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The Historical Origins of 'Open Science': An Essay on Patronage, Reputation and Common Agency Contracting in the Scientific Revolution

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The Historical Origins of 'Open Science': An Essay on Patronage, Reputation and Common Agency Contracting in the Scientific Revolution

Paul A. David

Abstract

This essay examines the economics of patronage in the production of knowledge and its influence upon the historical formation of key elements in the ethos and organizational structure of publicly funded `open science.' The emergence during the late sixteenth and early seventeenth centuries of the idea and practice of `open science' was a distinctive and vital organizational aspect of the Scientific Revolution. It represented a break from the previously dominant ethos of secrecy in the pursuit of Nature's Secrets, to a new set of norms, incentives, and organizational structures that reinforced scientific researchers' commitments to rapid disclosure of new knowledge. The rise of `cooperative rivalries' in the revelation of new knowledge, is seen as a functional response to heightened asymmetric information problems posed for the Renaissance system of court-patronage of the arts and sciences; pre-existing informational asymmetries had been exacerbated by the claims of mathematicians and the increasing practical reliance upon new mathematical techniques in a variety of `contexts of application.' Reputational competition among Europe's noble patrons motivated much of their efforts to attract to their courts the most prestigious natural philosophers, was no less crucial in the workings of that system than was the concern among their would-be clients to raise their peer-based reputational status. In late Renaissance Europe, the feudal legacy of fragmented political authority had resulted in relations between noble patrons and their savant-clients that resembled the situation modern economists describe as `common agency contracting in substitutes' -- competition among incompletely informed principals for the dedicated services of multiple agents. These conditions tended to result in contract terms (especially with regard to autonomy and financial support) that left agent client members of the nascent scientific communities better positioned to retain larger information rents on their specialized knowledge. This encouraged entry into their emerging disciplines, and enabled them collectively to develop a stronger degree of professional autonomy for their programs of inquiry within the increasingly specialized and formal scientific academies (such the Académie royale des Sciences and the Royal Society) that had attracted the patronage of rival absolutist States of Western Europe during the latter part of the seventeenth century. The institutionalization of `open science' that took place within those settings is shown to have continuities with the use by scientists of the earlier humanist academies, and with the logic of regal patronage, rather than being driven by the material requirements of new observational and experimental techniques.

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KEYWORDS: open science, new economics of science, evolution of institutions, patronage, asymmetric information, principal-agent problems, common agency contracting, social networks, invisible colleges, scientific academies

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Preface

This essay has evolved from another, entitled "Reputation and Agency in the Historical Emergence of the Institutions of 'Open Science'", the first draft of which was circulated for comment in May, 1991. Its development has been part of the work towards a book about the economic organization of research in science and technology in the West since 1550, provisionally entitled Patronage, Property and the Pursuit of Knowledge. That long-standing project drew early support from a grant from the Science and Society Program of the Mellon Foundation, and later from the Renaissance Trust (UK). Its initial conceptualization was shaped over two decades ago by my concurrent collaborative research with Partha Dasgupta on the economics of science. Weston Headley provided extraordinarily able research assistance during this project's early phase. My explorations of what for me was a new historical terrain were greatly encouraged and assisted by being able to begin reading extensively in the history and philosophy of science during 1990-91, when I held a Stanford Humanities Center Fellowship. Mario Biagioli, Noel Swerdlow, and the late Richard Westfall generously discussed their own archival researches relating to this subject; they also instructed me by supplying innumerable bibliographic references, only some small fraction of which have found their way into the present version.

The original draft from which the present work has developed was presented to the Conference on the "Economics of Conventions," organized by André Orléans and held under the auspices of the Centre de Recherche en Épistémologie Appliquée (École Polytechnique), Paris, March 27-28, 1991. Subsequent revisions benefited from discussions by participants in the Social Science History Workshop at Stanford University (Winter Quarter 1992), the Economic and Social History Seminars at Oxford, and at Cambridge (Spring, 1994), and a joint meeting of the Yale Economic History and Industrial Organization Seminars (Fall, 1994). Still further improvements were suggested by Joel Mokyr and other participants in the National Academy of Sciences Conference on Science, Technology and the Economy, organized by Ariel Pakes and Kenneth Sokoloff at the Beckman Center, Irvine, California, 20-22nd October 1995; presentation of the penultimate version at the 2004 Economic History Association Meetings (San Jose, CA) elicited still further well-intentioned suggestions for expanding the coverage of historical material and shortening the text. Most memorable, at this advanced stage in the career of this essay, is my sense of gratitude for the keen interest that colleagues took in various parts of my work, for the insightful comments and constructive critical advice offered so willingly by a diverse, wonderfully learned company of economists, economic historians and historians of science and medicine: Robert Auman, Robin Briggs,

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Avner Greif, Scott Mandelbrote, Deirdre McCloskey, Ian McLean, Joel Mokyr, Sheila Ogilvie, Trond Olsen, Paolo Rossi, Gerald Silverberg, and Noel Swerdlow. Sheila Johansson's keen editorial eye and enthusiasm for the historical subject as a whole, as well as for the contemporary policy sub-text of the argument, has been a source of continuing encouragement and effected many improvements in the many successive drafts. None of those whose help has been thankfully recorded here can be held to blame for whatever deficiencies may be found to have survived their corrective efforts.

A video recording of the author's July 3rd, 2008 lecture on "The Economic Logic and Historical Origins of Open Science'", which was delivered at CERN (Geneva, Switzerland) and based upon material presented and cited in the present article, has been made available for free on-line viewing and download from the CERN Archive: http://cdsweb.cern.ch/record/1113755.

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A "Presentist" Prologue

The programs of exploratory science and engineering research that today are critical for sustaining growth in the knowledge-driven economies also are particularly exposed to the unintended consequences of the movement that has aimed at strengthening and extending the system of intellectual property rights protections, and adapting it to the requirements of the digital information environment.[1] Adverse effects of higher access charges for data, information and research tools that the owners of legal monopoly rights are free to impose have exacerbated the impact of other policies embraced in a widening circle of countries. These measures aim to encourage public research institutes to "valorize" their results by patenting and direct private commercial exploitation, and they promote the exclusive licensing by research universities of patents awarded to those institutions on the basis of discoveries and inventions arising from the publicly funded work of their faculties. The lack of restraint in privatizing the public domain in data and information, especially in regard to the sharing of access to raw data-streams and information, tends to amplify the consequences of chronic under-provision of documentation and annotation activities -- which are necessary to maintain reliably accurate and up-to-date public database resources. Both trends therefore work to degrade the effectiveness of the research system as a whole.

Considered at the macro-level, "open science" and commercially oriented R&D based upon proprietary information together form a *complementary* pair of institutionally distinct sub-systems. The public policy challenge that needs to be faced, consequently, is to keep the two sub-systems in proper productive balance, so that the special capabilities of each may amplify the productivity of the other. But the former of these sub-systems, being based on cooperative behavior of researchers whose work is dependent on public and private patronage for support, is the more fragile of the pair; and more vulnerable to being undermined by the incursion of information disclosure restrictions motivated by the goal of privately appropriating rents from possession of new scientific and technical information. The "balancing act" for public policy therefore requires more than maintenance of adequate public funding for open science institutions and programs. It may call

[1] For entry points to the issues surrounding intellectual property rights and university-industry R&D collaborations, and references to the vast literature on the consequences of the Bayh-Dole and Stevenson-Wydler Acts in the U.S., see e.g., Smith (1990), David, Mowery and Steinmueller (1994), Mowery et al. (2004), David (2004, 2005, 2007a), David and Hall (2006); Guston and Keniston (1994) on university relations with the federal government; Branscomb (1994), Cohen and Noll (1994) on "privatizing" the National Labs. On the role of the public domain in scientific and technical data and information, see David (2003b) and other contributions to the same National Academy of Science Symposium publication.

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for deliberate measures to halt, and in some areas even reverse excessive incursions of claims to private property rights over material that would otherwise remain in the public domain of scientific data and information – in other words, for the protection of an “open science domain” from the regime of legal protections for intellectual property rights.[2]

Nevertheless, today there are many writers in the business press, academic economists, lawyers and policy makers who see the matter very differently. The centrality of information technologies and information goods in the phenomena that are associated with the New Economy suggested that the world has left behind the epoch of material capitalism and entered a new and different stage -- “Intellectual Capitalism.” Accordingly, on this view, the way forward calls for assuring the continued vitality of the market system through a combination of new institutional and technical innovations that would provide reliable control over “knowledge assets,” protecting holders of intellectual property rights from the potentially disruptive effects of the rapid advance of digital information technologies and computer-mediated telecommunications. In this vision of the brighter future, the dark threat that has to be contained is the one arising from the free and open circulation of ideas, data and information.

For readers with the range of present-day concerns that have just been reviewed, the thrust of this essay’s contribution to the social history of modern science is simply this: To pursue the policy path toward the vision of perfected “Intellectual Capitalism” could perversely lead the global enterprise of scientific research (and all that depends upon its sustained vigor) towards the truly darker past from which western European societies rather fortuitously managed to escape in the seventeenth century. The modus of that liberation – one could say of “enlightenment”—was provided by the inter-twinning of intellectual and institutional transformations that first coupled the Scientific Revolution of that era with the growing adherence to new attitudes and practices regarding the disclosure of “Natures’ Secrets.” This emergent ethos subsequently reinforced the institutionalization and stabilization of public patronage of researchers in the “Republic of Science” within a system of fruitful interactions with proprietary,

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[2] The case for this position is argued in David (2004, 2005), with regard not only to patenting but the potential effects on exploratory scientific research in many scientific field arising from legal protections of databases in the EU. On analytical and empirical questions concerning the seriousness of the so-called “anti-commons” arising from patenting of research tools in biomedical sciences, see, e.g., Walsh, Arora and Cohen (2003); and David (2003b) for somewhat divergent perspectives. Understandably, much of the science policy literature continues to focus upon issues of funding, but there are legal and other institutional impediments to academic research collaborations – and not only those relating to IPR. David (2006) and David and Spence (2003: esp. pp. 45-53; and 2008) examine these obstacles in the context of the hopes for a new age of global collaboration brought about by “e-Science and Grid computing infrastructures, and propose and a variety of ameliorative institutional innovations.

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commercially-oriented R&D activities that was away moving from predominant reliance upon secrecy and towards greater use of legal protections afforded by (open) patents.

To closely read the complex story of the transformations that created the ethos of open science is a beginning step towards fuller appreciation of the degree to which the constellation of differentiated institutions supporting and shaping the conduct of scientific and technical research is in reality a quite fragile cultural construct. We are dealing with the legacy of an extended, intricate and contingent historical process that cannot be assumed to have been produced by some underlying self-balancing, and auto-regenerative system that responds only to the imperatives of modern scientific technique and therefore requires neither social maintenance nor political protections. True, as is the case with many institutional structures, the institutional infrastructures of ‘‘open science’’ do possess a measure of plasticity. But elasticity has limits. Consequently, as is the case with other delicate pieces of (social) machinery, a substantial measure of caution and patience in seeking to first thoroughly understand the manner of its construction and its present workings would seem a minimum that should be asked of those having the power to tinker with this important institutional component of infrastructure for the modern system of innovation. That much precaution seems called for especially where reformers are moved to experiment and "innovate" in order to find a "quick fix" for one or another among the myriad (of often rather transient) societal problems that have brought themselves to the notice of people in elected offices.

Therefore, at such a time as this, when academic institutions and public research institutes are being encouraged to take ownership of intellectual property and actively exploit the commercial potential of their discoveries and inventions, and still other proposals for quite radical institutional changes affecting the organization and conduct of science remain in the air, it seems particularly appropriate to pause and look backward to the historical circumstances in which the social ethos and some of the key institutions supporting more traditional norms for academic science first emerged.

An examination of the strands of "internal" intellectual development in natural philosophy, and the nexus of "external" economic, social and political forces that shaped the early process of institutionalization of open science and its connections with the Scientific Revolution of the seventeenth century, may at very least elicit greater appreciation for the emergence of this complex of institutionalized behaviors in western Europe as an extraordinary piece of cultural good fortune. Perhaps, it will serve also to heighten an awareness of the potentialities of unintended consequences – both good and bad – that are latent in efforts to quickly re-orient complex and venerable social institutions in order to

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serve purposes very remote from those for which the course of their historical evolution has left them well adapted.

An inquiry of this kind must ask economists and economic historians to suspend their preoccupation with probing resource allocation mechanisms in the interior of the “black box of technology” -- long enough to seek a clearer view of what goes on inside the other major component of modern research and innovations systems, the still relatively under-studied “black box of science.” Although the open science system has begun to be explored using the tools of economic analysis during the past two decades,[3] far more remains known and understood about the evolving institutional structures affecting proprietary R&D resource allocation, and especially about the intellectual property protection mechanisms that enable private appropriation of research benefits. The desirability of closing this particular lacuna in the economics and economic history literatures has been as evident to those who have approached it within a broader framework of concern with the economics of institutions, as it is for those who have observed it from the perspective of science and technology studies.

Even before the ‘new economics of science’ had started directing attention to such a program or inquiry, Douglass North (1990:p. 75) characteristically perceived in it both a significant challenge and a promising opportunity:

> "The literature dealing with the origins and development of science is substantial, but I am not aware that much of it self-consciously explores the connecting links between institutional structures...and incentives to acquire pure knowledge."

That being too strong a hint to go ignored, a growing company of economic and technological historians, along with economists of an historical persuasion, have begun to turn their attention to studying the scientific foundations of the modern “knowledge economy.”[4] Nevertheless, this rapidly growing literature is

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[3] On the program to redress this comparative neglect through research in the microeconomics of resource allocation within publicly supported science, see Dasgupta and David (1987, 1994), David (1994b, 1998, 2002 2003b), and surveys by Diamond (1996), Stephan (1996), and David, Foray and Steinmueller (1999). Note should be taken of the harvest of interesting and ingenious quantitative studies emerging from a younger generation of economists, including Arora, David and Gambardella (1998), Geuna (1999), Carayol and Dalle(2000), Carayol and Matt (2003). Nathan Rosenberg (1982: Ch. 7), while not himself delving deeply into the ‘black box’ of the microeconomics of scientific research in universities and other non-commercial institutions, led the vanguard calling upon economists to recognize that the state of scientific knowledge should not be treated as exogenous to the economy’s development -- especially because the scientific enterprise continues to be shaped in many ways by specifically technological concerns.

[4] In addition to the works cited below as specifically germane to the institutional history on which this paper focuses, see the variety of topics addressed, e.g. by Lécuyer (1998), Lenoir

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preoccupied (quite justly) with pressing contemporary questions about the performance of these institutions and the people who work within them. It rarely pauses to ask how the modern world came to have two quite different and in some respects anti-thetical modes of organizing and allocating resources for the pursuit of reliable knowledge. The answer proposed in these pages is, then, a contribution to the economic history of modern science that may be read as offering insights from the past that carry implications for the present and the future.

The structure of this essay is straightforward: Section 1 describes what is meant here by "open science" and points out the puzzling aspects of the existence of this nexus of norms and institutionalized social conventions. Section 2 sets out a functionalist answer to the question of why modern societies have two different modes of organizing scientific research, but notes that an answer that may satisfy economists as to an existing institution's "logical origins" may not be the same as one concerned with explaining its "historical origins", and clearly it is not in the particular case at hand. Following a brief consideration of the problematic aspects of the few properly historical explanations that have been offered for the behavioral shift away from keeping secret such knowledge as could be discovered about Nature's Secrets, Section 3 turns to the core of the alternative thesis advanced here. It focuses on the consequences within the European system of noble patronage during the late Renaissance of the growing reliance of potential savant-clients upon new and more powerful mathematical knowledge. Practises of open challenge and disclosure that emerged in the early modern era are seen as a mode of coping with the exacerbated problems of asymmetric information within those principal-agent relationships, and one that was fortuitously aligned with the interests of the clients. Section 4 makes use of recent analytical results from the theory of common agency games to explore the consequences of the rivalry for prestige among noble patrons of the new, more mathematic sciences; that, together with the limitations on the spillovers among rival patron of the fruits of applying their clients' knowledge in the technical arts) meant that patron-scientist relations in that era were common-agency games in substitutes, rather than in complements, with accordingly different implications for the distribution of "information rents" among the players.

Section 5 then considers the early stages in the subsequent process of institutionalization of the new scientific disclosure practices, in the same spirit of "historically situated" microeconomic analysis". It examines correspondent relationships and reputation-building with the context of the early humanistic academies, and the broader "invisible colleges" and royal academies of the late seventeenth century. This further reinforces a skeptical reception of the notion the

(1998), and the broader, less institutionally specific treatments available to be found in Headrick (2000), Mokyr (2002), and Foray (2004).

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emergence of a "new age of academies" based upon state patronage was compelled by the enlarged material requirements of the new brand of science. Instead, it should be seen to have been a product of the same logic of princely patronage being played out in the nation-states of early modern Europe.

The essay concludes in Section 6 with reflections on the view of the institutions of open science as perhaps the most precious among the legacies left to modern capitalist societies by European feudalism, and offers some simple lessons for scientists and science policy-making today.

1. The puzzling phenomenon of "open science"

The subject matter of this essay will be seen to be situated squarely at the intersection among three areas of concern, namely, those of the new economics of science and the economic history of the production and distribution of knowledge, the economic analysis of social institutions, and the current policy debates over the future organization of national science and technology programs. With that more than ample motivation serving to frame the historical inquiry, the obvious first step must be to fix precisely upon the set of key institutionalized norms and practices of modern scientific research systems that deserve more explicit analytical and empirical attention than economists and economic historians hitherto have accorded to them.

The institutional features of prime interest here are those that distinguish most sharply the sphere of "open science" (supported by public funding and the patronage of private foundations) from both the organized conduct of scientific research under commercially-oriented proprietary rules regarding information, and the production and procurement of defence-related scientific and engineering knowledge under conditions of restricted access to information concerning basic findings and actual or potential applications.

1.1 *Ethos, norms and institutions*

Many of the key formal institutions of modern science already are quite familiar to academic social scientists, and therefore to economists and economic historians. It is a striking phenomenon, well noted in the sociology of science, that there is high degree of mimetic professional organization and behavior across the diverse cognitive domains of academic endeavor. Whether in the social sciences, or the natural and mathematical sciences, or the humanities for that matter, most fields have their professional academies and learned societies, journal refereeing procedures, public and private foundation grant programs, peer-panels for merit review of funding applications, organized competitions, prizes and public awards. Sociological studies of the sciences proper have documented the existence of commonly recognized norms and conventions, reflecting a clearly delineated

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ethos to which members of the academic research community generally are disposed to publicly subscribe, whether or not their individual behavior conforms literally to its strictures. The principal norms of "the Republic of Science" that famously have been articulated by Robert K. Merton (1973) sometimes are summarized under the mnemonic CUDOS: *c*ommunalism, *u*niversalism, *d*isinterestedness, *o*riginality, *s*kepticism.[5]

The *communal* ethos emphasizes the cooperative character of inquiry, stressing that the accumulation of reliable knowledge is a social, rather than an individual program; however much individuals may strive to contribute to it, the production of knowledge that is "reliable" fundamentally is a collective process. The precise nature and import of the new knowledge therefore ought not to be of such personal interest to the researcher as to impede its availability or detract from its reliability in the hands of co-workers in the field. Research agendas, as well as findings ought therefore to be under the control of personally (or corporately) *disinterested* agents. The force of the *universalistic* norm is to allow entry into scientific work and discourse to be open to all persons of competence, regardless of their personal and ascriptive attributes. A second aspect of openness concerns the disposition of knowledge: the full disclosure of findings and methods forms a key aspect of the cooperative, communal program of inquiry. Disclosure serves the ethos legitimating, and, indeed prescribing *skepticism*, for it is that which creates an expectation that all claims to have contributed to the stock of reliable knowledge will be subjected to trials of verification, without insult to the claimant. The *originality* of such intellectual contributions is the touchstone for the acknowledgment of individual claims, upon which collegiate reputations and the material and non-pecuniary rewards attached to such peer evaluations are based.

1.2 *The problem: In a world of secret knowledge, why "open" science?*

An essential, defining feature of modern science thus is found in its public, collective character, and its commitment to cooperative inquiry and free sharing of knowledge. While to most of us the idea of science as the pursuit of "public knowledge" seems a natural, indeed a primitive conceptualization, it is actually a social contrivance; and by historical standards, a comparatively recent innovation at that, having taken form only as recently as the sixteenth and seventeenth centuries.

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[5] See Merton (1973: esp. Ch. 13), Merton (1996: Chs. 20-24); also Ben-David (1991: esp. Parts IV and VI). For the mnemonic CUDOS, see Ziman (1994, p. 177).

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The epistemological transformation effected by the fusion of medieval experimentalism with Renaissance mathematics has been the subject of an enormous literature that focuses on tracing the intellectual foundations upon which those late sixteenth and early seventeenth century developments rested. Figure 1 reprises the familiar main lines of continuity in a schematic outline of the Scientific Revolution's antecedent intellectual sources.[6] But the late sixteenth and early seventeenth centuries also witnessed a transition from the previously dominant ethos of secrecy in the pursuit of Nature's Secrets, which gave way to a new set of norms, incentives and organizational structures. These institutional transformations reinforced scientific researchers' commitments to rapid disclosure and wider dissemination of their new discovers and inventions.

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[6] For a convenient entry-point into this literature, see Hall (1954 and 1983), whose theme is encapsulated by the following remark (1983: p. 30): "Perhaps the most obvious 'cause' for the scientific revolution that can be suggested is that it represents the selective flowering of certain medieval traditions." This does not dispute the view of the scientific revolution as a departure from, and reaction against the mainstream of medieval learning found in the universities, but points to resonances and suggests continuities between some new and distinctly non-Aristotelian currents that were present in medieval and Renaissance "precursors" and the mechanical philosophy of Galileo's epoch. Hall's theme is very much in line with the observations of Pierre Duhem [1861-1916] on the major advances in the treatment of mechanics and motion during the fourteenth century, and the notice given by Crombie (1953, 1959) to the contributions that Robert Grossteste [c. 1168-1253] made in giving empiricism an important place in medieval discussions of logic; and the subsequent successful use of experiments in medieval optics – such as the early fourteenth century study of the rainbow colors by Theoderic of Fribourg. On the significance of the revelation of Greek mathematics in the renaissance, and the fifteenth century European absorption of the algebraic advances made by Arabic mathematicians, see, e.g., Hall (1954: pp. 9, 11, 100, 170-71) and further discussion below, in section 3.2 . It should be noted that Shapin's (1996) recent provocative treatment of Scientific Revolution departs from the traditional historiographic attention devoted to identifying critical intellectual discontinuities and their precursors, presenting an account that stresses elements of continuity and change in the array of social practices by which scientific knowledge was produced.

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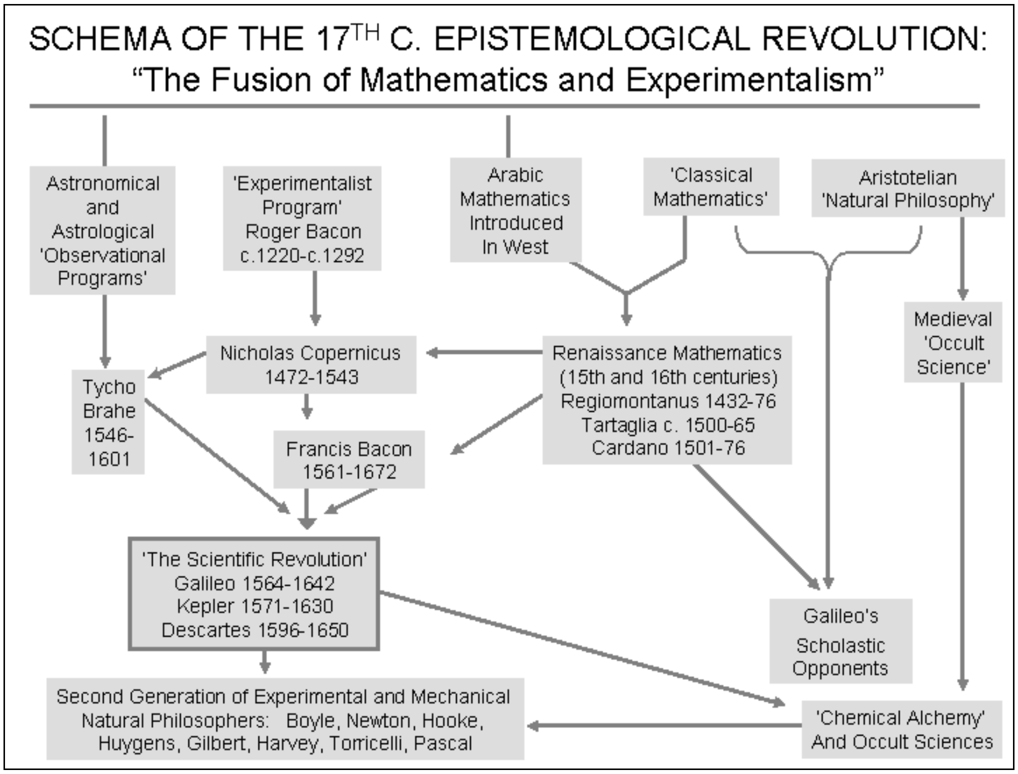

Figure 1

Rather surprisingly, the puzzle of how this came about has only lately come to be noticed among historians of science. The comparative inattention accorded to this particular problem may be attributable in large measure to the quite understandable continuing preoccupation of historians of western science with "the big questions": uncovering the origins and driving forces in the Scientific Revolution in 17th century, accounting for the uniqueness of the ensuing European achievements in the advancement knowledge in the natural sciences, and explaining why the flowering of scientific knowledge in other cultures, places and eras – notably classical Greece, Islam, and China – did not similarly attain a sustaining cumulative momentum.[7] Although forming a clearly distinct *problematique*, the neglected historical puzzle taken up here involves aspects of

[7] H. Floris Cohen's (1994) comprehensive historiography of the Scientific Revolution admirably helps disentangle the foregoing three quite different "problems." But it shares the silence of that literature on questions concerning the historical emergence of the ethos of "public knowledge" and "open science." This is somewhat less understandable in view of Cohen's otherwise penetrating critical expositions. But, a fixation of interest in those same, classic 'big questions" also characterizes recent contributions of economic historians such as Rosenberg and Birdzell (1986), Landes (1998), and Mokyr (1990, 2002).

