to move away from the course of action he had already charted.

It is thus not unreasonable for Urban to have felt betrayed when he realized what was in the *Dialogue*. Not only because Galileo did not treat Copernican ideas as hypotheses, but because he realized that the hope he may have entertained in 1624 to bring about peace between the Aristotelians and the mathematicians could no longer be achieved. Urban's political and social agenda was dead, and he was simply out-maneuvered by Galileo, who had fully and irreversibly undermined the plan to have a book which would have paved the way for a new spirited coalition of philosophers and astronomers. Rage piled upon rage, especially when he realized something he may have not considered in 1624 when discussing these matters with Galileo: if Galileo wrote a different book than the one they had discussed, then the pope could not have made public that Galileo delivered something different than what the pope had asked him to do. Urban could not afford to acknowledge—even to his closest associates—that he was disobeyed by Galileo. The mess he was in, because of the developments in the war and his eroding authority at home, made such an acknowledgment even more difficult. Galileo was a good student of power politics and for six years he neither showed signs that he fully agreed with Urban's suggestions nor did he express any serious objections.

The vibrant and almost cinematic description by many scholars of intellectual and courtly life in Rome offers a view of a complicated space of cultural, social, and political fluidity. It is also a view of Rome in which black magic was intensely practiced and predictions through horoscopes were the order of the day. It was a Rome of many collectives and meetings of the literati. It was, nevertheless, a Rome where various groups were (re)articulating their identities and were in search of audiences. This dynamism was gaining momentum under the acerbic and ascetic look of the Collegio Romano. Some of the literati and the virtuosi were entertaining iconoclastic views and some were exploring new intellectual territories. Wars have a way of subduing as well as liberating at least some people. Romans were trying to find a new equilibrium to counter the Counter Reformation, without questioning the (in)variables that held Catholics together. The complicated relations among the participants in the search of identities were further galvanized by the half-knowledge many of them had of what was happening in the skies. In the meantime, there was a war being waged, a war that could have been the end of Catholicism. Was the ebullience as well the machinations of various groups in Rome a way to get prepared for the coming end? Was it a different reckoning of the possible end, a different welcome of the unknown new state of affairs in contrast to what the stern and rather inflexible ideology of the Collegio Romano demanded? Surely the Barberini papacy was a catalyst. A catalyst, however, of processes with a much larger horizon. And this is what Urban understood. He understood that when the plans, orations, and recitals subsided, there would remain a fundamental conflict that could not be resolved through pamphlets, books, and Congregation edicts. Two social groups—the Aristotelians and the newly emerging natural philosophers, with irreconcilable differences and worldviews—would be vying for hegemony. There were those among the philosophers who saw the coming deluge and were trying to adapt. There were others, philosophers as well, who were intransigent. The mathematicians, however, were the "worst of them all." They knew the new language necessary to read and comprehend the "book of nature" when others could only speculate about its contents. Centuries of saving the phenomena had sharpened their language; it had almost come to perfection. Now they were demanding a new role since they were not satisfied with only saving the phenomena. They demanded an overall new role: saving the phenomena should now mean imprinting the real world. As it was the case for so many centuries, what was at stake was the jurisdiction for understanding reality. Urban understood that his reign was during a period when different social groups were strongly contesting for this jurisdiction. He was a learned and experienced man. He knew that such conflicts could not be dealt with administrative measures. He confided that much to others when he said that had he been heading the 1616 deliberations "things would not have come to that."

Realizing the ineffectuality of dealing administratively with such issues appalled him. Here was a social and political process unfolding before his eyes, and he suggested to Galileo a conciliatory strategy. More importantly—something that is often overlooked by historians—he was a member of the Congregation when both Kepler's *Epitome* was banned and the corrections to *De Revolutiobus* were inserted in 1620. Urban was an inside man: he knew the machinery of the Church and its limits while others thought of the boundless possibilities of the edicts and the decisions by the Inquisition. It was, of course, true that such processes guaranteed some stability for the Church. When they met with Galileo in 1624, Urban was convinced that such a strategy by the Church would lead nowhere, hence the agenda he proposed to Galileo since he knew that a conciliation could be brought about not through administrative measures but by the magnanimity of someone who wishes conciliation. In a way, Urban decided to distance himself from the Jesuits and his own lot. He was asking Galileo to distance himself from the Linceans and his own lot. Both would have to be estranged from their closest allies—but for a worthwhile cause.

But the course of events took totally different directions.

Urban had no choice but to silence Galileo forever. Urban's absolute defeat would become Galileo's absolute defeat. What Urban envisaged was cruelly undermined by Galileo. If in 1624 there was some flicker of hope that it may be possible with the help of Galileo to chart a new social agenda, and if Urban's hopes were rekindled a few months later with the *Reply to Ingoli*, then the 1632 *Dialogue* was like a prodigious yet solemn speech at the funeral of what Urban may have been silently nourishing for eight years. A pope who thought that Galileo would deliver, but he did not. The betrayed pope could not be magnanimous. He could not be understanding. He could not be forgiving. He could only be ruthless—not even allowing for a proper burial of Galileo.

The hypothesis we put forward about what happened during the six meetings between Urban and Galileo in 1624 helps us clarify a number of issues whose explanations were rather opaque or missing altogether. Like many other hypotheses, ours does not depend on any direct evidence. But while doing history, it is always enjoyable and intriguing to go beyond what direct evidence shows and pursue alternatives to what really(?) happened.

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Combining Physics and Politics During the Cold War

The Role of International Scientific Networks and Institutions

Roberto Lalli[1]

Abstract

The approach to historical epistemology pursued by Jürgen Renn and Department 1 has led to scholars delving deeper into the study of the structural role of scientific institutions in the production, circulation, and certification of knowledge. The focus on scientific institutions, on the other hand, necessarily requires an understanding of the political, economic, and social conditions that led to their establishment and shaped their activities. The process of integrating political and scientific matters is particularly central in international scientific institutions, as their scientific goals must be necessarily integrated and negotiated with their diplomatic agendas in the context of changing international political relations. In this contribution, I discuss a general framework based on network analysis and the concept of science diplomacy to analyze the relations between scientific and political aspects of international scientific institutions in their historical unfolding. This framework will be discussed in relation to case studies drawn from the activities of three different international scientific institutions devoted to physics during the Cold War: the International Union of Pure and Applied Physics, the European Physical Society, and the International Committee on General Relativity and Gravitation.

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Introduction

Institutions play a crucial role in the historical theory of knowledge put forward by Jürgen Renn in *The Evolution of Knowledge* (Renn 2020). In Renn's framework, institutions embody the potential of a society or a group to coordinate individuals' actions in their interaction with their environment. The systemic and self-referential character of institutions is essential for codifying, embodying, enabling, and transmitting knowledge. In turn, knowledge shapes the institutions' structure and provide the basis for the individuals involved to act within the institutional framework. Institutions might then be interpreted as knowledge systems or distributed knowledge that enable cooperative action.

The other most relevant concept about social relations playing a crucial role in Renn's theory of knowledge is that of networks. In the last years, various projects at Department 1 of the Max Planck Institute for the History of Science have cooperated to develop a conceptual and methodological framework for the application of network theory to the history of science called socio-epistemic networks (Renn et al. 2016; Lalli, Howey, and Wintergrün 2020; Zamani et al. 2020). A taxonomy has been developed that groups the different elements involved in the production and circulation of knowledge into three layers: the social, the material, and the conceptual networks. Once the proxies have been identified in the historical sources for each level, it is possible to apply tools from multi-layer network analysis (Bianconi 2018) to investigate the change over time of the structure of these complex networks as well as the impact of the changes in one level on the transformations in the other levels.

In Renn's theory of knowledge evolution, networks are deeply related to institutions: a network is a "more general social structure from which institutions [...] emerge as a more specific social form by adding regulative controls" (Renn 2020, 306). In other words, the establishment of a network is a social presupposition for the emergence and functioning of institutions. In turn, the regulative properties of institutions allow networks to maintain and reproduce relations among the pertinent elements of the network. Crucially, within the socio-epistemic network framework, the process of institutionalization at one level might also lead to the systematization or regulation of the other two levels. For instance, the process of institutionalization of a social network might cause a stabilization of the knowledge connected to that specific social network.

To elaborate on this approach, it is compelling to understand what the historical relations between growing socio-epistemic networks and emerging institutions have been for different kinds of institutions at different periods. Especially interesting case studies are those institutions whose goals are supposed to be preeminently scientific but also act at different levels. In my paper, I tackle this issue by discussing the role of international non-governmental scientific organizations devoted to physics (or subfields thereof). International scientific institutions have a very complicated, and historically contextual, role different from national institutions, for they are established and maintained in a context that requires continuous negotiations between individuals who belong to the different political, cultural, and social settings of their respective national environments. For this reason, such institutions often mingle their scientific function with a political/diplomatic one, either explicitly or implicitly.

In past theoretical analyses of international scientific institutions, historians and sociologists have tried to categorize these institutions focusing on two elements: their function and their mode of operation. Crawford, Shinn and Sörlin (1993) have proposed taxonomies for both elements. As far as their function is concerned, Crawford et al. identify the major role of such institutions in the pursuit of international, or in some cases global, standardization. The goals of defining standards accepted by all nations involved in the activities of international scientific institutions are, in turn, categorized into three distinct sets of activities: the pursuit of cognitive homogeneity, the establishment of communication standards, and the negotiations about technical standards. Crawford et al. also define two types of modes of operation distinguishing between *spontaneous* and *bureaucratic* institutions. The spontaneous ones are "institutions motivated by the interests of individual scientists who draw on national resources to hold world congresses, committees, coordinate projects," while the bureaucratic institutions are organizations where "cooperative schemes are […] outgrowths of government programs and therefore strongly influenced by national interests" (23–24). Aant Elzinga (1996a, 3–4), similarly, differentiates between *autoletic* and *heteroletic* organizations, the former serving "science as an end in its own right" and the latter supporting "transnational scientific cooperation on extra-scientific grounds." Whether explicit or not, the above-mentioned two-type taxonomies correspond to the distinction between intergovernmental and non-governmental organizations, which were formally defined from a legal perspective only after the UN Charter was drafted in 1946 (Martens 2005).

In my paper, I argue that while useful as a description of idealized scenarios, these taxonomies do not properly describe how historical international scientific institutions acted in the real world, for all international scientific organizations—including those that might be considered the most spontaneous among them—have always had a component that is related to extra-scientific goals. Even in those cases where institutions were built to pursue purely scientific ends, the actors involved had to necessarily take into account the international political context and enact regulations in accordance with international political relations, sometimes even with the ambition of influencing them.

Science Diplomacy as a Historiographical Framework

I argue instead that one should understand the activities of international scientific institutions within a framework that is now called *science diplomacy*—a term currently in use by politicians, political scientists, and, increasingly, science historians (Flink and Schreiterer 2010; Ruffini 2017; Turchetti et al. 2020; Adamson and Lalli 2021). While there are many biases in the current discourse on science diplomacy—due to the fact that most analysts are also advocates for the use of science in the soft power approach to foreign policy[2]—historians have long shown the deep interplay between the production and circulation of techno-scientific knowledge on the one side and the development of diplomatic relations on the other (see, e.g., Krige and Barth 2006; Steinhauser, Gutfreund, and Renn 2017; Turchetti 2019; Kraft and Sachse 2020). Here, the framework is simply meant to state that one should understand international scientific organizations also as science diplomacy agents, even when they are non-governmental.

