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From Book Censorship to Academic Peer Review

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Together with tenure, peer review is probably the most distinctive feature of the modern academic system. Peer review, we are told, sets academia apart from all other professions by construing value through peer judgment, not market dynamics. Given the remarkable epistemological and symbolic burden placed on peer review, it is surprising to find that so little research has analyzed it either empirically (in its actual daily practices) or philosophically (as one of the conditions of possibility of academic knowledge). While academics discuss it quite frequently, they do not frame it as an intellectual subject. Instead, they either confine it to private conversations or treat it as one of the practical aspects of the profession. Typically, peer review comes up in the context of personal complaints about the perceived incompetence (or other unflattering traits) of editors and referees. But when the dust settles, it is not uncommon to hear appreciative remarks for the referees' time-consuming and unpaid contributions, or to see them thanked in the acknowledgments.

It is not, however, the emotions elicited by peer review (as legitimate as they may be) that interest me here, but the fact that such a substantial body of usually polarized private or quasi-private comments seem to exist in the absence of a public analytical discourse about peer review. Why do we tend to perceive peer review as either good or bad, helpful or obstructive, but not as one of the fundamental conditions of possibility of academic knowledge and the construction of its value? With all the post-Foucauldian talk about the pervasiveness of discipline and its techniques in all aspects of modern life, it is puzzling that academics do not seem to recognize peer review as their distinctive kind of discipline, that is, as something that is simultaneously repressive, productive, and constitutive of their knowledge.

I cannot find in the history of peer review anything as dramatic as Foucault's juxtaposition of the early modern quartering of the bodies of criminals with the modern disciplining of the inmates' 'souls.' Still, it may not be out of place to view the long-term trajectory of the disciplining of printed texts as moving from early modern book-burning (the public material destruction of the text as object) to modern peer review (the internal disciplining of a text and its author). Peer review, then, may be an instance of the movement from 'disciplinary technique' to 'discipline' Foucault has discussed so often. Foucault also reframed modern notions of authorship by connecting

the emergence of the figure of the author not to notions of intellectual property (the author as creator) but to state censorship of books (the author as potential felon). But, quite surprisingly, he did not pursue his insight about the link between censorship and author function into the relation between peer review and discipline formation.

Moreover, peer review may constitute a different, perhaps anomalous kind of discipline. Unlike what Foucault saw in the cases of medicine and the penal system, in academia we find that the roles of the disciplined and the discipliner are often reversed during one's career. While the inmate does not become a penologist and the patient does not turn into a doctor (at least not by virtue of them being inmates or patients), in academia we start out as students (subject to our advisors' frequent reviews), but then gradually take on the reviewer function ourselves (depending on seniority, prestige, and willingness to allocate time and energy to reviewing, etc.). Peer review, then, may be the 'degree zero' of discipline in the sense that, being the condition of possibility for all disciplines of the modern academic system, its subject and object are not and cannot be permanently distinct. You need to have a certain kind of disciplined medical gaze to produce a certain nosology and a certain kind of medical subject. But because peer review is a discipline that does not produce subjects of knowledge (an inmate, a patient, etc.) but subjects that will then produce disciplined knowledge, it cannot be *permanently* exercised by a subject that is exterior to the subject being disciplined (otherwise the system would not be able to reproduce itself). In academia, then, subjects take turns at disciplining each other into disciplines.

In this essay, I look at a crucial stage in the genealogy of peer review: the transition from external to internal review of book manuscripts, that is, from state censors to academic reviewers. For a number of reasons, my examples come from the sciences. First, peer review was applied to scientific publications before it became standard in the humanities and social sciences, thus providing substantial evidence about the genealogy of peer review and its early direct relation to early book censorship. Second, peer review has been a hot topic in the scientific community for at least the last 10 years. Most laboratories and 'soft money' faculty positions (much more common in science than in the humanities and social sciences) are supported through peer-reviewed grants, not university salaries or in-house research funds. The amount of money distributed through peer review is staggering and, as many professional lives depend on it, the details of the peer review system are actively criticized, reviewed, and modified on a regular basis by scientists, funding agencies, and science administrators. This has produced a research database on scientific peer review that easily surpasses what we have for the humanities and social sciences.

