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Patronage, Reputation and Common Agency Contracting in the Scientific Revolution: From Keeping 'Nature's Secrets' to the Institutionalization of 'Open Science'

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This paper 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 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. The process of revision also subsequently 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. Also memorable, even at this advanced stage in the career of this paper are the trenchant comments and constructive critical advice provided by a wonderfully learned and mixed company of economists, economic historians and historians of science: Robert Auman, Robin Briggs, Avner Greif, Scott Mandelbrote, Ian McLean, Trond Olsen, Gerald Silverberg, and Noel Swerdlow. Sheila Johansson's keen editorial eye and ever-ready pen effected many improvements throughout this most recent draft. None of those whose help is gratefully recorded here should be held to blame for deficiencies that may be found to have survived their corrective efforts.

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 more favorable contract terms (especially with regard to autonomy and financial support) for the agent-client members of the nascent scientific communities. This left the new scientists better positioned to retain larger information rents on their specialized knowledge, which in turn tended to encourage entry into the emerging disciplines. They also were thereby enabled collectively to develop a stronger degree of professional autonomy for their programs of inquiry within increasingly specialized and formal scientific academies which, during the latter seventeenth century, attracted the patronage of rival absolutist States in Western Europe.

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Prologue: The situation of 'open science' in the modern world – a motivation

Science and technology policy and innovation policy broadly conceived have moved into prominence on the economic policy agenda of the European Union, where the Council of Ministers (meeting in Lisbon in October 2000) identified raising the R&D investment rate (by half again, to the level of 3 percent of GDP) as one key instrument for attaining the goal of improving the "international competitiveness" of the region. A second, concurrent strategic approach has been to call for more fundamental institutional reforms in public sector research and training. The Commission of the European Communities has been seeking to stimulate discussion and debate upon institutional and regulatory changes that would redefine the future role of the region's institutions of higher education and its public research organizations in what is hoped would become a more technologically innovative and business-oriented European Research Area.[1]

A sense of urgency has attended these discussions, which are not only reminiscent of, but explicitly reprises discussions and debates about the organization and public funding of R&D activities in the United States a decade ago. Similarly, there has been no evident hesitation on the part of many science and technology policy-makers in advancing and entertaining proposals for potentially far-reaching institutional innovations. It was not entirely coincidental that such issues were broached for serious discussion in the U.S. during the late 1990s, against a backdrop of unprecedentedly large contractions in the levels projected for real federal expenditures for both defence-related and civilian R&D over 1997-2002 interval.[2] Lively debates over science policy had erupted in the U.S. on many previous occasions. But, the most recent episode would seem to have been the first sustained occasion to see the fundamental questioning of many of the infrastructure institutions and organizational commitments that framed the nation's science and technology system since the major restructuring initiated by the 1945 report of Vannevar Bush.[3]

Even when the U.S. federal funding picture seemed to improve for basic research, opinion-leaders in the areas of science and education there, and their counterparts elsewhere have continued to ask whether universities should continue to be supported as the primary sites for conducting basic research in an "open" fashion which facilitates its close integration with graduate teaching. Some have opined that universities' emphasis on research is seriously detrimental to undergraduate teaching. Others wonder whether an 'academic research' environment is compatible with concurrent efforts to expand the sphere of collaborative R&D with industry, pro-active forms of "technology transfer," and to make more extensive use of intellectual property and other means of establishing a proprietary interest in the research activities of faculty, staff and students? Might it not be better to hive off both basic and

[1] See, e.g., Commission of the European Communities (COM 2003). Emulation of what is construed to be 'the American model" of closely coupling business and university research has emerged as a significant policy theme in EU member countries. See David (2004) for a critical examination of the currently fashionable European government proposals to follow the institutional experiment initiated by the Bayh-Dole Act (1980); and, in the UK particularly, to encourage universities in "managing" and "exploiting" their intellectual property assets (including the potential consultancy services of their faculties) in order to generate revenue support for research and teaching.

[2] See Boesman 1997, Koizumi, 1997, and Mowery 1997.

[3] On the Bush Report, the recurring issues in US science policy debates, and the prelude to the recent discussions, see, e.g., David 1996, Boesman 1997 and references therein.

applied research into specialized institutes, thereby resolving conflicts that arise between the universities' conduct of their traditional functions and the drive on the part of other organizations and agencies (both private and governmental) to control information flows in order to better exploit new findings? Issues similar to those concerning the future role of the university in the "national innovation system" also have arisen in discussions of moves towards 'privatizing' formerly state supported non-military research organizations as the National Laboratories in the U.S., and the civilian applied science institutes maintained by many governments elsewhere. Following perceptions of the "American model" in this matter, the re-orienting national research institutes towards application and commercial exploitation of their research output [4] The wave of sceptical re-consideration directed toward these long-standing institutions and familiar organizational practices, has not been unsalutary, because it has been largely informative even when not invariably constructive.[5]

The same cannot be said, however, regarding the ongoing global drift of institutional change towards a stronger, more extensive and globally harmonized system of intellectual property protections. In the view of an increasing number of observers, the consequence has been the to dangerous disturb the delicate balance between private property rights and the public domain in data and information, especially in the sphere affecting scientific and educational activities.[6] To state the point concretely and succinctly, the effective functioning of the institutions of 'open science' are today being increasing placed in jeopardy by the patenting of research tools, particularly in the areas of software and the biomedical sciences, and by the emergent conjunction of statutory protections for technical systems of "self-help" for copyright holders and *sui generis* legal protection of property rights in databases.

The latter, two-pronged formation threaten the effective "disintegration" of a cornerstone of the historical regime of copyright that emerged in the post-Gutenberg epoch, and which subsequently came to under-gird the institutionalized practice of open science. A foundational precept of the legal regime of copyright has been that whereas ideas, facts and their modes of expression "naturally" belong in the public domain, granting to private parties *temporary* possession of exclusive rights to exploit these may serve important, socially beneficial purposes. Notable among these has been the protection of the originators of expressive material from the economic damage of unfair competition by unauthorized copyists, in the same way that inventors are enabled to more fully appropriate the economic benefits of their creations by the awarding of patent rights. In the terminally "out-of-balance" intellectual property rights regime towards which we seem to be headed, the operating premise is that "information-goods" which can be fixed in digital form are "intellectual assets"; accordingly, they should be treated symmetrically with all other forms of private

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[4] For entry points to the vast literature, see e.g., David, Mowery and Steinmueller (1994) on university-industry R&D collaborations; Guston and Keniston (1994) on university relations with the federal government; Branscomb (1994), Cohen and Noll (1994) on the National Labs.

[5] Of course, all distributions have their extremes. See the effort in David (1996) to rectify one particularly flagrant outlier among the criticisms directed at the public funding of science and calls for reforms of a sweepingly destructive kind.

[6] On the role of the public domain in scientific and technical data and information, particularly in relation to exploratory research, see David (2003b); more generally, consult the other contributions to the National Academy of Science Symposium publication in which it appears. On patents, see, e.g., Walsh, Arora and Cohen (2003), and other chapters in the same source.

property, and therefore should be subject to *perpetual* private ownership under the protection of copyright and copyright-like statutes. Moreover, all technical means of enforcing those rights – through the deployment of digital water-marking, encryption and electronic digital rights management systems -- should be kept safe by legal restraint of technological means that might be used to defeat them.[7] In other words, 'technological self-help' for holders of intellectual property is to be reinforced by criminal sanctions against the use of 'technological self-help' by users of data and information-goods.