For inter-governmental scientific institutions this dimension is pretty much obvious, as governments have to formally agree on various matters, and therefore a diplomatic function is embedded in the structure of these organizations. One has just to think about the publicly recognized historical role of CERN in the Western European integration process by bringing together nations that a few years earlier had been enemies during

[2] For a critique of the notion and linguistic (mis-)uses of science diplomacy, see (Rungius and Flink 2020); for the concept of soft power, see (Nye 2004).

World War II (Hermann et al. 1987; Kohlrausch and Trischler 2014; Moedas 2016). CERN is clearly a major example that has been, and is, used as a model of science diplomacy to be exported, as shown by the recent establishment of the project Synchrotron-Light for Experimental Science and Applications in the Middle East (SESAME). SESAME brings together, within a single intergovernmental organization, countries with various political and territorial tensions among each other, including Egypt, Iran, Israel, Pakistan, the Palestinian Authority, and Turkey, among others (Schopper 2017; Höne and Kurbalija 2018; Rungius, Flink, and Riedel 2019).

While the science diplomacy dimension is certainly constitutive of inter-governmental organizations, this dimension is also unavoidable for most international non-governmental organizations. First of all, an academic tradition of international relations scholars has shown the impact on political relations of non-state actors—defined as social actors in international relations whose agendas are not necessarily aligned with those of their state (Risse-Kappen 1995). Non-governmental institutions are among those non-state actors, and their role in world affairs has become a major point of interest in transnational history in the last years (Iriye 2002). Secondly, most scientists involved in these organizations are in fact elite scientists who have had a high-level science policy (or in some cases even a political) role in their own country, and therefore their agendas cannot really be considered apart from national interests or governmental pressures. The level of scientists' alignment with national interests depends of course on various factors—political contexts, national regimes of control, individual trajectories—but it is certainly problematic not to consider that the scientists might act as covert or even overt state actors rather than as non-state actors. Finally, even if none of this directly happens, the scientists involved are led to discuss the political implications of the regulative frameworks of the institutions they are involved in, even when they would have preferred not to do so.

As a consequence of the above, one might argue that it is very rare for an international scientific institution to have a purely autoletic character (to use Elzinga's terminology). It is rather more interesting to ask what have been the actual historical relations between autoletic and heteroletic modes of operation in specific cases, how different political contexts shaped these relations, and, in turn, how the political/diplomatic dimensions shaped the regulative function in science that scientific institutions were supposed to play. In the next sections, I shall discuss these relations through examples taken from the history of three different international scientific institutions, mostly focusing on the Cold War period. I shall also highlight in which cases the historical analysis of these institutions might be useful to improve our understanding of the institutionalization process of socio-epistemic networks and to regulate their activities.

The Long History of the International Union for Pure and Applied Physics[3]

The first example is the IUPAP, which has just celebrated one hundred years since its foundation in 1922 (Lalli and Navarro 2022). In the framework of Crawford et al. (1993), the IUPAP is to be considered a *spontaneous* organization of physicists. However, this characterization would hardly describe the actual process that led to its establishment as well as its first 25 years of activity. It was funded within the umbrella of the International Research Council (IRC) that had the explicit political goal—codified in its statutes—to exclude Central Powers' scientists from international scientific cooperation led by victorious allied countries. The foundation of the IRC was born as a top-down initiative for strengthening wartime inter-allied cooperation, envisaged right from the start as an umbrella organization that would be structured in different unions devoted to different branches of scientific knowledge. In addition, membership in both the IRC and its unions was codified as national membership, implying that members were official representatives of countries. The diplomatic dimension was so central in this early period, that in some cases the scientific representatives of national members were the governments themselves.[4] During the interwar period, the IUPAP was framed as an institutionalized form for boycotting German physics, with the Germans responding by enacting schemes of counter-boycott. Even if attempts to increase the institutional relations with German physicists gradually increased after the mid-1920s—especially after the transformation of the IRC into the International Council of Scientific Unions (ICSU) in 1931—Germans did not join the IUPAP until the 1950s. Given the importance of German physics in the interwar period, one could hardly expect much from the IUPAP in terms of scientific impact. In fact, the organization was mostly inactive and its existence sparked

[3] This section is based on ongoing research projects that will be published in (Lalli and Navarro forthcoming).
[4] This was, e.g., the case of the representatives of the South African Union from 1922.

much criticism. Notably, Niels Bohr was extremely critical of the limited degree of internationalism expressed by the IUPAP and even declined the offer of becoming IUPAP president in 1934 (Fauque and Fox forthcoming; Navarro forthcoming; for older literature, see Kevles 1971; Forman 1973; Schroeder-Gudehus 1978; Greenaway 1996).

The IUPAP was refounded in the immediate post-World War II period on completely different bases with respect to the IRC.[5] Indeed, the involved officers decided that the IUPAP should be open to all countries where there was an active community of physicists, including former enemies as soon as the political conditions allowed—an approach that would favor the relatively rapid entrance of (West) Germany in 1952, even before the Federal Republic of Germany had acquired full sovereignty. The institutional context for the IUPAP refoundation was the 1946 agreement between the ICSU and UNESCO. This agreement reshaped the international scientific unions' functions, as their activities had to be aligned in some way with UNESCO agendas, even if this necessity was more perceived rather than formally requested. In turn, the ICSU and its unions rapidly became the institutions that would allow UNESCO to have an impact on the world of science in a non-governmental fashion, while UNESCO itself was instead intergovernmental (Elzinga 1996b; Petitjean et al. 2006).

This new framework had a deep impact on the activities of the IUPAP. Contrary to other scientific unions, the IUPAP had not created any topical commissions in the interwar period. From 1947 onward, it rapidly established various specialized and affiliated commissions dedicated to the promotion of international cooperation and exchange in specific branches of physics. This change was not only quantitative; it also changed the goals of the organization, which from then on went far beyond that of just establishing international standards on symbols, nomenclature, and units. There was also a subtler change as well. Some high-level officers started understanding the goals of the IUPAP as aligned with those of UNESCO. This interpretation of the new institutional framework was made clear by its former President Robert A. Millikan. He argued that the IUPAP should not sponsor any specific projects, but its action should rather be limited to those

[5] This second part of the section is based primarily on (Lalli forthcoming).

activities that were in line with the UNESCO's goals of promotion of world peace, especially by organizing international conferences.[6]

During the Cold War we can distinguish between two major historical phases. During the first one, from 1947 to 1957, the IUPAP—like most of these international organizations with a supposedly global character—was in fact dominated by Western membership and interests as well as ideology. A radical shift occurred after the mid-1950s, in relation to the changes in the USSR's foreign policies after the end of the Korean War and the death of Stalin. The Soviet Union started actively participating in the UN, UNESCO, and in most international scientific organizations (Ivanov 2002). The USSR officially joined the IUPAP at the 1957 General Assembly in Rome, and most countries under the Soviet sphere of influence followed suit. Even if some Eastern European countries were already formal members of the IUPAP, they had not been actively participating from the late 1940s until the entrance of the Soviet Union.

This enlargement of membership initiated a second phase, as it radically modified the way in which the scientists involved in the decision-making process perceived the regulative function of the IUPAP. The meetings of its Executive Council and of its scientific commissions were now also understood as venues for the negotiation and balance between the two Cold War blocs. At the same time, the scope of the IUPAP gradually increased with efforts to involve developing countries. The diplomatic function of the IUPAP became in this phase embodied in the work routinely done by IUPAP officers. This was made explicit by the then associate Secretary General and future President, Canadian physicist Larkin Kerwin, who in 1969 wrote: "The Union's purpose is to foster international physics meetings, more rapid dissemination of information and the establishment of international standards, units and nomenclature. Its *unofficial* goal is to make a contribution to general international understanding" (Kerwin 1969, 53; emphasis mine).

I shall now discuss two examples of this regulative role in relation to the Two Germanys and Two Chinas problems during the Cold War (these case are discussed in Liu, Yin, and Hu forthcoming; Olšáková forthcoming; Cozzoli forthcoming). After the Soviet Union

[6] Letter from Robert A. Millikan to Pierre Fleury, 19 May 1948, IUPAP Archives, Gothenburg secretariat, (hereafter IUPAP Gothenburg) series E2 "Correspondence with Council Members," Vol. 1, Folder "M," Center for the History of Science, Royal Swedish Academy of Science.

joined the IUPAP, the People's Republic of China (PRC) also made the official request to become a member, which was rapidly granted by the IUPAP Executive Council.[7] In the following months, the Republic of China in Taiwan and, one year later, the German Democratic Republic (DDR after its German name) also made their official request. These two requests enraged the PRC and the National Committee of West Germany, respectively. Cold War negotiations took place during the meetings of the IUPAP Executive Council in 1958–59, in which these requests were discussed. The outcome of these negotiations, also based on the path-dependency from previous decisions, led the IUPAP Executive Council to decide for accepting all of them, albeit without a unanimous agreement among its members. A crucial step in this process was to agree on unofficially changing the interpretation of the word "nation" in the understanding of national membership. The term "nation" was now understood as a "territor[y] that [was] scientifically independent" without having any political implications on the recognition of the independence status of this territory.[8] In spite of the attempts by IUPAP officers to convey this definition of territorial rather than national membership to the President of the Chinese Physical Society of Beijing, the PRC withdrew its participation, for it could not accept being an official member in an institutional body that recognized, at least implicitly, the Republic of China in Taiwan as a separate national entity.

It took decades before the PRC joined the IUPAP, and the eventual entrance of mainland China was again a result of negotiations with a clear diplomatic character. In order for the PRC to join, it was necessary to modify the statutes and eliminate the word "national" from the definition of members, which in 1981 became "liaison" members. Secondly, the name "Taiwan" also disappeared from the list of liaison members: both representative scientific organizations—in mainland China and Taiwan, respectively—were listed as different organizations of (one) China from 1984 onward.

The Two Chinas and Two Germanys issues were very general, and they emerged in similar terms in most international organizations based on a notion of national membership. The eventual resolution of the negotiation was basically the same in most cases. One would

[7] China had been a member from 1934, but after the end of the civil war in China and the establishment of the PRC in mainland China in 1948, contacts were interrupted and no fee was paid. The last time China was listed as a IUPAP national member was in 1951, but, unofficially, the membership terminated in 1948.
[8] Letter from Edoardo Amaldi to Nevill F. Mott, 24 November 1959, Sapienza University, Rome, Archives of the Physics Department, Fondo Edoardo Amaldi, Subfondo Archivio Dipartimento Fisica, scatola 106, sottofascicolo 4, Corrispondenza Presidente, 1957-1960.

then have expected that such negotiations should have happened only once, possibly within the ICSU—the umbrella organization of all scientific unions—and the regulative work done in that umbrella institution should have provided the framework for decision-making on these purely general political issues for all scientific unions. However, this was not the case. While in contact with other unions and the ICSU, officers at the IUPAP acted as if they had full autonomy in this decision-making process. This leads to the conclusion that each international scientific institution—no matter how embedded in a structured institutional organization—has done a performative function of negotiating political issues of its own in order to embody the final outcome as an internal autonomous process done by individuals, rather than accepting top-down decisions, in spite of the fact that the final outcome was the same for all those organizations.