Mythologies and Genealogies of Peer Review

Public images of science cast peer review as the ultimate guarantor of good science: scientists evaluate their colleagues' papers for publications, grant proposals for funding, and their personnel files for promotion. Peer review

preserves science's autonomy from potentially tainting social interests by keeping non-scientists out of decisions about scientific content. At the same time, peer review also seems to assure the state that good science is produced, that scientific publications are trustworthy, and that the tax monies allocated to science and medicine are distributed fairly and effectively. Peer review, in sum, is cast in two crucial and related roles: it guarantees good science while preserving the 'contract' between science and the state.¹

Like all mythologies, the public image of peer review has a purpose. It provides a sense or order, almost a unifying principle, to an otherwise chaotic set of professional practices, institutions, and interests that make up one of the largest, most dispersed, and most unregulated enterprises in modern society. Most knowledge-based or skill-based professions are subject to federal or state regulations, certifications, and possibly audits, but scientists and academics are largely exempted from those constraints. Although peer review is not a legal concept, it is invoked by scientists and academics as the axiom that informs most of their practices. It functions as an article in the tacit 'Constitution' of the social system of science (not unlike the way democracy functions in the discourses of modern liberal economies).²

The mundane reality of peer review is quite different. Its actual scope is not as comprehensive, and its performance record not as impressive as one may be led to believe. Also, its practices are too diverse to justify calling peer review a system. While the introduction of peer review of manuscripts can be traced back to the 17th century, its use for assessing funding requests is a post-World War II phenomenon (and largely limited to the USA, where the public funding of science grew to such levels as to raise concerns for the scientists' accountability to taxpayers). Even in the USA, where the NSF and NIH rely on peer review for funding decisions, there are substantial exceptions like defense-related work and other Congress-funded projects, not to mention the growing funding of academic research by the private sector, which is typically not managed through peer review.³ Not all scientific journals use external referees and, especially in the case of proprietary journals, editors may exercise great discretion over the extent of manuscript review. And depending on the checks and balances inherent in their research settings, different disciplines attach different value to peer review. Physicists, for example, rely on traditionally reviewed and printed articles but also on unreviewed preprints posted electronically.⁴

Performance is where actual peer review deviates the most from its ideal image. Empirical studies conducted in the wake of concerns about scientific misconduct have presented peer review not as a guarantor but as 'highly subjective, prone to bias, easily abused, poor in detecting gross defects, and almost useless in detecting fraud.'⁵ Widespread negligence, lack of skill, self-interest, and even plagiarism have been documented among referees. This has led *Nature's* editor to state that, at best, peer review is 'the least imperfect way of upholding the quality of scientific publications.'⁶ Independently from their competence or probity, the referees' expertise is necessarily tied to the present (not future) state of knowledge and entails discipline-specific notions of

relevance. This has resulted in documentable conservative biases in the system — biases that have tended to penalize innovative and interdisciplinary projects, including some that eventually led to Nobel prizes.⁷

The limited literature on peer review makes it impossible to trace the genealogy of these problems to the period before World War II.⁸ What is clear, however, is that peer review was a 17th-century development tied almost exclusively to the emergence of a new kind of institution: the royal academy. Both of the first two state-sponsored or state-chartered academies, the Royal Society of London (1662) and the Académie Royale des Sciences of Paris (1699), were granted the privilege to publish their own works. This was an extraordinary exception from the licensing and censorship systems that since the 16th century had been established by political and religious authorities throughout Europe in response to the perceived political and religious threats posed by the printing press.⁹ All texts, scientific or not, had to be reviewed and licensed in order to be printed and sold legally. The first scientific academies were not exempted from these requirements, but were allowed to administer them on their own.