Whatever the import of these recent legal trends may be for the future of commerce in digital music and video information-goods, the focus of the concern motivating this essay is that when taken in conjunction with broader pressures for the curtailment of public funding of knowledge production and distribution, they are likely to have retrograde effects upon the long-run vitality of scientific and technological research and all the societal benefits deriving therefrom. The advancement of knowledge is a cumulative process, one that depends on the rapid and widespread disclosure of new findings, so that they may be rapidly discarded if unreliable, or confirmed and brought into fruitful conjunction with other bodies of reliable data and information. "Open science" institutions funded from the public purse provide an alternative to the intellectual property approach to dealing with difficult problems that arise in the production and distribution of information under competitive market conditions.

Although not a perfect solution to those problems, and one that requires sustained public patronage of research agents who are enjoined to quickly disclose and freely share information about their methods and findings, as a mode of generating reliable knowledge, "open science" depends upon a distinctive non-market reward system to solve a number of resource allocation problems that have their origins in the peculiar characteristics of information as an economic good. The collegiate reputational reward system (conventionally associated with open science practice in the academy and public research institutes) does create conflicts been the ostensible norms of 'cooperation' in the full disclosure of methods and finding, and the impulses of self-interested actors to engage in non-cooperative behaviors. The latter also draw greater encouragement, if such was needed, from the prizes that await the victors in races to establish claims to priority of discovery or invention. Despite these and still other sources of inefficiency in way the open science regime affects the allocation of research resources, it is a mode of organizing and governing the conduct of science that can properly be regarded as uniquely well-suited to the goal of maximizing the rate of growth of the stock of reliable knowledge. It performs that function both directly through fundamental scientific discoveries and the devising of new tools and techniques for research, and well as indirectly through a variety of complementary interactions with the proprietary private-sector R&D regime that is oriented to produce and market applied science-based innovations.[8]

But, the programs of exploratory science and engineering research that are critical for sustaining growth in the knowledge-driven economies also are particularly exposed to the

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[7] For further discussion of the significance of the U.S. Digital Millennium Copyright Act (1998), and the European Union Directive for the Legal Protection of Databases (1996), and the likelihood that measures of both kinds will come into force in both the U.S. and the EU, see P. A. David (2003c).

[8] For further treatment of these points, which are not developed here, one may consult David (2003d) and the references supplied therein.

adverse effects of the high access charges for data, information and research tools that the owners of legal monopoly rights are free to impose. 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, thus have effect that augment and amplify the chronic under-provision of documentation and annotation activities necessary to maintain reliably accurate and up-to-date public database resources. Both can significantly 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 constitute complementary sub-systems. The public policy challenge that needs to be faced, consequently, is to keep the two sub-systems of open science and proprietary R&D in proper balance. This requires both adequate public funding of "open science" institutions and programs, and deliberate measures to halt, and in some areas reverse the excessive incursions of claims to private property rights over material that would otherwise remain in the public domain of scientific data and information.[9]

Nevertheless, today there are many writers in the business press, academic economists, lawyers and policy makers who see the matter quite differently. The centrality of information technologies and information goods in the phenomena that are associated with the New Economy has suggested that the world is now leaving behind the epoch of material capitalism and entering 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 "presentist" concerns that has just been reviewed, the thrust of the arguments set out in the following pages can simply be this: to pursue the policy path leading toward the vision of perfected "Intellectual Capitalism" would paradoxically lead us backwards, towards that truly darker past from which western European societies rather fortuitously managed to escape. The modus of liberation – one could say "enlightenment"—was provided by the inter-twinning of intellectual and institutional transformations that first coupled the "Scientific Revolution" of the seventeenth century with the emergence of "open science," and subsequently reinforced the institutionalization and stabilization of public patronage of "open science" in a healthy, productive system of interactions with proprietary, commercially-oriented R&D activities.

To closely read the history of those processes is a beginning step towards fully appreciating that the constellation of differentiated institutions that have supported and structured the conduct of scientific and technical research is a fragile cultural construct. It is the legacy of an extended, intricate and contingent historical process that cannot be assumed to have been generated by some underlying self-rebalancing, self-regenerating system that

[9] Much of the science policy literature has focused upon issues of funding, but legal and other institutional impediments to academic research collaborations, and a variety of ameliorative proposals are discussed by David and Spence (2003: esp. pp. 45-53), and David (2004).

requires no maintenance or protection. With this, as with other delicate pieces of (social) machinery, a substantial measure of caution and patience in seeking to more thoroughly understand its workings and the manner of its construction would seem a minimum that should be asked of those having the power to tinker with the institutional infrastructure of modern system of innovation; and particularly, when they are moved to do so by genuine aspirations fix one or another among the myriad (of often rather transient) societal problems that are likely to be brought to the notice of people in elected offices.

At such a time as this, when proposals for quite radical institutional changes are in the air, it therefore may be particularly appropriate to pause for a look backward to the historical circumstances in which the social ethos and some of the key institutions of modern 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, will at very least elicit some greater appreciation for the emergence of "open science" in western Europe as an extraordinary piece of cultural good fortune. It also may serve 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 serve purposes very remote from those for which the course of their historical evolution has left them well adapted.

Undertaking an inquiry of that kind asks that economists and economic historians suspend their probing of the interior of the 'black box' of technology, at least long enough to seek a clearer view of what goes on inside the other major component of modern research system, the still relatively understudied "black box of science." The open science system, if we may call it that, has only lately begun to be studied using the tools of economic analysis. Far more is understood about the evolving institutional structures affecting resource allocation and the mechanisms, and especially about the workings of the intellectual property mechanisms that enable private appropriation of research benefits, than is systematically known about the origins and effects of the corresponding institutional infrastructures shaping the economic organization of publicly supported R&D.[10]

The desirability of closing this particular lacuna in the economics and economic history literatures has been no less evident to those who have approach 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 begun to direct attention to such a program, Douglass North (1990:p. 75) characteristically perceived in it both a significant challenge and a promising opportunity:

[10] Within the past decade, however, the situation has begun to change. 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), the 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 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.

> "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 was too strong a hint to be ignored, a growing company of economic and technological historians, along with economists of an historical persuasion, lately have begun to turn their attention to studying the scientific foundations of the modern "knowledge economy."[11]

The subject matter of this essay therefore can seen to be situated squarely at the intersection between the concerns of the new economics of science and the economic history of the production and distribution of knowledge, the economic analysis of social institutions, and current policy debates over the future organization of national science and technology programs. With this more than ample motivation serving to frame the historical inquiry, the obvious next step must be to fix precisely upon the set of key institutionalized norms and practices of modern scientific research systems call for more explicit analytical and empirical attention than economists and economic historians have hitherto accord to them.

1. The puzzling phenomenon of "open science"

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,' 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 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 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. Within the sciences proper, however, there are recognized norms and conventions that constitute a clearly delineated ethos to which members of the academic research community generally are disposed to publicly subscribe, whether or not their individual behaviors conform literally to its strictures. The norms of 'the Republic of Science' that have famously been articulated by Robert K. Merton (1973) sometimes are summarized under the mnemonic CUDOS: communalism, universalism, disinterestedness, originality, scepticism.[12]

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[11] 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 LJcuyer (1998), Lenoir (1998), Headrick (2000), Mokyr (2002), and Foray (2004).