The Foundation of the European Physical Society

The second example concerns the historical process leading to the foundation of the European Physical Society (EPS) from the mid-1960s to 1968.[9] As we have seen, the IUPAP was an organization that was initially conceived as a top-down project designed as a coordination of scientific activities of allied countries during World War I. The involved individuals had to enter the existing organizational structure and gradually change it, often overcoming many difficulties due to the institutional inertia. The establishment of the EPS was certainly a result of a more bottom-up process, pursued however by physicists with an elite status within national and international communities. The EPS emerged as a result of the institutionalization of an existing network. The explicit goal of the promoters was that the EPS would help the existing network grow. More specifically, the regulative function of the EPS—as envisaged by the main promoters of the initiative—was to make a disciplinary sector of the European physicists' network a model of cooperation for the larger network. The social network that was being institutionalized was initially composed almost uniquely of particle and nuclear physicists involved at CERN, and the most active proponent was the then President of the Italian Physical Society, Gilberto Bernardini. The question is then why some actors in this growing social network started to pursue a process of institutionalization of their network. If it is true that the core network was the CERN network, what was the reason to have also the EPS,

[9] This section is based primarily on (Lalli 2021).

when they already had the CERN. Why did they need the EPS in the first place? What did they want to regulate?

At the very basis, the motivations lay in the necessity to address two issues. The first was that in the 1960s physicists connected at CERN were trying to strengthen particle physics research in their own countries. CERN was not sufficient, and in order for CERN to become more competitive against US laboratories, it was necessary that a large group of European physicists learned how to do big science, which was not the case in the early years of CERN's activity (Pestre and Krige 1992). What they needed was a set of agreements and regulations about how to build an overreaching infrastructure connecting CERN with small and medium-size particle physics laboratories in the various CERN member states. This is the effort that was being put forward with what was called the "European pyramid" of high-energy accelerators (Carson 2010). The EPS was a strategy to address this issue based on intergroup agreements getting rid of governmental representatives, present in the negotiations at CERN.

The second issue was the regulation of physics publications in Europe in order for them to become more competitive against mainstream journals of physics, namely those published by the American Physical Society (Khelfaoui and Gingras 2019). More specifically, the most urgent issue concerned the status of the journal of the Italian Physical Society, *Il Nuovo Cimento*. In the 1950s, *Il Nuovo Cimento* had enjoyed a rapid increase in importance and Bernardini's declared ambition was to have the journal recognized by European physicists as the main venue for the publication of CERN-related research results. At that time, ongoing projects of major private publishing houses to establish journals dedicated uniquely to particle physics greatly endangered the project of Bernardini. Therefore, behind the foundation of the EPS one can see the efforts by the President of the Italian Physical Society to defend a niche that the Italian Physical Society's journal was trying to occupy with a lot of institutional and personal efforts.

To summarize, the initial project of establishing the EPS was an attempt to build an institution that could provide a forum for negotiating these issues among European elite physicists and regulate the work concerning the establishment and functions of laboratories and journals. Neither proposal succeeded and in fact they were both abandoned right at the beginning, when national physical societies entered the negotiation in the funding phase. During this negotiation process, the EPS became mostly a federation of existing national physical societies in Europe, contrary to the initial project of a society mostly composed by individuals.

Within these agendas, the EPS—like the IUPAP—combined quite explicitly scientific and political goals. In official statements on the role of the envisaged society, scientific coordination and exchanges were described as deeply related to cultural and political goals, where the cultural and political arguments were similar to the discourses promoted at CERN and with the European movement of science being a tool for pursuing European cultural unity (Mobach and Felt 2022). This declared cultural and political purpose could have been wrongly considered an abstract and uninfluential rhetorical artifice, but eventually it was not. It combined with political aspirations in Czechoslovakia in the midst of the Prague Spring. Czechoslovak physicists saw the creation of the EPS as a way to implement ideals spread within the Prague Spring movement. In 1968 they declared their desire to join the initiative in spite of the opposition from the Soviet Union, East Germany, and other countries of the Soviet Bloc. The final meeting of the EPS steering committee was organized in Prague in May 1968—quite symbolical from a political standpoint. There, representatives from most countries of Europe could attend. Czechoslovak physicists had also requested to establish an EPS secretariat in the Eastern bloc, possibly in Prague, which was granted in that occasion. In that specific historical moment, the EPS project, initially proposed for quite different purposes, transformed into something different because it came to embody the needs and aspirations of communities that had not been part of the negotiations previously, sparking controversy within the Eastern bloc and explicit opposition from the Academy of Sciences of Soviet Union.

The negotiation within the Eastern bloc ended successfully for those communities who wished to join the EPS, and the foundation of a pan-European physical society was planned on 26 September 1968. Before its foundation, though, the invasion of Prague dismantled the hopes of those who were enthusiastic about the possible role of the EPS in facilitating greater political integration through scientific cooperation at the pan-European dimension. The EPS steering committee held an urgent meeting to discuss the meaning of the foundation of the EPS under the new circumstances and whether scientific institutions of countries involved in the invasion of Czechoslovakia would be accepted as members. This discussion occurred within the members of the committee without any reference to the different positions of their national governments on the recent political crisis. Even in this case, it appears that physicists involved in the decision-making process were dealing with political issues autonomously with no directive from their own country.

After a long and dramatic discussion, the EPS steering committee resolved to hold the foundation as planned, soon establishing a society open to all national physics organizations that wished to join. One of the motivations at the basis of this decision—supported by the Czechoslovak member in the steering committee František Janouch—was that this would have been the only way for the society to function as a network of support for Eastern Bloc's physicists who were suffering political discrimination in their own country. In short, in this tumultuous period right before its establishment, a new role for the EPS was envisaged, namely, that of a supporting institution for Eastern Bloc dissidents.

These were not void expressions of hope. It is what actually happened. The second President of the EPS, Swedish physicist Erik Rudberg, offered Janouch, who was suffering from political discrimination in Prague, a research position in Sweden, which was a neutral country (and would be so until Russia's invasion of Ukraine in 2022). Czechoslovak authorities did not initially allow Janouch to accept this position for years, but under pressures from the EPS he was eventually allowed to leave the country and accept the position in Sweden in 1973.

Here we have a case in which an institution conceived to provide regulative spaces for a specific network did not succeed within its scope but did eventually assume a function, dependent on the historical context, that was quite different with respect to what was initially envisaged. In this shift, the EPS turned out to be quite successful in some unexpected, interesting ways. Once the institution was there, it could be used and modeled according to individual needs and motivations, and this flexibility was eventually more useful in the diplomatic than the scientific arena in some moments of the Cold War.

The International Committee on General Relativity and Gravitation and the Renaissance of General Relativity

The third case is perhaps the most interesting one from the perspective of socio-epistemic networks. It concerns the activity of the International Committee on General Relativity and Gravitation (ICGRG), which later transformed into the International Society on GRG.[10] The ICGRG was an institution dedicated to the promotion of the field of general relativity and gravitation. In this case, we have one of the clearest examples of the institutionalization process of a socio-epistemic network. As explained in more detail elsewhere (Lalli, Howey, and Wintergrün 2019; 2020), we have been using a network approach within the framework of socio-epistemic network as presented in Renn (2020). Using network theoretical tools and concepts, we uncovered that the changing topology of the social network was chronologically related to evident changes in the semiotic and semantic layers, which suggests that a change at the social layer was causally connected to knowledge transformation in the field of general relativity. This case can then be understood as an evolving social network—with clear connection with knowledge production mechanisms—that rapidly institutionalized itself in order to promote the research field and stabilize it within the broader landscape of scientific research, especially by stabilizing the tradition of international conferences.

Even in this case, though, the diplomatic dimension played an important role and shaped the activities of the ICGRG since its early phases. The first-ever international conference to be completely dedicated to general relativity was the conference held in July 1955 in Bern, in the same period in which other conferences and encounters were shaping the neutral role of Switzerland in the phase of slight détente between the USSR and the USA, after the death of Stalin and the end of the Korean war (Strasser 2009).[11] The Bern conference was both favored and shaped by this transformation in diplomatic relations between the two Cold War blocs and the special role of Switzerland in this phase.

The format of the conference was modeled on that of existing international scientific organizations that were reorganizing international scientific exchange after World War II. These were the international scientific unions that, as mentioned above, were based on the concept of national membership. The scientists invited to the Bern conferences acted as national representatives who opened the meeting by describing general relativity as a field that was ideal to favor cooperation between East and West, implicitly because of the

[10] This section is based on (Lalli 2017; 2020).
[11] Two crucial events occurring in Switzerland in the same period of the Bern conference were the Geneve Summit—the first high-level political meeting between the USA, the USSR, France, and the UK after the 1945 Potsdam Conference—and the Conference on the Peaceful Uses of Atomic Energy, held in Geneva in August 1955.

absence of technological, and therefore military, applications.[12] This view was made public by André Mercier, the main organizer of the Bern conference and, later, the long-lasting secretary of the ICGRG until its transformation into a society. Mercier clearly perceived himself as an ambassador of peace precisely because of his work in the ICGRG.

Even before the establishment of the ICGRG, the very first steps of scientific community-building of the network were shaped by the existing framework of international scientific institution-making. This was evident not only from the formality embedded in the structure of the Bern conference, but also from the fact that the conferences were funded by the IUPAP. This reveals that a connection within this large framework was envisaged since the beginning even by those, like John Wheeler, who were proposing a US-style culture of communication, which was perceived as more democratic and less centralized (Blum and Brill 2020). In fact, Wheeler—a member of the ICGRG and one of the main promoters of general relativity in physics—was at the same time a representative of the IUPAP US national committee, which indicates how these institutions were connected from the social network perspective of the interlocking directorate.

In Renn's framework, one crucial function of the institutionalization process of a social network is to regulate the activity of the network itself and to support its epistemic goals. In the case of the GRG network and its transformation into a committee in 1959, we can see an example of this function. The institutionalization process led: firstly, to stabilize the tradition of organizing international conferences every three years; secondly, to create a Bulletin in 1962 for sharing information among individuals in the network; and, finally, even to publish a scientific journal dedicated uniquely to the field—*General Relativity and Gravitation*—from 1970. It is remarkable that the quantitative analysis of the dynamics of the social networks of collaboration in general relativity indicates a transformation in the network structure that temporally coincides with the institutionalization process, namely with the establishment of the ICGRG. In turn, it is remarkable how this transformation in the topology of the social layer coincided temporally with major transformations in the other layers of the socio-epistemic

[12] Ironically, the vague hope of producing anti-gravitational devices with the possibility of military applications was one of the reasons behind the increase of funds, both private and governmental, to support the field in the USA (Kaiser and Rickles 2018).

networks indicating the possible causal connection between institutionalization processes and knowledge transformation and stabilization.

However, the institutionalization process did not only perform this sort of social and epistemic regulative function, but it also brought tensions of different kinds related to different individuals' cultural and epistemic views. The most transformative of these tensions regarded the perception of some individuals in the growing network—the younger generation of Anglo-American physicists—that the ICGRG was a self-appointed body of people nobody had elected. The consolidation and spread of this perception eventually led to the transformation 0f the ICGRG into a society in 1974. While one might interpret this transformation as a democratization process, this process was also pursued in order to facilitate the closer institutional connection of the GRG network with the IUPAP. The elected Executive Committee of the newly established ISGRG became an affiliated commission of the IUPAP in 1975. On the one hand, some nodes in the social network wanted to change the institutionalization structure in order to make it more democratic. On the other hand, the institutional development led it to be more and more integrated into the overreaching structures of the institutionalization of international scientific cooperation during the Cold War, together with the various norms and rules about national balance in the executive committee—which also meant a Cold War balance, as I have described previously.