Peer review was introduced to select manuscripts to be published according to the new academies' printing prerogatives.¹⁰ These were not informal review procedures but statutory requirements spelled out in the charters that established these academies and granted them publication privileges. The dependence of the permission to publish on the fulfillment of peer review requirements was reiterated in the royal 'imprimatur' included in the early publications of the Académie des Sciences. These imprimaturs cited chapter and verse of the articles of the Académie's statutes pertaining to peer review, possibly to explain the peculiarity of its publication privileges to the readers.¹¹ So while peer review is now cast as a sign of the hard-won independence of science from socio-political interests, it actually developed as the result of royal privileges attributed to very few academies to become part and parcel of the book licensing and censorship systems.

State Book Licensing and Academic Peer Review

Why the French and English kings gave such extraordinary printing and licensing privileges to scientific académies is a matter of conjecture.¹² The absence of noticeable opposition from either printers' guilds or licensing boards to these very unusual developments suggests that there was already enough publishing and censoring to do without having to claim jurisdiction over a marginal kind of texts that were hard to print, sell, and review anyway. The censors in particular were kept already busy by the rapid increase in the production of printed books. In 1575 a licenser for the Congregation of the Holy Office in Rome exclaimed, seemingly out of despair for the task ahead of him, that 'the Holy Church would need all printing stopped for many years.'¹³

Farming out the licensing of certain kinds of books to state or church officials and institutions (like enlisting bishops or professors of theology to review religious manuscripts, or judges to license legal texts, or university chancellors to license educational books printed by university presses) may

have been an attempt to spread the burden of licensing.¹⁴ The case of royal academies, however, was quite different as they were given the status of independent licensers and publishers of their own work, and were not enrolled as corporate specialized licensers for the works of non-academics (though some individual academicals may have served, privately, as royal censors).¹⁵

More likely, the granting of these exceptional prerogatives to early academies indicates that there may have been little at stake in the texts whose publication they were to control. While the much-advertised cases of Galileo and Bruno (and, later, of Darwinism) have created the impression that the sciences were always on the verge of destabilizing religious authority and the political order morphed on it, these possibilities were marginal compared to those elicited every day by political news and commentaries, satire, or religious texts. Typically, scientific texts (especially of the descriptive type promoted by early academies) had little political relevance, provided marginal business opportunities, and posed little risk from the censors' point of view. In the 'republic of letters,' the good business opportunities, the censorship risks, and the political stakes were not in narrow technical publications like the *Mémoires* of the Académie Royale des Sciences or in the less technical *Philosophical Transactions*, but in the much more popular periodicals of news and book reviews like the *Journal des Savants*, the *Acta Eruditorum*, the *Giornale de' Letterati*, or the more *risqué* literary gazettes flowing out of the Netherlands (and often coming in under the radar of local censorship boards).¹⁶ The first genre may be compared to today's scientific and scholarly journals, while the latter resembles publications like the *Times Literary Supplement* or the *New York Review of Books*.

In any case, academicians were not exactly a seditious bunch.¹⁷ The Académie des Sciences was run by royal ministers, and candidates for membership were carefully screened before confirmation (a confirmation that, in principle, was the king's prerogative). Although in the case of the Royal Society we have a substantially weaker institutional connection between the crown and the academy, here too we find a close relationship between the court and the top cadres of the early academy. In both cases, because of the 'pre-disciplining' of academicians, the simple requirement that manuscripts had to be reviewed by the whole academy or by a committee made it almost impossible that anything controversial would go to press. The institutional contexts in which these texts were produced and the authors' direct dependence on the sovereign for their employment further reduced the probability that the work would be seditious in any way. If individual printers had much to lose by publishing unlicensed material, royal academies could have risked their entire corporate livelihood by not taking the review process very seriously.