[12] See Merton (1973: esp. Ch. 13); for CUDOS, see Ziman (1994, p. 177).

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 which is 'reliable' is fundamentally a social process. The precise nature and import of the new knowledge therefore ought not 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 therefor 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, form 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 to 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. Accompanying the epistemological transformation effected by the fusion of 'experimentalism' with Renaissance mathematics, the late sixteenth and early seventeenth centuries witnessed a transition from the previously dominant ethos of secrecy in the pursuit of Nature's Secrets, to a new set of norms, incentives, and organizational structures. These reinforced scientific researchers' commitments to rapid disclosure and wider dissemination of their new discoveries and inventions.

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 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.[13] Although forming a clearly distinct 'problematique,' the neglected historical puzzle taken up here involves aspects of the Scientific Revolution that may hold some of the answers to those "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.

[13] 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.

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. Medieval science, shaped by a political and religious outlook that encouraged withholding the "secrets of nature" from the "vulgar multitude," similarly 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 the idea in elusive, enigmatic terminology, 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." 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; according to Dobbs (1975: 27), "*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...."[14] 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).

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[14] 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).

Because the fruits of this mixture transcended the descriptive powers of ordinary language, on that account if on no other, they were hardly fit subjects for broadcast communications. Alchemical texts deliberately employed 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 who was well acquainted with 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."[15] 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.[16]

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, especially not those constrained by the regulations of the urban craft-guilds aimed at preserving the "mysteries" of their industrial arts.[17] Pamela Long (1991) has made 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 (1991) thesis in regard to the effects of medieval guild policies upon both secrecy and the dissemination of (craft) knowledge are somewhat

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[15] 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.

[16] On Newton's studies of alchemical texts and engagement in experimental researches, see also the monograph by Dobbs (1975) and the detailed evidence that Figala and Petzold (1993) present about his sustained personal contacts with the circle of fellow alchemists in London.

[17] Among recent discussions of medieval and early modern craft guild practices in this regard, see the excellent discussion by Long (1991: 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 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. This is implied by the points briefly noted in the following paragraphs, which draw upon the more "traditional" interpretations of urban craft guild regulations (e.g., Thrupp 1942, Throop 1963, Smith 1963, Unwin 1964) as displaying all the lineaments, and effects of monopolistic cartels.

subtle, and so when stating its details, some special care is appropriate. But the nub of the argument turns on the 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."[18] 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; 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.[19]

The craft-guilds anti-competitive practices had unintended as well as intended consequences impeding the diffusion of industrial production knowledge. First, 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. Secondly, 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 (those 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.

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[18] 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.)

[19]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 labor market 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 some detail by Long (1991: pp. 870-875) – but which have been construed quite differently by subsequent commentators (e.g., Merges 2004), ostensibly following the reinterpretation of the role of the craft-gilds by Epstein (1998).

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.[20] Indeed, the indirect effect of the guild's joint monopolistic (cartel) position created positive incentives for the members to guard information regarding idiosyncratic practices that supported product differentiation (distinctive shaping, coloration and finishing of their wares). The analytical argument goes this way: Product market monopolists have weaker incentives than do firms in competitive industries when it comes to introducing introduce technical innovations. Their readiness to invest in technological invention (their own, or those of others) will be the weaker on that account. This invention-inhibiting effect of monopoly vis-à-vis competition is the greater, the greater (more 'drastic') is the unit cost reduction afforded by the inventions in question, all of which has been made quite familiar by Arrow (1962). Therefore one hardly should be surprised by the fact that where entrenched craft-guild monopolies exercised control over an extensive national territory – which was still the case in France for most of the eighteenth century, it was only readiness of an absolute monarch to grant *privilPges* enabling production of new goods and the working novel techniques that operated to counteract those institutions' generally baleful influence upon industrial innovation.[21] Given the (rational) expectation on the part of a guild master that the other members of his corporation would not be any more favorably disposed towards the introduction of novelties into their trade than was he, the expected rate of obsolescence of their craft methods would be lower. Consequently, and the expected present value of devoting resources to keeping secret the "mysterie" surrounding existing products and methods would tend to be 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, 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; since that would eventually create journeymen for others to employ, an potential rival masters in the new locale, the 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.[22] Similarly, knowledge

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

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

[22] 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 overlook aspect of patents, see also David and Olsen (1992).

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 prevent their falling into the possession of maritime and commercial rivals.[23]

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 simply an epiphenomenon of the profound philosophical and religious reorientations that have been presented as underlying the Scientific Revolution of the seventeenth century? Or, should we instead see the Scientific 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 insight into the problem may be gained by turning to the economic logic of the organization of knowledge producing activities.

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

Indeed, it is quite possible to construct a functionalist account of the institutional complex that characterizes modern science, one that argues for the greater economic and social utility of the ideology of the open pursuit of knowledge and the norms of cooperation among scientists. Moreover, one can also demonstrate the "incentive compatibility" (with the norm of disclosure) of a collegiate reputation-based reward system grounded upon validated claims to *priority* in discovery or invention.[24] The core of the rationale that I have offered (in

[23] On maps and secrecy see, e.g., Boorstin (1983: 267-78). The one field of industrial technology that presents a striking exception to these generalization 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, notably *De re Metallica* by Agricola [Georg Bauer, 1490-1555] which appeared 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. (See, e.g., Hall (1983: pp. 241-242). It may be remarked that the mining of minerals was conducted by royal monopolies in the principalities of Europe during this era. Hence, the disclosure of techniques for the more efficient exploitation of these assets would not have been detrimental to the profitability of these enterprises. See Hirschliefer and Riley (1979) on the generality of this familiar point in the theoretical literature on the “appropriability problem.”

[24] A rationale of precisely this kind is developed by Dasgupta and David (1987, 1994). See David (2003a) for a more recent restatement and elaboration.

earlier papers written with Partha Dasgupta) to "account for" the peculiar information-disclosure norms and social organization of modern science is concerned with the greater efficacy of open inquiry and complete disclosure as a basis for the cooperative, cumulative generation of predictably reliable additions to the stock of knowledge. In brief, openness abets rapid validation of findings, reduces excess duplication of research effort, enlarges the domain of complementaries and beneficial 'spill-overs' among research programs.

The advantages of treating new findings as "public goods" in order to promote the faster growth of the stock of knowledge are to be contrasted with the requirements of secrecy for the purposes of extracting material benefits from possession of information that can be privately held as intellectual property. Suppressing the details of the argument which turns upon the difficulties of monitoring research effort, and the consequent need to tie the reward system to observable outputs, and hence to priority in revelation of purported 'findings', the main point can be put in the following overly stark, unqualified way. We may say that whereas Technology (*qua* social organization) is devoted to maximizing wealth stocks corresponding to the current and future flows of economic rents, and so requires the control of knowledge through secrecy, or exclusive possession of the right to its commercial exploitation, Science (*qua* social organization) is about maximizing the rate of growth of the stock of knowledge, for which purposes public knowledge and hence patronage or public subsidization of scientists is required. This functional juxtaposition suggests a logical argument for the existence and perpetuation of institutional and cultural separations between the communities of researchers forming 'the Republic of Science' and those engaged in proprietary scientific pursuits within 'the Realm of Technology': the two distinctive organizational regimes serve quite different but potentially complementary societal purposes.