This brings me to the last point. Even in this sort of institutionalization process that grew from the actual needs of a growing socio-epistemic network, the diplomatic dimension could not be avoided. This is clearly evident from Mercier's political position and the progressive institutional integration within the IUPAP. In addition, after the Six-Day War and the invasion of Prague, members of the ICGRG publicly expressed very different positions about how to handle the political crises within the committee's activities. An ICGRG international conference was planned to be held in Tbilisi, Georgia in September 1968. The Soviet organizers invited no Israeli scholars because of the disruption of diplomatic relations between the USSR and Israel after the Six-Day War. Either as a protest in support of their Israeli colleagues or as a reaction to the invasion of Prague, various members of the ICGRG, including Mercier as well as many scientists of the network at large, decided to boycott the Tbilisi conference. Others, including President Hermann Bondi, exposed very different views according to which scientists in international organizations should never use meetings for political demonstrations but should always strive to create the conditions for encounters. The different political positions within the ICGRG had scientific consequences. Those few physicists from the West who attended the Tbilisi conference—remarkably Wheeler and many scientists close to his scientific group—initiated important cooperation with Soviet physicists, the most productive and important being perhaps the cooperation between Kip S. Thorne and Vladimir Braginsky on the topic of gravitational waves (Braginsky and Thorne 1987).

Conclusion

In my paper, I have surveyed the relations between socio-epistemic networks and the emergence and functions of non-governmental international scientific institutions based on three case studies. By looking at how all these institutions dealt with political matters, it emerges that a taxonomy defining clear boundaries between autoletic and heteroletic institutions—or between spontaneous and bureaucratic institutions—does not completely capture the complexity of the modes of operation these institutions enacted under different historical conditions. In principle, all three institutions were spontaneous/autoletic according to the definitions of Crawford et al. (1993) and Elzinga (1996a), respectively. In fact, none of them was, or even could be, purely spontaneous/autoletic. Their actual mode of operation presented a balance dependent on how the individuals involved perceived the diplomatic role of institutions in specific historical moments.

Secondly, the cases reported here show very different institutionalization processes: from the top-down process of the IUPAP to the bottom-up one of the ICGRG. However, it appears that in all these cases physicists tried to accommodate to overreaching existing institutional frameworks in order to stabilize and regulate socio-epistemic networks. In turn, individuals, with much effort and time, tried to modify the existing structure on the basis of a learning process not only to support the growth and activity of socio-epistemic networks, but also to deal with political issues.

This brings me to the last point. In this sort of institutions, the science diplomacy dimension is inevitable. Individuals active in these institutions have to negotiate and to create balances that are diplomatic in nature and related to the current political situations and crises. This necessity to deal with the diplomatic dimension in some cases had epistemic consequences, in that different parts of the social networks related to the status of international political relations changed the structure of the social networks, which in turn had an effect at the epistemic level. This is the sense in which I propose interpreting these institutions as science diplomacy agents, rather than trying to insert them into ahistorical categories.

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Part IV. Knowledge Evolution and the Anthropocene

Crises and the Anthropocene

Ana Simões[1], Maria Paula Diogo[2]

Introduction

We propose to address history of science and technology as a disciplinary unit and aim to explore how history of science and technology as a part of the humanities may help to address problems of concern faced by present day societies.

We start from the theoretical background provided by the Science and Technology in the European Periphery (STEP) and Tensions of Europe (ToE), two independent research networks that coexisted from 1999 to 2014, eventually giving way to other groups. They both shared the will to dissect the conceptual meanings of Europe (Gavroglu et al. 2008).[3] With this framework in mind, we address two topics: crises and Anthropocene.

Both STEP's and ToE's agendas and our view on the debates on crises and the Anthropocene call for a *longue durée* approach and for an attitude of resilience against both technophobia and technophilia, thus contributing to ongoing debates by revealing past decisions, strategies, and options that shaped contemporary societies. We deem they are critical to better understand today's society and to propose active agendas that may influence public opinion and policy-makers. As such, it is our contention that these two interrelated topics deserve particular attention from scholars in our field (Diogo et al. 2019b; Diogo, Louro, and Scarso 2017; Diogo and Simões 2021b; Diogo and Simões 2016; Gavroglu et al. 2008).

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[3] See also STEP FORUM and https://www.tensionsofeurope.eu.

A Bit of the History of the History of Science and Technology: STEP and ToE

STEP and ToE have different, albeit complementary approaches to the history of science and technology, whereby both encourage national case studies and their relation to a larger regional, transnational, and/or European framework.

STEP's agenda stressed the concepts of appropriation, circulation, mediation, and innovation (Gavroglu et al. 2008), and was particularly concerned with the historicization of the notion of "European Periphery." Approaching local studies under such a theoretical umbrella, it is possible to reinforce the concept of co-construction of centers and peripheries in order to go beyond the corset of fixed geographies and to probe the idea of co-construction of "moving localities" (Raposo et al. 2014). It was also possible to bring to the forefront the perspective of active receivers that were often dismissed from global accounts based on a simplistic and static divide between the active Global North and the passive Global South, resulting in finally being able to fully participate in the ongoing debate on the relevance of science and technology as a global phenomenon, closely related to the discussion of progress itself (Diogo, Simões, and Gavroglu 2017). Topics such as experts, amateurs, and intermediaries; travels and circulation; and popularization and its various media, textbooks, and techno-political agendas have been under STEP's scrutiny. After the dissolution of STEP in 2014, mobility(ies) and urban spaces have profited from a discussion informed by STEP's rationale, often put forward by former STEP members. Together they have proved to be fertile territories for historical research that is relevant to the challenges that Europe faces today.

The ToE research network built upon two main conceptual tools: the idea of a hidden technology-driven agenda for European integration (as opposed to the explicit economic, financial, and political integrative policies), and the notions of linking and delinking that explore the ever-changing asymmetries within Europe and between the continent and the global world (Misa and Schot 2005).

The four main assumptions that supported the research framework of the first ToE program—*Inventing Europe: Technology and the Making of Europe, 1800–Present*—which gave way to the *Making Europe* 6 volumes book series,[4] were (i) the *longue-durée* narrative; (ii) the relevance of material networks, technological systems, and circulation of knowledge, practices, and artifacts; (iii) the need for placing the history(ies) of Europe in a global, worldwide dynamic perspective; and (iv) the need to review the current understanding of the process of European integration in order to highlight its bottom-up dimension.[5]

Beyond its research agenda, ToE encouraged a strong commitment to contemporary issues, claiming the relevance of history of technology in the discussion of topics usually labeled as political or economic and in proposing solutions to citizens and policy makers. In this context, it is only natural that the follow-up from the first ToE program, which started in 2016, encompasses a new ongoing research agenda—*Technology and the Making of Europe's Societal Challenges*—geared towards the big issues of our time, particularly those related to the concept of crises.[6] This is still uncharted territory for historians of technology insofar as technology has not been placed at the center of the current analyses of crises. It is therefore a tantalizing research avenue for historians of technology to understand how societal crises are shaped and/or mediated by technology.

History of Science and Technology and Contemporaneity

It is at the crossroads of STEP's and ToE's agendas, guided by their conceptual frameworks, that we highlight two topics—crises and Anthropocene—as research areas worthy of attention for historians of science and technology. We believe that both conceptual debates and archival research on these topics promise challenging results and show how the humanities—in this case history of science and technology—may be relevant assets in discussions occurring in present-day society on topics such as resources, migrations, security, urban challenges, mobility, pandemics, and environment.

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[4] *Making Europe* book series, 6 volumes under the coordination of Johan Schot and Philip Scranton. http://www.makingeurope.eu/

[5] Johan Schot, Ruth Oldenziel, Tensions of Europe Intellectual Agenda. Inventing Europe: Technology and the Making of Europe, 1800-Present, Position Paper, 2005, https://www.tensionsofeurope.eu

[6] Erik van der Vleuten, Position Paper, 2016. Jan Korsten, Eric van der Vleuten, Report kick-off workshop Technology and the Making of Europe's Societal Crises/Challenges, a Transnational History ca 1850-Today, 2016, https://www.tensionsofeurope.eu.

They are usually labeled as just political or economic, but it is evident that a multidisciplinary input encompassing reflections on the role of science and technology is fundamental in order to propose solutions to citizens and policymakers.

Crises

The rhetoric of and discourses on crises are rooted in a wider sense of loss and frustration very much based on the perception that the human ability to command nature does not lead necessarily to increasing well-being and that resources are indeed finite. Certainly, this is not new in academic discussions among historians of science and technology. The debate on technological pessimism and skepticism is a vivid example, and Kranzberg's first law, "Technology is neither good nor bad; nor is it neutral" (Kranzberg 1986), is a simple yet effective reminder that trade-offs are usually part of the relationship between humans, nature, society, and technology.

However, the idea of linear, continuous, and "good" growth and progress—which is at the heart of industrial capitalism—is still a *doxa* in the sense that for most people this constructed vision appears to be the only possible vision of reality. The growing evidence that this narrative does not match reality creates a civilizational instability that finds its natural habitat in a series of never-ending slow-moving crises, in a "rolling apocalypse" (Williams 2012) of contemporary history in the sense that there is an end at sight if humanity's actions in nature and society do not change drastically. These are concepts put forward by the historian of technology Rosalind Williams.

Pondering over the present predicament humanity finds itself in, Rosalind Williams discusses the concept of slow-moving crises as an alternative to the former longtime notion of crisis as a singular and abrupt event with dire consequences by acknowledging that crises, besides being events defined in space and time, are processes that unfold in extended periods and regions. They are part of a long sequence of what she dubs "slow-moving crises," in order to stress that searching for their causes or their aftermath leads to unavoidable deadlocks due to their integration into a multitude of inter-related events influencing and impacting each other, and eventually leading to the end of contemporary history, in a "rolling apocalypse." Her view on the concept of slow-moving crises also stresses the danger of easily discarding the actors that are part of the crises, regarding these long periods of "cascading crises" as "natural" phenomena that escape human agency.

Following her lead, we suggest that as historians, we should be able to utilize the lenses of science and technology to innovatively address the concept of crisis by asking different questions. A few examples include: how are crises enacted, entangled, and built across time and space? How do different actors perceive, use, mediate, and shape their interactions in moments of crises? Are science and technology a cause, a medium, or a solution in understanding crises and their multiple realities? Are contemporary crises shaped by past ones, and are there lessons to be learned? How can we seize discontinuities and continuities? Are science and technology the ultimate soft power in the context of crises?

Crises and Pandemics

The context of pandemics in which we still find ourselves in provides an illustration of the potential for Williams' concept of "slow-moving crisis" by forcing scholars to contextualize this specific crisis as a point in a long sequence of interlocked events, whose dimensions expand beyond the medical one. They include human-nonhuman relations, conditions for disease propagation and for public health control, confinement versus prevention strategies, individual freedom of choice and state control over individual actions, boundaries between democracies and autocratic regimes, and various gradations in the belief in the value of science and technology in society.

Similarly to how various other historians of science and historians of technology have used the opportunity provided by the pandemic to show how our disciplinary perspective can enrich present debates,[7] we examined it within urban history of science and technology in Lisbon. Various chapters of a recently-edited volume soon to appear in the series *Cultural Dynamics of Science* by Brill (Miralles Buil 2022, Nunes 2022) deal with public health issues in Lisbon at the turn of the nineteenth century and also study the protection of the port city and imperial metropolis from the ravaging impact of epidemics arriving mostly by sea. These chapters were the springboard for our revisitation of the theme

[7] Examples are papers and issues in journals such as Centaurus, Science and Education, or Cadernos Ibero-americanos de Direito Sanitário.

under the present context (Diogo and Simões 2021a). Next, we offer some conclusions from this analysis.