Being members of royal institutions, the academicians were already part of the state apparatus or, in the English case, were officially sanctioned by it. They were peers in an academic sense but, especially in the continental case, they were also — quite literally — peers of other state officials, including the censors. Recent research has shown that about 40% of Parisian royal censors in the 1750s were members of major royal academies, about 30% were associated to the university, and almost 36% were journalists (i.e. book

reviewers for literary journals).¹⁸ As shown by Anne Goldgar, while in this period the French royal censors were asked to assess only whether a manuscript was dangerous to the king, church, and morals, they often wrote long reports that closely resembled actual book reviews — reports in which they commented on the style, relative novelty, relevance, and other censorship-unrelated qualities of the book. She then shows that these surprising similarities between the genre of censorship and book reviews can be traced quite simply to the fact that many of the royal censors were also book reviewers for classic ‘republic of letters’ periodicals like the *Journal des Savants*.¹⁹

It was not uncommon for book review publications to be state-run or state-sponsored, especially on the Continent. One may think of the censorship system and book review periodicals as two products (one private, the other public) of the same state-based process.²⁰ For instance, the *Journal des Savants* opened in 1665 under the direct patronage of Colbert (Louis XIV’s chancellor) and the editorship of Monsieur De Salo, counselor to the Parliament of Paris. However, Salo’s printing privileges were revoked within three months due to the journal’s clash with the Catholic Church.²¹ The publication started again in 1666 under another court-friendly editor, the Abbé Gallois (also a member of the Académie des Sciences), who reported directly to the chancellor — the official in charge of both the book censorship system and the various royal academies. Gallois was also Colbert’s Latin tutor, giving him lessons in the carriage as he commuted between Paris and Versailles.²² Eventually, in 1701 the chancellor’s office took over the journal, appointing its various editors and controlling its finances.²³ The Abbé Bignon became the new chief editor of the journal, as well as the director of the censorship system.²⁴ He was also the superintendent of the Académie Royale des Sciences. Fontenelle became the science book review editor of the journal, while continuing to serve as the secretary of the Académie des Sciences. In that same period he was also a royal censor of scientific books.²⁵

Comparable intimacy between the state and literary reviews can be found earlier in the century. Periodicals of news and book reviews like the *Mercur*e and the *Gazette* were published from the 1630s by Theophraste Renaudot thanks to special printing privileges accorded by Cardinal Richelieu (who, in exchange, had direct input in the running of the journals).²⁶ Another leading news and book reviews periodical, the *Acta Eruditorum*, published since 1688 by Otto Mencke, a philosophy professor at Leipzig, displayed a cozy relationship with (and received annual subsidies from) the Elector of Saxony and his descendants.²⁷ Modeling itself after the *Journal des Savants*, the first Italian journal, the *Giornale de’ Letterati*, opened in Rome in 1668. Its principal promoter, Michelangelo Ricci, was a well-known mathematician, but also a cleric and a high-ranking censor for the Holy Office.²⁸ It was Ricci who, through the intervention of his colleague the Master of the Sacred Palace (the head of Catholic censorship), received the printing privilege for the *Giornale* from Pope Clement IX.²⁹ Ricci became cardinal in 1681. The journal’s first managing editor, Francesco Nazari, was also a church insider, having been the director of the Press of the Congregation for the Diffusion of Faith.³⁰

The closest relation between book review journals and state censorship was

laid by Leibniz in his ambitious but unsuccessful plan for the *Nucleus librarius semestralis* — something of a German version of the *Journal des Savants*. In 1668 Leibniz lobbied the emperor for a privilege to publish a semi-annual journal that would review all the books listed in the spring and fall catalogues of the Frankfurt book fair (the largest gathering in the early modern book business). He claimed that because the catalogues published only the titles of the books sold at the fair, they gave the buyers (or other readers who could not make it to the fair) no information about the contents and quality of these books and no reassurance about their religious or political orthodoxy. The *Nucleus librarius* was supposed to solve all that and, more importantly, to provide the emperor with a more efficient censorship system.³¹