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the Scientific Revolution that may hold some of the answers to the “big questions” that hitherto have tended to obscure it from view. That, at least, is a possibility broached at several points in the course of the present argument.

Even the most superficial acquaintance with the antecedent intellectual orientation and social organization of scientific research in the West suggests the utter improbability of a bifurcation of this kind, in which the germ of a new and quite antithetical mode of organizing the pursuit of knowledge appeared alongside the secretive search for ‘Nature's Secrets.’ Putting the point differently, virtually all of the antecedent conditions inveighed against ‘openness’ of inquiry and public disclosure of discoveries about the natural order of the world, much less of the heavens.

In classical Greece, science developed within the paradigm of competitive public debate, which operated to solidify knowledge into separate schools of thought and militated against collaboration among scientists directed toward a single goal. Similarly, medieval science, shaped by a political and religious outlook that encouraged withholding the Secrets of Nature from the "vulgar multitude,” made scant contribution to the development of the concept of openness, even though there were some individuals who thought it important to commit their knowledge to books meant to be shared with certain others. A work that played an influential role in moulding medieval attitudes toward the disclosure of knowledge was the pseudo-Aristotelian *Kitab Sirr al-Asrar* ("The Book of the Secret of Secrets") known to Europeans as the *Secretum secretorum*, which Lynn Thorndike (1950, v. II: 267) characterized as "the most popular book in the middle ages." It professed to reveal the deepest, esoteric wisdom of Aristotle, while promulgating in elusive, enigmatic terminology the idea that because this secret knowledge could make possible limitless things in the material world, it had to be kept hidden from the eyes of the "unworthy.” This was reinforced by other traditions in medieval literature that portrayed the goddess Natura as being modestly veiled, and hostile to an open disclosure of her secrets. The moral obligation to be circumspect in matters concerning the secrets of nature was thus, as William Eamon (1985: 325) has phrased it, "a conviction woven into the very fabric of medieval thought."

This stream of European thought harking backing to attitudes in classical antiquity, persisted beyond and was in some respects reinvigorated by the Renaissance. Thus, according to Paolo Rossi (2005), even in the early modern era discourse about the nature of the world had two faces, one revealed and the other hidden: “philosophers skillfully hid magic behind secret words, and this they did for altruistic motives: *si haec scientia hominibus esset discoperta*, *confunderent universum.*” (If this knowledge was revealed to all men, it would confound the universe):

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> "The obscure science was profound, for the very words that described the natural order were those given to Adam by God, and comprehensible to only a select few. What is striking about [these expressions of] the idea of secrecy is not the formulas themselves, but their immutability. The same authors, citations, and examples recur in occult texts through different periods in history. Cornelius Agrippa, for example, tells us that Plato forbade the disclosure of the mysteries, Pythagoras and Porphyry bound their disciples to secrecy, Orpheus as well as Tertullian demanded vows of silence, and Theodotus was blinded because he tried to penetrate the mysteries of Hebrew scripture. Indians, Ethiopians, Persians, and Egyptians spoke through riddles. Plotinus, Origen, and Ammonius' other disciples vowed never to reveal their teacher's dogma."

The imperative of secrecy was particularly strongly developed in the medieval and Renaissance traditions of Alchemy, where it was to persist throughout the seventeenth century and into the eighteenth -- side by side with the emergent institutions of open science. Alchemy was regarded as a form of personal knowledge, a "divine science" rather than a science of nature. B. Y. T. Dobbs (1975: p. 27) points out that "alchemy never was, and never was intended to be, solely a study of matter for its own sake"; it was not a rational branch of natural philosophy, but rather, "a way of life, a great work which absorbed all [of the alchemist's] mental and material resources…."[8] The knowledge whose possession was claimed by alchemists had to be gained through a combination of divine illumination and reason leading to inner sensations of secret understanding, on the one hand, and, on the other hand, experimentation ("labors at the furnace," so to speak).

Because the fruits of this mixture transcended the descriptive powers of ordinary language, on that account if on no other they were fit subjects for broadcast communications. Well into the era of the Scientific Revolution, authors of alchemical texts continued deliberately to employ obscure symbols, paradoxes, allegories and secret names, for the purpose of guaranteeing the protection of divine secrets and their retention within a small circle of intimates who were bound to secrecy. Robert Boyle [1627-1691] who was well acquainted with

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[8] Vickers (1984:9) makes the related point that in Renaissance Europe the occult mentality was not interested in nature for the sake of understanding *per se*; rather the underlying questions are primarily *self-interested*. Will I be happy? Wealthy? Will I be healthy? How long can I live? These are questions in which knowledge is an instrumentality, a means to an end, but a *private* rather than a *public* purpose. The socialization of modern "scientists" to reject of such questions as "unworthy" is recognized as an element in the Science-Technology differentiation found at the sociological level. This may be a legacy from the official rejection of occult studies in the Renaissance universities, even while they were being tolerated for *private* study (see Feingold 1984).

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members such a circle in London, found it apparent from "their obscure, ambiguous, and almost enigmatical way of expressing what they pretend to teach, that they have no mind to be understood at all, but by the sons of the art."[9] In the newer practice of "chemical alchemy" that was fostered in mid-seventeenth century England (through the fusion of mechanical philosophy with the older alchemical tradition), the notation and associated concepts became increasingly standardized, and on that account more readily decipherable. Yet, the maintenance of the tradition of secrecy and the cryptographic character of manuscripts persisted even in the voluminous unpublished alchemical writings of Isaac Newton – which continued throughout the century, and are reported by Newton's modern biographer Richard Westfall (1980) to have run to more than 1,200,000 words.[10]

Social and economic regulations, along with the relatively primitive and costly technologies available for scientific communications, reinforced the moral and philosophical considerations that were arrayed during the middle ages against open disclosure of discovered "secrets." Technological recipes normally were not broadcast by medieval and early modern Europe's craftsmen, and especially not those constrained by the regulations of the urban craft-guilds which aimed to preserve the "mysteries" of their industrial arts.[11] Pamela Long (1991) has made

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[9] See Eamon (1994: p. 341) for this extract from Boyle's "Skeptical Chymist" (1661). In reflecting upon the participation of leading scientific figures of this era such as Boyle, and Newton in these hidden, instrumentally purposive investigations, one cannot help but notice a striking and perhaps disturbing element of resemblance between the norms governing proprietary information guarded by the profit-motivated "start-up" companies in which academic scientists in the modern era have involved themselves, and the conventions of secrecy that traditionally prevailed among practitioners of the occult arts.

[10] This was more than Newton had committed to paper in all of his other "researches". On Newton's studies of alchemical texts and engagement in experimental research, see also the monograph by Dobbs (1975) and the detailed evidence that Figala and Petzold (1993) present about his sustained personal contacts with a circle of fellow alchemists in London.

[11] Among recent discussions of medieval and early modern craft guild practices in this regard, see the excellent discussion by Long (1991b:pp.870-881) of *secrecy* as the characteristic mode of protecting technical knowledge that was recognized to be of economic value. Epstein (2002, 2004) advances a reinterpretation of the economic effects of Europe's urban craft guilds, emphasizing instead their positive role in the dissemination of industrial knowledge and contributions to the region's technological dynamism. While the argument and evidence are on balance persuasive when taken in the context of the comparisons that Epstein (2002) draws between China and Europe in the renaissance and early modern epochs, they fail when offered in refutation of the view that guild regulations promoted industrial secrecy and obstructed the working of new inventions. That defect is detailed in the following discussion, which draws on the more "traditional" interpretations of urban craft guild regulations as having displaying all the lineaments, and effects of monopolistic cartels (e.g., Thrupp 1942, Thrupp 1963, Smith 1963, Unwin 1964).

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the important point that the medieval guilds fostered the conceptualization of knowledge of industrial practices as intangible assets, the economical value of which warranted protection. Secrecy, above all, was the characteristic mode through which the private benefits of craft knowledge were appropriated.

The economic aspects of Long's (1991b) thesis in regard to the effects of medieval guild policies upon both secrecy and the dissemination of (craft) knowledge are somewhat subtle, and so some special care is appropriate when summarizing its details. Nevertheless, the nub of the argument turns on the simple point that although information has the economic properties of a public good, it nonetheless may be held tightly secret for private exploitation, or it may be a secret that is collectively maintained among the members of a private consortium who are thus able to exploit it for commercial purposes as "a club good."[12] Although the craft guilds did nothing to prevent the first of these modes of private appropriation, one effect of their industrial policies facilitated the limited sharing of craft knowledge as "club goods." But, a secret is still a secret, not a matter that is "open" for wide diffusion as common knowledge. Medieval and early modern craft guilds did not draw the circles of knowledge-sharing so tightly as did the traditions in force among the alchemists, but their economic interests and regulatory powers worked in ways that restricted the dissemination and growth of technical knowledge more tightly that would be expected if the structure of their industries had been competitive.[13]

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[12] Although the existence today of trade secrecy statutes in some legal jurisdictions has led to modern the inclusion of trade secrets along with patents, copyrights and trademarks under the general rubric of "intellectual property," legal theory supports the view that the rights protected under trade secrecy laws are rights to enter contracts of confidentiality, and not strict "property rights." It is certainly undeniable that historically they derive from the common law governing master-servant relations. See further discussion and references in David (1993: pp. 30-32.)

[13] Craft guilds were chartered 'corporate' entities that governed the conduct of the major urban industries in Western Europe during in this era (and during much of the early modern period) very much in the manner of producer cartels. The guild-masters of a locale sought *collectively* to function very much along the lines of joint profit-maximizing monopolists in their respective product markets, and as monopsonists with respect to the labor market of their town. Thus, they regulated competition among the guild's members, restricted their output, and restrained masters from poaching apprentices and journeymen from one another. While they favored inducements for journeymen and less skilled works to settle in their city, they took steps to inhibit skilled artisans from leaving to work elsewhere, especially in neighboring locales that might otherwise form a market for the guild's transportable products; on some occasions, the might even resort to more direct, violent means of suppressing the establishment of competing production facilities in beyond the particular urban domain within which they legitimately were able to exercise regulatory controls. See, e.g., Thrupp (1963: pp. 230-280; 624-634). The foregoing summary, although stated in more abstract analytical terms may also be seen to fit closely the specific regulations and policies of the glassmakers' guild in Venice, described in splendid detail by Long (1991b: pp. 870-875) – but which have been construed quite differently by subsequent commentators (e.g., Merges 2004) ostensibly following the reinterpretation of the

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The craft-guilds' anti-competitive practices had unintended as well as intended consequences impeding the diffusion of industrial production knowledge. Within the town, the first-order effect of regulations forbidding poaching of apprentices, and the restraints on the raiding of skilled workers from the establishments of other guild masters, was to reduce opportunities for superior practices (workshop secrets) to be imparted to a larger number of artisans, thereby indirectly decreasing the chances for still wider dissemination beyond the ambit of the town. Further, while medieval and early modern cities and towns as a rule were open to migrants (indeed, in view of the net mortality the population of adults could be sustained only by in-migration), foreign journeyman who had not served as an apprentice to one of the guild's masters were as a rule not employable there. In view of the fact that few industrial craft practices were recorded in forms that would permit them to be transmitted except through immediate human agency, this restraint on the wider physical circulation of skilled artisans reinforced the geographical "balkanization" of technological knowledge.

Even though the absence of regulations against the movement of journeymen among the workshops of the town could facilitate exchanges of craft knowledge locally, it did not inhibit the efforts of individual masters to keep their own industrial secrets. Nor was it incompatible with guild sanctions against masters who carelessly permitted their skilled artisans to work in public view, where their methods might be observed by anyone that passed by.[14] Indeed, the indirect effect of the guild's joint monopolistic (cartel) position was the creation of positive incentives for the members to guard information about idiosyncratic practices, which in turn reinforced product differentiation (distinctive shaping, coloration and finishing of their wares, in the case of the Venetian glass-makers) and the maintenance of monopolistic competition in local markets.

The analytical argument supporting this reading of the evidence goes as follows. Firms with monopoly power in the markets for particular products generally will have weaker incentives to introduce product innovations than would equivalent firms in competitive markets. Their readiness to invest in developing inventions (their own, or those of others) with a view to introducing them in their industries will be the weaker on that account. The invention-inhibiting effect of monopoly vis-à-vis competition is stronger, the greater (more "drastic") is the unit cost reduction afforded by the inventions in question – a proposition that has been made familiar in economics by Arrow (1962). Therefore, one hardly should be surprised by the fact that where entrenched craft-

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role of the craft-gilds by Epstein (1998).

[14] Eamons (1994: p. 84) cites ordinances to this effect that were issued by the London Pewterers' Company in the seventeenth century.

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guild monopolies exercised control over an extensive national territory – as was still the case in France throughout most of the eighteenth century, it was only the readiness of an absolute monarch to grant *privilèges* permitting the production of new goods, and the working of novel techniques of manufacture, that counteracted the craft guilds' generally baleful influence upon industrial innovation.[15] Given the reasonably well-founded expectation on the part of a guild master that the other members of his corporation would not be any more favorably disposed than was he towards the introduction of novelties into their trade, the anticipated rate of obsolescence of their craft's methods would be slower. Consequently, the expected present value of devoting resources to keeping secret the "mysterie" surrounding already established products and methods would tend to be all the greater.

Of course, the alternative to secrecy (in its several forms) was disclosure under grants of privileges that would permit the revealed knowledge to be commercially exploited without hindrance. Prior to the enactment of patent laws (the first of which dates from 1474 in Venice) and the regular granting of "letters patent" providing monopoly privileges in exchange for the introduction of new arts, engineers and inventors were particularly reluctant to divulge the secrets of their inventions. Before the seventeenth century, therefore, the typical objective of the grant of an industrial "patents" was not to stimulate "invention" in the modern sense of the term; rather, it was to elicit the migration of foreign artisans would could introduce into the grantor's dominion, and establish therein a craft that was already known elsewhere. The necessity of training apprentices entailed sharing knowledge of the new art. Because that inevitably would create journeymen for others to employ, and eventually potential rival masters in the new locale, the grant of monopoly privilege by patent provided a period of protection from competitive entry that might suffice to reward the foreign artisan for the trouble and expenses of the move.[16]

Similarly, knowledge of recently discovered geographical secrets that had commercial value, such as trade routes, would be kept from the public domain. Maps based upon voyages of discovery in the fifteenth and sixteenth centuries were regarded as especially valuable and were either suppressed or guarded to

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[15] See Hilaire- Pérez (1991), and the more extensive treatment in Hilaire-Pérez (2000), esp. pp. 136-142.

[16] The modern English term "patent" derives from the medieval practice of announcing grants of privileges and protections by royal proclamations, or "Letterae Patentes," i.e., Open Letters. As early as the fourteenth century such grants were being employed by the English crown to encourage the introduction of foreign technologies such as the building of windmills, or the weaving of linen, through the transfer of skilled craftsmen from abroad. See Hill (1924), Prager (1944), David (1993). For analysis of the economic basis and implications of this generally overlooked aspect of patents, see also David and Olsen (1992).

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prevent their falling into the possession of maritime and commercial rivals.[17] The one field of early modern European commercial and industrial endeavor that presented a striking exception to these generalizations is that of metallurgy and metal mining, in which the sixteenth century saw a rapid proliferation not only of printed handbooks containing practical recipes (often alchemical in style), but informative books on mining engineering and assaying by German authors. Notable among these was *De re Metallica* by Agricola [Georg Bauer, 1490-1555] appearing in 1556, and the treatise on ores and assaying published in Prague in 1574 by Lazarus Ercker [d. 1593], superintendent of the mines in the Holy Roman Empire.[18] It should be remarked, however, that the mining of minerals was conducted by royal monopolies in the principalities of Europe during this epoch. Hence, the disclosure of techniques for the more efficient exploitation of these assets would not have been detrimental to the profitability of these enterprises.[19]

Why then, out of such a background of obfuscation and secrecy, should there have emerged a quite distinctive community of inquiry into the nature of the physical world, holding different norms regarding disclosure, and being governed by a distinctive reward system based upon priority of discovery? Why so, especially when in the modern context it appears that there is little in the chosen methods of (scientific) inquiry that would suffice to distinguish the investigative techniques used by university scientists working under the institutional norms of open science from the procedures that they (or others with the same training) would employ in the setting of a corporate R&D laboratory?

The emergence of the idea that humanity would benefit from the concerted collective pursuit of public knowledge, and of the conventions and norms supporting the practice of “open science" appears to have been a distinctive and vital organizational aspect of the Scientific Revolution. Is the social organization of open science then simply an epiphenomenon of the profound philosophical and religious reorientations that have been presented as underlying the Scientific Revolution of the seventeenth century? Or, instead, should we see the Scientific

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[17] On maps and secrecy see, e.g., Boorstin (1983: 267-78).

[18] See, e.g., Hall (1983): pp. 241-242. In discussing these and other 16th century German writings on mining and metallurgy, Long (1991a: p.325) notes that “wealthy investors with little specific knowledge of mining and metallurgy and holders of regalian rights both became ready patrons and consumers of mining literature.” The authors of these printed works wrote for these princely holders of mining right monopolies, and for “the expanded number of new practitioners whose skill in prospecting, mining, and processing metals provided the key to the profits of their employers.”

[19] See Hirschliefer and Riley (1979) on the generality of this familiar point on the economics information and the “appropriability problem”—viz. that controlling the tangible assets that are complements of information is as good as exclusive possession of the right to exploit that information.

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Revolution as the product of what might be called the "Open Science Revolution”? More reasonably, were these two discontinuities -- the one taking place in the social organization of scientific inquiry and the other transforming its intellectual organization -- interdependent and entangled with each other in ways that remain insufficiently understood? Some clearer insights into the problem may be gained by turning to the economic logic of the organization of knowledge producing activities.

2. Open Science’s ‘logical origins’ and the problem of ‘historical origins’

It is quite possible to elaborate a functionalist explanation for the “open” part of the institutional complex of modern science, by focusing on its economic and social efficiency properties in the pursuit of knowledge, and the supportive role played by norms that tend to reinforce cooperative behaviors among scientists.[20] This rationale highlights the “incentive compatibility” of the key norm of disclosure within a collegiate reputation-based reward system grounded upon validated claims to priority in discovery or invention. In brief: rapid disclosures abet rapid validation of findings, reduce excess duplication of research effort, enlarge the domain of complementarities and yield beneficial “spillovers” among research programs [see Figure 2]. Without delving too deeply into the details of the analysis, it should be noted that it is the difficulty of monitoring research effort that makes it necessary for both the open science system and the intellectual property regime to tie researchers’ rewards in one way or another to priority in the production of observable “research outputs.” The later thereby are become available for “validity testing and valorization” – whether directly by peer assessment, or indirectly through their application in the markets for goods and services.

[20] See, e.g., Dasgupta and David (1987, 1994); David (1998, 2003b, 2004).

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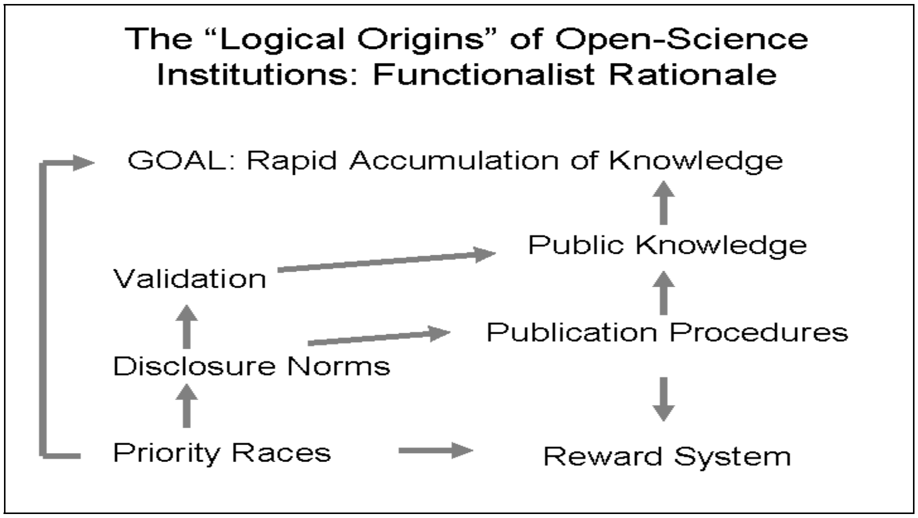

Figure 2

The specific functionality of the information-disclosure norms and social organization of open science rests upon the greater efficacy of data- and information-sharing as a basis for the cooperative, cumulative generation of eventually reliable additions to the stock of knowledge. Treating new findings as tantamount to being in the public domain fully exploits the "public goods" properties that permit data and information to be concurrently shared in use and re-used indefinitely, and thus promotes faster growth of the stock of knowledge. This contrasts with the information control and access restrictions that generally are required in order to appropriate private material benefits from the possession of (scientific and technological) knowledge. In the proprietary research regime, discoveries and inventions must either be held secret or be "protected" by gaining monopoly rights to their commercial exploitation. Otherwise, the unlimited entry of competing users could destroy the private profitability of investing in research and development.

One may then say, somewhat baldly, that the regime of proprietary technology (*qua* social organization) is conducive to the maximization of private wealth stocks that reflect current and expected future flows of economic rents (super-normal profits). Although the prospective award of exclusive "exploitation rights" yield this effect by strengthening incentives for private investments in R&D and innovative commercialization based on the new information, the restrictions upon use that secrecy or legal enforced intellectual property rights protections permit discoverers and inventors to impose work perversely to curtail the social benefits derived from that knowledge.[21]

[21] There is a countervailing hope, to be sure: the prospective rents of exploiting new

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By contrast, because open science (*qua* social organization) calls for liberal dissemination of new information, it is more conducive to both the maximization of the rate of growth of society's stocks of reliable knowledge and to raising the marginal social rate of return from research expenditures. But it, too, is a flawed institutional mechanism: rivalries for priority in the revelation of discoveries and inventions induce the withholding of information ("temporary suspension of cooperation") among close competitors in specific areas of ongoing scientific investigation. Moreover, adherents to open science's disclosure norms cannot become economically self-sustaining: being obliged to disclose (reasonably) quickly what they learn, and thereby to relinquish control over its economic exploitation, their research requires the support of charitable patrons or public funding agencies.

The two distinctive organizational regimes are thus seen to serve quite different purposes, which, when they co-exist at the macro-institutional level are complementary and may be highly fruitful in sustaining a flow of additions to the stock of reliable knowledge that can be profitably exploited through investments in developing new processes and products for the market. This functional juxtaposition suggests a logical explanation for their co-existence, and a rationale for the perpetuation of institutional and cultural separations between the communities of researchers forming "the Republic of Science", on one hand, and, on the other, those who are engaged in commercially-oriented R&D conducted under proprietary rules.

2.1 *Functionalism without context: the problem with 'logical origins'*

But, in what sense can that be taken to constitute "an explanation" for the institutions of open science? In seeking to uncover the "logical origins" of the institutions of modern science in their present-day functional value, this style of argument ignores questions of how they arose and have evolved historically. Rather, the best story that the logical origins approach based on social efficiency seems able to suggest about how these institutional arrangements came to exist is the supposition that they must have been instituted by some external agency, such as an informed and benevolent princes, or some other, rent-seeking political authorities possessing sufficient fiscal powers to create academies whose discoveries would enrich their realms and their tax revenues. But these are mere fantasies.

Yet, the ahistorical "logical origins" style of functional explanation quite obviously falters when one asks how could it have been otherwise? Though what spontaneous and undirected process possibly driven by the self-interests of the

knowledge under those restrictions on its disclosure could induce greater private efforts in research, yielding new knowledge of social utility.

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individual human actors, might the norms of full disclosure and cooperation in the search for knowledge ever have come to be established to begin with? After all, the modern economic analysis of the so-called "appropriability problem" identified by Richard Nelson (1959) and Kenneth Arrow (1962), emphasizes that openness in science sets the stage for a market failure due to free-riding behavior: the beneficiaries are reluctant to pay for the costs of generating new knowledge, since they expect it will be freely disclosed to them.

In order to respond satisfactorily to this last line of objections, we have to inquire into the institution's "historical origins." These may or may not be the same as the "logical origins" that we are led to perceive by considering the *contemporary* functional value of the open mode of scientific inquiry in a social and economic setting historically distant from that it which it first emerged. Of course, once the idea of the open cooperative pursuit of knowledge among scientists had been translated into established practice, even among very informal and loosely organized networks of researchers, it would become more reasonable to entertain the hypothesis that the functional value of that mode of inquiry could commend it to other, more self-conscious groups bent upon some collective purpose. Thus, kings and princes and their ministers, legislative bodies, and other public patrons subsequently might play important parts in the story of the formal institutionalization of those norms. Indeed, historians of science have at various times entertained the view that the creation of grand academies of science under royal patronage in England and France during latter part of the seventeenth century was an important organizational discontinuity that emerged as a benign response by these early modern European states to the growing material requirements of the new sciences. But, that particular interpretation of the events mingles many errors with some truths -- as will be seen in due course (from section 5's discussion of the so-called "Fontenelle thesis"). Before getting ahead of ourselves, however, it is necessary to consider what the historians of science -- not the economists -- have had to offer by way of accounting for the surprising emergence of "openness" in science.