Epidemics are part of the web of interactions which mark the urban space, introducing disruptive elements in a fragile balance of forces, either because they suddenly alter the status quo, or because they force changes in urban spaces and in the behavior of those who inhabit them, from experts to common citizens. From the medieval Black Death to COVID-19 in 2020 and 2021, this pattern persists, justifying the renewed vision of urban history as encompassing urban history of science, technology, and medicine.

Looking backwards, the European rate of urbanization accelerated in the nineteenth and twentieth centuries, including both the expansion of large central European cities like Paris, Berlin, and London; and smaller peripheral cities like Lisbon and Porto. The new nineteenth-century city is mostly a Haussmannian city: modern and cosmopolitan, conceived to serve an emerging middle class. Hidden infrastructures (sewage, lighting, water supply), visible infrastructures (gardens, new avenues, sidewalks, transport, leisure, and consumption areas) as well as public health and hygiene regulations were at the heart of the new urbanity built by different classes of experts. Scientists, engineers, and physicians formed a new technocracy, which assumed a fundamental role in deciding urban matters and in informing political decisions.

However, the glare of the Haussmanian city has made invisible, to the eyes of contemporary modernity, another city within the new city. It was the city of the poor, that is, of the working class that migrated to the cities and lived in precarious housing and hygienic conditions and neighborhoods. This city becomes visible during epidemics because diseases find in it the opportunity to blossom and expand. This happened in Lisbon during the epidemics of the late nineteenth century, and continues to this day.

It was from the perspective of science, technology, and medicine that the issue of working-class housing and neighborhoods was exhaustively debated in Portugal and abroad during the late nineteenth century, not only as a privileged space for the application of techno-scientific knowledge and public hygiene and health policies, but also as a space for the exercise of power, in the sense of Foucault's concept of *dispositif*.

It becomes clear that our case studies stemming from Lisbon at the turn of the nineteenth century, including medical debates on the origin of diseases, discussions on the virtues of containment versus prevention mechanisms, on the impact of the various dimensions of epidemics (medical, social, economic, and political), on the diversity of specific local responses, and the call for their articulation with global actions provide reflections that are useful to assess the COVID-19 global pandemic.

Historical research on how societies have previously dealt with epidemics and pandemics enables to better navigate the current moments of uncertainty and bewilderment—and articulate future actions effective in reversing the conditions that stopped the world in the twenty-first century. It is, of course, impossible to predict dates and contours of the occurrence of future epidemics. Nonetheless, it is possible to create the necessary local, national, and global conditions—at the environmental, social, institutional and political levels—so that we cease to be the agents responsible for the propagation of epidemics. More than react, we need to anticipate. Additionally, foreseeing means drastically re-evaluating our relationship with the planet—with the world around us. If we fail to do this, that is, if we address this pandemic as a singular event and miss to see it as part of a succession of "slow-moving crises" that compose what Williams called a "rolling apocalypse," in the coming pandemic we risk fulfilling the famous prediction of Karl Marx in the opening of *The 18th Brumaire of Louis Bonaparte*: "History repeats itself, the first time as a tragedy, the second as a farce."

Anthropocene

Our second topic—how to address the concept of the Anthropocene and its relevance as a historical category within the problematics of history of science and technology—is closely related to the concept of crises.

Science and technology are at the core of the Anthropocene. In our opinion, historians of science and technology should engage in the debate surrounding the Anthropocene by (i) discussing if the concept is useful as a historical category; (ii) using their own analytical tools to dissect some of the assumptions behind the concept; and (iii) blending concepts used in our field of expertise with the rationale behind the Anthropocene.

One of the aspects that troubled us the most during our first immersion in the Anthropocence debates was the realization that most accounts of the Anthropocene were and still are de-historicized, that is, human agency (translated into economic and political systems), dissolves into a generic narrative, which oscillates between Jean-

Jacques Rousseau's mystification of nature and George Orwell's dystopia of omnipresent technological surveillance. Furthermore, as far as science and technology are concerned, the idea of a quasi-autonomous technosphere is particularly close to technological determinism—and is oblivious of the human nature of science and technology. Finally, another disturbing implication of the ahistoricity of the usual concept of Anthropocene is the use of "we" and "us" as if human society was a homogeneous, flat, and free-floating reality.

A growing number of authors consider that the discourse on the Anthropocene inherently emphasizes the urgency of global solutions for a global problem caused by humanity as a whole, intentionally levelling socio-economic differences and concealing political conflicts (Malm and Hornborg 2014; Moore 2015). These authors propose alternative concepts to describe the "age of humankind," particularly by stressing the role played by different forms of capitalism (including the so-called state capitalism) in the unbridled exploitation of natural resources, thus bringing to the forefront the divide between those who explore and those who are explored. Colonial and postcolonial studies also respond to this criticism by discussing how European colonial science, technology, and medicine anchored a new global epistemology and ideas of progress and growth that profoundly changed the very concept of ecology, both in colonial and postcolonial periods.

In this new context, there are numerous possibilities for historians of science and technology to participate and help reinforce the human dimension in the present narratives on the Anthropocene by following the changes in natural and human landscapes using science and technology as a heterogeneous ensemble of mechanisms that enforce and reinforce power over nature, framed by economic and political objectives.

In our view, the primary issue concerning time and the Anthropocene is not the building of a chronology of events or the discussion of its starting point, but the addition of temporality—of *a longue durée* perspective—to the discussion in order to seize the intertwined movements of different historical scales. As historians of science and technology (but the same applies to philosophers and sociologists of science and technology), it is our responsibility to encourage a historicized account of the Anthropocene.

Furthermore, the Anthropocene framework enables us, historians of science and technology, to test many of our concepts and theoretical apparatuses in order to assess their plasticity and robustness. Examples of such concepts are biopolitics, biopower, and biofacts; technologies of the body, of the land, and of the state; *milieu intérieur*; technopolitics and technoeconomics; technological determinism (and its criticism); as well as actor-network theory and practice theory. In a nutshell, the history of science and technology can contribute by enriching the ongoing debate on the Anthropocene as well as the extent to which this concept is useful for historical analysis.

Anthropocene and Lumpennature

In what follows, we give a specific example which we have been developing. It is an example that makes clear how input from the history of science and technology may help dissect the nature-culture/technology dichotomy and help reassess the nature of nature, which is central to the Anthropocene debate. By following the changes in natural and human landscapes—and by using science and technology as mechanisms determined by economic and political objectives—we argue that they not only enforce and reinforce power over nature, but that they are also responsible for the demise of the utopian separation between nature and culture/technology. As such, they are the reason behind our proposal to introduce the concept of *lumpennature* (Diogo, Louro, and Scarso 2017; Diogo et al. 2019a; Diogo, Câmara Leme, Scarso 2022). We expand upon the analogy of the Marxist concept of *lumpenproletariat* in order to highlight the demise of nature as an independent entity and to stress its constitutive technological dimension. Therefore, we move beyond the idea of "technologization of nature" to assert the creation of a "technological nature/technonature," that is, a *lumpennature*.

Historians of science and technology have recently introduced concepts such as "naturalization of technology" and "technologization of nature." They do not imply, though, a perception of "nature" and "technology" as balanced sides of a pair. In fact, in both instances, technology is the driving force, either by imposing itself as a "second" nature and reclaiming its status as a "form of life" (Winner 1986)[8] or by domesticating

[8] Winner uses Wittgenstein's term of *Lebensform* (Philosophical Investigations and On Certainty) to highlight how technology reflects a particular form of life.

and controlling nature by transforming landscapes and ecosystems to enhance food and energy supplies, both at local and global scales.

On the other hand, philosophers and historians emphasized all throughout the twentieth century that past historical accounts did not make room for nature. That is, there was an absence of nature *per se in* history and *as* history—in the sense that, in mainstream narratives, nature is not taken as an actor. By reacting to this persistent situation, they have brought attention to the visible and invisible entanglements of nature, technology, and humans. Let us be reminded of the *École des Annales* (Bloch, Febvre, and Braudel), Kranzberg and Pursell, Benjamin, Schatzki, the 1960s debate on the culture-nature divide, and more recent scholarship on the topic (Benjamin 1986; Bloch 1931; Braudel 1949; Febvre 1935; Kranzberg and Pursell 1967; Schatzki 2003; Williams 2010).

Having this context in mind, we propose going a step further by introducing the notion of a "technological nature." In this new concept, we include artificial and digital representations of the wild that fulfill our inborn affiliation with the environment without engaging with nature itself.[9] This entails programs related to wildlife while simultaneously destroying the existing natural habitats; creating more perfect, artificial waves to substitute natural sounds, but also decreasing global warming by putting deflectors in the Earth's orbit without addressing the causes for the warming of the planet. In this sense, this new "technological nature" is a *lumpennature*, deprived of its primal values and inherently framed by technology. We are aware that this "technological nature" contaminates the essence of what has been perceived in the past as "true nature," eventually leading to its dissolution as a category (McKibben 1989, 58). We are reminded of McKibben's statement: "We have deprived nature of its independence, and this is fatal to its meaning. Nature's independence is its meaning—without it there is nothing but us."

Conclusion

By combining concepts such as crises and Anthropocene with the theoretical frameworks of both STEP and ToE, historians of science and technology can historicize and endow them with new dimensions or with dimensions which have remained in the dark up until

[9] Psychologists are particularly attentive to this issue. See https://depts.washington.edu/hints/.

now. The relevance of local contexts, transnational expertise and objects, colonialism, and imperialism are just a few examples of topics to be reappraised.

Stemming from a new perspective centered around Europe, but also deeply critical of Eurocentrism, the consideration of the dynamic evolution of extended global networks enables to relocate the building of local asymmetries to new grounds. Following the reflections of Clapperton Mavhunga's on one of the Anthropocene Campuses, we extend the question of "Whose Anthropocene?" to "Whose crises?"[10] The recognition of the dire impact of asymmetries on present-day problems calls for solutions involving ample participation of diverse constituencies, displaying the importance of history of science and technology as a particular area within the humanities.

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[10] Clapperton Mavhunga's "Whose? Reading the Anthropocene and the Technosphere from Africa" in the *Anthropocene Curriculum* https://www.anthropocene-curriculum.org/contribution/whose

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Earth System Science in Political Context

Ursula Klein[1]

Introduction

Earth system science (ESS) provides the most important scientific background in current debates about the Anthropocene. As you may know, Earth system scientists aim to study the Earth as a single, integrated system. They wish "to build a unified understanding of the Earth" (Steffen et al. 2020). Early on, these scientists have additionally formulated a second practical-political agenda. To complement their research on the functioning and evolution of the Earth system, they also assess the risks of human-induced global change and propose solutions to maintain the quality of human life (National Research Council 1986a).

Humans have been thinking about the Earth as an integrated whole for millennia: in religion, ancient philosophy, early modern cosmology, eighteenth-century geography, nineteenth-century geology, and so on. In a recent paper by leading Earth system scientists on the history of their science, the authors even claim that Earth system science shares some basic beliefs with old folk wisdom. "For tens of thousands of years," they state, "indigenous cultures around the world have recognized cycles and systems in the environment, and that humans are an integral part of these" (Steffen et al. 2020). So, what is new in recent Earth system science? These same practitioners of ESS quoted above provide the following answer: "… it was only in the early 20th century that contemporary systems thinking was applied to the Earth, initiating the emergence of Earth system Science."