According to Leibniz, books that were offered for sale at the fair but had not been reviewed and approved by the *Nucleus* could be confiscated on the spot and the publisher (who was likely to be in one of the fair's stalls) easily prosecuted. The *Nucleus* would have functioned simultaneously as a journal and as an arrest warrant. Leibniz's plan, however, was not accepted. The official reason was that it would be impossible to grant an *a priori* privilege for a periodical, but more likely the *Nucleus* was perceived as Leibniz's thinly veiled attempt to turn himself, in a single move, into chief censor and primary literary and philosophical judge of Germany and the empire.³²

From Risk Control to Quality

While today it is said that peer review ensures the readers of the trustworthiness of the text in front of them, and assures taxpayers that their monies have been put to good use by scientists, its genealogy suggests that, at first, the interests protected by peer review were primarily those of the state and its academies, not those of the broader scientific or scholarly community. As peer review developed within the logic of royal censorship, its protocols were not significantly different from traditional book licensing but were applied to specific categories of low-risk (and relatively low-business) texts. It seems, then, that there was a double connection between the early modern state and the development of peer review. Peer review was closely modeled after book licensing because the state required that all texts be licensed. At the same time, the very state that required book licensing was also relatively unthreatened by scientific texts and therefore supported the development of something like a parallel licensing system.

But while there was a substantial overlap between members of royal academies, state censors, book review writers, and editors of state-sponsored literary journals, such an overlap does not imply an identity between peer review and book censorship. The crucial difference between the two systems was a *spatial* one: it was the *provenance* of the texts and their *positioning* vis-à-vis the *state* that informed what review protocols were applied to them. My hypothesis is that texts produced within the state apparatus were peer-reviewed while texts that were not produced within that framework were processed by the censorship and licensing system. Peer review was for 'domestic products' (texts produced within state academies or by authors

pre-disciplined by direct ties to the state). Censorship was for 'foreign imports' (foreign books, or domestic manuscripts produced by authors who were not directly connected to and corporatively supervised by the state). News was also treated as a 'foreign import.' Actually, because news 'happened' completely outside the control of the state, it could be seen as the epitome of this 'foreign imports' category. Not surprisingly, state-controlled journals did treat news like books: both had to be selected, reviewed, and censored before publication. Distance from the state meant reduced control and higher risk, and it was the relative risk a text posed to the state that differentiated the jurisdictions of peer review and state censorship.

The domestic/foreign distinction helps reframe the notion of 'peer.' What differentiated texts that became peer-reviewed from texts that continued to undergo censorship was not primarily the discipline they belonged to nor their authorial status (because even factual reports like news could be censored). It is not that certain kinds of texts began to be peer-reviewed (as opposed to being censored) because they belonged to a professionally mature discipline — a discipline that could provide competent peers for self-reviewing. It was the combination of being relatively harmless and produced by dutiful state subjects whose corporate survival depended on royal support that created the conditions of possibility for these claims becoming the subject of peer review. Of course, the existence of a community of peers was necessary too, but the genealogy of such a community (and the very notion of 'peer') was not at all external to the logic of the early modern state. The Académie des Sciences and the Royal Society were corporations of royal subjects, and royal subjects became peers by virtue of being academicians.

The defensive logic of peer review framed the academies' parameters of publishability which, I believe, tended to be defined both broadly and negatively. Such parameters were quite broad because the scientific community itself was a broad construct. Also, because printable copy was a rare commodity in the first decades of scientific publishing, too narrow parameters of relevance could have deprived the fledgling system of scientific communication of the critical mass it needed to sustain and expand itself. It was only through publications that academies could recruit peers, extend their networks, or encourage the establishment of other academies elsewhere. And parameters of publishability were defined in the negative because, given the heterogeneity of the community and its many interests, it would have been impossible to define quality in any other way but by indicating what was not publishable. While individual academicians may have had strong opinions about what counted as a good claim or about who was right or wrong in any given dispute, all academies were substantially more conservative on these matters because of the risks they were taking by publishing (and appearing to endorse) a given claim or text.³³ By default, academies seemed to adopt a version of the state censors' purely negative filter-like role, that is, the detection and prohibition of dangerous texts.