2.2 *Historical origins: the problems with an "idealist" explanation*

Among the very few historians of western science who have directly addressed this aspect of the origins of modern scientific institutions, William Eamon (1985, 1994: esp. Ch.10) is notable for having not only documented, but proposed to explain the sixteenth century shift in the conception of science from that of the discovery and preservation of nature's secrets within elect brotherhoods of scientists, to complete and public disclosure of new knowledge. Following the work of Webster (1970) and others, Eamon depicts the transformation that occurred in seventeenth century England as the product of converging intellectual

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movements of reform. One of these had been initiated by the polemic of Francis Bacon [1561-1626] against the tyranny of philosophical systems ossified by unchanging subservience to intellectual "authority." It took concrete form in the Baconian program to foster the progress of knowledge by reorganizing the scientific community for greater cooperation and communication along lines inspired by the mechanical arts. A second movement identified by Eamon was that taking place in Puritan social reform politics, and particularly the influence of the ideas advanced by the circle around Samuel Hartlib [d.1662], who saw collaboration among scientists and inventors as means of achieving universal knowledge, the unity of religion, and the improvement of human welfare.

The closest that Eamon (1985, 1994: esp. Ch.10) comes to offering a materialist explanation for the emergence of open science, however, is to suggest that the progress of the "useful arts," as set down in the literary form of "books of secrets" and in accounts of the results of experimentation in the alchemical tradition, eventually became available for circulation as printed books. According to this argument, the *virtuosi* of the early seventeen century were thereby furnished with a model of what a distributed, open organization of knowledge acquisition might do for the advancement of scientific understanding. Thus, in the *Novum Organum* published in 1620, Bacon could contrast the power of cumulative improvement and confirmation by many practitioners of the useful arts with the stagnation of thought in the ancient philosophical traditions. On this view, natural philosophers were led by the technological writers' evidence of the progress around them to reappraise the collaborative, social nature of knowledge acquisition in the artisan tradition, and they generalized upon this to arrive at a prescription for the reorganization of investigations into the workings of nature.

A number of serious difficulties bedevil this nonetheless interesting line of explanation. The first concerns those "books of secrets" about the practical arts. It is undeniable that with the spread of literacy in the late middle ages some technological practitioners began to compile what Eamon (1994: pp. 82-89) initially describes as "craft secrets," in handbooks intended for the training of other artisans or to "stake claims to their inventions." Were one to go no further, it would be easy to form the impression of a surprising fashion spreading among ordinary craft-artisans who sought to reveal the very knowledge upon possession of which their livelihoods depended. Such a picture would be not only paradoxical, but misleading. Out of "literally dozens of examples of the writings craftsmen produced" by the end of the fifteenth century, Eamon presents an illustrative "sample" consisting of three texts. Significantly, they form a rather different picture from the one Eamon's discussion suggests -- although he does not comment upon it. First in the sample of documents is the manuscript treatise on the arts of painting, glassmaking and metalwork by Theophilus, a twelfth-century German Benedictine monk. Next comes a better-known treatise on the

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care of vineyards and orchards, composed c.1350 by the Bavarian, Gottfried of Franconia. Most probably he too was a cleric, who had acquired his skills when engaged with just such matters on the Wηrzberg estates of the prince-bishop, which were under the administration of ecclesiastical ordinaries.[22] The third exemplar is a 1389 manuscript on metallurgy by a Nuremberg "blacksmith and experimenter, " which opens with a learned reference to "meister alkaym" and goes on to detail numerous recipes for substances supposedly efficacious in quenching and tempering of knives.[23] It seems rather unlikely that the writer intended it for circulation beyond the circle of alchemical adepts.

My purpose in entering into these details goes beyond noticing that monastic artisans and horticulturalists would be better equipped to produce literary works than the literate craftsmen of this era. While that much is apparent to Eamon (1994: p.82), his discussion omits notice of the obtrusive coincidence that the two "sharers of artisinal secrets" actually were not directly engaged in business. They belonged to religious orders, where they might have hoped to advance their status by calling attention to their expertise – or perhaps they were seeking to attract support for wider European research-travels. These were, in any case individuals who would have had something to gain, rather than something valuable to lose by advertising what they knew; their stories therein hold a clue about the directions in which to look for explanations of the later emergence of the more widespread disclosures of "expert" knowledge.[24]

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[22] Gottfried reported his extensive travels, in the course of which he appears to have exchanged information and techniques on orchard care with an English Benedictine monk in Flanders, Nicholas Bollard, another who wrote a book about the planting and grafting of fruit trees. Eamon's surmise that both of them were clerics seems more than merely plausible.

[23] The contents of the latter seem (at least to this non-"adept" reader) to belong less to craft practice than to the tradition of arcane medieval magical and experimentation described by Thorndike (1950: II). If Newton could hold a Cambridge fellowship which also supported his alchemical pursuits, then, three centuries earlier a Nuremberg blacksmith also might make enough of a living to support his sideline as a *magus*.

[24] A similar point can be made in regard to the 15th century scribal (manuscript) books that appeared in Italy and south Germany on the mechanical arts. Long (1997) points out that these works (most of which were written in Latin, and often were lavishly illustrated and sumptuously bound) were encouraged by the patronage of elites, because in that era "the practice and representation of rulership came to be closely associated in particular ways with technological power and the mechanical arts."(p. 3) Military technology (including artillery, war carriages and gunpowder, as well as the construction of fortified building) figured prominently among the topics, because "the praxis of military leadership came to be closely associated with armaments and techniques—in contrast to most ancient models, in which generalship was perceived to rest character and qualities of leadership….In this environment technical books, whether lavishly illustrated and thus a visual manifestation of power, or predominantly textual, involving the explication of rational principles, came to be highly appropriate gifts for princes." The authors of these technical works were not members of craft guilds, but rather were "free agents" in search of

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The next difficulty concerns Eamon's contention that by the early seventeenth century the mathematicians and scientific *virtuosi* had fallen under the spell of Bacon's vision, persuaded by the correlation between the evident progress of industry and growing circulation of revealed practical knowledge in printed "books of secrets." Suffice it to say that this part of the argument sits most awkwardly alongside Eamon's ample acknowledgments that secrecy remained the norm in the realm of technical invention and industrial practice; that, consequently, the Royal Society of London's efforts during the 1660s and 1670s to compile a Baconian "History of Trade" was notably unsuccessful in opening up the crafts to public view.[25] He might have noticed, too, that in contemporary Paris the newly founded royal academy of sciences was hardly more effective than the Royal Society in drawing forth and codifying industrial knowledge from craft practitioners. Not until the middle of the next century did a marked shift of become evident in this regard, signalled by the concurrent appearance of the *Encylopédie* (1751-65) and the *Descriptions des arts et des métiers* (1761-88) -- the Académie des Science's own fulfilment of the old Baconian quest for a History of Trades.

Indeed, from his study of the Académie des Science's archives covering the preceding era, Robin Briggs (1991:p. 39) concludes that before the 1690's the institution's "genuine aspirations towards utilitarian science had only a minimal bearing on industry." This was due not only to the inclinations of the academicians toward the abstract treatment even of mechanical questions that had recognizable technological applications, and to the ineffectuality of their understanding of matters in applied chemistry. Craft-guild obstructions also continued to impede direct inquiries into existing industrial operations.[26] Pamela

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patrons, and plainly used these presentation volumes as advertisements for their services, indeed, sometimes indicating that not all the secrets were to be found in the descriptions of the techniques on view. The choice of audience, and language (save for the manuscript books in German containing recipes for gunpowder), coupled with the expense of these volumes, renders it very unlikely that they circulated widely or that their contents came to be diffused beyond the circle of elite readers and their servitors.

[25] See Eamon (1994: pp. 7, 299, 342-345, 348). Bacon (1994: pp. 26, 197-198) calls for the compiling of "histories" of the mechanical arts as "Instances of the Ingenuity or Hand of Man" (as well as compilations of "Boundary Instances," i.e., anomalies and monsters in Nature). The project was taken up as the "History of Trades" by the early Royal Society of London, many of whose founding members regarded their institution as the realization of the utopian community of cooperating scientists envisaged in Bacon's *New Atlantis.* Interestingly enough, it was on the very eve of the Society's formal organization, when the project was under active discussion, that one of its distinguished future members warned of the deliberate obscurantism of the alchemical tradition as one among the obstacles that lay in the path of such an endeavor: see the passage from Robert Boyle's "Skeptical Chymist" (1661), quoted by Eamon (1994: p. 341).

[26] The early cohorts among the academicians appointed were strongly pre-disposed to pursue practical questions at an abstract level, as Briggs (1991:pp.45-46) makes plain in the

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Long (1991: p. 916) concurs in the latter view, refering to project of the *Descriptions des arts et des métiers* as "an attempt to penetrate the secrecy that was commonplace among craftsmen."

Where that not enough, one further problem deserves notice. If Baconian ideology of open knowledge thoroughly pervaded the Puritan reformers gathered around the social philosopher Samuel Hartlib in London, and was therefore fervently embraced by the community of public-spirited new scientists in England who came under that influence, one would not expect to find that the older habits of secrecy could persist even within those circles. Yet, we have the counter-example of Isaac Newton, and other distinguished scientific figures such as Robert Boyle.[27] Newton never published a single scientific paper based upon the intense researches he devoted to the production of "philosophical mercury," and recorded in his manuscripts in abundant and painstaking detail. In the view of B.Y.T. Dobbs (1975), the leading modern historian of Newton's alchemy, that reticence hardly constitutes grounds for the surmise that Newton's extensive observations of "animated mercuries" amalgamated with gold were the products of a disordered mind. Nor is it right to suppose that having failed in an irrationally deviant enterprise, Newton's taciturnity regarding those investigations was due simply to has having had nothing of any interest, let alone public interest to disclose about the business. Instead, Dobbs adduces a piece of his correspondence with Henry Oldenberg, the Secretary to the Royal Society, in support of an alternative and simpler explanation for Newton's public silence about this aspect of his work. Namely, that he thought it was not safe to "go public" with alchemical knowledge.[28] Evidently, *mentalité* is a complicated

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following, characteristically ironic illustration: "Roberta, Huygens, Edmé Mariotte and Jacques Buot investigated the optimum size wheels for ploughs and carts, agreeing that big ones were preferable, but there is no sign of any rustic experiment to test this theoretical conclusion." Even had they been naturally impelled towards engaging in "industrial field research," such inquiries would not have proceeded without obstruction. In 1670 the Académie was ordered by Colbert prepare models of the machines in use at a ribbon manufacture in Chevruese and at a silk-twist manufacture in Paris, which he wished to have placed in his library. To accomplish this, Colbert also had to direct that those manufactories permit an *éleve* of the Académie (Antoine de Niquet [1641-1726], who had engineering training) to visit the premises in order to inspect and take measurements of the machinery. See Briggs (1991:p.45); on Niquet's background and career, see Sturdy (1995: pp. 129-133).

[27] Like Newton, Boyle was involved in a London-based circle of alchemists, and, as Eamon (1994: 341-342) notes, felt an obligation to keep the secret the knowledge that had been imparted to him in that context.

[28] Newton, who believed so strongly in the *prisca sapientia* -- an original wisdom or knowledge in the ancients which had been mostly lost to mankind -- also took the old alchemical writers at their word, accepting that the secrets they sought to penetrate involved "other things besides ye transmutation of metalls," things of an obscure nature, premature disclosure of which risked bringing "immense dammage to ye world." See Dobbs (1975: 14, 194-196).

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matter. Two quite opposed attitudes concerning the desirability of revealing one's scientific discoveries could and did co-exist within the mind of this paragon of the Scientific Revolution. One was linked with the old traditions of secret alchemy, the other with the open practices of the new “mechanical philosophy”.[29]

Although it would be only sensible to reject firmly any *wholly* materialist disregard for the power of intellectual currents to motivate alterations in social behavior and institutions, one must remain less than fully persuaded by the arguments of historians of science that the institutional innovations of open science followed simply and directly from a wholesale overthrow of the medieval outlook and traditions of secrecy; and that, fortuitously, they first came to be crystallized in the Italian academies that attracted noble patronage early in the seventeenth century, setting a model for more formal institutionalization in the Royal Society of London and the Académie Royale des Sciences, and subsequently in the many scientific bodies subsequently created in their image.[30]

Part of the problem with this mode of explanation is that it gives all the action to a few institutional reformers, portraying most among the new breed of "scientists" as passive participants who accept the new ideology, and docilely accommodate to the entailed revolution in their reward structure. Implicitly, they appear to altruistically offer themselves for the new, collaborative crusade for the improvement of society by means of the advancement of knowledge, even though this will oblige them to share potentially valuable information freely with others. A still greater difficulty is that it remains unclear why this reform movement should have swept the ranks of those who had been dealing in the secrets of nature, yet stopped when it reached those who dealt in the secrets of the technological and commercial arts. Rather than banishing secrecy and universally instituting full disclosure, two distinctive modes of organizing and motivating those the knowledge-seeking business had been brought into co-existence.

2.3 *Functionalism in historical context: the kernel of the argument*

Therefore, rather than viewing the organizational innovations associated with the ethos of open science as somehow deriving automatically from the intellectual changes represented by the new style of ‘scientific’ activity, the contention here is that the emergence of the norms of disclosure and demonstration and the rise of “cooperative rivalries” in the revelation of new knowledge, had independent and

[29] See the related conclusion of Figala and Petzold (1993: p.10-191) that Newton’s fusion – or was it a “straddling” -- of Renaissance and Enlightenment approaches in his theoretical designs was supported by his personal acquaintances with a natural philosopher and with a “hermetical philosopher,” upon both of whom he called for assistance during his later career in London.

[30] On this interpretation of formation of the pioneer public scientific societies, see Brown (1934/1967), Ornstein (1963).

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antecedent roots. Specifically, they were functional responses to heightened asymmetric information problems that had been posed for the Renaissance system of court-patronage of the arts and sciences. The pre-existing informational asymmetries between noble patrons and their *savants*-clients had been exacerbated by the claims of mathematicians and the increasing practical reliance upon new mathematical techniques in a variety of "contexts of application." Disclosure of both new knowledge and reliable techniques for solving practical problems offered a means for the mutual validation of claims to expertise, and public challenges and competitions among the mathematically adept provided a vehicle for building reputational renown. This is core of the thesis advanced in the following sections of this essay.

No historical development of any consequence, and certainly no significant new institutional formation is likely to explicable by reference to a single cause. There usually are a multiplicity of factors and conditions that play a variety of contributory roles, some of which are enabling or facilitating of the new departure and therefore may be held to have been necessary even though they did not have a "precipitating" influence. Moreover, persisting alterations in specific social, political or economic arrangements emerge and acquire sustaining momentum from the confluence of forces that already are present in the social system, from the mutual alignment (and re-alignments) of the actions of individuals and groups, whose respective concerns and intentions may at their basis be quite different, but who find – or suppose – that their distinctive purposes will be served by making common cause with one another at least for the moment.

Yet, it would far exceed the ambitions of this essay to comprehensively identify and depict all the conditions and forces that entered consequentially into the emergence of open science practices and the coalescence of a new ethos governing the manner in which Nature's Secrets were to be pursued. Instead, the kernel of the present argument focuses upon the configuration of precipitating causes, factors that in themselves may not have been sufficient to account for the changes, but whose addition to other, pre-existing (background) conditions enables us to understand why these institutional changes occurred when and where they did, and not earlier or elsewhere.

Thus, to make a very simple illustrative point, it could be said that egotism and vanity are attributes that would impel mathematicians and scientists, like others, to wish to call attention to themselves by displaying their accomplishments for the admiration of others. But, were these basic human qualities to be proposed as having had an important role in the emergence of open science practices, one would have to ask why they had not come into play long before the latter part of the sixteenth century, and in some societies other than those in Western Europe. That is not to say that the readiness to publicly proclaim one's achievements, and

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to claim novelty for intellectual accomplishments in philosophy or science or in the expressive arts, has been a constant in human history. Indeed, it is certainly relevant to acknowledge that the Renaissance had seen a notable shift in the conceptualization of human agency and in the legitimacy of drawing attention to one's personal accomplishments, including responsibility for the introduction of novelties. The merging of the traditions of Boethius, St. Thomas and Dante yielded a conceptualization of "Fortune" as a causal element in the universal design: it was the impulse that assists individuals to succeed at the right moment, an impulse that is seen to operate through the gifts of reason, will-power and free will that have been granted to man. To boast of one's Fortune in this sense might be thought immodest, but by the time that had become the public style of the *condottieri* in fifteenth-century Italy, to do so no longer was regarded as tempting the gods. Important though it may have been for what eventually was to come, this European cultural shift had occurred at rather too early a date to plausibly "account for" developments that only became visible more than a century later.[31] The same point applies also with regard to the new disposition of Renaissance theologians, philosophers, scientists and mathematicians to cease combing through the works of Aristotle in order to find quotable authority for their own constructions and conclusions.

Nevertheless, there were concurrent developments that, while not playing central roles in the explanatory argument presented here, have been identified as factors facilitating or abetting more open discourse among those seeking to uncover Nature's Secrets, and the wider public circulation of information about their discoveries and inventions. Two among these are assigned auxiliary parts in the present argument, and therefore appear on either side of the core structure in the schematic, summary representation provided by Figure 3.

On the Figure's left side, the prior development of the printing and publishing trades, following Gutenberg's innovations in the mid-fifteenth century, is recognized as a "background" condition that both facilitated and created incentives for scientific authorship, and hence for the broader distribution of claims to new discoveries. On the right-hand side of the Figure one finds

[31] Burckhardt (1929: p. 482) remarks that it was probably the *condottier*i in fifteenth century Italy who first ventured to boast loudly of their fortune.

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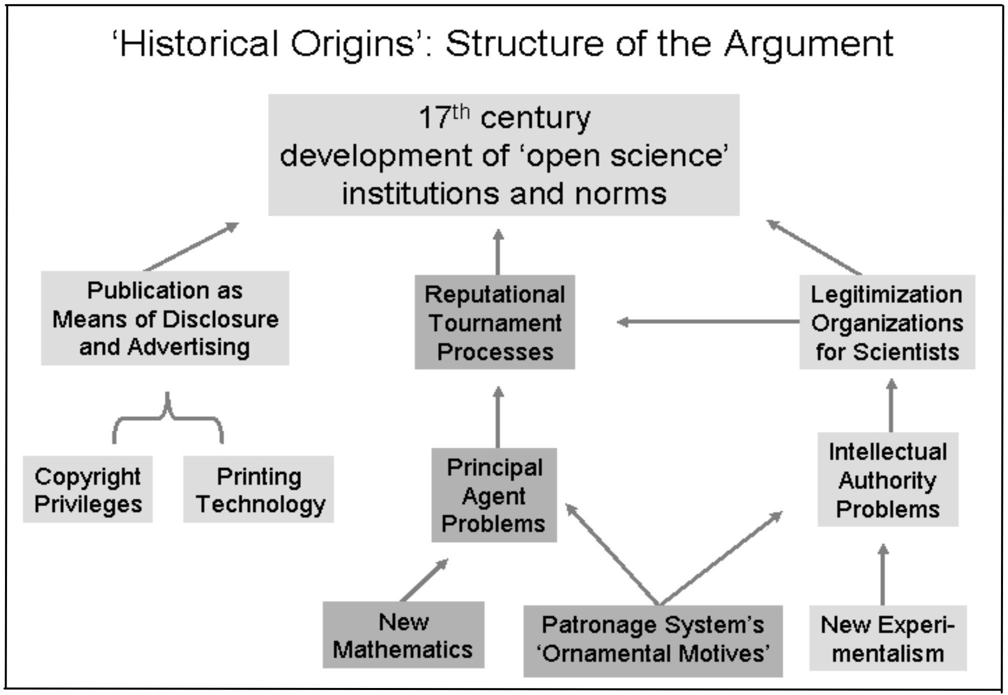

Figure 3

acknowledgement that the flowering of empiricism called for open demonstrations of experimental results, as well as displays of novel natural objects and artefacts; that this need could be accommodated by presentations before gatherings that included not only "scientific peers" but, importantly, members of local elites, whose presence and patronage furnished social legitimacy and a measure of material support for the proceedings of the early "scientific academies."

Although much can and has been said about the development of these two sets of historical circumstances, the points to be underscored here concern the respects in which neither of them reasonably can be assigned a precipitating role in the transformations that are of central interest. The patronage system, like the humanistic "academies" that lent themselves as venues for the early seventeenth-century assemblies of those infected by the new spirit of scientific inquiry, was not something new; these were pre-existent, well established institutional structures in western European society. Although the contributions of Mario Biagioli (1989, 1990, 1993) and other modern scholars attests persuasively to the significance of their intertwining in shaping the public presentation and reception of scientific discoveries during this era, they stop well short of having shown that

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the mode of "social legitimization" was a critical factor initiating the more open pursuit of Nature's Secrets.[32]

By contrast, the advent of printing with moveable type has been proposed considerably more boldly as a potent causal force in the scientific movement that "transformed the Book of Nature."[33] Elizabeth Eisenstein's (1979) work emphasizes the role of the printing press in the dissemination of uncorrupted classic texts, and graphic representations produced from engraved copper plates, thereby creating a stable body of common knowledge to which scientific practitioners throughout Europe who read Latin were able to refer. Her researches thus provide much evidence elaborating upon George Sarton's observations about the "special importance for the history of science" of Gutenberg's technological breakthrough. [34] But the bearing of the latter upon the transformation of scientific thinking during the two following centuries was substantially weakened by Eisenstein's recognition of the validity of Sarton's qualifying observations on the same subject. These directed attention to the decidedly "retrogressive" character of the surviving incunabula, to the tendencies of the early printers to perpetuate previous literary trends in imitation of the copyists and cater to the stultifying "inertia" of the texts that continued to be in demand in the universities, rather than promoting the circulation of more innovative scientific writings.[35] Eisenstein (1979: pp. 558-559) therefore concluded that "the effect of early printed technical literature on science and technology is open to question" – even though the advent of printing with moveable type undoubtedly had played an important longer-term

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[32] The early scientific societies and the question of the relationship between institutional evolution and the changing intellectual orientation and material requirements of early modern science are examined in greater detail, below, in Sections 5.1 and 5.3.

[33] See Eisenstein (1979: Part Three), and particularly Ch. 6, which opens with the assertion (p. 520): "The shift from script to print helps to explain why old theories [in science] were found wanting and new ones devised even before telescopes, microscopes, and scientific societies had appeared." But nothing in the pages that follow supports that particular claim, although they document the point that Latin-reading university professors (however reluctant they may have been to embrace the new mechanical philosophy of the early 17th century) had increasing commerce with artisan printers after "bookhands were replaced by typefonts and master printers took over from university stationers and scribes."

[34] See Sarton (1957: pp. 116, 119); Sarton (1958: pp. xi, 89) quoted by Eisenstein (1979: pp. 507-508).

[35] Eisenstein (1979: 512-514) on this point offers a strained exculpation of the early printers who simply "took over from the stationer and the 'slavish copyist' who handled materials that were in steady demand." They were "merely trying to continue, as earlier stationers had done, to serve a reading public that was oblivious to the kind of 'intellectual progress' modern scholars might regard as obvious now." This does seem to fall rather short of the point that might be called for in a thesis depicting "the printing press as an agent of change" in intellectual and cultural matters.

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role in the advancement of science. One added source of doubts on this score stemmed from the abundant evidence that although printing reduced scribal copying errors, especially in technical and scientific drawings and diagrams, it was long-familiar images -- such as the representation of the Ptolemaic system, and of Galen's theory of the circulation of blood in the human body -- that were most widely reproduced in illustration of "scientific texts." This had the quite perverse effect of reinvigorating and sustaining in the minds of literate laymen some of the very same notions about natural phenomena that contemporary scientific circles had rejected.

With more specific regard to the effects of the advent of the printing press in promoting scientific advances through open publication, the overall picture is no less ambiguous, as it presents a mixture of positive and negative elements. Certainly, something must be said of the contribution made by Renaissance printers in putting into circulation uncorrupted texts and engravings that made Euclid's geometry widely available, and in stabilizing symbolic notations that played an increasing part in broadening the circle of mathematical literacy. Particularly noteworthy, too, was the pioneering venture of into printing of Regiomontanus, the great Renaissance mathematician who in 1470 had been able to use the patronage of a wealthy Nuremburger in order to establish both an astronomical observatory and a printing press in that city.

But, as Eisenstein acknowledges, this was more a harbinger of things to come than a transformative event in late Renaissance mathematics and astronomy: trigonometric tables and series of Ephemerides continued to be turned out by the apprentices that had been trained in Nuremburg, along some technical treatises, but many of Regiomontanus' manuscripts remained unpublished for more than a half-century following his pre-mature death in 1476.[36] By contrast with this story of an emblematic "scientist-printer," it is the enduring influence of the teachings of Regiomontanus upon the subsequent "mathematization" of scientific and technological practices that will be seen to occupy a considerably more central position in the argument of the following sections.

3. Noble patrons, mathematicians, and principal-agent problems

The system of aristocratic patronage of creative activity -- the patronage of artists, architects, musicians and savants by bishops, kings, dukes and princes -- had become firmly rooted in Western Europe during the Late Renaissance.[37] As

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[36] Eisenstein (1979: p. 586-87) presents Regiomontanus as being "the first but not the last of a new breed" – in setting a model for "scientist-printers" who were to follow, and notices a number of professors at Nuremburg and Tubingen who undertook to continue his program of publishing ephemerides.

[37] With special reference to patronage of mathematicians, see e.g., Feingold (1984: Ch. VI);

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such, it constituted a key feature of the socio-economic context within which the Baconian program in Natural Philosophy emerged. This conjuncture was particularly important for the institutional development of open science.