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

In what sense can that be taken to constitute an explanation? In seeking to uncover the "logical origins" of the institutions of modern science in their presently observable functional value, this style of argument ignores the details of their historical evolution. Rather, the most persuasive economic rationale that can be constructed along those lines would seem to presuppose these arrangements were instituted by some external agency, such as an informed and benevolent political authority endowed with fiscal powers. Of course, once the idea of the open, cooperative pursuit of knowledge had been translated into established practice, even among very informal and loosely organized networks of scientists, it would become quite 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 its formal institutionalization.

Consequently, where the ahistorical functionalist, "logical origins" style of explanation falters most obviously is when we demand to be told why and how -- through a spontaneous and undirected process, possibly driven by the self-interests of the individual human actors -- the rule of full disclosure and cooperation in the search for knowledge, could ever otherwise have come to be established in the first place. After all, the modern economic analysis of the so-called "appropriability problem" identified by Nelson (1959) and 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.

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 historically distant social setting.

2.2 *Historical origins: the problems with Eamon's 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, he depicts the transformation that occurred in seventeenth century England as the product of converging intellectual movements of reform. One of these was Francis Bacon's polemic against the tyranny of philosophical systems ossified by unchanging subservience to intellectual "authority", and his 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 is found by Eamon in Puritan social reform politics, and particularly the influence of the ideas advanced by the circle around Samuel Hartlib, who saw collaboration among scientists and inventors as means of achieving universal knowledge, the unity of religion, and the improvement of human welfare.

The nearest Eamon (1985,1994:esp.Ch.10) comes to offering a materialist explanation for the emergence of open science 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 the argument, this furnished the *virtuosi* of the early seventeen century 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, Francis Bacon [1561-1626] contrasted the power of cumulative improvement and confirmation by many practitioners, with the stagnation of thought in the ancient philosophical traditions. Natural philosophers, according to Eamon, had been 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 difficulties bedevil this 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 were 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, an illustrative "sample" of only three texts is presented. Significantly, they form a rather different picture, one on which Eamon discussion fails to comment. First in the sample 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 care of vineyards and orchards, composed c. 1350 by the Bavarian, Gottfried of Franconia -- most probably he too was a cleric who and acquired his skills when engaged with just such matters on the Whrzberg estates of the prince-bishop which were under the administration of ecclesiastical ordinaries.[25] Eamon's third offering 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.[26] It seems rather unlikely that 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 gain support for wider European research-travels?). These were people that 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.

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.[27] 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

[25] 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.

[26] 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 If Newton could hold a Cambridge fellowship which also supported his alchemical pursuits, three centuries earlier a Nuremberg blacksmith also make enough of a living to support his sideline as a magus.

[27] 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).

century did a marked shift of become evident in this regard, signalled by the concurrent appearance of the *EncylopJdie* (1751-65) and the *Descriptions des arts et des mJtiers* (1761-88) -- the Acad**J**mie des Science's own fulfilment of the old Baconian quest for a History of Trades. Indeed, Robin Briggs' (1991:p. 39) study of the archives covering the preceding era concludes that until 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 continued to impede direct inquiries into existing industrial operations.[28] Pamela Long (1991: p. 916) concurs in the latter view, referring to project of the *Descriptions des arts et des mJtiers* as "an attempt to penetrate the secrecy that was commonplace among craftsmen."

Where that not enough, yet another problem deserves notice. If Baconian ideology of open knowledge thoroughly pervaded the Puritan reformers around the social philosopher Samuel Hartlib [d. 1662], 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.[29] In the view of B.Y.T. Dobbs (1975), the leading modern historian of Newton's alchemy, the fact that Newton never published a single scientific paper based upon the intense researches he devoted to the production of "philosophical mercury" (and which his manuscripts recorded in great detail) should not lead us to surmise that Newton's 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 would have 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.[30] Evidently, *mentalité* is a complicated matter. Two quite

[28] 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 following characteristically ironic illustration: "Roberta, Huygens, Edm**J** 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 academy 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 *Jleve* of the Acad**J**mie (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).

[29] 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.

[30] 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 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).

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 alchemy, the other with the practices of the new “mechanical philosophy”.[31]

Although it would be only sensible to firmly reject any *wholly* materialist dismissal of the power of intellectual currents to motivate alterations in social behavior and institutions, I 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 Acad**J**mie royale des Sciences that subsequently was followed by the many scientific bodies created later in that image.[32]

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 further 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 communities in the knowledge-seeking business had been brought into co-existence.

Instead of simply viewing the latter organizational innovations as somehow deriving automatically from the intellectual changes represented by the new style of ‘scientific’ activity, I suggest that emergence of the norms of disclosure and demonstration, and the rise of “cooperative rivalries” in the revelation of new knowledge, had independent and antecedent roots. They constituted a functional response 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.

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[31] 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.

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

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

The system of aristocratic patronage of creative activity--the patronage of bishops, kings, dukes and princes--had become firmly rooted in Western Europe during the Late Renaissance.[33] As 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 motivation: *utilitarian* and *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. Westfall (1993: pp.64-66) has found that among the 630 individuals listed in the *Dictionary of Scientific Biography* as having been born between the decade of Copernicus' birth (the 1470s) and 1680, fully three quarters had engaged in some area of applied technology, including medicine. Among the 363 scientists not involved in any way in the medical or pharmacological arts, two-thirds had some technological accomplishment.[34] A mathematician of Johann Kepler's intellect, for example, could make himself useful if the circumstances so demanded. When 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; and when he was in 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.[35] All this may be labelled the *utilitarian* motive.

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[33] See with special reference to patronage of mathematicians e.g., Feingold (1984: Ch. VI); Westfall (1985), Biagioli (1989, 1990); Rose (1977).

[34] Thus the surprisingly high showing among those who were "technologically involved" does not simply reflect the presence of many medical practitioners in the original sample. Westfall (1993:pp. 65-66) removing from the list all 238 of the 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). For good measure, another 29 practising pharmacologists can be removed, which reduces the number of those remaining to 363. Only 118, or one-third of those, were without any recorded technological involvements.

[35] 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 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").

But, at least at 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 also been consistent in displaying a desire to surround themselves with creative talents whose achievements would enhance not only their self-esteem, but their public image, their reputation, those aspects of grandeur and ostentatious display which served to reinforce claims to status. Poets, artists, musicians, chroniclers, architects, instrument-makers and natural philosophers have often found employment in aristocratic courts, 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 relationships offering material and political support in exchange for service often were 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 existed in 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.[36]

The motivations for entering into a patron's role which reduce to symbolic acts of self-aggrandizement, will here be called *ornamental*. Motives of the *ornamental* kind for patronage of “savants” and “virtuosi,” however, should be understood to have been not less instrumental in character than were the utilitarian considerations previously mentioned. The public display of "magnificence,” in which art and power had become allied, was a stock item in the repertoire of Renaissance state-craft.[37] Prodigal expenditure on 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.”[38] But it soon took on other, more distinctly political colorations.

[36] 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.”

[37] 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” – specifically the writings of Vitruvius on the use of machinery in festivals “to please the eye of the people,” and, considerably more informatively, the newly recovered mechanics of the School associated with Hero of Alexandria. On Renaissance engineers engineering, see Gille (1966).

[38] 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 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

Mary Hollingsworth (1998:13-14) underscores this point with forceful clarity in regard to Renaissance patrons of architecture:[39]

> "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."