The review practices of the earliest French royal academy, the Académie Française, seem to contradict this pattern, but can be actually shown to confirm

it. Founded in 1635 by Cardinal Richelieu under the direct patronage of Louis XIII, the academy was put in charge of establishing the canon of the French language, its poetry, and its eloquence.³⁴ It was expected to produce dictionaries of language, poetry, and rhetoric by collecting and selecting its judgments on the literary texts it examined.³⁵ Although it was not granted publishing privileges like those of later science academies, its work was structured around intricate review practices. These were spelled out in its statutes that contained a level of detail unmatched by any description of peer review we find in the statutes of either the Académie des Sciences or the Royal Society.³⁶ It is also clear that the Académie Française, unlike its scientific counterparts, operated according to specific parameters of quality and was not shy about making them public. This can be explained by noticing that the very subject of the Académie Française — French language — was a very well established topic, with millions of ‘practitioners.’ The academy’s royal mandate was precisely to canonize that field, not to develop it. Establishing parameters of quality — ‘règles certaines’ — was exactly what the Académie Française was supposed to be about.³⁷ Furthermore, its judgments were specifically limited to formal issues — issues of style, not content — which drastically reduced the political or epistemological stakes behind those judgments.³⁸

Instead, in the case of scientific academies we see that, as in book censorship, risk was the driving concern behind the review. However, the notion of ‘danger’ was reframed as it was transferred from the state to its academies. Book censorship was designed to prevent the publication of news or views that could destabilize the state. The aim of peer review was more modest: to avoid the publication of claims produced by academicians that could bring disrepute to the academies themselves and point to their failure to live up to the book licensing privileges granted by their royal patrons. So while state censors did not need to worry, at least in principle, about the quality of the claims put forward in a manuscript except in so far as they may have offended the state or the church, academicians needed to care about the robustness and originality of the claims, not just their political orthodoxy, because the publication of weak, old, dogmatic, or fraudulent claims would have reflected negatively on the institution itself and may have endangered royal support. Although quality is normally considered a positive notion, the construction of parameters of quality was an inherently negative process predicated on the avoidance of an increasingly more specialized kind of ‘corporate danger.’

Another fundamental risk that academies had to consider was that of extinction due to lack of development. Being already well established, the state perceived books as something to be controlled more than promoted. But academies were new kinds of institutions (often in direct competition with universities) whose future existence could not be taken for granted. While academies needed to control publications, they also needed to promote them in order to promote themselves. I believe that around 1700 the dictum ‘publish or perish’ applied primarily to academies, and secondarily to individuals. Academic books and journals were distributing information, but they were also establishing a new kind of cultural market and advertising a certain

way of doing natural philosophy based on networks of ‘philosophical commerce.’

While in the first part of the 18th century research publications became necessary requirements for academic membership (a new requirement not demanded of university professors), those same publications were feeding academic journals. If journals were not supporting academies with revenues from subscriptions and advertising, they were providing them with the kind of prestige and recognition that fostered royal support. Publications, then, became a credit-carrying object, and these ‘academic banknotes’ needed to be printed, not only censored. It was at this point, I believe, that the academic peer review system began to differentiate itself more substantially from that of state book censorship. State censorship focused almost exclusively on the risk books could produce if they were published, while academic peer review slowly reframed that risk in two ways. First, the risk that had to be controlled was directed to the academy (and only secondarily to the state). Second, risk was derived not only from publishing books, but also from not publishing them.