3.1 ***Motivations for patronage -- the utilitarian and the ornamental***

Aristocratic patronage systems have reflected two kinds of motivations: the *utilitarian* and the *ornamental*. Most rulers have recognized some need in their domain or in their courts for men capable of producing new ideas and inventions to solve problems connected with warfare and security, land reclamation, food production, transport facilities, and so forth. The *potentes* among men have long sought the services of those who professed an ability to reveal the secrets of Nature, and of Destiny, and if one had the wits, there was always a living to be made in satisfying such needs. Moreover, there were many among the active participants in scientific advances during the sixteenth and seventeenth century who had not only the wits but the inclination to be actively engaged in one or another area of applied ("technological") practice.

Richard Westfall (2001: p. 329) reported finding that among the 631 individuals listed in the *Dictionary of Scientific Biography* as having been born between the decade of Copernicus' birth (the 1470s) and 1680, all but 118, or 82.6 percent of all those whose career details are available, were engaged in some area of applied technology, including medicine.[38] But this strong showing by those that were "technologically involved" reflects the presence of a very sizable medical community: of the 513 who were thus involved, fully 267 were physicians and practicing pharmacologists (see Table 1, in section 3.2). Thus, the group of 346 that had some involvements in the practical arts other than medicine constituted two-thirds (67.4%) of the entire number that had any engagement with "technological practices", and about 55 percent of all the leading scientists of the epoch.[39]

Westfall (1985), Biagioli (1989, 1990); Rose (1977).

[38] This period of observation closes (by design) on the eve of the decade that saw the publication of Newton's *Principia*. From Table 1 it is seen that of those 631 individuals, the career details of 621 were sufficiently clear to allow identification of their scientific domains and their areas of involvement with technological applications. The 10 obscure cases, however, must have been among the 118 for whom Westfall (1993: pp. 65-66) could not find an indication of involvement with technology. So, for consistency with the 621 total obtained by removing those 10 cases, Table 1 gives 108 as the number without any technological involvements. The percentages in the text that refer to the total group of notable scientists therefore take 621 as the denominator, and so are slightly higher than those based on Westfall's (2001) total.

[39] Westfall (1993:pp. 65-66) reports removing 238 individuals who were engaged in "medicine" (having earned medical degrees, pursuing medically related studies in anatomy, physiology, and surgery, or practising medicine for a livelihood), and another 29 practising

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The count of the total number of distinct technological involvements that are recorded for the 246 non-medical scientists who had any "applied" engagement with technology is 278, according to Westfall's data (2001:p. 329) – from which it is apparent that at least some among them must have found practical applications of their knowledge in more than one field. The distribution of specific instances by technological field, however, is remarkably uniform -- approximating 20 percent in the cases of each of the three classic *ars* (technical practices): hydraulic engineering, military engineering and navigation. Involvements with the new cartography (scientific map-making on mathematical principles) accounted for a larger (36 percent) share in the total. That should not be surprising, when one considers that production of more precise maps and plans itself was an art that came into play when hydraulic engineers planned irrigation systems, canals, and projects to re-channel and contain the flow of rivers. The same held when military engineers laid out their designs for fortifications, and when maps were prepared of coast-lines of continents and islands, and star-charts were drawn for the aid of navigators.[40]

The point here is that the scientists in this age who had become progressively more familiar with the new mechanical philosophy also were adepts in one, and sometimes in many of the more intensely "mathematized" practical arts. A mathematician of Johann Kepler's intellectual abilities, for example, was ready to make himself useful in mundane practical matters when the circumstances so demanded. Indeed, the challenge of practical problems could stimulate innovations of wider scientific importance. After he succeeded to Tycho Brahe's place in Prague in the service of the Emperor Rudolph II, one of Kepler's duties was the casting of horoscopes; but when he was visiting Linz, Austria, in 1612 -- a year in which the wine harvest was exceptionally big, he undertook to make improvements on the then crude methods used for estimating the volumes of wine-casks on the estate of his host, which yielded formal advances in volumetric techniques for solids of revolution.[41] The interests of patrons in such connections may be labelled the *utilitarian* motive.

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pharmacologists, leaving 246 who had some involvement in the other technological fields. There is a discrepancy between the implied total of 364 shown in Table 1 for the non-medical community and the figure of 394 that appears in Westfall (2001:p. 329). But the latter appears to be a misstatement, as it would arise from the reduction of the 631 total by removing only (237) physicians and not the 29 practicing pharmacologists. In constructing the top panel of Table 1, the data from Westfall (1993) has been followed, save for making the grand total 631.

[40] As small a proportion of the 246 as 6.5%, or about 16 individuals, would thus be able to generate the 32 extra instances of technological involvement (278-246) had they worked in cartography along with the 2 classic engineering fields.

[41] See Boyer (1985: 354-58). What puzzled Kepler was how the wine merchants were able to gauge the volumes of their casks, since the latter were of such variegated sizes and shapes. Kepler collected the results of his volumetric meditations--which led him to use the method of

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But, at least until the end of the fifteenth century any very direct "utilitarian" value to the elites of having in their service such intellects as those of the new breed of natural philosophers appears still to have been rather subsidiary to the status-enhancing patronage of individuals who were recognized winners of reputational tournaments. Kings and princes, and the occupants of positions of power more generally, have been consistent in displaying a desire to surround themselves with persons of creative talent whose achievements would enhance not only their self-esteem, but their public images, their reputations, and those aspects of grandeur and ostentatious display that serve to reinforce claims to political authority and elite status. Poets, artists, musicians, chroniclers, architects, instrument-makers and natural philosophers often have found employment as clients of aristocratic patrons, both because their skills might serve the pleasures of the court, and because their presence their "made a statement" in the competition among nobles for prestige. These dyadic patron-client relationships, which offering material and political support in exchange for uncommon services often proved to be precarious, too much subject to aristocratic whims and pleasures, or to the abrupt terminations that would ensue on the demise of a patron. Nonetheless, they were prominent during this era as part of a well-articulated system characterized by elaborate conventions and rituals that provided calculable career paths for men of intellectual and artistic talents.[42]

Those motivations for entering into a patron's role which reduce to symbolic acts of self-aggrandizement, will here be called *ornamental*. Motivations for the patronage of “savants” and “virtuosi” that belong in that category, however, must be understood to have been not less instrumental in character than the previously described “utilitarian” considerations. The public display of "magnificence,” in which art and power had become allied, was a stock item in the repertoire of Renaissance state-craft.[43] Prodigal expenditure on

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infinitesimals to find the volumes of various solids of revolution, including ones not even considered by Archimedes -- in a book that appeared in 1615 under the title: *Stereometrica doliorum* ("Volume-measurement of Barrels").

[42] See Biagioli (1990, 1993) for documentation of the assertion that Italian patronage relationships in the era of Galileo were elaborately structured, and far from idiosyncratic and "chaotic.”

[43] See Strong’s (1984) study of the alliance of “art” and “power” in public festivals marking politico-military “triumphs,” and their evolution into theatrical “spectacles” at court over the period 1450-1650. Particularly intriguing in the present context is the employment of Renaissance artisan-engineers to create dramatic effects in these spectacles. Strong (pp. 36-40) comments on the engagement of Brunelleshci and Leonardo in such activities in Florence as “indicating how a vigorous medieval tradition that made use of engineering to achieve scenic and mechanical effects in the liturgy and in mystery plays began to be expanded and developed under the impact of the study of the texts of classical antiquity.” The latter refers, specifically to the writings of Vitruvius on the use of machinery in festivals “to please the eye of the people,” and, considerably more

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achieving splendour was a princely virtue, a matter of central importance for both the Renaissance and Baroque court. This idea initially was articulated by the Florentine humanists who looked back to St. Thomas's praise of magnificence as a virtue, and through that source ultimately to the *Nichomachean Ethics* of Aristotle: "…great expenditure is becoming to those who have suitable means to start with…, and to people of high birth and reputation, an so on; for all these things bring with them greatness and prestige."[44] But it soon took on other, more distinctly political colorations.

Mary Hollingsworth (1998:13-14) underscores this point with forceful clarity in regard to Renaissance patrons of architecture:[45]

> "For them, art was not a statement of their aesthetic sensibilities; it was the prime vehicle for the display of their achievements, their status, political ambitions or commercial prowess, their religious beliefs and their civic pride. The magnificent palaces and their lavish decoration commissioned by governments, guilds and individuals were designed to demonstrate the wealth and power of their owners. Architecture, above all, was visible, permanent and expensive. They understood its value as propaganda. Pope Nicholas V [1397-1455] insisted that magnificent buildings were essential to convince ordinary people of the supreme power of the Church. In 1496 the Venetian diarist, Domenico Malipiero, reported that his government had begun to build a costly clock tower at a time of economic instability to demonstrate that the state was not bankrupt."

Over the course of the sixteenth century, the less durable furnishings of the court and even the ephemeral court spectacles and festivals aligned with the celebration of "magnificence" and "liberality" became instruments of ideological persuasion, projecting images of the harmony and legitimacy of a hierarchical social order based on political power. The display of magnificence was thus

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informatively, to the newly recovered mechanics of the School associated with Hero of Alexandria. On Renaissance engineers and engineering, see Gille (1966).

[44] Quoted by Strong (1984: p.22), who adds that in the cases of Catherine de-Medici and Charles I, the importance of demonstrating "regal magnificence" through the production of court spectacle was such that "their greatest creations came during periods when the crown was not only heavily in debt but almost bankrupt." They would have done better to follow the policy advocated by Machiavelli in Chapter 16 of *The Prince*, which recommends avoiding excessive "liberality" even at the risk of acquiring a reputation for meanness. This is quite in keeping with the general tenor of Machiavelli's counsel, that the ruler should be ready to act against the virtues that give men a good reputation in order to "preserve the state."

[45] See also Hollingsworth (1996: esp. Ch. 9) on the role of art and architecture as "state propaganda" in sixteenth-century Venice, aimed at impressing visiting foreign dignitaries with might of the Republic and reinforcing "the myth of harmony and stability at home."

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transmuted from an obligation of greatness into a means of reinforcing claims to prestige and authority in a politically and economically insecure world.[46]

From the patrons' point of view, therefore, most of the ornamental services that clients of the court might provide had "positional" value.[47] Although having an accomplished artist or and adept astronomer-astrologer in one's court was altogether a good thing, far better if they were persons of greater accomplishments and renown than the clients to be found at the courts of rivals. Of course, this was true as well of some among the utilitarian services. Possessing sophisticated military equipment and fortifications was good for security and warfare, but it was even better if one's preparations for armed conflict were more sophisticated than those of potential enemies. But, competition among patrons gave added strength to the ornamental motivation for the maintenance of "distinguished" clients. The pressures that Europe's ruling families felt to attach to their households and courts artists, musicians, savants and other "clients of distinction" (whose recognized special accomplishments and capabilities would enable them to render noteworthy services) were thus augmented by the instability of the political order and the proximity of numerous rival rulers and their courts.

If inventions and discoveries that met utilitarian needs were to be really useful, they would in many cases have to be kept secret or at least not advertised indiscriminately. This obviously applied in the case of some military devices,

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[46] In examining the economic and social conditions in Italy that underlay the growth in the demand for art during the "High Renaissance," Goldthwaite (1993: esp. pp. 48-49) notes that the degree of mobility within the elite ranks was far greater there than elsewhere in Europe; "[p]rincely patronage, so much more variable and better financed in Italy than elsewhere in Europe, was largely conditioned by the inherent instability of the state system arising from what Burckhardt called the illegitimacy of power….The sixteenth century saw much fluctuation in the fortunes of the aristocracy and a continual injection of new blood." Great expenditure on public building in Italy was not the exclusive prerogative of the lay and ecclesiastical nobility, for, as Goldthwaite (p. 188) points out: "Oligarchs as well as princes viewed the city as the natural setting for the physical expression of their authority….Urban renewal was one way to celebrate the consolidation of power by the local oligarch, even if under the auspices of an outside government….The establishment of the Venetian state in the later fifteenth century was the occasion for celebration by local elites; and the ruling class in Vicenza, Brescia, Bergamo, and other towns found an appropriate expression of the new arrangements in public building, often in imitation of Venetian models."

[47] The term "positional goods" has come to be used in contemporary reference to goods that are desired not for their intrinsic utility (satisfactions derived from their consumption), but because possessing more of these than other members of the society confers status satisfactions. Unique goods, the limiting case of commodities in inelastic supply -- such as a famous painting, or the lot on the housing estate that commands the most splendid view, or the highest salary in the organization -- exemplify commodities that are said to hold a positional value (in addition to other desired attributes).

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battle formations, and geographical knowledge concerning valuable trade routes. By contrast, it is in the nature of the ornamental motive that it must be fulfilled by disclosure of marvellous discoveries and creations, indeed, that the client's achievement be widely publicized. It was very much in the interest of a patron for the reputations of those he patronized to be enhanced in this way, for their fame augmented his own. Galileo understood this well, as was evident from the adroit way in which he exploited his ability to prepare superior telescopes for the Grand Duke of Tuscany, Cosimo II de' Medici: he urged his patron to present these to other crowned heads in Europe, whereby they too might observe the new-found moons of Jupiter that Galileo in the *Sidereus Nuncius* (March 1610) had proclaimed to be "the Medicean stars."[48]

A patron thus might display either of two quite different dispositions towards inventions and discoveries made by the creative talents in his domain or Court: maintaining reticence, if not insisting on outright secrecy in some instances, while actively promoting public disclosure in others. This dual disposition is understandable in the light of patrons' dual motivations for supporting the production and acquisition of new information about the material world. Two distinct and ultimately conflicting attitudes towards knowledge--identified above as the respective hallmarks of the regimes of "open science" and "proprietary technology"-- were therefore reflected jointly in the dispositions and behaviors of aristocratic patrons in the late Renaissance, and hence also those of their clients.

3.2 *Mathematics and the heightening of informational asymmetry problems*

But, like all prospective employers of specialized 'experts', these patrons faced a recurring problem: how were they to select from among the contending applicants for clientage? Men seeking patronage had naturally to display their skills, their accomplishments. This in principle could be done either publicly, with a view first to earning renown, and thereby obtaining employment. Or it could be done privately, within the restricted circle of the patron and those who served him. Yet, the public revelation of novelties and the restricted modes of disclosure on the part of inventors and discoverers seeking patronage were in each case

[48] See Drake (1957, 1978), Westfall (1985), and Biagioli (1990, 1993: Ch.1) for extensive discussion of the significance of Galileo's telescope in the context of the patronage system. Biagioli's (2000, 2006) subsequent revisionist argument regarding Galileo's withholding of information about the telescope's construction from other astronomers who were potential scientific competitors, lends added strength to the interpretation provided here: the peculiarity of the telescope (as was later true also in the case of the microscope) lay in its self-demonstrating nature *for patrons.* Consequently, the need for peer-validation of Galileo's expertise, and the disclosure of this knowledge about the instrument's construction, was corresponding attenuated – a point on which there is more to be said, below [in sect. 3.3b].

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responsive case to the incentives to advertise the would-be clients' talents. If a patron were always capable of evaluating the achievements of those seeking patronage and clients already in his service, matters would be relatively simple. Performance would be the guide for screening, hiring, rejecting, firing and retaining inventors and discoverers. Were the patron musically inclined and educated, for example, it is likely that he would have more fully formed ideas as to the special abilities he was seeking to patronize in choosing a court composer or musical tutor. Biographies of composers, artists, philosophers and the patrons are replete with examples where men of greater creative talent sought, in various ways, to come to terms with their patrons' too well articulated tastes and fancies.

By the end of the sixteenth century, however, the circumstances surrounding the patronage of *savants* no longer were so straightforward. The problem of asymmetric information is inherent in the principal-agent relationship between a patron and a client who possesses (or claims to possess) some special expertise. But the problems this could pose had grown obtrusively more serious in all the fields of intellectual and artistic endeavour that were touched by the evolution of Renaissance mathematics. With the rise of algebra, the geometry of conic sections and trigonometry, the new mathematics had fused classical with Arabic elements and been transformed into what for most patrons was a dauntingly esoteric area of expertise. The problems and methods with which the mathematical disciplines of sixteenth-century Europe were concerned were, indeed, far more esoteric and intellectually demanding than the ubiquitously useful humanistic pursuit that had gathered a growing number of influential proponents in the course of the fifteenth century.[49]

These changes, of course, had not come all of a sudden. They were heralded by the widening practical application of mathematics during the Renaissance -- to bookkeeping, mechanics, optics, surveying, cartography, and

[49] Though these developments are presented here as constituting a substantial discontinuity, both in epistemological terms and in their implications for the workings of elite patronage of learning in the Renaissance, it is appropriate to notice the existence of medieval precursors who had expressed appreciation for role of the mathematics sciences in "physics" (as well as for "experimentalism"). Leff (1968: Ch. 5), points out the intellectual indebtedness of the scientific revolution of the seventeenth century to the mathematical physics tradition deriving from Robert Grosseteste [1168-1253], which was established at Oxford and the University of Paris during the thirteenth and fourteenth centuries. Roger Bacon [c.1220-c.1292], who had studied at both places and lectured on Aristotle at Paris in his youth, much admired Grosseteste's mastery of the mathematical sciences to which subject (and optics) he devoted his best writings (see Lindberg 1992, pp. 222-229). The following translation of a Baconian dictum, clearly reiterating a view asserted by Aristotle, is the epigraph for Boyer's (1985: p.272) chapter on European mathematics in the Middle Ages: "Neglect of mathematics works injury to all knowledge, since he who is ignorant of it cannot know the other sciences or the things of this world."

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even art.[50] Indeed, an important feature that differentiated the art of the Italian and German Renaissance from medieval art was precisely the novel relationship that the former had developed with mathematics during the fifteenth century, through the formalized use of perspective for the plane representation of objects in three-dimensional space. This close connection, forged in the treatises on perspective by Leon Battista Alberti (*Della pictura*, written in 1453, was printed in 1511) and by Piero della Francesca (c. 1478), was no less evident in the work of Leonardo da Vinci (1452-1519). His *Tratto della pittura* opened with the admonition: "Let no one who is not a mathematician read my works."[51] Long anticipating the insistence of Galileo and his school that any comprehension of nature would have to be expressed in mathematical terms, in the third decade of the sixteenth century Oronce Fine', the first professor of mathematics at the new College royal in Paris, set out the following motto on the frontispiece of his *Protomathesis* (1532):

> "Since thoughtful Nature has created by number and measurement, and then enclosed each thing in its own weight; you will not be able to understand the proper causes of things unless you establish the numbers, and are at the same time a geometer."[52]

These expressions of the necessity of possessing mathematical knowledge were continuing a rhetorical tradition in Renaissance learning that had been established at least since the mid-fifteenth century. Perhaps the most notable among those who had presented a broad, humanist case for mathematical learning was Johannes Müller of Konigsberg [1432-1476], the great Renaissance mathematician who styled himself Regiomontanus.[53] In the course of the invited

[50] For introductory surveys of mathematics and mathematical publishing during the Renaissance (particularly, 1450-1575) see Sarton (1955: Ch. 151-165); Boyer (1985: Ch. XV); and Feingold (1984) on the changing place of mathematics in the curriculum of the English universities in this period.

[51] Quoted by Boyer (1985: p. 326). For further details on the mathematical aspects of the works of della Francesca, see Field (1993).

[52] Quoted by Keller (1985:p. 354), who points out that the Renaissance advocates of mathematics would also have been likely to look for numerological or astrological types of mathematical explanations for the world around them, whereas men like Galileo (and Mersenne, for that matter) found in mathematics the most certain and permanent form of earthly knowledge.

[53] On the work and influence of Regiomontanus, see Swedlow (1993), who points out the interpretation of the Scientific Revolution of the seventeenth century as the successful fusion of the "classical" mathematical sciences with the Baconian "experimental" sciences overlooks the more gradual Renaissance transformation of mathematics into something resembling its modern form. This was a development in which Regiomontanus, in whose work science was allied closely with humanism, played a signal role. Swerdlow (1993: 148-151) paraphrases, summarizes and

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lectures in astronomy that he delivered in Padua during April 1464, Regiomontanus issued the following rhetorical challenge to his listeners:[54]

> "To whom, by immortal God, are these worthy studies, not only useful, but even in part essential? For in the first place I pass over all mechanics and artisans, to whom geometry would give much direction if they had learned its precepts, whether they attempted to construct buildings, or conduct water, or transport heavy objects....Then I pass over the bankers, who greatly increase their wealth by skill in computation. I pass over the throng of armor bearers and soldiers, to whom geometrical contrivances are useful for hurling missiles and aiming siege engines....What finally shall I recall of the makers of musical instruments, to whom I have so frequently pointed out their error in dividing measuring devices?
>
> "I pass over all the mechanics in order that the utility of mathematics to liberal studies may be fully shown. Do you not know how frequently the Peripatetic Philosopher makes use of mathematical examples? Nearly all of his writings are fragrant with mathematical learning, as though no one who has neglected the quadrivium of the liberal arts may be considered capable of understanding Aristotle....Does it, I pray, seem to you of little significance that he places our [mathematical] sciences in the first degree of certainty, considering that only one who has expertly understood them is knowledgeable?
>
> "Also alluding to this, a certain Plotinus, an Academic, said, 'Would that all things were mathematical!' so seized by disgust was he with the other arts, which can surely be considered nothing but a mass of discordant opinions....Do not the followers of Aristotle today rather impudently tear at most of his writings with some risk, uncertain whether he intended to speak of names or of things? How many branches, differing both from each other and from their trunk, has this sect sent forth?....Consequently, the more leaders philosophy has, the less it is understood in our time....not even Aristotle himself, if he came back to life, would be thought adequately to understand his disciples and followers. This no one unless mad has dared to assert of our sciences, since neither age nor the customs of men can take anything away from them. The theorems of Euclid have the same certainty today as a thousand years ago. The discoveries of Archimedes will instil no less admiration in men to come after a thousand centuries than the delight instilled by our own reading."

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comments upon a portion of the April 1464 astronomy lectures in, "An Oration by Johannes Regiomontanus Delivered in Padua in a Reading of al-Farghani," which contains the marvellous passage quoted in the text (below).

[54] Unpublished translation by N. M. Swerdlow (1994), kindly supplied in private communication with the author.

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A century after Regiomontanus, the practical "fruit" of mathematics had become a commonplace of prescriptions for humanist educational reform – as evidenced by the fact that the Jesuit order, striving to maintain a leading position in contemporary education, had fully embraced this trend.[55] In the Society's rationale for its instructional curriculum in Germany, published in 1586, the case for mathematics was argued in what by then were well-rehearsed terms:

> "[Mathematics] teaches poets about the rising and setting of the stars; teaches historians the situation and distances of various places; teaches logicians [?*analytici*] examples of solid demonstrations; teaches politicians truly admirable methods for conducting affairs at home and during war; teaches physicists the manners and diversity of celestial movements, of light, of colors, of diaphanous bodies, of sounds; teaches metaphysicians the number of the spheres and intelligences; teaches theologians the principal parts of the divine creation; teaches jurists and canonists calendrical computation, not to speak of the services rendered by the work of mathematicians to the state, to medicine, to navigation, and to agriculture. An effort must therefore be made so that mathematics will flourish in our colleges as well as the other disciplines."[56]

Similar arguments were made (perhaps not coincidentally) in the same year by the German Jesuit mathematician Christoph Clavius [1537-1612], who prescribed the identification of mathematical passages in Aristotle as pedagogical exercises, and laid particular stress on the mathematical disciplines' importance for an understanding of "natural philosophy:"

> "Physics cannot be understood correctly without [the mathematical disciplines], especially what pertains to that part concerning the number and motion of the celestial orbs, of the multitude of intelligences, of the effects of the stars, which depend on the various conjunctions, oppositions and other distances between them, of the divisions of continuous quantities to infinity, of the tides, of the winds, of comets, the rainbow, haloes, and other meteorological matters….I omit an infinity of examples in Aristotle, Plato and their most illustrious interpreters which

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[55] As Dear (1988: esp. Chs. 3-4) has shown, traditional scholastic learning, including Jesuit teachings on the importance of mathematics, strongly influenced the thought of Marin Mersenne [1588-1648], the Mimimite friar who carried on a prodigious correspondence with Hobbes, Pascal, Descartes, Fermat and others during the 1630's and 1640's. Mersenne took the mathematical disciplines as the foundation for the existence of certainty in knowledge and therefore as the basis of his refutation of Pyrrhonian skepticism.

[56] Translation from 1586 *Ratio studiorum et institutiones scholasticae Societatis Iesu per Germanium olin vigentes collectae* (Berlin, 1887-1894:2: pp.141-142), presented by Dear (1988: pp. 44-45).

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can in no way be understood without some knowledge of the mathematical sciences."[57]

Two points concerning the critical position of the advances that were taking place in mathematical learning deserve special emphasis in the context of the present thesis. The first is that the claims of mundane practical utility as well as application in gaining insight into the natural order were not just “hype” by fifteenth and sixteenth century professors of mathematics: what they knew and knew how to create were powerful tools that would transform there major fields of technology whose roots reached back to antiquity – hydraulic engineering, military engineering, and navigation; and it would create an essential new, fourth field of application, cartography (map-making on mathematical principles) that directly impinged on the other three.[58]

Richard Westfall (2001), in a posthumously published paper, observed that those domains of practical application of mathematics became more thoroughly ‘mathematized” because “they pertained primarily to the existing structure of power and wealth” since they addressed matters of commerce, irrigation and the food supply, defence, and the delineation [by map-making] of jurisdictions within states and external borders where they confronted others.”[59] Following this line of explanation further, Westfall argued that the seventeenth century flowering of European mathematics -- which saw “the most creative period in mathematics since the age of classical Greece,” was in some important part what economists would characterize as an endogenous, derived response to

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[57] English translation in Dear (1988: p. 45), from Christopher Clavius, “Modus quo disciplinae mathematicae in scholis Societatis possent promoveri,” *In Monumenta paedogagical Societatis Iesu quae primam Rationem Studiorum 1586 praeccessere* (Madrid 1901: p. 472). See e.g., Sarton (1955:p.138); Boyer (1985: pp. 334, 353) on Clavius, who taught mathematics at the Collegio Romano. He produced a long string of textbooks used by the Jesuit colleges throughout Europe -- covering arithmetic, algebra, geometry, gnomonics, the astrolabe, the *computus* (abacus), trigonometric and astronomical tables -- and advised Pope Gregory XIII on the reform of the calendar in 1582. .