The concept of "systems thinking" and its meaning in the framework of Earth sciences will be explored in this contribution. It should be noted, however, that systems thinking did not originate in this specific context. On the contrary, it was already a fashionable approach in the post-World War II sciences, which was then "applied" (from outside so to speak) to the sciences of the Earth.

[1] Max Planck Institute for the History of Science, Berlin

In the 1980s, ESS was established as a new approach in the Earth sciences through a number of initiatives on national levels, mainly in the U.S., and through the founding of new international scientific organizations such as the World Climate Research Programme (WCRP) in 1980 and the International Geosphere-Biosphere Programme (IGBP) in 1986. In the following four decades, ESS was subject to changes on many levels: with respect to its institutional organization, political agenda, the degree of cooperation between scientists and scholars from the social sciences and humanities, and its approaches and methods as well as concepts and understanding.

I will concentrate here on the latter issue: conceptual changes and their political implications. Insight into the changes and development of the generalized understanding by scientists of the Earth system over time can be achieved by analyzing certain key publications that assess the overall state of knowledge with a view of its practical consequences and options for political actions.

Earth system scientists distinguish three methods of knowledge production (Steffen et al. 2020, 58–59):

1. Observation and data gathering
2. Modeling the Earth system
3. Syntheses and assessments.

While the first two methods are highly technical endeavors, "syntheses and assessments" are presented in relatively non-technical texts. They are written in natural language (mostly English) and typically illustrated by "conceptual models," that is, block diagrams and pictures. In stark contrast to Earth system scientists' empirical research and modeling practices, which are a black-box for most Anthropocene activists and policy-makers, the publications involving syntheses and assessments are accessible to a broader audience.

Syntheses and assessments are thus an important means of communication. In addition, I argue that they have another important function: it is mainly through writing and discussing such kinds of texts that Earth system scientists articulate and reflect on their overall understanding of the Earth system. The importance of this second function of syntheses and assessments is often underestimated, both by practitioners of ESS and by historians and philosophers of science. Like all empirical methods, observation and data gathering provide knowledge only of isolated parts of the Earth. By contrast, modelling and simulation provide insight into the system behavior of the Earth, yet they are not entirely comprehensive either. Furthermore, they are formulated in mathematical language. Model building and simulation of the Earth system by no means yield self-evident natural knowledge, much less generalized synthetical knowledge or assessments about their practical relevance. Comparable to mathematically formulated physical theory, Earth system models require interpretation, which involves natural language (and sometimes pictures or diagrams). In other words, there is a huge epistemological gap between scientists' practice of modeling on the one hand, and syntheses and assessments on the other. Bridging the gap would require systematic reflection on alternative interpretations. At present, however, the main format of this type of reflection is the collective preparation of publications that address a broader scientific audience and policy-makers. Far from being embedded in "fundamental research" on a theoretical level, Earth system scientists' third method—syntheses and assessments—actually meshes theoretical objectives with "applied research." What is termed "syntheses and assessments" always appears in mixed politico-scientific texts.

Views of the Earth System in the 1980s

I come to the main part of my talk beginning with the predominant understanding of the Earth system in the 1980s. A major step toward the institutional organization of ESS in the early 1980s was the preparatory work carried out in the U.S. by the Earth System Sciences Committee founded by NASA.[2] In a report from 1986, which is now famous for the "Bretherton diagram" of the Earth system, the committee members described ESS as an endeavor that simultaneously performs basic and applied research. With respect to the practical consequences of ESS, optimistic overtones clearly prevail in this report:

> The pursuit of an *improved quality* of life upon the Earth goes hand in hand with the search for greater scientific understanding of the Earth itself. The application of basic science to human needs *is today proceeding more vigorously than ever* before. (National Research Council 1986a, 9, emphasis added)

Of course, the Earth system scientists were well aware that there is a significant difference between a technical system and the Earth system: the Earth system undergoes changes in history. Indeed, they pointed out that the understanding of the Earth system's historical change is a major objective of ESS. The language they chose in this respect is

[2] For the historical context of ESS, see also (Conway 2008) and (Rispoli and Olsáková 2020).

interesting. The term “evolution” they use to describe the Earth’s changes in history means that the Earth system changes slowly and gradually, at least during long extended periods of history, rather than abruptly. There is also a tendency to emphasize the system’s stability or the limits of its variability. This understanding is expressed, for example, in the following statement:

> The reality of global change stimulates us to understand its causes and to determine the limits of variability that arise through interactions among the components of the Earth system. (National Research Council 1986a, 20emphasis added)

We will see that this understanding of the Earth system, which corresponded to some extent to the contemporary view of stability, management, and control of technological systems, underwent dramatic changes in the subsequent decades. But let us now look at another form of systems thinking in the 1980s, namely systems ecology, thus complementing our brief overview of the historical context of the introduction of systems thinking in Earth sciences.

The view that the Earth system is relatively stable and undergoes slow, gradual changes is clearly articulated in another U.S. program of the time, which studied the Earth from the perspective of systems ecology. In the late 1970s, The National Research Council founded a Committee on Planetary Biology, sponsored by NASA, which developed a program for a “global ecology” or a “science of the biosphere.” In the committee’s report from 1986, stability of natural ecosystems is highlighted:

> The continuing stability of certain ecological systems … is critical for the survival of human beings. Since we have no adequate understanding of what leads to stability of natural systems, research is needed to identify and then collect the relevant data from which a theory of the stability of ecosystems can be derived. … The persistence through time of ecosystem function implies some kind of stability, but the factors determining stability are not known. (National Research Council 1986b, 28)

The committee members also assumed that there must be certain “regulatory mechanisms” that guarantee stability and persistence, which were essential for ecosystem (National Research Council 1986b, 28–29).

As systems ecology refers to local or regional ecosystems, the interesting question is of course whether this view of local ecosystems could be extended to the global scale of the entire “biosphere” or the hypothetical “global ecosystem.” It is only in passing that our systems ecologists state:

> Since, over geological time, the constraints change and the [global] ecological system moves to new states, the biosphere seems to be less a stable … system. In contrast to this perspective *forced on us* by a global point of view, ecological theory has assumed that the biotic systems at local scales are stable entities that resist or react to changes in a relatively constant environment. Thus, attention has focused on controls that permitted the system to return to its initial state following perturbation. (National Research Council 1986b, 53, emphasis added)

Based on their empirical knowledge about the history of the Earth, the systems ecologists conceded that their assumption of a stable global ecological system was problematic. However, their empirical knowledge had not yet propelled them to seriously question the received *theoretical* understanding of global natural systems as stable entities that undergo gradual, linear change. Even though, a program for biosphere science or global ecology was not implemented in the newly founded international research platforms of the late 1980s (such as the International Geosphere-Biosphere Programme), it brought to the fore a widespread form of systems thinking that did not go unnoticed by Earth scientists with physics and chemistry backgrounds.

Contemporary Earth System Science

The term "system" was long associated with the notion of stability and controllability, which implied that, as a rule, the historical change of a system proceeds gradually and slowly. Now, some thirty years later, the vast majority of Earth system scientists no longer accept this view. In their syntheses and assessments, contemporary Earth system scientists are considerably less optimistic with respect to both their epistemological and political agenda. They now also highlight the potential instability of the Earth system, its tendency to change abruptly, and its vulnerability in view of the global dimension of human-induced changes. I now come to this later period.

In a recent paper, Jürgen Renn elegantly summarizes this new understanding of the Earth system observed in contemporary ESS:

> The Anthropocene as a concept is also the result of a new of kind of Earth science: a transition from geology to Earth System science whereby our planet can be understood as a *nonlinear complex system* with many feedback loops. According to this new understanding, the Earth System is not only subject to uniform change processes but can also achieve *tipping points* that lead to such *catastrophic changes* as Snowball Earth events, which have happened several times in the past. This is why some also speak of a "new *catastrophism*." (Renn 2020, 381, emphasis added)

Let me give a few examples that illustrate this new understanding of the Earth system as an utterly vulnerable system that can be pushed over certain thresholds beyond which irreversible, disruptive changes may occur. In recent syntheses and assessments of Earth system scientists, the notions of "tipping points" and "tipping elements" play a central role. The term tipping point is used broadly, not only in scientific contexts. It usually means a critical threshold at which a small perturbation can qualitatively alter the state of an object or of a system. In ESS, tipping points were first recognized in certain regions of the Earth, such as the Amazon rainforest and the Greenland ice sheets. These regions are called "tipping elements." They are particularly vulnerable parts of the entire Earth system because of their internal dynamics of change. "We offer a formal definition," Timothy Lenton and colleagues point out, "introducing the term 'tipping element' to describe subsystems of the Earth system that are at least subcontinental in scale and can be switched—under certain circumstances— into a qualitatively different state by small perturbations. The tipping point is the corresponding critical point—in forcing and a feature of the system—at which the future state of the system is qualitatively altered" (Lenton et al. 2007, 1786). "Tipping elements," writes another group of authors, are "important features of the Earth System that are not characterized by linear relationships but can instead show strongly nonlinear, sometimes irreversible, threshold-abrupt change behavior" (Steffen et al. 2020, 59). In a similar vein, a third group of authors observes that there is growing evidence that "seemingly stable conditions are followed by periods of abrupt, non-linear change, reflected in critical transitions from one stability domain to another when thresholds are crossed." A corresponding notion belonging to the practical-political agenda of ESS is "planetary boundaries" defined as the limits of change within which human societies can operate safely (Rockström et al. 2009).

While tipping elements are still confined to the regional scale of the Earth, the newer concept of "tipping cascades" refers to the planetary scale and to changes of the whole Earth system. I quote again a group of leading Earth system scientists:

> More recent research has focused on the causal coupling between tipping elements … and their potential to form cascades. Tipping cascades could provide the dynamical process that drives the transition of the Earth system from one state to another, effectively becoming a planetary-level threshold. (Steffen et al. 2020, 59–60)

The authors add the following, again with a view to the societal consequences of human-induced global change:

> Research on tipping elements and cascades highlights the ultimate risk … of destabilization of the Earth System as a whole. (Steffen et al. 2020, 60)

If the Earth system as a whole is destabilized, it does not collapse the way a house does during an earthquake. Rather, it shifts abruptly to a qualitatively different state. What kind of state could that be? To quote from another source:

> Our analysis suggests that the Earth System may be approaching a *planetary threshold* that could lock in a continuing rapid pathway toward much hotter conditions—Hothouse Earth. This pathway would be propelled by strong, intrinsic, biogeophysical feedbacks difficult to influence by human actions, a pathway that could not be reversed, steered, or substantially slowed. (Steffen et al. 2018, 8257, emphasis added)

A "Hothouse Earth" would not be just a few degrees hotter than today; it would be much hotter. The authors do not say exactly how hot, but they point out clearly that we have to expect "much hotter conditions."[3] Further, they state that "hothouse Earth" means not merely that our economy and social life would be disrupted, but rather that "the habitability of the planet for humans" is at risk.[4] That is, our physical survival is threatened.