But, 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 transmuted from an obligation of greatness into a means of reinforcing claims to prestige and authority in a politically and economically insecure world.[40]

From the patrons' point of view, therefore, most of the ornamental services that clients of the court might provide had "positional" value.[41] Although having an accomplished artist or

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men a good reputation in order to "preserve the state."

[39] 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."

[40] Goldthwaite (1993: esp. pp. 48-49), in examining the economic and social conditions in Italy that underlay the growth in the demand for art during the "High Renaissance," 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 Burckhart 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 eceliastical nobility in Italy, 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."

[41] The term "positional goods" has come to be used in referring to goods that are desired not for their intrinsic utility (satisfactions derived from their consumption), but because to possess 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 (in addition to other desired

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 one's rivals. Competition among patrons gave added strength to in particular to the ornamental motives for supporting such clients. The pressures that Europe's ruling families felt to attach to their service artists, savants and others persons of distinction – clients, whose creative achievements and capabilities would enable them to render special unique services, were 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, at least not advertised indiscriminately. This obviously applied in the case of some military devices, battle formations, and geographical knowledge concerning valuable trade routes.[42] By way of 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."[43]

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, but 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 "Science" and of "Technology"-- were therefore reflected jointly in the dispositions and behaviors of aristocratic patrons in the late Renaissance.

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attributes) a positional value.

[42] Strikingly, among the non-medical set of distinguished contributors to the sixteenth and seventeenth century science studied by Westfall (1993: pp. 65-66), the two-thirds who also had practical technological involvement were virtually engaged in the fields where mathematics figured importantly: 41 percent in engineering (divided about evenly between military and hydraulic engineering), 19 percent in "navigation" and 37 percent in "cartography."

[43] 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) 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.

3.2 *Mathematics and the heightening of informational asymmetry problems*

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 both were responsive to 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 the circumstances surrounding the patronage of *savants* were no longer so simple. 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.[44]

These developments, 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 even art.[45] Indeed, an important feature that

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[44] 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."

[45] For introductory surveys of mathematics and mathematical publishing during the Renaissance (particularly,

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."[46] 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."[47]

These expressions of the necessity of possessing mathematical knowledge were continuing a rhethorical 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 Mhller of Konigsberg [1432-1476], the great Renaissance mathematician who styled himself Regiomontanus.[48] In the course of the invited lectures in astronomy he delivered in Padua during April 1464, Regiomontanus issued the following rhetorical challenge to his listeners:[49]

> "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

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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.

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

[47] 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.

[48] 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 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 contained the marvelous passage quoted in the text (below).

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

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."

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.[50] 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,

[50] 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.

> and to agriculture. An effort must therefore be made so that mathematics will flourish in our colleges as well as the other disciplines."[51]

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 with [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 can in no way be understood without some knowledge of the mathematical sciences."[52]

By the late sixteenth century the continuity between the rhetoric of the humanist educational tradition the increasingly advertised importance that a newer and more sophisticated brand of mathematics held for students of natural philosophy was posing an unprecedented problem for would-be patrons of the *virtuosi.* [53] 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.[54] This obviously would be a 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 really were 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.

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[51] 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).

[52] English translation from Christoph 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), in Dear (1988: p. 45). On Clavius, who taught mathematics at the Collegio Romano, 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, see e.g., Sarton (1955:p.138); Boyer (1985: pp. 334, 353).

[53] 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 Ram**J**e (Ramus) in Paris, and John Dee in London, see Keller (1985).

[54] See Feingold's (1984: 192) conclusion 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.

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 exacerbated nature of these informational asymmetries thus posed problems for patrons, they would perforce be problematic for the intellectually talented who hoped to enjoy such material support, social status and political protection as noble patronage could afford.

3.3 *Coping with informational asymmetries in the patronage system*

How could a patron be reasonably assured that he had not by mistake taken into his service an incompetent, or worse, a charlatan, whose exposure as such would only reflect badly upon his own repute among rival patrons of the arts and sciences? To put the problem in modern words: how were court mathematicians and natural philosophers who claimed to draw heavily upon their command of mathematical skills to be screened for their ability? Or, how were men of "science" and mathematics to obtain credentials for employment when aristocratic employers were becoming increasingly incapable of directly evaluating the quality of their work?

Challenges and Public Contests, Reputational Competition and Priority Disputes:

As 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 was creating 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 seems particularly pertinent in this context to notice that from the mid-sixteenth century onwards -- which usually is taken as the start of the era of modern mathematics -- there was a perceptible quickening of communication among Europe’s adepts in algebra. Moreover, this development featured increasingly open discussions, recorded in correspondence that conveyed claims of new results and information about new techniques, as well as new mathematical conundrums and solutions of long-standing puzzles.

Public boasts and the issuing of challenges created occasions for the arranging of public contests among the Renaissance mathematicians.[55] 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.[56] According to Tartaglia’s account, it was the news of the existence of an algebraic

[55] 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 *Rechenb cher* (1529) of Adam Reisse, the celebrated German algebrist.

[56] This was the so-called “*cosa* and cube problem”: $x^3 + ax = b$, where a, b > 0. (The *cosa*, being Italian for “thing,” referred to (x) the unknown quantity. The method Fior employed 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 on his deathbed passed the secret to Fior. 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

solution of the cubic that stirred his interest in taking up the problem, and in 1535 his declaration of that intent led 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 stumped by the 30 questions that Tartaglia posed (which involved the more general cubic form). This public triumph soon brought the winner an invitation to visit the famous physician and mathematician Girilamo Cardano [Jerome Cardan, 1501-1571], who held out prospects of a patronage arrangement for him in exchange for information about Tartaglia's method.[57]

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 form 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 Viet**J**, 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.[58]

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 was brought to a high art by Pierre de Fermat.[59] 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 ensuing extensive correspondence with the Minimite friar in Paris, Fermat posed 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.

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to the latter's chronology.

[57] See Boyer (1985: 310-312). Cardan apparently intimated that he would arrange for Tartaglia to meet a prospective patron, and, by swearing an oath to keep the secret, induced the latter to reveal his technique for solving the cubic $x^3 + ax^2 + bx = c$, which he subsequently published in his *Ars Magna* in 1545, acknowledging he had learned of it from Tartaglia.

[58] Boyer (1985: pp. 334-342) relates this story, noting that although Viet**J** 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.

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

In a setting in which public reputations for intellectual accomplishments began to matter as a signal of individual's capabilities, disagreements about who had done what, and who had done if first, were more likely to become a serious matter—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 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."[60] 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, those developments in social communications perhaps further reinforced the participants' motivation to establish claims to enduring fame. [61]

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 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 Gilamo Cardano (Cardan) and their respective supporters, and the bitter dispute over priority in formulation of the geoheliocentric system starting in 1584-85 between Tycho Brahe and the mathematician Nicolai Rymers Ursus.[62] These were notable precursors of the well-known

[60] 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.

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

[62] See Boyer (1985: 310-312), who applies the adjective "sordid." Cardan did add insult to injury, in justifying having broken his pledge (o never reveal Tartaglia's method for solving the cubic), on the ground that

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.[63]

An opportunity for 'direct confirmation'-- 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" he had established within the community of client-scientists who already had managed to gain distinction for their expertise. That, of course, would require forming their own judgement, by inference from personal verification of an achievement that could be verified without a command of the very same sort of esoteric knowledge that the would-be client claimed to possess. 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.[64] 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. Nor was it the great works for which he is now acclaimed, the *Dialogue Concerning the Two Chief World Systems* (1632/1967) and the *Discourse on the Two New Sciences* (1638/1974), as those were products of his later years. Rather, Galileo was able to make a name for himself by first constructing a telescope remarkably better than those that previously had been built in northern Europe, which, on being presented to the Venetian Senate in August 1609, quickly won him tenure and a remarkable salary increase.