[58] The development of the techniques of map-making in the sixteenth century occurred virtually without any antecedent traditions in Europe. Westfall (2001: pp.325-327) points out that there were few proper maps in Europe before 1500, perhaps only 30 are known in England; that the first printed edition of Ptolmey’s *Geography* did not contain any maps, and that for most readers of that work the maps that appeared in the second (1477) and subsequent editions were “almost certainly the first maps they had ever seen.” As for the medieval *mappaemundi,* according to Woodward (1987) they were more akin to theological documents than maps: of the 1,100 that are extant, fully 900 appear as illustrations at the beginning of manuscripts – the work of copyists and illuminators, rather than cartographers.

[59] Westfall (2001: p. 330) continues in this vein, remarking: “It is no surprise then that the same men who patronized art, music, and literature also patronized those who could provide essential technical services.”

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the to the growth of demands for new technologies in precisely those fields which were particularly amenable to the application of more powerful tools for mathematical analysis and computation. Refracted through the interests of elite patrons' practical concerns for technical services in those domains of application, this shift in demand shaped training and careers in mathematics in ways that further reinforced, and eventually accelerated the cumulative creative advance not only of that field, but the whole of the new "mechanical physics".[60]

In a positive feedback process such as the one that has been extrapolated here from Westfall's (2001) "externalist" argument, it is difficult to identify whether the initiating impulse really had come from the derived "demand pull" side, rather than from a prior shift in the supply of more useful methods of analysis and calculation. The latter impulse, having arisen from Renaissance mathematicians' absorption of learning from the Arabic world, has been emphasized by the more traditional, "internalist" account of these historical developments. But perhaps solving the identification problem is not so important in this context, and rather than becoming entangled in the externalist vs. internalist controversy, out recognition of the growing practical utility of mathematics in during this era should direct the discussion to a less familiar and yet more critical issue: Why should the possessors of these mathematical techniques have begun to reveal to others the very tools that increasing were perceived to be so helpful permitting them to offer the range of valuable technical services that interested powerful patrons? Why not reveal the applications while keeping secret the methods that yielded those valuable results?

Clearly, this kind of knowledge did not fit easily into the medieval formula of the *prisca sapientia* -- an original wisdom or knowledge in the ancients which had been mostly lost to mankind. Rather than being the wisdom of the ancients or the word God had imparted to Adam, the algebra and its applications quite evidently was a novelty. That could be made part of a larger explanation in terms of an intellectual movement toward a culture of "openness," which would be distinct from, yet reminiscent of the previously examined thesis advanced by William Eamon. It would derive some force from a second feature worth noticing about the presentation of Renaissance mathematics as an integral element of humanist knowledge: it's effect was to bring early modern mathematicians within the larger orbit of idealized social norms for literary and philosophical transactions among a free, egalitarian, trans-national community of scholars that was associated with "the Republic of Letters" -- a metaphor that

[60] Schoellman (2004), drawing upon evidence from Feingold (1984), emphasizes the role of the incentives for training created by the eventual emergence of well-delineated career paths – including mathematical tutoring for the young in the households of elite patrons – as an important supply side factor in the delayed but rapid rise of English mathematics that marked the 17th century.

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during the fifteenth century was growing in currency and becoming more and more elaborately articulated.[61] By the close of the seventeenth century, the virtues of this marvellously rational and enlightened realm were being proclaimed in terms such as those employed by the author of "News from the republic of letters," the French literary critic Pierre Bayle [1647-1706], in the following translation by Ian MacLean (2006) of a passage from Bayle's *Dictionnaire historique et critique* (1696):

> "The only dominion ('empire') recognized [in the Republic of Letters] is that of truth and reason; under their auspices, war can be innocently waged on anyone [....] Everyone is both sovereign and answerable in law to everyone else. The laws of society have not prejudiced the independence of the state of nature, in relation to error and ignorance; every individual has in this respect the right to take up arms and can exercise this right without asking permission from those in government…The criticism levelled at a book tends only to show that its author lacks this or that degree of insight (enlightenment); but even with this lack of knowledge, the author can enjoy all the rights and privileges of this society, without his reputation as a man of honour and a good citizen suffering the slightest hurt; in making the errors in a book known to the public, a critic is arrogating nothing to himself which affects the Majesty of the State [i.e., the *Respublica Literaria*].

In this and other, similar effusions, one may see readily recognizable allusions to the norms resembling those of communalism, universalism, scepticism, and openness. But clearly, this was an overly idealized depiction of the state of affairs in the emergent "Republic of Science." Indeed, MacLean (2006: pp. 11-12) finds that depictions such as Bayle's glossed over the parochialism of many of the networks of correspondence, the fissures that cropped up along lines of national factionalism, the sometimes acrimonious disputes, personal affronts, and tones of social condescension, all of which were to be found in the contemporaneous "Medical Republic of Letters" that emerged in Europe during the century preceding the Thirty Years War.[62] This suggests

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[61] Ian MacLean cites 1417 as the date of the earliest recorded appearance of the phrase '*respublica literaria'* – in a letter by Francesco Barbaro, praising Poggio Bracciolini for having made available lost works of classical antiquity to "a scholarly community united by 'the bond of letters' transcending frontiers and generations" (see MacLean, 2006: pp. 3-4). I am grateful to have been able to draw upon the new material in this unpublished manuscript, kindly communicated to me by Dr. MacLean.

[62] Modern use of the phrase "Republic of Science," introduced by Michael Polanyi (1962), transparently evokes the broader humanist metaphor of a *respublica literari*. MacLean's recent enquiry into the nature of the "Republic of Medical Letters" in the sixteenth century rests on works preserved by the post-1520 flourishing of the genre of printed collections of "letters" that

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that intellectual enthusiasm for the metaphor of a free community engaging in the pursuit of truth by the rational means of open, impersonal deliberation and debate was limited in its power to overcome the baser impulses and narrower interests of the human beings that would have to be involved in translating the vision into a reality. An approach that acknowledges a greater role for self-interested actors therefore seems a more promising route towards accounting for the willingness of the mathematically adept scientists of this epoch to begin to relinquish a secret hold on their techniques, and ultimately on Nature's Secrets as well.

3.3 *Coping with informational asymmetries in the patronage system*

By the late sixteenth century an unprecedented problem for would-be patrons of the *virtuosi* had been fashioned by the continuity between the rhetoric of the humanist educational tradition and the increasingly advertised importance that a newer and more sophisticated brand of mathematics held for students of natural philosophy.[63] As has been seen, statements about the natural world had become so mathematical and technical in their form that neither the typical head of a noble house, nor his advisors, were prepared by virtue of their own talents and training properly to judge the quality of work associated with the new learning.[64] This obviously would be a special concern where the motivation for patronage of natural philosophers and other savants was primarily of the ornamental sort.

But, that was not the limit of the problem. Whether or not advanced mathematical skills were really required to satisfactorily perform most of the utilitarian services, or to provide that part of humanist instruction to the children of elite families, was beside the point. How was an inexpert principal to know how much his prospective agent needed to know? For those would- be patrons who could afford the expenditure, was it not best to try to employ the most competent? If the exacerbation of these informational asymmetries thus posed problems for patrons, they would perforce be problematic also for the

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were composed by the representatives of the new breed of academic physicians. Many of the medical writers were among the leading scientific figures of their epoch, as may be seen from Westfall's data, as in Table 1 below, and some -- like Girilamo Cardono (of whom more will be said, below) also were stellar mathematicians.

[63] See Keller (1985), especially on the program and rhetoric developed on behalf of a mathematical education by a number of western European writers during the 1570s and 1580s - including Nicolo Tartaglia, and G. F. Peverone in Venice, Pierre de la Ramée (Ramus) in Paris, and John Dee in London,

[64] Feingold (1984: 192) concludes that by the seventeenth century the vast majority of England's upper classes "pursued the mathematical sciences as one more accomplishment in a many-faceted, but by no means profound, education." On the patronage and social position of mathematicians in Venice and Florence at the end of the sixteenth century, see Rose (1977) and Biagioli (1989), respectively.

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intellectually talented individuals – if they hoped to enjoy such material support, social status and political protections as noble patronage might provide.

The foregoing argument's double premise is that accomplished scientists possessed of advanced mathematical learning were extensively engaged in quite practical technological (non-medical) pursuits and that during this era they typically found support in the patronage system. If this needs quantitative confirmation, it is to be found in Table 1, which presents results derived from a study of the accomplishments of the 1470-1680 birth cohort listed in the *Dictionary of Scientific Biography,* an undertaking initiated by the late Richard Westfall.[65] Among the 621 notable scientists whose career details were not too obscure to be useful for the analysis, 195 individuals have been identified as having made some contribution to mathematics -- approximately 31.4 percent of the total, and well more than a majority (53.6%) of the 364 leading scientists who were neither physicians nor pharmacologists.[66] The latter group are referred as the "non-medical" scientists in Table 1, were it is seen that they comprised almost 57 percent of the total number of notable scientists.

These scientist's involvements with technological fields was extensive, as has previously been noted: from the compilation of data in Table 1 it is found that such involvements were present for more than two-thirds of the non-medical (including pharmacological) contributors (67.6%). From that one might correctly surmise that the individuals contributing to the mathematicians' tools-kit in this period would figure prominently among the ranks of the scientists who also were applying their mathematics for one or another practical purpose. Nevertheless, the degree to which that was the case is quite striking: the field-by-field comparisons in Table 2 show that the frequency of involvements in each of the indicated five "main technological areas" -- on the part of the notable scientists who had made mathematical contributions of one form or another -- approached

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[65] Westfall (2001:pp.328ff) reports finding 631 biographies, 1 more than was reported in Westfall (1993), his first discussion of this study, and Table 1 (below) starts from the larger number. But it then discards 10 individuals whose career details are obscure.

[66] Westfall's (2001) text mentions 205 "mathematical contributors" but a recalculation, whose results are shown in Table 2, below, yields 195 as the number that is consistent with the 621 "useable" biographies. In any case the difference between the percentages (31.4% and 32.5%) is inconsequential.

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Table 1: Extent of Notable Scientist's Involvement in the Practical Arts During the Sixteenth and Seventeenth Centuries

Major areas of practical application of scientific knowledge
by the 631 individuals born during 1470 - 1680
who are listed in the *Dictionary of Scientific Biography*

Main areas of scientific specialization & involvement with practical applications	Number of Individuals	As a percentage of scientists with known careers: in all scientific areas	in the non-medical sciences
Total birth cohort	**631**		
Those with obscure career details	10		
Scientists with known careers:			
Total in cohort	**621**	**100.0%**	
Total in Medical Sciences	**267**	**43.0%**	
Physicians	238	38.3 %	
Practicing pharmacologists	29	4.7 %	
Total in Non-Medical Sciences	**364**	**56.8%**	**100.0%**
Involved with some techno-logical application:	**246**	**39.6%**	**67.6%**
in 4 "mathematized" areas*	98	17. 1%	29.1 %
in instrument-making	8	2.2%	3.2 %
in other areas	140	22.5%	38.5%
Without significant techno-logical involvements	**108**	**19.0%**	**32.4%**

***Notes:** Military engineering, hydraulic engineering, navigation, and cartography.

Sources: Underlying data from Westfall (1993, 2001). See text and footnotes for details.

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54 percent among the non-medical" scientists. When a more exacting standard is imposed, taking only the mathematical contributors whose accomplishments in that regard were neither minor nor incidental to their career and livelihood, it is seen from the upper panel of Table 2 that almost 73 percent of the latter group were involved in the five main technological areas where mathematical methods were being applied.[67]

The lower panel of Table 2 provides more a detailed view of what these mathematicians were doing in their pursuits of practical applications, by examining the distribution of the 166 recorded instances of technological involvement. Due to the presence of some who worked in multiple domains, these average slightly more than one instance per career mathematician. Matching the 'instances of involvement" in each of the five areas of application against the number of career mathematicians identified as having been engaged in that area, the average mathematician's propensities for applying themselves in different technological fields is seen to have be quite uniform across the five main areas. Among those contributing in military and hydraulic engineering and cartography, the proportions are essentially identical at 0.61; among those who worked primarily in the area of navigation techniques the figure is only a bit below (at 0.59), and the proportion was not much lower (0.55) for the contributors to scientific instrument-making.

It is appropriate to focus upon the group of "career mathematicians" to confirm that the mathematically adept scientists who might make themselves useful by applying their knowledge in practical (utilitarian) pursuits would have faced the other side of the same problem of informational asymmetric that confronted their prospective patrons. The career mathematicians were the preponderant number among all the "mathematical contributors," and those for whom elite patronage would have been an especially important source of livelihood. Therefore it is reassuring to find that their biographical details reveal that 104 of them, or 71.2 percent, actually did find one or more patrons in the course of their careers.[68]

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[67] This follows Westfall's suggestion that such a standard is appropriate in identifying "career mathematicians" from this source. The excluded individuals in this exercise comprised 30 whose contributions were deemed by Westfall (2001:p. 330) to have been of an "incidental" nature, and another 30 whose career details are either not known, or known sufficiently to indicate that mathematical expertise was irrelevant or at most only incidental to their livelihood. Two among the mathematical contributors identified by Westfall were physicians -- the famous Cardano surely being one of them. So, only the other physician was discarded, yielding a total of 146 "serious career mathematicians." Among the 30 cases omitted on the grounds that their mathematical contributions were "incidental" to their careers and livelihood there were 10 nobles, 9 scholastic philosophers, and the other physician.

[68] Notice that these results, when taken in conjunction with the details given in the preceding footnote, imply that of the total number (185) of "notable but non-noble scientists" who

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Table 2: Extent of Participation in the Practical Arts by Mathematicians among the Notable Scientists of the 16th & 17th c.

Technological involvements of individuals born between 1470 and 1680 whose entries in *The Dictionary of Scientific Biography* notes their having made some mathematical contribution

Scientists with known careers in non-medical sciences who made any mathematical contribution*	Number of individuals	Contributors' share in the sub-population of: all non-medical scientists	"career mathematicians"
All mathematical contributors	**195**	**53.6%**	
"Career mathematicians"			
Total group:	**146**	**40.1%**	**100.0%**
Involved in the 5 main technological areas of applied mathematics	**106**	**29.1%**	**72.6%**
Not involved in mathematical technologies	**5**		**3.4%**

	Number from: all non-medical scientists	"career mathematicians"	Percentage from the "career mathematicians" (col.2) / (col.1)
Recorded involvements in mathematical technologies:	**285**	**166**	**58.2%**
Military Engineering	57	35	61.4 %
Hydraulic Engineering	56	34	60.7 %
Navigation	56	33	59.2%
Cartography	101	56	60.9%
Scientific instruments	14	8	55.4 %
Instances of involvement in mathematical technologies per scientist in the whole group:	0.783	1.137	

* *Notes*: "Non-medical"scientists exclude physicians and pharmacologists, as in Table 1. Career details are obscure for 10 of 205 scientists found by Westfall (1993, 2001) to have made mathematical contributions.

Source: Complied from underlying data in Westfall (2001: pp. 329-330). See Table 1 for non-medical population count. See text and footnotes for further details.

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are identified has having made any sort of mathematical contribution almost 56 percent were engaged in some way as clients within the patronage system.

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Given this situation, how were elite patrons to have been reasonably assured that they weren't taking into their service as client a putative mathematician who was an incompetent, or worse, a charlatan, and whose eventual exposure as such would only reflect badly upon his own repute among rival patrons of the arts and sciences? Even if the public embarrassment of such an association could be tolerated, there was a potentially costly risk of putting large military or hydraulic engineering projects in the hands of persons who might lack an adequate command of those lately more "mathematized" arts. To re-state in modern terms the problem as it appeared from the other side of the "market for patronage: How were court mathematicians and natural philosophers who claimed to draw heavily upon their command of mathematical skills to be screened for ability? Reflexively, how were men of "science" and mathematics to obtain credentials that recommended them for employment, when aristocratic employers were becoming increasingly incapable of directly assessing their competence before commitment their own prestige or significant material resources to the practical projects they might ask a client to undertake?

(a) Challenges and Public Contests, Reputational Competition and Priority Disputes:

Because this central problem of the patronage system was rooted in the growing asymmetry of information that the new mathematical learning associated with the Scientific Revolution had created between client-agents and patron-principles, it might well be expected that modes of coping with it would first begin to arise in that arena. It is particularly pertinent, therefore, to note that from the mid-sixteenth century onwards -- which usually is taken as the start of the era of modern mathematics and its practical application -- there was a perceptible quickening of communications among Europe's adepts in algebra. This development came increasingly to feature open discussions in correspondence that conveyed claims of new results and information about new techniques, as well as solutions of long-standing puzzles, and newly posed mathematical conundrums. Although the circulation of printed books and pamphlets eventually came to play an increasing important role in the broadcast dissemination of scientific and technical information -- including mathematical methods, the beginnings of this trend are not ascribable to the agency of the printing press. Personal letters were and would long remain the favored medium for conducting knowledge transactions at a distance, and, in the course of the sixteenth century, inter-personal correspondence across Europe had become swifter and more reliable. Private letters were conveyed regularly by diplomatic couriers and the postal dispatch systems that began serving the principal cities and the aristocratic courts.[69]

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[69] Merton (1938 [1970]: pp.216-217) has emphasized the importance of the system of

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Public boasts and the issuing of challenges created occasions for the arranging of public contests among the Renaissance mathematicians.[70] One such event was the famous competition in the 1530's, involving the algebraic solution of cubic equations in which Niccolo Tartaglia [c.1499-1557)] convincingly demonstrated his command of a technique far more powerful than the method that used by Antonio Maria Fior. The latter, a mathematician now judged to have been of only "mediocre" ability, nonetheless had been making something of a name for himself on the basis of being able to find the real roots of a simple form of the cubic.[71] The method that Fior employed, however, was not one of his own devising; it been discovered by his mathematics professor at Bologna, Scipione del Ferro (c.1465-1526), who not did make it public but when on his deathbed had passed the secret to Fior, his student. According to Tartaglia's account, it was the news of the existence of an algebraic solution of the cubic that stirred his interest in finding a solution-method for this problem. His announcement of that intention led in 1535 to the arrangement of a contest between them. Tartaglia scored a complete victory, having been able to devise a means of answering all 30 of the questions that were set for him by his opponent, who was completely stumped by the 30 questions that Tartaglia posed in turn, because they involved the more general cubic form. This public triumph soon brought the winner an invitation to visit Girilamo Cardano, the famous physician and mathematician. Cardano, having held out prospects of a patronage arrangement in exchange for knowledge of Tartaglia's method, secured it in due course, and went on to publish it in his *Ars Magna* in 1545.[72]

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stagecoaches and postal services that was in existence by the end of the seventeenth century in facilitating frequent and rapid social communications among scientific "correspondents". Access to these channels of communication was among the advantages that came with a position at one of the greater princely or ecclesiastical courts.

[70] See Boyer (1985: pp. 311-12, 341) for examples of "contests" involving famous mathematicians of the sixteenth century. Quite evidently, this was a continuation, albeit on an increasing scale, of earlier practices involving open competitions among proponents of rival arithmetic methods. See, e.g., Bell (1937); Boyer (1985: pp. 309) for a depiction of a contest between an "algorist" and an "abacist" in the *Rechenbηcher* (1529) of Adam Reisse, the celebrated German algebrist.

[71] This was what contemporaries called the "*cosa* and cube" problem (the *cosa* being Italian for "thing," i.e., the unknown quantity, x), and referred to in modern mathematical histories as the "depressed cubic" equation: $x^3 + ax = b$, where $a, b > 0$. . The details of the ensuing Tartaglia-Fior contest are drawn from the account by given by Boyer (1985: pp. 311-12), and O'Connor and Robertson (2004) – adhering to the latter's chronology.

[72] See Boyer (1985: 310-312). Cardano apparently intimated that he would arrange for Tartaglia to meet a prospective patron, and, by swearing an oath to keep the secret which Tartaglia had held for four years after his triumph over Fior, induced him to reveal the technique for solving the cubic ($x^3 + ax^2 + bx = c$) by reducing it to the "depressed cube" problem. It is significant that

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The tradition of open challenges involving ever-more sophisticated mathematical problems flowered with the progress of the discipline as the century drew to a close. In 1593 a Belgian mathematician, Adrian van Roomen [1561-1615] challenged anyone to produce a solution to an equation of the forty-fifth degree, and the ambassador from the Low Countries to the court of Henry IV, learning of this, taunted his hosts that France had no mathematician capable of responding to the challenge from his countryman. National honor was thus at stake, as well as a chance for a French mathematician to display his talents in a very public, high status arena. The opportunity was seized by the gifted Francois Vieté, who famously saw a trigonometric transformation that would enable him to find the positive roots – which he proceeded to do with such speed that van Roomen, greatly impressed, felt obliged to pay him a visit.[73]

Occasions of this kind became increasingly frequent in the following period, as *virtuosi* issued public challenges for others to find answers to problems which they had already solved — a practice in which Galileo and others had indulged, but which subsequently was brought to a high art by Pierre de Fermat.[74] Although Fermat was ostensibly unmotivated by simple aspirations for employment as a mathematician, being securely settled in Toulouse as a lawyer and councillor to the Parlement there, he was hardly indifferent to the fame which he was quickly to acquire -- as one of the leading mathematicians of his time, once he had been brought into contact with Marin Mersenne. In the course of an

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when Cardano subsequently published the method, and his general solutions to the family of cubics in the *Ars Magna,* he acknowledged having learned of it from Tartaglia. But, he pointed out that he had released himself from his oath of secrecy to Tartaglia by virtue of discovering Scipione del Ferro's own description of his solution method – on a visit he made with Lodovico's Ferrari (Cardano's secretary) to Bologna in 1543. There he was able to examine the notebook of Scipione that had come into the possession of the latter's son-in-laws Hannibal Nave, a mathematician who in 1526 had taken over his lately deceased father-in-law's lecture course of at the University of Bologna, along with the alias "dal Ferro." I am indebted to Todd Schoellman's (2004) mention of Cardano's scrupulousness in explaining his violation of his oath of secrecy, although, for this aspect of the oft-retold Tartaglia-Cardano affair I have relied upon O'Connor and Robertson's (2004) account, the details of which appear to differ from those in the source cited by Schoellman.

[73] Boyer (1985: pp. 334-342) relates this story, noting that although Vieté made deep, generalizing contributions in the fields of algebra and trigonometry, which moved the mathematics of his day closer to the modern conception of the discipline as a form of reasoning rather that a collection of specific solution-tricks, he did all that in his spare time. He first trained as a lawyer, and had a career as a parliamentarian and in the diplomatic service. So his acceptance of the public challenge should probably be ascribed to the honor of the occasion (and ego-gratification?), rather than prospects of material benefits. But ego-gratification often is a comparative proposition, and so that motivation could gain strength for the increasing frequency with which others, with material gain in mind, were entering public competitions.

[74] On the period in which Descartes and Fermat dominated the mathematical scene in France, see Boyer (1985: Ch. 17). O'Connor and Robertson (2004b).

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ensuing extensive correspondence with Merseene, a Minimite friar in Paris, Fermat brought a succession of challenges (and, as others soon came to understand, reminders of his talents) to the attention of the circle of mathematicians and physicists with whom Mersenne maintained an active correspondence.

In a setting in which public reputations for intellectual accomplishments began to matter as a signal of and individual's capabilities, disagreements about who had done what, and who had done it first, were more likely to become serious matters -- and not only for the individuals immediately involved. Referees would need to take a position on such matters. Interestingly enough, Robert Merton (1938/[1970]:p. 169) remarked that the latter part of the sixteenth century appears to have seen the first outcropping of numerous public disputes among mathematicians and scientists over claims to "priority" in discoveries or inventions, some among which became sufficiently vehement, protracted and marked by personal rancor to have attracted much wider and enduring notice.

Many of these squabbles over priority arose from the conjunction of essentially independent developments of instruments, or of observational findings. Their clustering in this period could be attributable to the well-known phenomenon of "multiplicity" that has been a documented pattern of scientific advance, with "simultaneous discoveries" being made on particular topics and in "hot" field where new, fruitful ideas and strategies of inquiry are "in the air."[75] Promising ideas get into the air through social communications, however, so that improvements in the speed and range of messages passing by word of mouth from travelling scholars, or by the greater volume of correspondence that had begun to flow through improved postal systems in this particular era, also may have been a significant contributory factor. The continuing increase in the density and ramification the interconnected "circles of correspondence" among scientist and mathematicians in the seventeenth century reinforced these developments. Moreover, by contributing to the growing momentum of the "internal" intellectual development of the mathematical disciplines, the quickening flow of social communications among scientists perhaps further reinforced the participants' motivation to establish claims to enduring fame. [76]

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[75] The sociology of controversies over scientific priority is treated famously by Merton (1973: Ch. 14 in his classic 1957 paper ["Priority in scientific discovery"]; also pp. 334, 340). See Lamb and Easton (1984: Ch. 11) on the statistical evidence and causes of "scientific multiples," specifically in connection with the genesis of priority disputes. Dasgupta and David (1987, 1994)] discuss the economic significance of priority and priority disputes in modern scientific research settings.