Let me now briefly address the question of why the Earth system may become unstable. The main answer given by scientists to this question is: there are both positive (amplifying/destabilizing) and negative (corrective/stabilizing) feedback mechanisms in the Earth system. As you may know, the term feedback mechanism comes from cybernetics. However, cybernetics highlights negative feedback mechanisms that correct perturbations and thus regulate the stable functioning of a machine or another system. By contrast, Earth system scientists now agree that the global Earth system is not fully regulated by negative feedback mechanisms. It does not function like a cybernetic machine, as James Lovelock's cybernetic Earth system theory, the famous Gaia theory, would have it.[5] Further, the Earth's feedback mechanisms do not exist in a permanent state. If a part of a feedback mechanism undergoes change, the entire mechanism may be weakened or even disrupted. In other words, Earth system scientists believe that feedback mechanisms have a history, which implies that they do not have the universality of natural laws.

[3] See also (Steffen et al. 2018, 8257).
[4] See also (Steffen et al. 2018, 8256).
[5] See (Lovelock 1979).

Conclusion

Clearly, the current understanding of the Earth system by Earth system scientists is significantly less optimistic than it was in the 1980s. Their new understanding of the functioning of the Earth system and the risks of human-induced global change may be called catastrophism, presupposing the term catastrophe does not refer to external agents of disruption.[6] Contemporary ESS tells us that the natural Earth system has the *inherent* potential to become unstable and shift abruptly to another state.

This view implies that the notion of evolution is no longer considered to be appropriate for describing historical changes of the Earth system. The immense number of deep climate changes and biological mass extinctions in the long history of the Earth question the previous belief in the Earth's slow, gradual change. In historiography, abrupt and nonlinear change that leads to a qualitatively new state of a political system is also called revolutionary change. In fact, some Earth system scientists have substituted the term revolution for the older term evolution of the Earth. A recent book publication is thus entitled *Revolutions That Made the Earth.*[7]

As a consequence, in recent ESS the issue of stability and resilience of the Earth system is at the forefront of research. "ESS now faces two critical research challenges," a group of Earth system scientists observes. "First, how stable and resilient is the Earth System? … Secondly, how can we better understand the dynamics of human societies?" (Steffen et al. 2020, 61). Given the fact that the new understanding of the Earth system as a potentially unstable system is linked to the physical concept of "unstable systems" –and also given the fact that Jürgen Renn started his career as a historian of physics—I conclude with a quotation from Ilya Prigogine:

> Classical science emphasized order and stability; now, in contrast, we see fluctuations, instability, multiple choices, and limited predictability at all levels of observation. … In the classical view—and here we include quantum mechanics and relativity—laws of nature express certitudes. When appropriate initial conditions are given, we can predict with certainty the future, or "retrodict" the past. Once instability is included, this is no longer the case, and the meaning of the laws of nature changes radically, for they now express possibilities or probabilities. … We are

[6] Geologists' traditional notion of catastrophes in the history of the Earth meant events caused by external agents such as collisions with asteroids or even supernatural agents that caused the Great Deluge.
[7] See (Lenton and Watson 2011).

observing the birth of a science that is no longer limited to idealized and simplified situations but reflects the complexity of the real world. (Prigogine 1997, 4–7)

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Evening Lecture: “Caminante, no hay camino, se hace camino al andar…”

Luca Lombardi

The last time I met Jürgen Renn (during a nice walk in Berlin’s Tiergarten), I asked him what gave him that charge of optimism that is one of his finest qualities. I inquired because I myself tend more and more toward pessimism, against which I nevertheless try to fight.

Why? Because after a lifetime in which I had thought and hoped that we had left the scariest part of human history behind, I had to change my mind and resign myself to the fact that some basic characteristics of humankind, at least in these last hundreds of thousands of years, have not changed. In fact, we are still in what I call “The Age of War,” the era that includes all the periods within the Holocene and also the period in which we currently find ourselves, the Anthropocene: actually, they are all sub-periods of this age. Will we ever succeed in getting out of it? Violence, oppression, war: are they unavoidable characteristics of humankind? Or will there perhaps be—in hundreds or thousands or hundreds of thousands of years—an age in which our current era will be considered as the true prehistory of humanity? We must admit that we cannot be sure that, in this hypothetical and distant era, human beings will still exist.

At the moment, the narrative that humanity has fashioned of itself and of the God it has invented in its image and likeness does not bode well. It explains and justifies, in a fabulous way, the nature and destiny of humanity. As a matter of fact, everything depends on Adam and Eve and their—human, all too human—disobedience. One thing leads to another: from Adam and Eve, Cain and Abel were born. Among the descendants of Cain there is Iubal, considered the inventor of music. This casts a shadow on an art that is considered the symbol of beauty and purity. It is. And it isn’t. In the same way that man is wonderful and brilliant, he is also stupid, terrible, and highly dangerous. Sometimes in the same person. Take the case of Richard Wagner, an intense and outspoken anti-Semite, whom Thomas Mann called “ein Ungeheuer von Genie” (a monster of genius), the author—besides some masterpieces of all time—of the infamous pamphlet “Das

Judentum in der Musik" (Judaism in Music). When, in Vienna in 1881, during a performance of Lessing's *Nathan der Weise*—a play on religious tolerance among Jews, Christians, and Muslims, the theater burned down, Wagner said: "All Jews should be burned during a performance of *Nathan*."[1]

Coming back to Cain and his descendants, I also think of one half-brother of Iubal, Tubal-Cain, who is credited with the invention of weapons. The Bible commentator Rashi (acronym of Rabbi Shlomo Yitzhaqi, 1040–1105) interpreted his name,Tubal-Cain, as the one who would "aromatize [he uses this curious term, sometimes also translated as 'spicy'] and redefine the profession of Cain by making weapons for assassins."

In short, due to the unbecoming behavior of Adam and Eve, humanity was not born under the best auspices. Its path also seems to be predestined over the centuries and millennia, up to Russia's war of aggression against Ukraine, which has now been going on for almost five months and threatens to ignite a new world war.[2]

This is a reason for pessimism. Of course, we must consider that everything is in constant motion. Humans themselves, having arrived on Earth with things substantially done, have not stopped for a moment since then—although often giving the impression of spinning in circles—and there is no doubt that in the course of these past 250,000 (or even 500,000 years) they have made tremendous progress. Especially in the last thousands of years and in particular in the last hundred years. We live today in a world that, despite its eyesores, is absolutely magical. People nowadays do things that in the past not even a god or goddess would have dreamt of being able to do.

This is undoubtedly a reason for optimism. Although that optimism must remain moderate, because in many parts of the globe—and also next to and among us—we continue to hate, to fight, to annihilate each other. After all, we are still in the Age of War. But what is this instinct of destruction and self-destruction that turns us against our fellow men, against other animals, and against our own home, this wonderful planet that in a short time humanity has depleted and half-destroyed?!

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[1] I dedicated to the specific theme of anti-Semitism my composition *Warum?* for a string quartet (2006). A few years later I also made a version of it in which information on the two-thousand-year history of anti-Semitism, from Paul of Tarsus to the present day, is interpolated between the movements.

[2] I read this text on July 13, 2022.

A few days ago, I read an interview with Jürgen Renn published in *Die Zeit*[3] that announced the creation of a new institute: the "Max Planck Institute of Geoanthropologie," which will be based in the city of Jena and will study the interrelationships between the geosphere and man-made systems.[4] This is great news, and I am sure that important impulses for the protection of our planet will come from this institute.

What, if any, could the role of art and music be in helping make the Earth an increasingly habitable place, not just for those who already lack nothing, but genuinely for everyone?

Alas, music that once seemed to have been most powerful (see Jericho and the seven trumpets—actually seven shofars[5]) has meanwhile weakened considerably.[6] And in any case, it cannot intervene directly and immediately. Music's message may not reach its intended audiences for a long time: it's like a message in a bottle, which, if read by someone, must be put back in the bottle and entrusted again to the waves of history. But its effectiveness is uncertain and often disappointing. Schiller's wish, amplified by Beethoven in his *Ninth Symphony*, "Alle Menschen werden Brüder" (all men become brothers), is still, despite being ritually repeated *ad nauseam*, a utopian program. And then, again: brothers, yes, but possibly not like Cain and Abel. And then, again: The *Ninth Symphony* was one of Hitler's favorite pieces. What does this tell us?

Still, these testimonies of great men in history are what prevents us from giving up hope. I am thinking—in addition to Beethoven's *Ninth Symphony* or his opera *Fidelio*, for example—of Schönberg's *A Survivor of Warsaw*; of the *Babi Yar Symphony*, to texts by Yevtushenko, by Shostakovich; of Britten's *War Requiem*; of the opera *Il Prigioniero* by Dallapiccola; of *Il canto sospeso* by Nono; of *Ich wandte mich und sah alles Unrecht, das*

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[3] Interview available at https://www.zeit.de/2022/27/juergen-renn-max-planck-institut-mensch-erde.
[4] Announcement of the creation of the Institute at https://www.mpiwg-berlin.mpg.de/news/max-planck-institute-geoanthropology-be-directed-jurgen-renn.
[5] A ritual musical instrument made from the horn of a ram or other animal, used on important Jewish public and religious occasions.
[6] See also my *About the power of music and the powerlessness of composers*.
German and English versions: Hanse Institute for Advanced Study. 1999. *Annual Report*. p. 56–66, 67–73.
Spanish version: 1999. De la potencia de la música y la impotencia de los compositores. In *Cuadernos de Veruela. Anuario de creación musical* (3): 35–51.
German version: 2000. Von der Macht der Musik und der Ohnmacht der Komponisten. In *Merkur – Deutsche Zeitschrift für europäisches Denken*, 54 (2): 312–323.
The issue is also addressed in Lombardi, Luca. 2006. The Artist and the Power: Remarks on my Operas *Faust* and *Dmitri*. In *Construction of Freedom and Other Writings*, ed. Jürgen Thym, 269–284. Baden-Baden: Valentin Koerner.

geschah unter der Sonne (I turned and saw all the wrongs that took place under the sun), Bernd Alois Zimmermann's last composition before he committed suicide.

This does not mean that every composition must have a civil or political message. Life is made up of many different aspects, and it is right that music reflects them all. There is music and music. In any case, the music of the great composers of the past is, beyond any extra-musical program, an implicit anticipation of a different world, which we don't know will ever exist—a sliver of utopia. But there are moments in which an artist cannot close his eyes to reality, but must take a stand because "music is not an ark on which one can survive the flood" (Brecht). Nor can we fatalistically wait for the catastrophe, but—albeit with the weak but vital means of music—make our contribution to avoid it.

Finally, a brief description of the musical pieces that the excellent pianist, Alessandra Ammara, will realize. I have chosen three short piano compositions of mine.

- The first piece is *Albumblätter*, which I wrote at the turn of the year 1967. I was 22 years old, and it was the first composition in which I confronted myself with the avant-garde of the time, even before going to Cologne—at that time the mecca of New Music—to study with Karlheinz Stockhausen and Bernd Alois Zimmermann, two composers who differ very much from each other.
- The second one I wrote back in 2002 for the 50th birthday of Wolfgang Rihm, one of the most important German composers of today. This piece is part of a cycle entitled *8 Saluti* (8 Greetings).
- The third piece is from 2014 and is called *Mendelssohn im jüdischen Museum Berlin* (Mendelssohn at the Jewish Museum of Berlin), where it was first performed by Roberto Prosseda (husband of Alessandra Ammara and an excellent pianist himself). It is one of my many compositions dedicated to Jewish themes and to the memory of the tragedies that the Jewish people have suffered due to human wickedness and stupidity (qualities that are, alas, stubbornly persistent).