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. On Tyco and Ursus, see Thoren (1990: pp.255-256, 260-261, 390-96).

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

[64] 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; the University of Padua was 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.

Proceeding to build a still more powerful instrument for his own use, Galileo soon thereafter 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.[65] From the viewpoint of Galileo's career, the true beauty of this heavenly discovery lay in the fact that to confirm it 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. The historian of science, Richard Westfall (1985) called 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. The ducal family of his native Florence was the most obvious target. 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 noted, 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.[66] Westfall (1985: 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 the new science who were seeking public reputations that would command the attention and the patronage of princes.[67] 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 the astronomical observations he had made with it, ultimately were referred to other "expert" astronomers for corroboration.

[65] 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.

[66] 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. Galileo's earlier ploy, in presenting the telescope to the Venetian Doge, also succeeded in getting the Senate to renew his professorship at Padua for life, at a salary that was almost doubled. Although he left Padua for the service of the Grand Duke, in Florence, the resentments stirred among his rivals for patronage there--like those he kindled upon arriving in Florence--would be a source of Galileo's later troubles, including those with the Jesuits, according to Westfall (1985). Drake (1980) advances the radically revisionist but very plausible thesis 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. But for quite a different and enormously controversial reading of Galileo's Trial before the Inquisition, see Redondi (1987).

[67] The microscope-makers of the day, however, enjoyed some parallel advantages.

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) notes, 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 respond to make a response -- an invitation that was reinforced both by the ambassador's "own exhortation" and a request from his patron and employer Rudolph, that he express his opinion in the matter. In the spring of 1610, Galileo used 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 [68]

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." 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 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."

[68] 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) account for this, and the other instances cited in this paragraph.

Although there surely is some merit in essaying such a reading of the historical evidence, patently, the preceding statement goes rather overboard.[69] 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 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 surely is right 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, to consider 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 either, but 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.[70]

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[69] As Noel Swedlow has remarked (in private communication, April 1994):"Think of all the nonentities that held similar court positions!"

[70] 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 things 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."

4. Patronage, competition, and common agency: some 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 should similarly be noticed as deserving of 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.[71] 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.[72] 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 by social historians of science in the contributions in Moran (1991), including 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, and the Munich Court of Ferdinand Maria, the Elector of Bavaria (r. 1654-1679). 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 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.

We should not suppose that written, legally binding contracts sealed the patron-client bond. On the other hand, the nature of the dyadic relationship 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. From that discussion what emerges clearly -- as one might well suppose -- is that the patron determined how casual or close a connection he wished to form with any

[71] 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 he 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 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).

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

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 legitimation offered by the patronage received from many small patrons into the equivalent of that which a great single patron would afford, even if the 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. Since the core of the system involved competition for prestige gained by 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. Since 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 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 the 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 could be proffered to a different patron? Indeed, it would. Following Dixit's (1995) intuitive exposition of common agency games, we may seen 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 principles.

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 will eventually 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.[73]

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 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.[74]

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

[74] 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

The fact that very few scientists could achieve such an apotheosis in their profession during the seventeenth century, and so had to balance between the competing expectations of an array of patrons carried other implications, however, which worked to ameliorate their material situation. It is important in this connection 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 *complementaries* among the differentiated services they performed for their multiple patrons. This situation arose from the dominance in this historical epoch of patrons' concerns with the "ornamental" rather than the "utilitarian" value of scientist-philosophers, and among its direct consequences was that the structure of the incentives offered to clients, weakened as it was, still remained more favorable to the scientist-philosophers than would have been the case were patrons willing content themselves with the benefits of knowledge ‘spill-overs’ from the inquiries supported by courts other than their own. When, in a later era, the utilitarian applications of scientific knowledge became paramount, there would tend to be greater complementarity between the services that could be provided to different patrons by a common agent, and the possibilities of free-riding on work undertaken for other patrons tended to diminishing the bargaining power of individual researchers.

The preceding line of argument is consistent with the conclusions emerging from a recent formal analysis of the common agency problem by Lars Stole (1991). Stole shows that (for the case in which the agent must choose either to accept multiple principals or none) 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 avoid a market failure if what principals wanted was access to a public good, which all could enjoy if it was collectively provided, but which their (misguided) competition for more privately appropriable forms of benefits prevents from being produced by their common agent.

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 is a fairly obvious amplification of the conclusions from preceding analysis, albeit one that is often overlooked. A strong central state would create a monopsony situation in the market for patronage, whereas in Renaissance Europe one had many contending 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.

The Reformation acquires a new significance -- if that is conceivable -- when seen in this context: the emergence of Protestant princes in the North of Europe reduced the sphere within which the one universal source of authority, the Catholic Church, could exercise its political and social influence over lay patrons of science. Although this did not suffice to

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efficiency properties.

spare Galileo from trouble -- 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, further, 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 by leaving greater rents to the individual scientists, and creating super-star winners of reputational tournaments, the workings of the patronage system induced more people to enter the profession. On the other hand, the fact that patrons sought "positional services" from their scientist-clients operated to focus the latter' 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 science.

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 open pursuit of scientific knowledge under the auspices of State-sponsored academies. In the period from the 1660s to 1793, some 70 officially recognized scientific organizations have been identified as having been founded specifically on the models provided by the Royal Society of London (founded privately in 1660 and receiving charters from Charles II in 1662 and 1663), and the Académie royale des Sciences created established in 1666 by Louis XIV (on the initiative of Jean-Baptiste Colbert).

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.[75] Although justified by both the wider influence they 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 emerging nation state has tended to deflect scholarly interests away from the generic features in the workings of the early learned societies and scientific academics -- the lesser and the greater, the many organized under private patronage as well as those grand objects of State support.

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

Another correlate of this particular historiographic fixation (doubtless equally unintended) was the excessive emphasis accorded to the aspect of discontinuity in the historical record of institutional development, identified in the 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, but tended to be taken as given, and natural among the scientifically curious. It also 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 the rational institutional response to the dawning of the new scientific methodology.

But, the findings of more recent historical investigations, and the new perspectives that can be brought to these matters by economic analysis, point towards a rather different historical interpretation. Just as it has been argued in the foregoing pages 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 the interpretation of the ‘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 this reinterpretation.

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 was, in a sense, addressing the central problem of informational asymmetry in the late Renaissance patronage system.[76] 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.[77] But, in the course of seventeenth century the formation and growth of these networks -- 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

[76] 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).

[77] 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, were not an innovation of the seventeenth century

the creation of more and more formal scientific bodies that progressed from the "assemblies" and salon-like discussion groups gathering for scheduled meetings under the auspices of sponsor, to academies having elected members whose researches drew support from private patronage.[78] The extent to which the characteristic open style in which those organizations' affairs came to be conducted was not merely symbiotic with the system of elite patronage, but was directly responsive to its requirements (as well as dependent upon its support), are questions that remains to be answered through renewed inquiries into the primary sources. Though those matters cannot be settled here, neither need they be. It is enough for the purposes of the present argument to notice the ways in which participation in the early academies and scientific societies conveyed benefits upon their members.