[76] Boyer (1985: p. 367) connects the reorientation toward the internal development of mathematics with the formation and expansion of an interconnected community: "not since the days of Plato had there been such mathematical intercommunication as during the seventeenth century….From the seventeenth century, therefore, mathematics developed more in terms of inner

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Disputes are more likely to become acrimonious, however, when personal dignity and public honor become engaged, which frequently was the case where one of the parties was aggrieved by the perception that there had been a breach of trust on the part of a confidant; or disconcerted by the unexpected and pre-emptive announcement of "their" discovery or invention – claimed by another participant (however distantly situated) in the growing networks among corresponding scientists. Emblematic of the developments in this era were the "sordid controversy" that erupted in the 1540s between Nicolo Tartaglia and Girlamo Cardano (Cardan), and their respective supporters, and the bitter dispute over priority in formulation of the geo-heliocentric system that started in 1584-85 between Tycho Brahe and the mathematician Nicolai Rymers Ursus.[77] These were notable precursors of the well-known seventeenth-century wrangles over priority involving distinguished controversialists, in which Galileo disputed with Christopher Schneider over who had made the first observations of sunspots; and Newton engaged Robert Hooke in battles for priority of discoveries in the fields of optics and celestial mechanics, before entering into his protracted and aggravating fight with Leibniz over the invention of the differential calculus.[78]

(b) An opportunity for visual confirmation of wonders--the telescope and Galileo:

For patrons seeking to judge the qualifications of potential scientific clients, there was an alternative to seeking reassurance in the "collegiate reputation" the latter already had established within the community of established client-scientists. That, of course, would require forming their own judgements, by inference from the patrons' verification of the candidate's demonstrated expertise, or salient accomplishments that could be ascertained without having a command of the very same sort of esoteric knowledge that the would-be client claimed to possess. (Otherwise, there is no escape from the infinite regress problem: who verifies the

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logic than through economic, social, or technological forces, as is apparent particularly in the work of Descartes…"

[77] On Tycho and Ursus, see Thoren (1990: pp.255-256, 260-261, 390-96). Boyer (1985: 310-312), applies the adjective "sordid" to the Tartaglia-Cardan affair, but notes that the latter did add insult to injury, by justifying having broken his pledge to never reveal Tartaglia's method for solving the cubic -- on the grounds that Tartaglia had not been the originator but had received a hint of the general solution from another source. This allegation remained unsubstantiated, although, as Boyer notes, Tartaglia himself was not above publishing the results of other mathematicians without crediting them. See O'Neil (1991) for an interesting discussion in of the ensuing extended controversy between Tartaglia and Cardan, although the interpretation offered there in terms of "intellectual property ownership" is somewhat strained.

[78] See Merton (1973: pp. 286-288); Biagioli (1993: pp. 57-58, 63, 68, 70; 2006: pp. 161-214) specifically on Schneider and Galileo and the sunspots; Westfall (1980: Chs. 7, 14) on controversies involving Hooke, and the priority dispute with Leibniz, respectively.

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verifier?) The opportunities to do this were quite limited, but where they did present themselves, an applicant for patronage obviously would find it quicker to build a reputation by responding to them directly. The effectiveness of the tactic in such exceptional circumstances is evident from a well-known incident in the career of Galileo.

When Galileo burst upon the European intellectual scene in 1609-10, he was already forty-five years old and scarcely known outside two narrow circles in Venice-Padua and Florence.[79] One should notice that it was not mathematics that catapulted him to instant fame from the comparative obscurity of a professorship at the University of Padua (where his tenure was about to expire) and the proprietorship of a small instrument-making business in the town whose most successful product was a novel "military compass" sold with either tutorial instruction, or a printed pamphlet. Nor was it the great works for which he is now acclaimed, because the *Dialogue Concerning the Two Chief World Systems* (1632/1967) and the *Discourse on the Two New Sciences* (1638/1974) were products of his later years. Rather, Galileo first was able to make a something of name for himself by constructing a telescope remarkably better than those that previously had been built in northern Europe – an instrument that he presented to the Venetian Senate in August 1609, and quickly gained him promotion to life tenure at Padua at a remarkably higher salary.[80]

As Galileo soon discovered, the downside of this coup was that the Senators had fixed his new salary for life. It was not long before he had again set about grinding the lenses for a more powerful telescope, and was embarked on a program of astronomical observations that might bring him greater renown and perhaps more munificent patronage.[81] The ducal family of his native Florence was

[79] Drake (1980: Ch.2) provides a wonderfully concise and nuanced account of Galileo's early years. It should be said that Galileo's move to a mathematics professorship at the University of Padua in 1592, on the strength of his teaching reputation at Pisa and with the support of early patrons, had brought him into greater academic prominence. Padua had passed under Venetian sovereignty in the late fifteenth century, and its university had benefited from the uniquely enlightened tolerance of Venetian rule – becoming renowned throughout Europe for its school of medicine, as a center of (Aristotelian) natural philosophy, and for a faculty of mathematics that was second only to that of Bologna. See Biagioli (1993: 30-32) on patronage in Galileo's early career.

[80] Galileo's presented this the telescope to the Venetian Doge, pointing out its utility for sighting ships at great distances – of relevant in naval combat, but also to alert merchants to the impending arrival of cargoes at the port. The Senate renewed his professorship at Padua for life, at a salary that was almost doubled.

[81] Drake (1980) suggests that Galileo's dissatisfaction with the fixed stipend at Padua prompted his decision to begin the course of observations of the lunar surface, and of the planets -- which yielded the "Medicean Stars." No success in academic life goes unpunished, however. Although that second coup enabled Galileo soon to leave Padua for service at the Medici court in Florence, according to Westfall (1985), the resentments stirred among his rivals for patronage at

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the most obvious target. Very soon thereafter Galileo published the pamphlet *Sidereus nuncius* ("The Starry Messenger," March 1610) announcing that the new stars he had been able to observe near Jupiter were in fact satellites circling that planet.[82] From the viewpoint of Galileo's career, the true beauty of this heavenly discovery lay in the fact that to confirm it at a basic level required little or no expertise, just reasonably clear vision, access to the new telescope, and directions as to where in the night sky one should look.

Richard Westfall (1985) calls attention to the adroitness with which Galileo exploited this aspect of his achievement, using it to the fullest advantage within the context of the patronage system. For some time beforehand he had unsuccessfully been seeking to attract the attention of a princely patron who might free him from the burdens of teaching, and give him the leisure and security to pursue research and writing. So now he named his new-found moons of Jupiter "the Medicean stars," quickly composed the *Sidereus Nuncius* to proclaim their existence publicly, and dedicated the work to the Grand Duke of Tuscany, Cosimo II de' Medici. As Westfall notes, Galileo took the opportunity to send the Grand Duke "an exquisite telescope," so that he might see for himself the sort of heavenly secrets that his would-be client had the power to reveal. Moreover, when that gambit had paid off handsomely and he had been taken into the Grand Duke's service, Galileo persuaded his patron to let him prepare excellent telescopes to be presented (only at the Duke's orders) as gifts to the other rulers of Europe -- so that they, too, could not doubt the existence of the new celestial bodies that bore his name.[83] Westfall (1985: p.4) described this as "an inspired manoeuvre whereby Galileo enlisted the Grand Duke as his public relations agent."

Such self-promotional genius, and such readily transportable, auto-confirmatory devices, however, could not be deployed by most practitioners of

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Padua--like those he kindled upon arriving in Florence--would be a source of Galileo's later troubles, including those with the Jesuits. Drake (1980), however, advances a different and at the time radically revisionist thesis -- viz., that Galileo was zealous to protect the Church from conflicts between its theological positions and the new science, and recognized his real opponents to be the university-based natural philosophers defending the purely "theoretical" Aristotelian system of science. For yet another, more recent and still more controversial reading of the sub-text of Galileo's trial before the Inquisition, see Redondi (1987).

[82] See Drake (1957, 1978); Westfall (1985), and Biagioli (1990, 1993: Ch.1) for extensive discussion of the significance of Galileo's telescope in the context of the patronage system.

[83] The Grand Duke favored Galileo with a gold chain, a medal, and in June 1610, an appointment as "Chief Mathematician of the University of Pisa and Philosopher to the Grand Duke, without obligation to teach and reside at the University or in the city of Pisa, and with a salary of one thousand Florentine scudi per annum." Boorstin (1983: 321). Biagioli (1993:104) notes that this was a remarkable stipend, comparable to that of the highest court official and at least three times that of any highly paid artist or engineer of the day.

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the new science who were seeking public reputations that would command the attention and the patronage of princes.[84] Scientific reputations would, in more typical circumstances, have to be built first among professional peers who were equipped to evaluate one's claims, and even in the case of so seemingly transparent an instrument as the telescope, questions of the reliability of Galileo's announcements of his astronomical discoveries ultimately were referred to other "expert" astronomers for corroboration.

The pattern of distribution of Galileo's telescopes along with copies of the *Siderius Nuncius*, nevertheless, illustrates in a particularly striking way the intimate connections that existed between the patronage system and the formation of networks of communication among the new scientists. Indeed, as Westfall (1989: 35) remarks, in this information dissemination process it was the Medici ambassadors who provided what modern communications engineers would recognize as the system's "physical transport layer." For example, the astronomer Johannes Zugmann had read the copy of the *Siderius Nuncius* which had been sent to his patron the elector of Cologne, and Ilario Altobelli, an astrologer and astronomer and friend of Galileo's in Rome, received the copy sent to Cardinal Conti. Kepler obtained the *Siderius* from Giuliano de' Medici (the Medici ambassador to the court of Emperor Rudolph II), who summoned him to the Medici palace in Prague where he was read a letter from Galileo inviting him to make a response. This invitation was reinforced by both the ambassador's "own exhortation," and a request from Rudolph (his patron and employer) that he express his opinion in the matter. In the spring of 1610, Galileo was able to use the supportive response in Kepler's *Conversations with the Sidereal Messenger* as testimony to the international recognition of his discoveries, in order to counter doubts that were being circulated about the reliability of his telescopic observations. [85] Evidently, the role of the patron's emissaries in these transactions involved something beyond mere "message transport." Mario Biagioli (1993: 58) has argued that "Galileo did not use pre-existing diplomatic and aristocratic communications networks simply because they were practically convenient....The use of diplomatic connections -- of diplomats who partook of the status of the prince they were presenting -- gave Galileo credibility."

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[84] The microscope-makers of the day, however, enjoyed some parallel advantages. Their story would seem to deserve close consideration from the same epistemological perspectives as Biagioli's (2000, 2006) careful analysis of the differences between producing a "discovery" that actual and potential patrons might see for themselves, and obtaining independent confirmation that what could be seen through the instrument was not merely an artefact of the latter's construction, and had been interpreted correctly by the self-proclaimed discoverer.

[85] Kepler did respond, in his *Conversation with the Sidereal Messenger*, which he dedicated to "the ambassador of Prince Medici Grande Duke of Tuscany, himself a Medici by birth," who had "sought this service from me." See Biagioli's (1993: 56-58, 95-97; also in Biagioli, 2006: ch. 2) account for this, and the other instances cited in this paragraph.

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Nevertheless, exactly what sort of "credibility" this represented remains a rather complex and contentious question. Biagioli (1990, 1993: 3) takes the position that social and political status translated directly into scientific credibility. He presents the late sixteenth and early seventeenth century European court, with its etiquette and appended rituals of aristocratic patronage, as contributing crucially to the "cognitive legitimation of the new science by providing venues for the social legitimation of its practitioners, and this, in turn, boosted the epistemological status of their discipline." In a further, bold elaboration upon this interpretation, Biagioli (1993: 59) declares:

> "If it is a bit naive to consider scientific credibility as related only to peers' recognition, even in modern science, such a view is seriously misleading when used to interpret the construction of scientific credit and legitimation in early modern science. I think it would be useful to suspend for a moment the 'natural' belief that Galileo, Kepler, and Clavius earned their titles (e.g., in the case of Kepler, 'Mathematician to the Emperor') because of their credibility and the quality of their scientific work. As a thought experiment, we may think, instead, that they gained scientific credibility because of the titles and patrons they had."

Although there surely is some merit in essaying such a reading of the historical evidence, the preceding statement goes rather overboard.[86] Advancement to prominent positions within the hierarchy of patronage may well have transmitted a signal that enhanced the professional scientific standing of the individuals involved, yet it is difficult to suppose that such a signal would long retain much scientific credibility-value, for either peers or patrons, were it known that advancement to such positions was purely a matter of aristocratic caprice or ecclesiastical politics. By the same token, it would seem that to dismiss "public reputation," and particularly the judgement of peers as being without importance as a basis for individual scientific credibility in this historical context errs by neglecting to consider the needs of highly placed patrons. Surely it was a matter of concern to protect their own status from the embarrassments that would ensue

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[86] As Noel Swerdlow aptly remarked: "Think of all the nonentities that held similar court positions!" [Private communication, April 1994.] Indeed, Biagioli's (2000, 2006) subsequent treatments of the story of the telescope have simply emphasized the distinction between gaining "credit" with noble patrons and having one's scientific discoveries confirmed by well-known peers – while suggesting that being in good standing with a highly placed patron could help to elicit public expressions from other savants of their confidence in one's announced discoveries. This modified position reasonably allows a potential for errors in the reputation-building process. Of course, valid is that is as a general point, it is not particularly germane in regard to the "credit" that came to Galileo for his announced observational discoveries regarding the moons of Jupiter, the "imperfect" (mountainous) surface of the earth's moon, or the sun-spot "blemishes" on the solar disc.

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from endorsing incompetents and outright charlatans. It would be reckless to disregard the significant risks inherent in a client's exposure to challenges brought by other clients, and would-be clients – challenges that possibly might be directed ultimately against the patron, a process that is amply documented by Biagioli's own researches.

Of course, it is correct to have pointed out that the patronage system operating in the Renaissance took into account many things about a mathematician-philosopher besides the attestation of peers as to his "technical" proficiency. There were family connections and alliances, political and theological factions, social graces, and much else besides that might be considered relevant when selecting a client who would be useful in advancing the prestige of a patron. So, there must have been "trade-offs," whereby more gifted scientists were passed over in favor of the less gifted but more diplomatic, or the socially better-connected. Such things are not unheard of in the modern era, and we may suppose that personal attributes that today would be dismissed as "scientifically irrelevant" exercised far greater influence in the careers of Galileo, Kepler, and their contemporaries.

On balance, however, it seems most reasonable to maintain that the survivors of reputational trials in diverse fields of arcane knowledge acquired "credibility," not because high-status patrons conferred it upon them through acts of appointment to serve at court, but because contemporaries would have understood that patrons could not have afforded simply to act upon their personal judgements in such esoteric matters. Rather, they had endorsed the outcome of an "external" screening process that "testified" to their clients' expertise. The more public the screening, and the wider the consensus of views that were manifestly independent of the patron, the more "trustworthy" the choice would be when judged in terms of the latter's interests.[87]

[87] To pursue the modern parallel: it is correct to say that a scientist today gains credibility from the fact of receiving an appointment to an endowed professorship at a prestigious university (although that is not the sole route available). But, this is not because it is supposed that the Dean, President, or Chancellor of the university in question has judged the individual's competence, any more than it would be presumed that the appointing officials had given weight to the individual's family connections and personal manner. What counts is the presumption that the reputation of their institution and their own reputations as good institutional stewards are matters of concern to the administrators; that they therefore would have acted prudently, insisting that the candidates for professorships be vetted by some other agency, acknowledged by others to be competent -- indeed, on average, more competent than they themselves to make such evaluations. Of course, the beauty of the thing is that the implied admission of ignorance will be their surest claim for exoneration should the appointment turn out to have been a mistake: "We took the advice of experts."

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4. Patronage, competition, and common agency: economic implications

Although for purposes of the foregoing exposition heavy use has been made of the story of Galileo and his involvement in the system of patron-relations in the Italian courts, the situation and experiences of many other notable scientific figures in courts throughout Western Europe similarly deserve notice and closer analytical examination. As has already been remarked, Johannes Kepler served in the court of Emperor Rudolph II in Prague, a position in which he succeeded Tycho Brahe.[88] Galileo's illustrious student, Torriceli, succeeded him as court mathematician in Florence, and his friend Borelli, who did pioneering theoretical work on the possibilities of flight, lived in Rome as the protégé of the retired Queen of Sweden.[89] Leibniz served the electors of Hanover for forty years as a diplomat and advisor, and so on. Innumerable additional instances of court patronage involving both the greater and the lesser luminaries among the scientists of this era have been documented in studies by social historians of science of the court of Prince Henry of Wales [d.1612] at Richmond Palace, the Court of Rudolph II and the Habsburg circle in the mid-seventeenth century, the Munich Court of Ferdinand Maria, the Elector of Bavaria [c. 1654-1679], and still others.[90] But the mere multiplication of these examples does not in itself do much to advance our understanding of why the client-scientists of this era appear to have flourished under the patronage regime as their numbers swelled. That is the problem to which we must now turn.

The competition among patrons for mathematician-clients, as an aspect of the aristocratic rivalry for prestige and power in Renaissance Europe, proved beneficial to the "new philosophers" and induced entry into the emerging profession of "science." The central analytical issue in this proposition concerns the structure of "common agency" contracts. It has been suggested that we think of the relations between patrons and mathematician-clients in terms of principal-agent arrangements, characterized by the ability of the patron-principal to stipulate the terms on which favors, monetary stipends, valuable gifts, and social and political connections would be dispensed in return for "ornamental" or

[88] Although born into a noble family, and possessing sufficient means earlier in his life to furnish his magnificent observatory on the island of Uraniborg with all the latest in astronomical instruments, on 1 May 1598 went Kepler received the Rudolph's invitation to come to Prague, he was without a patron, living in exile from Denmark and in need of ready money. He then delayed his journey for three months, it is presumed in the hope that the printing of his lunar theory would be completed with a suitable dedication to the Emperor, before presenting himself to his prospective patron in Prague. See Thoren (1990: Ch. 12).

[89] See Mokyr (1990: pp. 73, 84, 169) on Leibniz, Torcelli and Borelli, respectively.

[90] See, e.g., the detailed contributions in Moran (1991), and the broad perspective of Cohen (1994: pp. 205-208.

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utilitarian services. Such patrons faced a problem more general than the one posed by the fact that they were not competent in most cases to evaluate the technical expertise of their mathematician-clients; the latter might acquire information in the course of a patronage relationship which could be used to advance the client at the possible expense of his patron's interests.

One should not suppose that written, legally binding contracts sealed the patron-client bond, but the nature of these dyadic relationships was neither unstructured nor entirely idiosyncratic. That there were well-established "conventions" and "rituals" governing such exchanges has been shown by Biagioli's (1993) examination of the system of patronage in Galileo's time. What emerges clearly from that study -- as one might well suppose -- is that the patron determined how casual or close a connection he wished to form with any particular client, from among the set of supplicants. Indeed, it was within the discretion of the patron as to whether or not contacts through "brokers" or intermediary clients would be invited, or reciprocated.

Further, it is clear that except for the few great successes, like Galileo and Kepler, mathematician-scientists with a public reputation would accumulate a number of patrons. Our perception of the patronage system as involving exclusive dyadic relationships between a patron and his client, which would perhaps be replicated with more and more numerous clients as one moved up the ladder of wealth and social prestige, is based on the familiarity with the later stages in the careers of a few, eminently successful practitioners of the new science. To be able to rise to the position where a great prince was your patron -- as Rudolph II was the patron of Kepler, or Cosimo II was Galileo's -- was the grand ambition of those who entered this system from the lower ranks of the social order. According to Biagioli (1990: 12-13), Galileo was quite conscious that one could not compound the social legitimacy provided accumulating many small patrons into the equivalent of that which a great single patron would confer, even if the immediate material benefits might be comparable. Writing early in 1609 to a Florentine courtier, Galileo made it plain that he regarded a career that involved serving many low status patrons for piecemeal compensation to be "cheapening," a sort of prostitution ("*servitu meretricia*"):

> "Regarding the everyday duties, I shun only that type of prostitution consisting of having to expose my labor to the arbitrary prices set by every customer. Instead, I will never look down on service a prince or a great lord or those who may depend on him, but, to the contrary, I will always desire such a position." (Translation by Biagioli 1990:13)

A patron who had to "share" a client with others would naturally try to structure the terms of the arrangement so as to have his interests served above all others. Since the core of the system involved competition for prestige gained by

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association with the famous, services yielded by clients had a strong "positional goods" aspect; several patrons might gain prestige relative to others if the public reputation of their common client were to be enhanced, but they would also be vying with one another to capture some greater share of the glory. But, insofar as what the scientist-clients had to offer was "novelty," at any point in time the welfare ('satisfaction') of several patrons could not be jointly advanced to the same degree. In that sense the services provided by a client to his several patrons were "substitutes" rather than "complements": the same treatise could not be dedicated publicly to more than one patron, and it could risk the rupture of a valuable relationship to present the same 'gift,' whether that of a new astronomical discovery, or an exotic botanical or zoological specimen, in two noble courts.

4.1 *Common agency games and the distribution of information rents*

With multiple patrons to satisfy in order to sustain their professional life-styles, and, *a fortiori* to further elevate their position, the clients would find it tempting to turn to others such as students, apprentices, and colleagues further down on the scientific career ladder, to help with the work. But that would be exactly what would concern their potential patrons. Would it not be likely that if they offered a substantial incremental reward, in order to induce a more impressive dedicated offering from their client, the result would be that the resources would be used to subsidize the production of another 'gift' that might be proffered to a different patron? Indeed, it would. Following Dixit's (1995) intuitive exposition of common agency games, we may see how the two-part incentive schemes provided by the patron-principals in this situation would interact, and what that implies for the equilibrium of the (tacit) game between the principals.

Dixit's simplified model supposes there are only two principals (patrons) -- call them *F* for Frederick, and *R* for Rudolph -- trying to influence a single agent (*C*, the client) who controls the performance of two scientific projects *f* and *r*, each of which is expected to yield a dedicated result. Principal *F* is primarily interested in the outcome of *f*, while *R* is intrigued by the prospects of *r*. While the outcomes eventually will be disclosed for all to see, the amount of effort that *C* devotes to the tasks is not observable by the principals. But inasmuch as the agent's time or effort is limited, more spent on *f* will necessarily mean less being spent on *r*, and vice-versa. Therefore, principal *F* will be disposed to hold out the offer of an incentive scheme that responds positively to greater elaboration, or faster completion of project *f* ('*f*-output'), and negatively to *r*-output -- perhaps penalizing *C's* prior publication of an elaborately dedicated work for *R*, by reducing of the supposedly fixed portion of the client's stipend.[91]

[91] Conveniently, in the model analyzed by Dixit (1995) there is a constant term as well as a

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Similarly, *R*'s incentive scheme rewards the agent positively for greater *r*-output, and penalizes the production of more *f*-output. Now the problem is that if either principal were to offer a 'high-powered' incentive scheme, which proffered very large marginal rewards for their favored form of client-service (*r* or *f*, respectively) a response to it by the agent would result in the implicit transfer of some resources to the other principal. If the agent hurries to complete project *r* in order to obtain the enlarge 'reciprocation' of his 'gift' by *R,* the penalty imposed by dissatisfied *F* would be tantamount to the levy of a tax upon the marginal payoff received from the satisfied principal. Some of *R*'s 'gift' to *C* is, in effect, passed back to *F* -- inasmuch as C's participation in the scientific undertakings is predicated upon obtaining a minimum payoff. If the two principals can recognize this, offering high-powered schemes will not seem desirable for either of them. But, each would find it attractive to offer some incentive to their client, because the tax that the other would levy on *C*'s marginal rewards for pleasing them is less than one-for-one, and so allows for some inducement of effort on their behalf. In the final outcome, as Dixit (1995) shows, the Nash equilibrium of the game of strategy between the principals is that the overall power of these incentive contracts is rather low. Little wonder that a scientist operating within this patronage system would express a desire, as we have seen in the case of Galileo, to escape from the servicing of multiple patrons and be taken up in an exclusive relationship with the court of a great prince.[92]

The fact that during the seventeenth century very few scientists could achieve such an apotheosis in their profession, and consequently had to balance between the competing expectations of an array of patrons, carried other implications that actually tended to ameliorate their material situation. It is

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variable conditional reward, so that the incentive schemes follow the two-part structure of the patronage contracts that we have seen to be characteristic of court patronage systems. Dixit (1995: 4) points out that the level of this 'sure payment' can be adjusted to make sure that the agent is willing to accept the contract, given that there is a measure of uncertainty surrounding the outcome of these scientific projects.

[92] Dixit (1995) demonstrates, in the context of the simplified model, that there are different levels of efficiency associated with the equilibrium of the game, according to the assumptions made about what can be observed, and who can cooperate with whom. The maximum efficiency for the system is attained in the ideal state where everything (input efforts and outputs) can be observed by all, and all principals bargain with the agent. But, acknowledging the informational asymmetry between the principals and the agent, a second-best optimum is achieved when all the principals cooperate explicitly, in which case they behave as one patron; not having to worry about free-riding by other principals, the preferred contract offers the agent high-powered incentives. One may suppose, however, that it was the freedom from the time occupied in interactions with numerous patrons, and the prospect of high marginal rewards for scientific accomplishments that made the prospect of having a single (wealthy) patron attractive to Galileo -- rather than a concern for the fact that the resulting second-best optimum would dominate the multiple patron Nash equilibrium in terms of its allocative efficiency properties.

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important in this regard to emphasize the point that the prevailing circumstances of common agency, as has just been seen, created a relationship of *substitutes* at the margin, rather than *complementarities* among the differentiated services they performed for their multiple patrons. This condition arose from the dominance in this historical epoch of patrons' concerns with the "ornamental" rather than the "utilitarian" value of scientist-philosophers. Among its direct consequences was the tendency for the structure of the incentives offered to clients, weakened as it was, to nonetheless remained more favorable to the individual scientist-philosopher than would have been the case were patrons willing to content themselves with the benefits of knowledge spillovers from the inquiries supported by courts other than their own. In a later era, when the utilitarian applications of scientific knowledge became paramount, there would be generally greater complementarities between the services that could be provided to two different patrons by a common agent, and the possibilities of free-riding on the results of projects undertaken for other patrons tended to diminish the bargaining power of individual researchers vis-à-vis those who employed them.