Judaism and the fight against anti-Semitism form an important strand of my work, to which belongs also my most recent composition, *Novembernacht*, in memory of the 1938

Pogromnacht in Germany. I wrote it for András Schiff, one of the great pianists of our time.[7]

Another strong interest of mine is that of our planet, nature, and all other animals. I consider the latter being similar to humans, though of course also different from us, and also therefore I have been a vegetarian for many years now.

I have great respect for life in all its forms. I could say that, even not being a believer, this is my personal religion. To this strand of my interests belong compositions such as *Infra* for ensemble (which I wrote on behalf of a scientific institution[8]) or *Terra* (earth) and *Mare* (sea), two orchestral compositions which I would like to complement in the near future with *Vento* (wind) and *Sole* (sun).

Alessandra Ammara has also brought with her two miniatures of mine, which were written in the last few weeks. The first is called *My Hope for Ukraine*.[9]

The same word, "hope," also recurs in the second piece, *Sorge und Hoffnung* (worry—I could also translate it as anguish, distress, or anxiety—and hope).[10]

After this reflection that I shared with all of you in this marvelous venue[11], I would like to return to the conversation with Jürgen Renn that I mentioned at the beginning. I have come to the conclusion that the alternative is not between optimism and pessimism. Rather, we must continue walking, inventing the road ourselves, as suggested by the title of this text, which I took from a poem by Antonio Machado ("Caminante, no hay camino, se hace camino al andar … " / wanderer, there is no way, the path is created by walking …).[12] We must proceed along the path that we create along the way, with realism, trying—*trotz alledem* (despite everything)—not to lose hope.

[7] The premiere is scheduled for June 27, 2023 in Bochum (Ruhr Piano Festival).

[8] For the inauguration of the Hanse Institute for Advanced Study in Delmenhorst, Lower Saxony in 1997.

[9] In this piece I paraphrase the Israeli anthem, *HaTikvah* (The Hope), expressing in this way also my hope for my second fatherland (or rather, motherland).

[10] I wrote it for the forthcoming 80th birthday of a dear friend, the musicologist Jürgen Thym.

[11] The speech and the concert took place in the Miramare Castle Park (Trieste), at sunset, in front of the sea.

[12] The poem by Machado is from 1907. The composer Luigi Nono tells that in the mid-1980s he visited a monastery near Toledo, and on a wall there he saw the inscription: "*Caminantes, no hay caminos. Hay que caminar*" (Walker, there is no path, yet you must walk), which is strongly reminiscent of Machado's verse. In the last three years of his life, Nono composed a trilogy of works whose titles all derive from that inscription: *Caminantes… Ayacucho* (1986–87, Ayacucho is a city in southern Peru that was the scene of a rebellion against the Spanish in the early 19th century); *No hay caminos, hay que caminar… Andrej Tarkowskij* (1987); and, his last composition, *"Hay que caminar" soñando* (1989). Strange enough, he did not mention Antonio Machado, whom he certainly was aware of. There is, however, a significant difference

Enunciating this word makes me think of when, as a boy, I accompanied on the piano the wife of a friend of our family—the sociologist Franco Ferrarotti—who interpreted an aria by Alessandro Scarlatti: *La speranza, mi tradisce, mi si mostra e poi svanisce …* (hope betrays me, shows itself and then disappears …).[13]

between the two quotes: Machado says that the road must be created as one proceeds, thus assigning the choice of the path and its destination to the wanderer himself. In the sentence quoted by Nono, instead, there is no choice, one must walk, wherever the path leads. I would say that this version, rather than being pessimistic, is desperate. But how and why to live if you are hopeless? It is probably no coincidence that Nono wrote these compositions, already ill, at the end of his life.

[13] In Trieste I took the liberty of singing this line.

Final Remarks

Bernd Scherer

I would like to thank Jürgen Renn for his wonderful collaboration on the Anthropocene project—a project we both worked on together during the last ten years, or, as we just learned, during the hottest decade in human history.

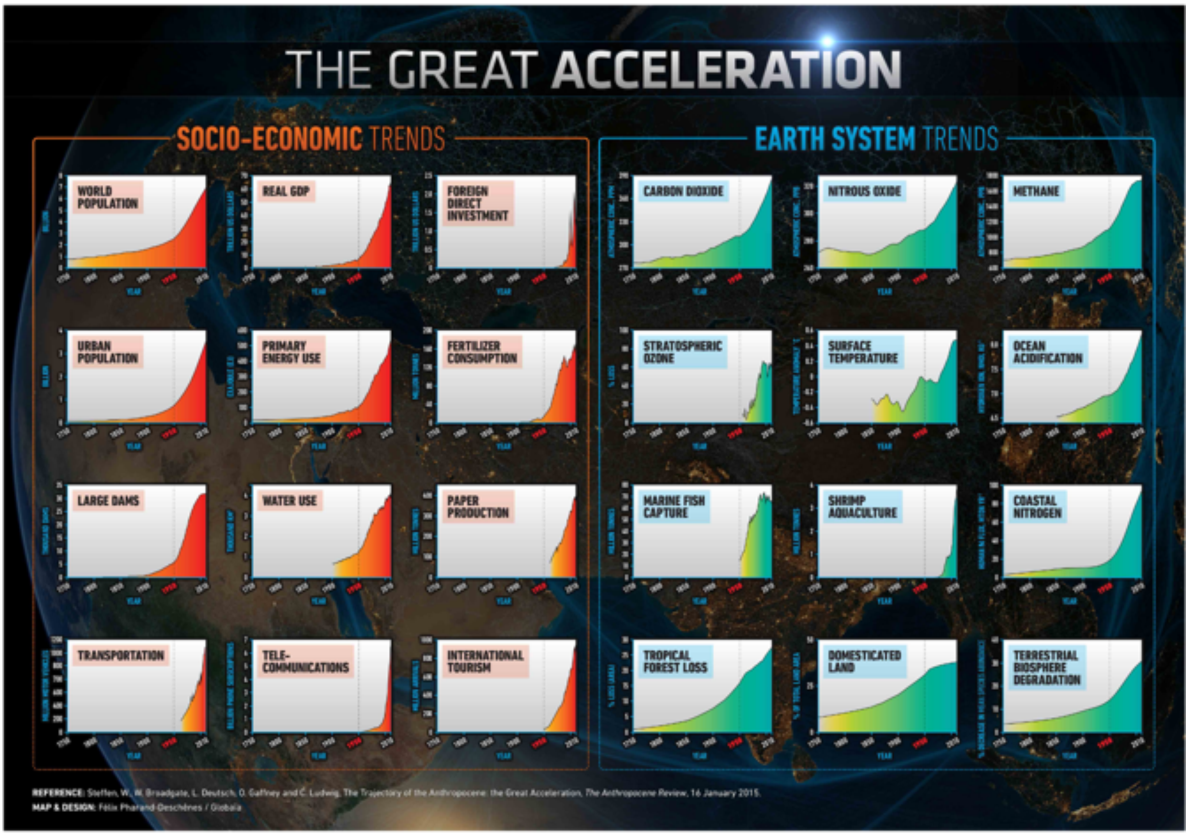
It was an extremely inspiring collaboration during which I got to know Jürgen as an open-minded and generous person who was ready to engage himself in what became a very experimental long-term project to develop new forms of knowledge production.

Jürgen already mentioned that everything that should be said has been said. So, please bear with me if I say the same thing once more, only a little bit differently.

In my closing remarks, I would like to connect some points which have been raised during the conference with what I consider major insights from the Anthropocene project. Here, I will draw upon the last speakers and pinpoint some implications of the concept of the Anthropocene—implications which the new Institute of Geoanthropology may consider. It is a reflection on the role of humans in the Anthropocene, including the scientists and the role of knowledge production.

Since we must take into account the human factor in the Anthropocene, I will be short after three days of complex presentations and discussions. I have 7 points:

1. **My starting point** is that we are confronted with an existential crisis, represented by these graphs which articulate what Earth System scientists call the **Great Acceleration** (see figure below). The graphs represent major parameters that define the Earth system. We can see exponential growth starting around the middle of the twentieth century. All these parameters are induced by human action. That hints to the fact that we humans are not only intervening in nature but are disbalancing the Earth system as a whole.



2. **Change of basic categories**. These developments deeply impact our categorical system, for example the nature/culture divide. Have a look at the image below: this used to be a garden to grow tomatoes. The garden has been transformed into a machine. Natural and cultural processes are now completely intertwined. What we used to regard as nature is now an outflow of human consciousness. The separation of the natural sciences on the one hand and the cultural and social sciences on the other does not do justice to these new phenomena anymore. However, it is not only the outer world but also the inner world that is changed by these anthropogenic developments.

Armin Linke. Greenhouse, El Ejido, Spain, 2013 (ReN_007774_ 19). Courtesy Armin Linke and Vistamare I Vistamarestudio, Pescara | Milano.

3. **The creation of new worlds**. This anthropogenic development is driven by a close cooperation between the sciences and technology in capitalist economies. Instead of helping us understand the world and gain insights about it, scientific knowledge is nowadays applied more and more to construct new worlds in ever shorter time spans. New technological generations follow each other in periods of just five to ten years. We are creating new worlds faster than we can understand them.

4. **New forms of knowledge production**. These new realities require new forms of knowledge production. Since the development of new disciplines would take too long to keep up with the pace of our own worldmaking, knowledge production in the arts or in the context of activism has begun to play an important role.

5. **The role of rationality**. The anthropogenic development which led to the crisis we are in now can be understood as the outcome of unintended side effects. By trying to solve the energy problem with nuclear energy, we created nuclear waste as an unintended side effect. By trying to end the hunger problem with synthetic fertilizers, we contaminated soils and waters. These unintended side effects are realities that are overlooked by the rational approach to problem solving. They are the dark side of what we regard as

rational development. For some time, these unintended side effects were hidden in the shadow of a linear development model, but now they are coming to the fore ever faster.

6. **The role of scientists in this existential crisis.** Scientists are not just observing the anthropogenic world. They are observers *and* actors, and they are affected by these processes. There is no longer an outside perspective that is completely detached from what is being observed.

The pandemic already demonstrated the possibility that global infrastructures, and thereby the world we constructed and used to live in, can implode in a very short time.

In my view, this is a wake-up call for scientists to take responsibility. However, I would differentiate here between the basic and the applied sciences.

Within the basic sciences, which I consider fundamental, the challenge is to imagine a radically different world. They should use their freedom to develop radically different conceptual frames. How the imagination can be modelled in an evolutionary Earth System is an interesting question. But I am convinced that the new MPI on Geoanthropology which Jürgen was able to found will pursue exactly this objective.

The applied sciences on the other hand should concentrate on practical solutions for our actual problems in a dynamic relationship with the basic sciences. In doing so, they should avoid contributing to an economic model that presupposes unlimited growth.

7. **My last point**. Before the pandemic, I spoke with the famous German filmmaker Alexander Kluge. In this talk, Kluge was imagining a possible collapse of the global system. This possibility reminded him of the year 1945, after the Second World War, the zero hour. The whole country was in ruins. Each day people had to face questions of survival. For Kluge, this situation made clear which are the fundamental cultural techniques, habits, and strategies necessary to survive on an individual and on a collective level. Whenever Kluge in his later life was confronted with major problems, he reminded himself of this zero hour. One (but not the only) challenge for our knowledge systems is to prepare ourselves for such a situation.

When we receive nowadays reports about more than 800 million people on the planet being undernourished, we should remind ourselves that these people are already experiencing the collapse of their worlds.

Max Planck Institute for the History of Science

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