Early Practices

From its informal inception in 1666 until 1688 the Académie des Sciences published only collective works. The initial membership amounted only to 15 academicians divided in even smaller disciplinary clusters who met twice a week to present and discuss their work. Given the context it would be difficult to draw the line between authoring and reviewing, especially when the outcome was a collective publication. After 1688 the Académie allowed its members to publish their own individual works through external publication channels, but in this case they could not mention the writer’s membership in the Académie.³⁹ This changed again in 1699 when the king gave the Académie its official statutes and legal standing. The statutes no longer bound the academicians to collective authorship, but made their privilege to display the term ‘academicien’ on their books conditional on internal peer review. Article 30 of the statutes read:

The Academy will examine all works that academicians propose to have published; it will give its approval only after a complete reading in the meetings, or at least only after an examination is made by those the Academy has designated to prepare a report; and no academicians shall use the title academicien in his writings unless that work has been approved by the Academy.⁴⁰

Peer review emerged as a distinct procedure only with the introduction of individual authorship, that is, when the Académie’s name and status was attached to works that had been authored only by some of its membership. Article 46 linked these peer review practices to book licensing:

To facilitate the printing of the various works that academicians may produce, His Majesty allows the Académie to chose a printer to whom, as a result of being chosen, the King shall have delivered the necessary

privileges for printing and distributing the works of the academicians approved by the Académie.⁴¹

This article gave the Académie the extraordinary royal privilege to have works of its members printed without the imprimatur of the royal board of censors (though at first this may have not made much of a difference as the academy's chief official was already heading the state censorship system).⁴² The connection between Articles 46 and 30, between the privilege to publish and the statutory requirements for peer review, was confirmed in the text of the royal privilege that was included in 1702 in the first publications of the Académie after the granting of the 1699 statutes. These texts articulated the Académie's statutory printing privileges in great detail (spelling out what topics and what kinds of publications the Académie could publish), but also made them contingent on the peer review requirements listed in Article 30, which they explicitly cited.⁴³

The history of peer review at the Royal Society of London bears important similarities with the French case, plus a few interesting twists. In December 1663, the council of the Society resolved that:

No book be printed by order of the council, which hath not been perused and considered by two of the council, who shall report, that such book contains nothing but what is suitable to the design and work of the society.⁴⁴

As in the Académie des Sciences, internal review and licensing did not apply to members of the Royal Society who opted for publishing through normal, non-academic channels.⁴⁵ In London this meant the Company of Stationers, the body that had controlled book printing and censorship on behalf of the crown since 1557.⁴⁶ And as in the Académie's case, the peer review procedures developed by the Royal Society were part of its book licensing system — one that had to fulfill the same purpose and legal requirements of the Stationers' procedures.⁴⁷ The Society was bound to develop such procedures if it wished to take advantage of the book licensing privilege included in the charter it had received from Charles II in 1662:

Full Power and authority is given and granted unto the said Society, from time to time to choose one or more Printers and Gravers, and by writing sealed with the Common Seal of the Society, and signed by the President for the time being, to grant them power to print such things, matters and businesses concerning the said Society, as shall be committed to them by the Council from time to time; The Said Printers and Gravers being sworn before the President and Council in form before specified, which President and Council are empowered to give the said Oath.⁴⁸

The minutes of the council's meetings indicate that the Society complied with the legalities associated with its licensing privileges, like selecting its official printer and developing the peer review procedures cited above.⁴⁹ These procedures were applied, as shown by the licensing of books by Hooke, Grew, Petty,

Wallis, Evelyn, Willoughby, Malpighi, and Newton — all members of the Society.⁵⁰ For instance, in 1671 Nehemiah Grew's manuscript of *Anatomy of Plants* was given to Henri Oldenburg (the secretary of the Society), who read it and passed it to another member, John Wilkins, who after reading it gave a very positive report to the whole Society urging them to read it too. We do not know whether they took his advice, but at least the president did read the manuscript before signing the license.⁵¹ Similarly, on June 22, 1664 the Council decided that:

In case Mr Hooke's microscopical observations should be printed by order of the society, they might be perused and examined by some members of the society; the lord viscount Brouncker was desired to undertake this perusal, and to communicate the manuscript, after his perusal of it, to whom of the society he should think fit.⁵²

On other occasions, the minutes of the council mention book manuscripts handed out to two or three members to read and report on (though not necessarily with publication in mind).⁵³ Usually, this procedure was adopted for technical texts not likely to be understood by the general membership. More manageable manuscripts were read and discussed at the weekly meetings, entered in the register-books, and occasionally approved for publication. Some of the members' books were read and approved chapter-by-chapter, as the installments were completed.⁵⁴