The preceding line of argument is consistent with the conclusions emerging from the formal analysis of the common agency problem by Lars Stole (1991). For the case in which the agent must choose either to accept multiple principals or none, Stole shows that when the agent-services are substitutes (complements) the contracts designed by principals produce less (more) inefficiency in the production of services than would result from having a sole principal. They also leave the agents with more (fewer) "information rents." A monopsony thus would tend to extract the "producer surpluses" from the agents, whereas competition on the buying side of the market allows them to retain more "rent" on their specialized knowledge. On the other side of the ledger, however, a monopsony might have avoided a market failure -- if what principals wanted was access to a “public good” that all could enjoy if it was collectively provided, but which their (misguided) competition for more privately appropriable forms of benefits prevented from being produced by their common agent(s).

It should be remarked that in Europe the politically-inspired reputational rivalries among noble patrons to secure more of the attentions of their frequently shared client(s) worked to allow the latter to retain more "rents" from the information each of the latter possessed -- in comparison with the situation that would have obtained were there only a single *possible* patron on the scene. This fairly obvious amplification of the conclusions from preceding analysis is too often overlooked. A strong central state would create a monopsony situation in the market for patronage, whereas in Renaissance Europe one had many contending principalities courts. The latter was better for the scientists, who were less at the mercy of arbitrary political authority and economic power than would otherwise have been the case.

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The Reformation acquires further, new significance when seen in this context -- if that is conceivable: the emergence of Protestant princes in the North of Europe reduced the sphere within which the Catholic Church, the one universal source of authority, could exercise its political and social influence over lay patrons of science. Although this did not suffice to spare Galileo from trouble (for, indeed, many of his troubles might be attributed to the Jesuit Order's perception that successful prosecution of the Counter-Reformation required vigorous suppression of all challenges to the Curia's authority in matters of theology), it undoubtedly made it more difficult to maintain a European monopsony in the market for intellectual services.

It may be asked, then, whether the competition among patrons also was good for "science." There are two effects to be considered here: the effect on the development of the profession, and that upon the content of the knowledge being produced. With regard to the former, it would appear that the workings of the patronage system induced more people to enter the profession by leaving greater rents to the individual scientists and creating super-star winners of reputational tournaments. On the other hand, the disposition of patrons to seek "positional services" from their scientist-clients operated to focus the latter's attention less upon the production of knowledge that had a public goods dimension, and more upon putting their knowledge to use in forms that served the interests of particular patrons. A centralized system of patronage, although likely to be worse from the viewpoint of its effects upon the scientists' autonomy and social status, might have been better able to focus attention on those lines of inquiry that would yield complementary benefits to those who supported their research. One might regard that as an outcome particularly attractive to the myopic.

5. Open science, 'invisible colleges' and the 'new age of academies'

The foregoing, necessarily compressed treatment of immensely complex matters has focused upon the economic aspects of patronage in the production of knowledge, and the latter's influence upon the historical formation of key elements in the ethos and organizational structure of open science. Those developments preceded and laid the foundations for the later seventeenth and eighteenth century institutionalization of the more public pursuit of scientific knowledge under the auspices of State-sponsored academies. Some 70 officially recognized scientific organizations have been identified as having been founded in the period from the 1660s to 1793 -- specifically, on the models provided by the Royal Society of London and the Académie Royale des Sciences. The first of these was established privately in 1660 and received charters from Charles II in 1662 and 1663; the latter institution was founded by Louis XIV in 1666 on the initiative of his Minister of Finance, Jean-Baptiste Colbert).

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The activities of these two archetypal State foundations, and the ensuing formal institutional "reorganization of science" in Europe that they inspired, have received much attention from more than one generation of historians of science.[93] Although justified by both the wider influence that those state foundations exerted, and the depth of the archival material available for study, the emphasis placed upon the first pair of academies to be established under monarchical patronage in era of the European nation-states emergence has had an unfortunate effect. It deflected scholarly interests away from the generic features in the workings of the early learned societies and scientific academics, both the lesser and the greater -- the many that had been organized under private patronage, as well as those grand objects of regal support.

Another correlate of this particular historiographic fixation (doubtless also quite unintended) was the excessive emphasis that has been accorded to the aspect of discontinuity in the historical record of institutional development, which has been presented in the literature as "an institutional reorganization of science" that marked the closing third of the seventeenth century. This contributed to further obscuring the broader significance of the conditions underlying the antecedent emergence of an "open science" ethos -- conditions that had not ceased to be relevant for the functioning of the new academies, even though the latter tended to be taken as given and natural among the scientifically curious.

Moreover, it reinforced a long-standing disposition of the historical literature, especially that relating to the great French Academy of Sciences and its imitators on the continent, to present the organizational shifts associated with the new State patronage arrangements as having been induced more-or-less automatically, as a rational institutional response to the dawning of the new scientific methodology. In other words, the interpretation placed on these events has assigned causal priority to the epistemological and methodological aspects of the new sciences, including their technical practices and material requirements, rather than viewing the power and influence manifested by those coupled developments to have co-evolved with the changes that had occurred in the supporting institutional infrastructure.

The findings of more recent investigations by historians of science and the new perspectives that can be brought to these matters by economic analysis, however, point towards a quite different way to read this second phase of the process of institutional evolution. Just as the foregoing pages have argued that the intellectual reorientation represented by the Scientific Revolution cannot properly be held to have been a motor cause of the emergence of the "open" mode of searching for Nature's Secrets, so there are good grounds to resist interpreting the

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[93] See, e.g., Brown (1934 [1967]), Hirschfield (1957[1981]), Orenstein (1963), Hahn (1971), Hunter (1981).

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'New Age of Academies' as a radical institutional departure brought about by the economic imperatives of the new scientific practices. The following brief treatment of these large matters is intended to do no more than indicate the main analytical and empirical foundations for the reinterpretation advanced here.

5.1 *Understanding the early scientific societies: reputation, club goods, and the logic of 'invisible colleges'*

The formation of networks of correspondence and the flowering of "invisible colleges" among mathematicians and the experimentalists in the new science, in a sense, was addressing the central problem of informational asymmetry in the late Renaissance patronage system.[94] Such networks, augmented by an expanding volume of printed pamphlets and treatises, provided an arena in which challenges could be issued, contests and competitions could be staged, and collegiate reputations could be both secured and widely broadcast.[95] But, the formation and growth of these networks in the course of seventeenth century -- and with them, a system of continual peer-group evaluation based on public demonstrations of individual creative achievements -- was quite clearly reinforced and institutionalized through the creation of more, and more formal scientific bodies. These progressed from the informal "assemblies" and salon-like discussion groups gathering for scheduled meetings under the auspices of private sponsors, to academies having elected members whose researches drew support from elites, and ultimately from regal patronage.[96]

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[94] Price (1963: 83ff) adopted and conceptually extended the seventeenth century term "invisible college," used by Robert Boyle in describing the small group of natural philosophers whose intellectual transactions with one another anticipated the formation of the Royal Society. Invisible colleges, according to Price (1963:76), confer upon each scientist "status in the form of approbation from his peers, they confer prestige, and above all, they effectively solve a communication crisis by reducing a large group to a small select one of the maximum size that can be handled by interpersonal relationships." See Merton (1986) on the relationships between the concepts of reference group, invisible college, and deviant behavior in science. For a reformulation and defence of Merton's emphasis on the ethos of cooperation and norms of disclosure, see David (1998, 2003c).

[95] Although the scope and intensity of activity in these networks of correspondence increased greatly during the late sixteenth and early seventeenth centuries, correspondence among scientists (including the chemical alchemists), like the previously noted exchanges, challenges and contests among Europe's mathematicians, was not an innovation of the seventeenth century

[96] Lux (1989: Ch. XXX) traces the evolution during the years 1665-1666 of a provincial *assembée,* that had formed in the French town of Caen, into the Académie des Physiques (physiology) du Caen. This process took place under the patronage of a local figure of noble parentage who as a young man had made a reputation in the French world of *belles lettres* and had "connections" to people close to Louis XIV. The institution flourished briefly, and in 1672 received royal patronage -- the first among France's provincial institutions to do so.

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The characteristic open style in which those organizations' affairs came to be conducted was not merely a ritualized social feature of the way client-patronage relations were conducted in the courts of Europe's elites. It was more directly a response to the requirements of the patronage system as well as being dependent upon its support. This raises some intriguing historical questions that remain to be answered through renewed inquiries into the primary sources. But these matters cannot be pursued here, nor need they be: it will be sufficient for our immediate purposes to examine the ways in which participation in the early academies and scientific societies conveyed benefits upon their members.

The obviously pertinent question is this: Why should *individuals* seek to participate in associations where they would be expected freely to divulge the results of their investigations into matters that might otherwise be a source of personal gain? What incentive was offered for such cooperation – aside from an institutionalized setting in which such conduct was made a formal obligation (publish or perish!) of career advancement? Two principal answers come to mind. Firstly, there was a certain status conferred by being accepted as a "correspondent" by already established "scientists-philosophers." Association with persons of great repute carried some "signalling value" for the correspondent, which continued to be relevant in a setting where private patronage was consequential. Secondly, there is the "exchange value" of the information to which such associations might provide access: division of intellectual labor confers advantages to those who need not work in isolation and instead may draw upon codified knowledge and the (tacit) expertise of others for help in solving particular problems. Access to such "networks" of assistance must be purchased by credible proffers of worthwhile "material" or accreditation. The matters warrant being considered a little more fully, taking them in turn.

(a) The Signalling Value of Correspondent Status:

This is a generic property that is not restricted to "correspondent" relationships among scientists, but its seventeenth century significance in that context is nicely conveyed by Richard Westfall's (1980: 541) account of Newton's relations with the astronomer John Flamsteed, which reveals how the latter viewed the question of responding cooperatively to Newton's request for access to his lunar observations. What he wanted from Newton was acceptance as a "philosophic peer," and he replied in the following way to Newton's offer to pay him for the trouble of copying some astronomical observations: "All the return I can allow or ever expect from such persons with whom I corresponded is only to have the result of their Studies imparted as freely as I afford them the effect of my paines." Flamsteed emphasized that admission to this reciprocal status involved

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approbation; he wrote to Newton that his "approbation is more to me than the cry of all the ignorant in ye World."

It is apparent that the would-be correspondent's motivation might be vanity or ego gratification from such acceptance into peer-ship, but it also could spring rationally from an instrumental desire to be accorded status that would enhance prestige with third parties. Here the mechanism at work is "passive patronage": a person who can claim acceptance as a correspondent by a recognized "great", a "star scientist" (in modern parlance), might hope to use this to raise their status in the eyes others. The signal can be discounted, however. Flamsteed wanted to be thought of as a scientific peer, whereas Newton viewed him (privately) as an inferior but a useful collaborator. Those at the top of the hierarchy of recognized status may thus benefit by being able to accord some (passive) patronage to those below them, a derived effect that is important in reputational work organizations and provides one of the benefits of winning a reputational tournament. Nevertheless, more than vanity might well be involved: Flamsteed truly was devoted to his astronomy and had labored long and hard, under difficult conditions and with little reward. Quite naturally he wanted Newton's scientific help, as well as well assistance in furthering his career.

(b) Network Membership:

Unlike signalling value, network membership benefits can be thought to have a substantive basis for both researchers and their patron-employers, whether or not the final disposition of the knowledge that is acquired will be public disclosure or private exploitation in some directly productive activity. The network members have solutions to problems at their disposal, which they are prepared to share and thereby create a loose coalition within which some degree of non-compliance with the norm of mutual help can be tolerated. While it is true that the latter form of "deviance" would result in an equilibrium in which the "common pool" was degraded by the private reservation of certain information, regular access to even some measure of help from a group of highly expert "peers" would remain a valuable asset for the individuals who are admitted to the society's membership.

5.2 *Enforcing the ethos: the disclosure norms and knowledge-sharing games*

To restate the thrust of the foregoing considerations, it is possible that cooperative behavior within a limited sphere can emerge and be sustained without requiring the prior perfect socialization of researchers to conform, altruistically, to the norm of full disclosure and cooperation. This is a rather straightforward instance in

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which insights from the theory of repeated games are applicable to explaining cooperative behavior among potentially rivalrous researchers.[97]

To sharpen this point, we may start simply, by considering two researchers working towards the same scientific goal that involves the solution of two sub-problems, and suppose that each has solved one of the problems. Once each gets the other solution, it will be a matter of writing up the result and sending it off to the corresponding secretary of a scientific society, or a journal for publication -- the first to do so being awarded priority. Now suppose, further, that the write-up time is determined by a random process and the agents are symmetric in that regard; if each gets both halves of the problem at the same moment, each will have the same (one-half) probability of being the first of the pair to submit for publication. Whether the winner will be awarded priority will depend, however, on whether or not some other researcher has obtained the full solution and sent it off already. The question is: should the first researcher hope to get some part of the solution-payoff and follow the strategy (S) of sharing information with the other one, or instead adopt the strategy (W) of withholding?

If, without prior communication they play the strategy pair (S, S), they can proceed immediately to the write-up stage; if they play (S, W) the second member of the pair will be able to proceed to the write-up, and the opposite will be true if they play (W, S). Should they both withhold (W, W), they must both spend further time working on the other problem. It is evident that if they are only going to be in this situation once, the rule of priority alone will induce each of them to withhold, and they will end up (collectively, if not individually) at a relative disadvantage vis-à-vis other researchers who are hurrying to publish. If nobody else has the full solution yet, society also will have been forced to wait needlessly, because each member of the pair has a dominant (private) strategy of withholding what she knows.

This game has the structure of a classic two-person "Prisoners' Dilemma." It is well known that if a game having this structure is played repeatedly an escape from the pessimal outcome is possible, so long as the future is not discounted too heavily, and where the players can expect the other member of the pair to remember, and punish their failure to play cooperatively by sharing. (Indeed, there is a so-called "folk theorem" to that effect).[98] But, the value in the future of developing and maintaining a good reputation for sharing has to be large

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[97] The following parallels material developed in Dasgupta and David (1994).

[98] For a non-technical introduction to the literature on the repeated Prisoners' Dilemma and its broader implications, see Axelrod (1984). The "folk-theorem" of game theory holds that (if future payoffs are discounted by each player at a low rate) in the "super game" obtained by repeating a finite, two-person game indefinitely, any outcome that is individually rational can be implemented by a suitable choice among of the multiplicity of Nash equilibria that exist. See Rubinstein (1979, 1980), and Fudenberg and Maskin (1984).

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enough to discipline the self-interested researcher into adhering in the present to the “sharing” mode of behavior. If repetitive play comes to an end, or if the future is valued only slightly, cooperation will unravel from the distant terminal point in the game, right back to its inception.

But that is not the end of the matter. As there are other researchers in the picture, we should really be considering an **n**-person game, involving the solution of an **m**-part problem, where **n** > **m**. Now the question of sharing information becomes one of sharing both the knowledge you have acquired in your own investigations, and such information as you have gained in communication with others. It is obviously advantageous to be in a group in which information will be pooled, because that will give the group members a better chance of quickly acquiring all **m** parts of the puzzle and being the first to send it in for publication. On the other hand, individuals may behave opportunistically, by exchanging what they have learned from one group for information from people outside that group while withholding some knowledge from others within their group. In that way they could expect to do still better in their current race for priority of publication. Because others would see that such "double-dealing" will be a tempting strategy, however, cooperation will be unlikely to emerge unless "double-dealers" (who disclose what you tell them to third parties, but don't share their knowledge fully with you) can be detected and punished. What is the form that retribution can take? Most straightforward will be punishment by exclusion from the circle of co-operators in the future; and even more severely, not only from the circle that had been "betrayed" but from any other such circle. This may be accomplished readily enough by publicizing "deviance" from the sharing norms of the group, thereby spoiling the deviator's reputation and destroying his acceptability among other groups.[99]

What, then, is the likelihood that this form of effective deterrence will be perceived and therefore induce cooperative behavior among self-interested individuals? If a group, i.e., "a research network" numbering **g** players (**g** ≤ **n**) is large, identifying the source(s) of "leaks" of information and detecting instances of failure to share knowledge within it will be the more difficult. It is worth remarking that the power of a large group to punish the typical deviator from its norms by ostracism tends to be enhanced by the higher probability that all those individuals with whom potential deviators will find it valuable to associate are situated within the group.

In other words, the expected loss entailed in being an "outcast" is greater when there is only a fringe of outsiders with whom one can still associate. But this consideration is offset by the greater difficulties the larger groups will encounter

[99] See Greif (1989), and Milgrom, North and Weingast (1990) for analysis of repeated games of incomplete information that have this structure.

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in detecting deviators. Smaller groups have an advantage on the latter count, and that advantage also enables them to compensate for their disadvantage on the former count. The more compelling is the evidence that a particular individual had engaged in a "betrayal of trust," the more widely damaging will be the reputational consequences for the person thus charged. Hence, the prospect of unambiguous detection and attribution of deviations (from recognized norms regarding the disclosure and non-disclosure of information) augments the deterrent power of the threat of ostracism. This instrument can be more readily available to any group that remains small in relation to the total population of individuals with whom an excluded group-member could form new associations.

The foregoing suggests that small cooperative "networks" of information sharing can be supported among researchers, because cooperative behavior furthers their self-interest in the race for priority, and denial of access to pools of shared information would place them at a severe disadvantage vis-à-vis competitors who participated in similarly small networks.[100] Does that imply that the normative content of Merton's communalistic norm of disclosure really is redundant, and so plays no essential role in fostering conditions of cooperation among citizens of the Republic of Science? Not at all! For, it can be seen that networks of cooperative information sharing will be more likely to form spontaneously if the potential participants start out by expecting others to cooperate, more so than if they expect "trust" to be betrayed; and cooperative patterns of behavior will be sustained longer if participants have reason to expect refusals to cooperate will be encountered only in retaliation for transgressions on their part.

A distinct, but closely complementary line of theoretical analysis also supports the thrust of these conclusions. Social communication networks of scientists formed by the intersection of memberships in restricted "clubs" for private knowledge-exchanges are very likely to form spontaneously and be sustainable when their numbers remain small. But, the emergence of a stable or expanding "invisible college," created by the interconnections among small social networks, is more difficult to achieve in the absence of an articulated ethos that raises the expected propensities of individuals to regularly and frequently disclose

[100] These "circles" or "networks" which informally facilitate the pooling of knowledge among distinct research entities on a restricted basis can exist as exceptions to both the dominant mode of "public knowledge" characterizing open science, or the dominant mode of "private knowledge" characterizing private sector R&D. Thus, von Hippel (1988) and others have described how in fact firms tacitly sanction covert exchanges of information (otherwise treated as proprietary and protected under the law of trade secrets) among their engineer-employees. Participants in these "information networks" who accepted money or remuneration other than in kind, most probably, would be dismissed and prosecuted for theft of trade secrets. For further discussion of employed engineers whose "trading in trade secrets" is tolerated by their employers, see von Hippel (1988); and David (2001) for a proposed the explanation of the observed practices.

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to others in their local network their own pre-publication results and methods, or their evaluations of the scientific claims of others. The problem is that small ensembles relying on trust and frequent contacts permitting personal monitoring are unlikely to be able to function well enough in reaching substantial "closure" on novel scientific propositions – at least not well enough to enable them to attract further membership. Consequently, under the conditions specified, the dynamic behavior of the "invisible college" (i.e., the ensemble of those in very small but overlapping social networks) prevents it from expanding beyond quite restricted (stable equilibrium) dimensions, and thereby attaining the size and diversity of membership that would transform it into rapid generator of contributions to reliable knowledge.[101]

Moreover, when the norms of behavior (i.e., the "custom" within the network in question) are common knowledge, and therefore are part of the shared socialization among all the potential members of networks, detecting and reporting deviant behaviors that warrant punishments will be a more clear-cut matter. Potential deviants will think it more likely that the retribution of ostracism from a particular network will be attended by more widely damaging reputational consequences. It is evident from this that even if the process of socialization among scientists were weak and quite imperfect, the common "culture of and ethos of science" makes it much more possible for the rule of priority to engage the self-interests of researchers in reinforcing adherence to the norms of disclosure. This particularly likely to be the case among those restricted circles of colleagues that Derek Price (1963), borrowing a seventeenth century phrase, famously referred to as "invisible colleges."[102]

The restriction of the circle of correspondence and conversation was not simply a matter of the constraints imposed on travel and written communications. It was a reflection of the economic incentives that impelled individuals to seek entry into these early scientific groups, especially when formal academies made public the existence and identities of their elected members. Those scientists who

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[101] This is shown in the formal modelling exercises based on properties of the stochastic "percolation process" in David (1998, 2002). In this framework the role of a well defined cooperative ethos, re-enforcing spontaneous probabilistic disclosures of pre-publication results and tentative assessments of the reliability of others' findings, serves to establish a baseline probability of "openness" for the agents in the system. If the latter is high enough to allow the network to attract new cooperating agents, the induced improvement in the cognitive performance of the larger ensemble, in turn, leads to more frequent information disclosures. Pursuing the details of this analysis further, however, would take the discussion too far afield from its present historical focus.

[102] Of course the enforcement mechanism described here is neutral with reference to the content of the norms. It could, by the same token, serve to enforce the preservation of "club secrets" when shared interests among the members were sufficiently strong to lead to the articulation of "rules" toward that purpose.

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were admitted to academies obviously would be able to advertise the fact, and hope to collect the resulting "status" rents from their patrons and employers. Rents depend upon scarcity, however, and so admission plainly could not be left open to all. If it were, much of the private *raison d'etre* which the foregoing discussion has identified would soon dissipate, along with the reputation of the academy. Membership selections had to be on the basis of a scientist's ability to contribute to the creation of shared scarcity rents, rather than diluting them. Such a basis could be provided, of course, by social connections, but not decisively so, because in these circles publicly disclosed scientific achievements were the foundation of sustained collective prestige.

Consequently, there was motive aplenty for the individual members of these "parliaments of scientists" to collectively assume the role that could be proclaimed as that of guarding of "scientific excellence" in a world where neither the public nor the lay patrons of science could distinguish good scientists from inept or fraudulent ones. That this commitment eventually would impart a decidedly conservative bias to the scientific proceedings of "the most prestigious academies" would follow, as the night follows day.

Public demonstrations of talent were made more feasible by the institutionalization of scientific assemblies, the publication of "transactions" -- the printing of papers and scientific treaties carrying the imprimatur of the Society or Academy – as well by competitions and the awarding of prizes for the finest achievements. Parliaments of scientists such as the Royal Society of London and the Parisian Royal Academy of Sciences became, among other things, an important and increasingly predictable milieu within which professional scientific reputations could be secured. Furthermore, as has already been argued, the most efficient means open to professional communities for conferring this status among researchers was by the assignment of credit for priority in discovery or invention.

Academies and honorary professional societies also served the second of the beneficial purposes previously identified, and upon which there is much commentary in the historical literature concerned with invisible colleges. They provided an identified, institutionalized environment within which members could draw upon the help of peers to solve their scientific problems. This is more than merely the open transfer of knowledge at meetings. Scientific exchanges, with their attendant evaluation of individual work, may be seen as a form of "patent-pooling" arrangement – albeit without the patents – among the holders of complementary knowledge assets.[103] Scientific societies, like the informal networks of correspondents that they institutionalized, supported a loose coalition of researchers who engaged in repeated altruistic exchanges of their specialized

[103] On economic efficiency and patent pools, see, e.g., Shapiro (2001), Lerner and Tirole (2004).

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knowledge, but also more personal and reciprocated assistance. Their formation thus was socially as well as privately beneficial in these effects upon the productivity of researchers.

To be sure, such sharing arrangements are likely to be less than ideal: scientists in competition with one another would be expected to behave strategically in regard to how much information should be shared, even with fellow academicians, and when deciding which experts in what fields of scientific inquiry should be admitted into their company. But, among those within the society, the norm of cooperative disclosure (especially when accompanied by acknowledgements of priority) provided the basis for repeated, reciprocal information transactions that on balance would be conducive to further enhancing the members external reputation individually and collectively -- both directly as well as indirectly -- by assisting their research and publications.

There can be little doubt that it was more socially efficient to have instituted these formal professional bodies than not. Yet, one should not therefore suppose that such considerations, any more than the internal imperatives of the new methods being employed in the new experimental and observational sciences, were sufficient historically to initiate the institutional movement that made State patronage of formal scientific academies a ubiquitous attribute of modern societies.

5.3 *Continuity, context and change in the institutional organization of science: a reconsideration of the Fontenelle thesis*

Numerous scholarly studies grounded on the archives of the late seventeenth century academies have portrayed the onset of a distinctive phase in the institutionalization of modern science as having been brought about by the growing scale and costs of the new modes of scientific inquiry.[104] The view is that the advent of the expensive new laboratory science (not to mention the new observational astronomical observatories) proved too costly for private patrons, thereby creating "a crisis" in the mid-1660s that was solved by the institutional innovation of State support under the regal patronage of Louis XIV.

Among historians of science, this explanation has come to be known as the "Fontenelle thesis,"[105] was its first articulation was due to Bernard Le Bovier

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[104] This is a theme that has been elaborated in regard to the history of the Académie Royale des Sciences and its continental *sequelae*, the founding of the Royal Society being presented as an exception. See, e.g., Hall (1956:pp. 196-197) and Hall (1983: 218-220). See Lux (1989: pp. 4-7) for a historiographic introduction.

[105] See Lux 1989: pp. 4-7. For de Fontenelle's scientific career the historical memoirs published in the 1720s and 1730s, see Stroup (1993: esp., pp.19-23, 202); Sturdy (1995:pp. 239-241).