The immediately pertinent question is: why should *individuals* seek to participate in associations where they would be expected freely to divulge the results of their investigations in to matters that might otherwise be a source of personal gain? What incentive is offered for such cooperation -- outside of an institutionalized setting in which such conduct was made a formal obligation (publish or perish!) of career advancement? Two answers come to mind. First, there was a certain status conferred by being accepted as a "correspondent" by already established "scientists-philosophers" persons of great repute. This carried some "signalling value." Second, there is the "exchange value" of information: division of intellectual labor confers advantages to those who need not work in isolation but can instead draw upon codified knowledge and tacit expertise of others to solve particular problems. Access to such "networks" of assistance must be purchased by proffers of worthwhile "material" or accreditation. These points may warrant being considered more fully, taking them in turn.

The Signal Value of Correspondent Status: This is a generic property, not restricted to relationship between scientists. A passage from Richard Westfall's account (1980: 541) suggests how the astronomer John Flamsteed regarded his cooperation with Newton's request for 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 approbation; he wrote to Newton that his "approbation is more to me than the cry of all the ignorant in ye World."

From this it is apparent that the would-be correspondent's motivation could be vanity or ego gratification from such acceptance into peer-ship, and/or a rational instrumental desire to be accorded status that would enhance one's prestige with third parties. Here the mechanism at work is "passive patronage": if I am able claim acceptance as a correspondent by a recognized "great," a star, it raises my prestige with others. But, the signal can be discounted -- Flamsteed wanted to be thought a peer, whereas Newton viewed him (privately) as an inferior but useful collaborator. Those at the top of the hierarchy of recognized status benefit by being able to accord some patronage to those below them, a derived patronage effect that is important in reputational work organizations. This is a benefit of winning a

[78] (Lux (1989: Ch. XXX traces the evolution during the years 1665-1666 of a provincial *assembJe* formed in the provincial French town of Caen into the AcadJmie des Physiques (physiology) du Caen 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. This institution flourished briefly, and in 1672 received royal patronage (as the first among provincial institutions to do so.)

tournament. Nevertheless, more than vanity might 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.

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. It is a "patent-pooling" arrangement without the patents--a loose coalition, in which some degree of non-compliance with the norm of mutual help will occur, and so yield an equilibrium in which the "common pool" is degraded by the private reservation of certain information. Nevertheless, access to help from "peers" remains a valuable asset for the individual.

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 which insights from the theory of repeated games are applicable to explaining cooperative behavior among potentially rivalrous researchers.[79] To sharpen this point, one should start simply by considering two researchers working towards the same scientific goal which 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 that if both get 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 he knows.

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

This game has the structure of a classic two-person "Prisoners' Dilemma." It is well known that an escape from the pessimal outcome is possible, if the game is played repeatedly, the future is not discounted too heavily, and the players 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).[80] However, the value in the future of developing and maintaining a good reputation for sharing has to be large to discipline the self-interested researcher into adhering to the sharing mode of behavior in the current period. 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.

Yet 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 not only what you have learned yourself, and what you have been told by 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 other within their group. In that way they might can 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.[81]

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 ostracizing him 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.

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[80] 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).

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

But this consideration is offset by the greater difficulties the larger groups will encounter 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, unambiguous detection and attribution of deviations (from recognized norms regarding the disclosure and non-disclosure of information) augment the deterrent power of the threat of ostracism that can be wielded by 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.[82] Does this imply that the normative content of Merton's communalistic norm of disclosure is really redundant, and 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 by expecting others to cooperate, 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.[83]

Furthermore, when the norms of behavior involved (i.e., the "custom" within the network in question) are common knowledge, and therefore 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

[82] 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 firms in fact 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.

[83] A distinct, but closely complementary line of theoretical analysis has been developed that also supports the thrust of these conclusions. The modelling exercises in David (1998, 2002) show that social communication networks of scientists formed by the intersection of memberships in restricted “clubs” for private knowledge-exchanges are very likely to for spontaneously and be sustainable when their numbers remain small. But, in the absence of an articulated ethos that raises the expected propensities of individuals to regularly and frequently disclose pre-publication results and methods, and their evaluations of the claims of others to others in their local social networks (i.e., there “club”), the ensemble formed by the inter-connections among those entities is unlikely to be able to function well enough in reaching substantial “closure” on specific scientific questions to enable it to attract further membership. Consequently, under the conditions specified, the dynamic behavior of the “invisible college” (i.e., the ensemble) prevents it from expanding beyond the small (stable equilibrium) dimensions to attain the size and diversity of numbers that would transform it into rapid generator of contributions to reliable knowledge. To pursue the details further, however, would take the discussion too far afield from its present historical focus.

socialization among scientists were weak and quite imperfect, the common "culture of Science" makes it much more possible for the rule of priority to engage the self-interest of researchers in reinforcing adherence to the norms of disclosure, at least among those restricted circles of colleagues that Derek Price (1963) referred to as "invisible colleges."[84]

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 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 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; for, 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.

5.3 *Continuity, context and change in the institutional organization of science*

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, 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 through the assignment of credit for priority in discovery or invention.

In fact, professional societies served another and complementary purpose, which has been much commented upon in the literature on 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 a mere open transfer of knowledge. Scientific exchanges, with their attendant evaluation of individual work, may be seen as a form of "patent-pooling" arrangement--without the patents – among the holders of complementary knowledge assets.[85] Scientific societies, like the informal networks of correspondents that they institutionalized, supported a loose coalition of researchers who

[84] 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.

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

engaged in repeated altruistic exchanges of their specialized knowledge. Their formation thus was socially as well as privately beneficial in these effects upon the productivity of researchers.

To be sure, one would expect such sharing arrangements 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 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 would on balance be conducive, both directly and indirectly (by assisting their research and publications) to further enhancing the external reputation of the members. There can be little doubt that it was far more efficient to institute these formal professional bodies than not. But, we 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 had been sufficient, historically, to initiate the institutional movement that made State patronage of formal scientific academies a ubiquitous attribute of modern societies.

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 being brought about by the growing scale and costs of the new modes of scientific inquiry.[86] The view that the advent of the expensive new laboratory science (not to mention the new observational astronomical observatories) proved too costly for private patrons, creating "a crisis" in the mid-1660s that was solved by the institutional innovation of State support under the regal patronage of Louis XIV, has come to be known among historians of science as the 'Fontenelle thesis.'[87] Although this explanation itself is hardly of recent origin, having been first articulated early in the eighteenth century -- by Bernard Le Bovier de Fontenelle [1657-1757], a permanent secretary of the Acad**J**mie royale des Sciences and the author of the first history of that institution, one might suggest that this line of explanation had particularly strong resonance for historians of science writing in the 1960s and 1970s, when discussion of the phenomenon of "Big Science" and its funding requirements were very much in the air. [88]

During the past two decades, however, 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 developments was

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[86] This is a theme that has been elaborated in regard to the history of the Acad**J**mie 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.

[87] 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).

[88] 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).

manifested in the appearance around the end of the sixteenth century of numerous privately patronized scientific societies and 'academies.'