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de Fontenelle [1657-1757], a permanent secretary of the Académie Royale des Sciences and the author of the first historical account of that institution. One may suggest that however ancient its provenance, the modern revival of this thesis was not entirely unrelated to the new and potent resonance that it had acquired for historians of science writing in the 1960s and 1970s, when policy discussions of the phenomenon of "Big Science" and its correspondingly big funding requirements and new arrangements for public sector governance and support were very much in the air. [106]

During the past two decades, however, the evidentiary basis for the Fontenelle's line of explanation for the transition to state patronage of science has been questioned; a more persuasive case has been made for viewing the post-1660s phase in the evolution of the institutions of modern science as the continuation of a much broader cultural movement that had been taking place in Europe outside the medieval universities. One significant aspect of those historical developments was manifested in the appearance around the end of the sixteenth century of numerous privately patronized scientific societies and 'academies.'

Quite strikingly, it has been found that seventeenth century science proper played only a very minor part of that wider intellectual reorganization. Of the 2500 learned societies that James McClellan (1985) estimates were created in Europe between 1500 and 1800, at least 700 had been formed during the sixteenth century alone. While some of these organizations were scientific in purpose, these were not prominent among the pre-1550 vanguard. The overwhelming majority of these academies were formed in response to interests far broader than anything resembling the organized pursuit of science. David Lux, the historian of France's scientific academies, stresses that there is no necessity of a close coupling between the nature of cognitive pursuits and their supporting organizational form, and reminds us that young intellectual wines can mature well old institutional casks (Lux [1991: pp.189, 196]):

> "[T]he traditional points of departure for discussing organizational change in science -- della Porta's *Accademia Secretorum Naturae* [founded in Naples, 1589] or Cesi's Accademia dei Lincei [founded in Rome, 1604] -- offer nothing to suggest the intellectual novelties of sixteenth-century science produced real organizational change....Rather than producing organizational change, sixteenth- and seventeenth-century science followed other

[106] On the growth of large-scale research ("big science"), see the essays edited by Galison and Hevly (1992), starting with Hevly's insightful "Afterword" (pp. 355-363). For U.S. science policy and funding for highly capital-intensive "big science" in the post World War II decades, and the reorientation of opinion that occurred in the 1980s, see Smith (1990: pp. 11, 14-15, 156-157).

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> intellectual activity into new organizational forms. Indeed, in strictly organizational terms there is no obvious justification for attempting to isolate science from other forms of intellectual activity before the end of the seventeenth century. Nor is there any obvious justification for portraying science as honing the cutting edge of organizational change....Despite the literature's claims about novel science creating needs for new organizational forms, the institutional history of science across the sixteenth and seventeenth centuries actually speaks to a record in which scientific practice changed only after moving into new organizational forms."

Thus, it is quite unwarranted to try to explain the developing *practice of open science* simply as a consequence of the opportunistic adoption of the humanist "academy" of the late Renaissance as a convenient organizational mode for pursuing investigations of the natural world. Similarly, it would be just as misleading to suppose casually that the motives that led individual participants in the new scientific disciplines to embrace open science and the new methods of inquiry it deployed must have provided a sufficient force to impel the ensuing reorganization of their scientific activities under the auspices of State-sponsored institutions.

As in the treatment of the emergence of cooperation among the mathematicians and scientists of the late sixteenth century, so in examining the subsequent institutionalization of modern science, functionalist historical explanations should be contextually grounded upon an analysis of the incentives that shaped the behaviors of the actors involved. To apply this approach to the circumstances of the founding of the Académie Royale des Sciences, it is essential to begin by acknowledging that a very significant actor was Jean-Baptiste Colbert, Louis XIV's minister of finance and the navy. Moreover, the context of Colbert's initiative in creating the this institution makes it inescapable to suppose that in his actions, and those of his King, the decisive considerations emanated from the political logic of the absolutist State, rather than the structures of social interaction among researchers or the material requirements of sustaining advances on the frontiers of observational and experimental science.

The specific point of departure here is that the foundation of the Académie Royale des Science was connected with a train of events set in motion by Louis XIV's determination in 1661, immediately upon the death of Cardinal Mazarin, that henceforth he would serve as his own prime minister. The announcement of this decision publicly signaled the monarch's intention to rule in his own right, tearing up the extensive networks of political patronage that previously had formed around great ministers of state like Richelieu and Mazarin, and particularly those grand nobles who, in the recent *Fronde*, had mounted a direct military challenge to the monarchy – albeit one that was unsuccessful. It followed from this that *le Roi Soleil*, the unique star around whom the politics of French

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society was to revolve, should take the role of uncontested and universal patron of the intellectual and cultural achievements that were expected to embellish his regime.[107]

To further underscore this point, and its continuity with the previous argument regarding the significance of ornamental motives for noble patronage, it should be noted that three years before the announcement of Louis XIV's approval of the Académie Royale des Sciences, Colbert had initiated the founding of the Académie Royale des Inscriptions et Médailles on February 3, 1663.[108] That was but a modest precursor of his grand scheme for the reform of learning in the realm, which contemplated the establishment of a universal academy under royal patronage incorporating four sections: philosophy, literature, history and mathematics. This plan, however, soon met with determined opposition from existing "corporations" in Paris – particularly the University, the Parlement and the guilds, so that only the part providing for the Académie des Sciences would survive to receive the King's patronage.[109]

It is relevant, too, that in giving shape to this new body, Colbert and his advisers envisaged the academicians as being occupied with the more theoretical and intellectually respectable range of scientific matters. The model appropriate for a royal foundation, even in that field, would seem to have been something closer to the culturally prestigious Académie FranHais. This is especially striking, for Colbert himself was hardly disinterested in the promotion of industry and industrial invention, and, at least since the 1650's the idea of utilitarian benefits ("improvements in the conveniences of life") flowing from research had been a frequent subject of discussion in French scientific circles such as the private Academy Montmor. Indeed, at the very time that Colbert's plans for a "grand academy" were being drawn up, an elaborate proposal for a *Compagnie des sciences et des arts* was circulating in Paris, envisaging improvements in navigation, flood control, better maps, inventions of machines and medicines.[110]

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[107] For a recent account of the context of patronage in seventeenth-century France, and of the political circumstances surrounding Colbert's initiative in founding the academy of sciences, see Sturdy (1995: Chs. 3-4), which supports the interpretation advanced by Lux (1989, 1990).

[108] Subsequently renamed the "Académie Royale des Inscriptions et Belles-Lettres" by royal decision on January 4, 1716). Initially it was nothing more than an informal work group co-opted from the members of the Académie Française under the monarch's exclusive control, and tasked to provide Latin mottoes and inscriptions for the various monuments and medals that commemorated the noble deeds of the king, and the prestige of the French monarchy in general. See Leclant (2004).

[109] See George (1938: pp. 379-386); Lux (1989: pp. 55-56) and Lux (1990) on the grand plan of Colbert. For the bearing of this on the Fontenelle thesis, see Lux (1989: pp.53 ff).

[110] See Briggs (1991:pp. 40-42). This frankly utilitarian plan had been drawn up by Christian Huygens, a correspondent of the private Academy Montmor in Paris. On the

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The Fontenelle thesis has been further undermined by David Lux's (1989) demolition of one of the putative factual bases upon which it had been erected. Supposedly, a suspension of the activities of a number of private scientific academies in Paris in the mid-1660's had precipitated Colbert's intervention to resolve the "funding crisis" by creating an academy that would receive State support. Yet, there had been no precipitating crisis, no evident exhaustion of the resources and consequent "failure" in the private patronage of science in France during 1665-66. The reported suspension of the work of several Parisian scientists, and the discontinuation of the regular meetings that had been hosted there by the polymath *savant* Melchisédec Thévenot [1620-1692] turn out not to have been causes, but instead consequences – first, of the rumours about the progress of Colbert's plans, and then of the announcement confirming his success in persuading the King at least to found a royal academy for the sciences.[111]

Under the finely graduated hierarchical political system of patronage in France at this time, and following the declared intention of the Crown to displace the aristocracy from claims to the trappings of power, including the splendors of grand acts of patronage, the discontinuation of the Thévenot and other scientific academies under private patronage in Paris was only to be expected once Louis XIV had announced that a royal academy would be formed.

The advent of State sponsorship of science in France and Britain during the late seventeenth-century, however, should not be depicted as bestowing unalloyed blessings upon the open science movement. Indeed, in some respects it had retrograde effects. Institutionalization of cooperation in formal organizations facilitated "regulation of conduct" on behalf of the common interests of "insiders," – the membership of the academy. One should not be surprised that such "a company of scientists," finding themselves occupying the top-most echelon the top of the status hierarchy among such academies in France would set rules that restricted members' freedom to communicate with "outsiders." Whereas many among the less prestigious *assemblées* and provincial academies had

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Montmorians, see the footnote immediately following this.

[111] The specific reference is to the meetings at the home of the polymath *savant* Melchisédec Thévenot [1620-1692], who in 1662 had organized (and continued to support) an academy in Paris in the two years following the disbanding of the Monmort Academy in 1664, a decade after its founding by Henri-Louis Habert de Monmort [d. 1679]. See Lux (1989:pp.55-56), on the details of timing on the continued meetings of the Thévenot circle until the announcement of the new royal foundation. See Sturdy (1995: pp. 16-21), on other Parisian scientific assemblies, for Thévenot was not the only private host for such gatherings in Paris during the years 1662-1665, although his 'cabinet' appears to have drawn the most illustrious company. It appears that deterioration in the personal finances, as well as in the health of Monmort added to his dissatisfactions with the ill-tempered disputes and factional rivalries among members of his academy, and so contributed to its closing. This hardly lends substance to the picture of an institutional "crisis" driven by the cost of doing the new science.

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welcomed the establishment of regular exchanges of scientific information with other learned societies, the young Académie Royale des Sciences recorded in its minutes for 15 January 1667, that "the business of Academy should be kept secret and …communicated to outsiders only with the approval of the Company."[112]

Prominent among the early academicians' motivating concerns was the forestalling of pre-emptive publication of their findings by foreigners. The academy's first permanent secretary, although generally enthusiastic about exchanging discoveries with foreign institutions, nonetheless justified the rule of secrecy, on the grounds that research was motivated "by the hope of gaining fame through priority."[113] Thus, at this primordial stage of formal institutionalization one can see the tension between the role played by priority in the reputational reward system, and the norms of openness in science—and its lamentable resolution. Changes in the regulations went even farther this direction in 1688, by forbidding the members to publish without the permission of the Company, and stipulating that permissions were to be granted only after the Academy had examined the manuscript in question. De facto arrangements of the same nature were already in place at the London counterpart of French academicians' institution: a member's paper submitted to Henry Oldenberg, secretary to the Royal Society, would have to have been read at one of the Society's meeting and find approval before being forwarded by him for printing in the *Philosophical Transactions*.[114]

Membership of the new learned societies thus conferred substantial "club goods" benefits to researchers in ways other than the effects of signalling their

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[112] Stroup (1993: p. 205-206) notes that another concern revealed by commentaries in the archives was to protect the new academy from public ridicule that would be all the more biting for being informed about its internal proceedings. This was not a paranoid delusion in the age of Molière, and other play-writes and satirists for whom philosophers and physicians were stock figures of fun. The Royal Society also endured its share of mockery on the London stage in this era.

[113] The phrasing is that of Stroup (1993: p. 206), who comments further: "This view was no less common in the seventeenth than in the twentieth century, despite protestations in both eras about cooperation." Similar concerns were voiced some years earlier by a member of prestigious Academia del Cimento, the Italian mathematician and physiologist A. Borelli, who informed a patron of his preference "to go slowly in beginning this correspondence with those gentlemen of the Parisian [Monmort] academy, since in writing, on cannot do less than communicate something or other, and I fear that this may give those foreign minds an opportunity to rediscover [sic] the things; I am speaking of the causes, not the experiments." Middleton (1971:p.300), as quoted by Stroup (1993:p. 207).

[114] Westfall (1980: p. 239) reproduces a relieved reply from Newton (10 February 1672) to Oldenberg's letter that brought the news that the paper on colors – on which Newton had labored for many years – had been read and "met both with a singular attention and an uncommon applause," and so could be published in the *Philosophical Transactions*.

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talents and achievements as scientists. Those elected would gain access to shared knowledge under institutionally "regulated" terms of cooperation that reduced the risks of their new discoveries reaching the ears and eyes of (external) rivals for scientific priority and fame. That there was also some societal gain from exercising controls over the "quality" of scientific expertise signalled by membership in prestigious academies and societies seems beyond contention.[115] But, the signals of quality emitted by the political process of electing new members hardly were perfect. Undoubtedly, something of value to society was sacrificed in restricting their admission of members with an eye to the effects the candidates' election would carry for the institution's external repute -- even in the judgements of those less well-informed about the scientific merits of the cases.

The same must be said of the academy's efforts to protect collective professional status by requiring general assent before members were permitted to have their discoveries and inventions printed for general circulation by the academy's "house organ." To be generous, one might conclude that what had been attained was a second-best (or maybe third-best) social outcome -- gaining for the "insiders" the efficiencies of exchanging scientific information as a club good, but losing the possibilities of greater positive externalities from more closely approaching a scientifically meritocratic, universally open regime of cooperation in the pursuit of knowledge.

With the growth of these societies in power and prestige, and with the coming of the Industrial Revolution, and in its wake the multiplication and spread of organized centers of learning, private aristocratic patronage and the values it had imparted to the early institutionalized forms of State patronage began to lose strength. The social institutions of science, and in particular the mechanisms for generating collegiate reputations persisted nonetheless. In large part this may be ascribed to the fact that they provided familiar models of arrangements whereby scientists could be screened for employment, and rewards could be allocated by agents and agencies acting on behalf of the new sources of patronage -- namely, industry and the state.

In a much later era, beginning in Germany mid-way during the nineteenth century, modern scientific research was introduced and became established regularly as a university-based activity. A proliferating number of state-supported "academic" research institutes adapted for their use the organizational structures that had become familiar in government bureaucracies, along with the

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[115] Restriction of entry into the medical and legal profession today has been rationalized in a similar manner in a classic essay by Arrow (1963). Medieval urban craft-guild regulations that effectively imposing quality standards, and the employment of hallmarks indicating the place of manufacture, are read likewise, as instances in which non-market restraints that were privately beneficial to insiders (by protecting their collective reputation of their specialized wares) also conveyed "quality control" benefits to consumers.

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institutionalized practices of the scientific academy.[116] Nevertheless, the fundamental problems of reputation and agency upon which the economic analysis here has focused did not disappear. They re-surfaced in these new organizational settings. University patrons, both private and public, along with academic administrators and professors even today are confronted with informational asymmetries and agency problems. Collegiate reputational reward mechanisms operating in academic research communities today parallel in many respects those which have been seen to have characterized the system of European court patronage. In the sciences, especially, academic institutions and individuals also continue to seek ways to mediate the conflicts between the organizational logic of preserving modes and norms open inquiry, and the lure of capturing economic rents from their information about new discoveries and inventions.

6. Conclusions and Implications: the legacy of European feudalism -- and a cautionary tale for modern science policy-makers

The moral to be drawn from the story related here is not simply that the more things change, the more they stay the same.[117] There is in it, too, a bit of historical irony that seems well worthy of notice, especially as it serves to underscore the tenacity of the past's hold on the incrementally evolving institutions that channel the course of economic change.[118]

The nub of this historical twist is that an essentially pre-capitalist, European aristocratic disposition to award patronage for the purposes of enhancing rulers' political powers -- symbolically through competitive displays of "magnificence" -- came to confer value upon those who pursued knowledge by following the "new science" in the late sixteenth and seventeenth centuries. Such men were deemed worth supporting because the public reputations gained by their achievements had an *ornamental* instrumentality for their patrons, at least as much as because their knowledge equipped them to devise technologies that would directly advance their patrons' economic, military or sanitary interests.

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[116] Lenoir (1998), and Lécuyer (1998) provide economists with convenient, brief and insightful points of entry into both the history and the historiography of university-based research, and its connections with the perceived needs of industry and the state in mid-nineteenth-century Germany and twentieth-century America (pre-WW I), respectively. One may consult Vereek (1992) for an interesting economic interpretation of the less well known organizational reform of the German universities and research institutes along Prussian bureaucratic lines, which took place during the last two decades of the nineteenth century.

[117] The French expression has more punch*: Plus ça change, plus c'est la meme chose.*

[118] On "path dependence" in the dynamics of economic systems, see, e.g. David (1988, 1992, 1993b, 1997b, 2001a, 2005, 2007b).

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The norms of cooperation and information disclosure within the community of scientists, and their institutionalization through the activities of formal scientific organizations thus emerged (in part at least) as a response to the informational requirements of a system of patronage in which the competition among noble patrons for prestigious clients was crucial. Likewise, the initiation of State patronage of scientific academies was propelled as much by the ornamental motives of absolute monarchies as it was by an appreciation of the new knowledge as a potential foundation of wealth and power. These developments echoed the former rivalries among the noble houses of the principalities that eventually were absorbed in the formation of Europe's ascendant nation-states. They were thus part of the legacy of fragmented political authority left by western European feudalism, and in this regard they paralleled the conditions of "common agency" contracting in the late Renaissance relations among clients and patrons.

A comparison therefore might be drawn with the alternative circumstances of a monolithic political system, such as had prevailed elsewhere -- as in the Heavenly Empire of China, to cite a well-known case in point. In place of any comparably dominant single principal-patron, the multiplicity of Western Europe's contending noble courts created conditions that were more favorable for the agent-client members of the scientific community. This was so both in terms of the "information rents" they were able to retain on their specialized knowledge, and their collective development of greater professional autonomy.

More than one economic historian's speculations about the reasons for the material ascendancy of "the West" has drawn attention to the possible significance of the contrasting political environments of late medieval and early modern Europe, on the one hand, and that of contemporaneous China on the other.[119] The more familiar suggestion is that the degree of military security and centralized political control achieved under the Ming dynasty (1368-1644) left the reception and retention of technological innovations hostage to the whims of a single court; that it removed the pressures that were experienced by rival European rulers and had led them to encourage the growth of economic activity within their territories as a basis for tax revenues.[120] David Landes, in *The Wealth*

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[119] Joseph Needham (1969) posed the problem of why it was that though Chinese civilization had been "more efficient than 'occidental' civilization in applying human natural knowledge to practical human needs" between the first century B.C. and the fifteenth century A.D., Western Europe emerged as the technologically and industrially more dynamic society in the centuries that followed. But, Needham's "reason why" had more to do with class and culture, than with political structure: for all its rationalism, China's Mandarin bureaucracy could not make up for the lack of a "mercantile culture" that he saw as the core of Europe's capitalism and expansionism.

[120] See, e.g., Rosenberg and Birdzell (1986), p. 137: "In the West, the individual centers of competing political power had a great deal to gain from introducing technological changes that promised commercial or industrial advantage, and hence greater government revenues, and much

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and Poverty of Nations (1998: p. 38) presents a political variation on the same theme in the following characteristically robust formulation: "Ironically, then, Europe's great good fortune lay in the Fall of Rome and the weakness and division that ensued....in those middle years between ancient and modern, fragmentation was the strongest brake on wilful, oppressive behaviors. Political rivalry and the right of exit made all the difference."

That view resonates with the cogent observations made by Joel Mokyr in *The Lever of Riches* (1990:pp.178, 232) and *The Gifts of Athena* (2002: Ch. 6), which point to the greater scope that centralized political control might allow for the suppression of technological innovations and new commercial practices.[121] On balance, then, the argument that fragmented control offered protections from resistance to technical innovations which might disturb incumbent seigniorial or bureaucratic elites, or otherwise disrupt established economic interests (of the guilds) and so jeopardize the sovereign's fiscal base, seem quite rather more to the point than does the notion of rival princes encouraging technological innovations as part of their quest for new revenues.

That much having been granted, the thesis advanced in the foregoing pages has a new and somewhat different thrust. It is directed towards accounting for the paradoxical observation that the "scientific revolution" is a West European cultural product, despite the remarkable record of previous scientific inquiry and technological accomplishments in China that has been so richly documented by Joseph Needham (1954) and his collaborators.[122] Its novel burden is the idea that

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to lose from allowing others to introduce them first."

[121] But, on the other side of the coin, one also must observe that in the European context, where entrenched craft-guild corporations (monopolies) exercised control over an extensive national territory – as was the case in eighteenth-century France, it was the absolute monarch's ability also to grant *privilèges* to independent inventors that served to counteract the chartered monopolies' generally baleful influence upon industrial innovation (see Hilaire-Pérez 2000). Obviously, the question is complex and cannot be readily resolved here.

[122] Joseph Needham believed that the emergence of the "scientific revolution" in Europe rather than China, like the rise of industry, was attributable to the "mercantile culture" found in the one place and not the other. China's agrarian bureaucratic civilization, for all its interest in nature and technological precocity was lacking in those "bourgeois values" that are held so vital in Marxian analysis. Being deprived of the opportunity to fuse scholars with craftsmen, China thus was unable "to bring to the fusion-point the formerly separated disciplines of mathematics and nature-knowledge" (Needham, 1956: p. 34, as quoted by Rosenberg and Birdzell, 1986: p. 88). This thesis has not gone without criticism in the specialist literature (see, e.g., the points reviewed by Mokyr (1990: p.230-231), and Cohen (1994:pp.449-482). The argument advanced here concerning Europe, however, is not meant to be construed as another attempt at a grand "social class" explanation -- indicting the absence in China of European vassalage and "aristocratic values," the culture of noble patronage, and the sublimation of feudal conflicts in rivalries for prestige among the princely courts.

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that the critical institutional side of the scientific revolution of the seventeenth century -- which saw the pursuit of new knowledge carried on under the patronage of rival political authorities at numerous geographically dispersed and culturally diverse courts and academies -- contributed not only to the flourishing of science, but to preserving the advances that had been made in the stock of reliable knowledge upon which further research could build. It was able to do so by providing the *protection of statistical independence* from the workings of the variety of systematic forces and exogenous disturbances to order that could interrupt the advancement of science in any one place and time, and that often had done so. By the same token, the practices of open science that developed within the European political and social context were conducive to maintaining the exchange of information through radiating networks of distributed and intellectually variegated actors, thereby stimulating and imparting momentum to cumulative process that uniquely characterized the advancement of science in that region of the world.

In the course of a lively and comprehensive review of the state of the historical literature on China's "failure" to sustain the technological supremacy that it had attained vis-à-vis Europe at the end of the fourteenth century, Joel Mokyr (1991:p. 223) suddenly recasts the question in the following way:

> "China's lack of progress after 1400 is striking not only in the light of Europe's success, but also compared with it own performance in the previous centuries…The European experience seems to suggest that nothing succeeds like success….Why does such a *cumulative* path-dependent model not work for China?"[Emphasis added].

Although no really satisfying response was forthcoming from Mokyr's ensuing pages, this rhetorical restatement nonetheless was insightfully provocative. The arguments tentatively advanced here, which have pointed to the functional value of distributed open science communities in providing a mode of insurance against exogenous interruptions to knowledge accumulation and erasures of the society's collective memory file, can perhaps be taken as a belated suggestion of the direction in which historians of science and technology should continue searching for answers to Mokyr's question.

The existence of the vital background conditions in Europe, of an extensive contiguous territory over which political power was decentralized to multiple, contending centers of authority, of course, was contingent upon a distinctive aspect of the region's cultural and political history. The logic of vassalage institutions in the medieval epoch had given rise to a political landscape of fragmented and contending principalities, each governed on the basis of personal authority. Thus, baldly to summarize the present thesis: the emergence of the characteristic institutions and organizational features of open science that have

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played so vital a role in generating the sustained material achievements of the era of modern economic growth, may be said to be western Feudalism's greatest gift to Capitalism.

There is, then, a broad lesson to be drawn for the present from this extended inquiry into the historical origins of open science institutions. The methods of modern science themselves were not, and still are not sufficient to create either the unique cultural ethos associated with the Republic of Science. Nor did they suffice to impel a transition to the public patronage of specialised scientific academies and kindred institutions whose rule and norms of behavior reflect and advance the collective, cooperative purposes of researchers engaged the open pursuit of knowledge. The historical record provides scant assurance that the methodological power and technical sophistication of modern scientific research alone can safely be relied upon to permit only those modes of organization and governance that will sustain the functional attributes of institutional infrastructures that support the open science regime, however conducive that later may have been to the rapid growth of the stock of reliable knowledge.

The institutions of open science, rather than having emerged and survived as robust epiphenomena of a new organum of intellectual inquiry, are thus seen to be independent and in some measure fortuitous social and political constructs. They are cultural legacies of European history that continue profoundly to influence the systemic efficacy of the scientific research process. Being in some significant degree exogenous to actual scientific practice, these features of the institutional landscape in the world's representative democracies can be subjected more easily to substantial re-design, or otherwise manipulated as potent instruments of science and technology policy. This is both a good and a worrisome thing. Wise and forward looking policy-making for this sensitive part of the modern institutional infrastructure needs to pay particular heed to the complex and contingent character of these organizational instruments' evolution, and therefore to respect the potential fragility of the institutional matrix within which modern science has flourished.

Open science norms and institutions are a social innovation whose workings must be continually re-created as "social facts". This regenerative process depends upon the scientific practitioners themselves recognizing that much of the "power" that their research communities possess for the successful pursuit of reliable knowledge derives from their personal appreciation of and commitment to behaviors that conform broadly to the informal "cognitive norms," as well as to the formal regulations governing their activities. In short, the sustained functionality of these vulnerable institutional legacies ultimately rests not in the hands of some distant, unknown and hopefully wise designers of public policy, but upon scientists' acceptance of responsibility for day-to-day individual

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and collective actions in support of “good scientific conduct conjoined with good technique,” and for transmitting that ethos to the future scientists who look to them as mentors.

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