Indeed, seventeenth century science proper has been found to have played only a very minor part of that wider intellectual reorganization. Of the 2500 learned societies that are estimated by James McClellan (1985) to have been created in Europe between 1500 and 1800, at least 700 were formed during the sixteenth century alone. While some of these organizations were scientific in purpose, those were not in the pre-1550 vanguard; the overwhelming majority of them were formed in response to interests far broader than anything resembling the organized pursuit of science. The historian of France's scientific academies David Lux (1991:pp.189,196) has stressed the absence of 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:

> "the 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 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 find an explanation for the developing *practice of open science* simply in the early opportunistic adoption of the humanist "academy" of the late Renaissance as an organizational mode for pursuing investigations of the natural world. But, equally misleading is the casual supposition that the motivations of individual participants in the new scientific disciplines for embracing open science and the new methods of inquiry it deployed were a sufficient force to impel the ensuing re-organization 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, historical explanations should be contextually grounded upon an analysis of the incentives that shaped the behaviors of the actors involved. In the case of the founding of the Acad**J**mie royale des Sciences, a very significant actor was Jean-Baptiste Colbert, Louis XIV's minister of finance and the navy. Moreover, the context of the initiative he took creating the this institution makes it inescapable to ignore that the decisive consideration were those emanated from the political logic of the absolutist State, and neither the structures of social interaction among researchers nor the material requirements of sustaining advances on the frontiers of observational and experimental sciences.

The obvious point of departure here is an appreciation that the foundation of the Acad**J**mie royale des Science was connected with a train of events set in motion by Louis

XIV announcing (upon the death of Cardinal Mazarin) in 1661 that he would serve henceforth as his own first minister. That had 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 only recently had mounted (unsuccessful) a direct military challenge to the monarchy. It followed from this that that *le Roi Soleil*, the unique star around whom the politics of French 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.[89]

To further underscore this point, and the continuity of the previous argument regarding the significance of ornamental motives for noble patronage, one may note 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.[90] 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.[91] 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 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 at least since the 1650's. Indeed, at the very time that Colbert's 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.[92]

[89] 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).

[90] 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).

[91] 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.).

[92] 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 Montmorians, see the footnote immediately following this.

The Fontenelle thesis has been undermined further by David Lux's (1989) demolition of one of the putative factual bases upon which it had been erected. Supposedly, a the 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 "crisis" by creating an academy that would receive State support. Yet, there was 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* MelchisJdec ThJvenot [1620-1692], turn out not to have been causes, but instead were consequences -- first of the rumours about the progress of Colbert's plans, then of the announcement confirming his success in persuading the King at least to found a royal academy for the sciences.[93] Under the finely graduated hierarchical political system of patronage in France, 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), once Louis XIV announced a royal academy would be formed, the discontinuation of the ThJvenot and other scientific academies under private patronage in Paris was only to be expected.

The advent of State sponsorship of science in the late seventh-century, however, should not be depicted as bestowing unalloyed blessings upon the open science movement. Institutionalization of cooperation in formal organizations facilitated "regulation of conduct" on behalf of the academy-members' common interests. One cannot 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 *assemblJes* and provincial academies had welcomed the establishment of regular exchanges of scientific information with other learned societies, the young AcadJmie 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."[94]

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

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[93] The specific reference is to the meetings at the home of the polymath *savant* MelchisJdec ThJvenot [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 ThJvenot circle until the announcement of the new royal foundation. See Sturdy (1995: pp. 16-21), on other Parisian scientific assemblies, for ThJvenot 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. But this hardly lends substance to the picture of an institutional "crisis" driven by the cost of doing science.

[94] 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 MoliPre, 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.

institutions, nonetheless justified the rule of secrecy, on the grounds that research was motivated "by the hope of gaining fame through priority."[95] Changes in the regulations went 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 French academicians' counterpart institution in London: 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 met with approval before being forwarded by him for printing in the *Philosophical Transactions*.[96]

Membership of the new learned societies thus conferred substantial "club goods" benefits to researchers in ways other than the effects of signalling their 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 a societal gain in exercising controls over the "quality" of scientific expertise signalled by membership in prestigious academies and societies seems beyond contention.[97]

But the signals of quality emitted by the political process of electing new members were hardly 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 say 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.

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[95] 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).

[96] 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 "mett both with a singular attention and an uncommon applause," and so could be published in the *Philosophical Transactions*.

[97] 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.

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, itself lost its 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 institutionalized practices of the scientific academy.[98] But 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 caution for modern science policy

But the moral to be drawn from the story related here is not simply that the more things change, the more they stay the same. There is a particular historical irony, too, 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.[99] The nub of it is simply this: 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 at least as much because the public reputations

[98] Lenoir (1998), and L**J**cuyer (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.

[99] On the theme of "path dependence" in the dynamics of economic systems, see, e.g. David (1988, 1992, 1993b, 1997b).

gained by their achievements had an ornamental instrumentality for their patrons, as because their knowledge equipped them to devise technologies that would directly advance their patrons' economic or military interests.

The norms of cooperation and information disclosure within the community of scientists, and their institutionalization through the activities of formal scientific organizations, 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 by an appreciation of the new knowledge as a potential foundation of wealth and power. These 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 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 might therefore 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 dominant single principal-patron in Western Europe, the multiplicity of contending noble courts tended to be more favorable to 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.[100] 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; and that it removed the pressures experienced by rival European rulers which led them to encourage the growth of economic activity within their territories as a basis for tax revenues.[101]

In *The Wealth and Poverty of Nations* (1998: p. 38) David Landes 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

[100] 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.

[101] 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 to lose from allowing others to introduce them first."

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." This 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), pointing to the greater scope that centralized political control might allow for the suppression of technological innovations and new commercial practices.[102] On balance, 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 said, the thesis that advanced here 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, so richly documented by Joseph Needham (1954) and his collaborators.[103] Its burden is the idea that 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 did 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 often did 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-

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[102] 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 *privilPges* to independent inventors that served to counteract the chartered monopolies' generally baleful influence upon industrial innovation (see Hilaire-PJrez 2000). Obviously, the question is complex and cannot be readily resolved here.

[103] 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" -- held so vital in Marxian analysis; and thus, being deprived of the opportunity to fuse scholars with craftsmen, 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 uncriticised in the specialist literature (see, e.g., the points reviewed by Mokyr (1990: p.230-231), and Cohen (1994:pp.449-482). The argument I have constructed 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.

à-vis Europe at the end of the fourteenth century, Joel Moyer (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 mine]. Although no really satisfying response was forthcoming from Mokyr’s ensuing pages, this rhetorical restatement was nonetheless 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 technology should continue searching for answers to that 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 predicated upon the a distinctive aspect of the region’s 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 state my summary thesis: the emergence of the characteristic institutions and organizational features of open science that have played so vital a role in generating the sustained material achievements of the era of modern economic growth, well may be said to be western Feudalism's greatest gift to Capitalism.

There is a second, corollary proposition, to which the historical experience recounted here also lends weight. 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 thus 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. Rather than emerging and surviving as robust epiphenomena of a new organum of intellectual inquiry, the institutions of open science are independent and in some measure fortuitous social and political constructs. They are cultural legacies of European history that continue to profoundly influence the systemic efficacy of the scientific research process.

Features of the institutional infrastructure of public science, being thus in some significant degree exogenous to actual scientific practice, may be subjected to substantial re-design and otherwise manipulated as potent instruments of science and technology policy. In this sensitive area, however, wise policy-making for the future must pay especially careful heed to those organizational instruments’ own complex and contingent histories, and so respect the potential fragility of the institutional matrix within which modern science has evolved and flourished.

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