There are, however, several open questions, some left unanswered by the sources, others probably made unanswerable by the state of flux of the Society's early review procedures. For one, the sources are generally silent about the Society's level of editorial engagement with these texts.⁵⁵ Then, it remains unclear whether different protocols were followed to review manuscripts for publication as opposed to published books donated to the Society. Also, we know little about how extensive the discussion of these texts during public meetings might have been. Because the Society's corporate life hinged primarily on the public discussion and subsequent registration of texts or experiments and, only secondarily, on its role as a publisher, it is difficult to draw a clear line between the review-like procedures followed for experiments performed at its meetings, texts sent to the Society for its perusal, and manuscripts considered for licensing and publication.

What is clear, however, is that when it came to publishing the early Society behaved more like a licensing board than a modern editor or academic publisher. If a text was deemed to contain 'nothing but what is suitable to the design and work of the society,' it was licensed and published apparently without changes.⁵⁶ If, instead, it was found to be outside of the Society's parameters of disciplinary, methodological, or stylistic acceptability, it was not even likely to be entered in the register book.⁵⁷ This is not surprising if one pays attention to the wording of the Society's review policy — one that mentions suitability, not quality. It seems that the first purpose of the review was to filter out books whose topic fell outside of the licensing jurisdiction of the Society — one that was limited to natural philosophy.⁵⁸ Beyond that, the reviewers seemed to limit themselves to assessing whether a text could fit the

broad brand of empirical and non-dogmatic natural philosophy the Society was trying to promote. As in traditional book licensing, the review was about making sure that a text did not make unacceptable claims rather than to certify that it made good claims (or help it improve those claims according to specific disciplinary parameters).

The issue was not only to develop a system for enforcing certain parameters of quality, but to define those very parameters.⁵⁹ At first, the Society did not present itself as an eager publishing house that solicited submissions on which to exercise its editorial judgment. Instead, it tended to 'offer' publication to some texts it had received for perusal but that had not been necessarily submitted to it for publication. In effect, the Society retroactively turned a few of the texts it received and approved into 'submissions' which it then 'proceeded' to accept, license, and print. This *a posteriori* publishing strategy helped the Society avoid being put in the hot seat of editorial judge. Instead, it could make editorial decisions without having to defend or spell out the rationale for such decisions, something it would have had great difficulty doing.

We know little of the Académie's parameters of publishability largely because until 1699 it published very little and only works authored collectively by its members. When publications happened, they did not seem to involve particularly difficult judgments as the Académie was a very small institution divided up according to disciplinary clusters, each of them with its specialized styles and criteria of relevance. Furthermore, its use of collective authorship may have been not so much an expression of a Baconian ideal of cooperative intellectual work but a self-disciplining arrangement which made each author into another's censor. While the Royal Society operated more in a 'market environment' and needing to publish in order to develop its own authority and networks, the Académie was, until 1699, an almost completely autarkic institution paid and legitimized by Louis XIV, and thus had little need for publications. Most of its early publications were handsome, large-format, beautifully illustrated royal coffee table books, indicating that the king prized quality (or maybe looks) over quantity. But even after the changes in its publication policies in 1699, the Académie did not need to spell out its publication parameters because, unlike the Royal Society, it continued to publish work mostly by its members, that is, by people who were *a priori* defined as producers of publishable work. Unlike the fellows of the Royal Society, the members of the Académie had to jump through many hoops to get there. There were so many 'security checks' written into the tight academic sociability, that, in the end, criteria of publishability were probably embodied in the academicians themselves. To put it differently, the academiciens had been pre-reviewed so much as people, that their texts did not require much further reviewing. Criteria of membership and criteria of publishability overlapped.

Foreign Texts and Foreign People

The transfer of peer review protocols from books to journals proved compli-

cated for both the Académie and the Society, though for different reasons. As mundane as the difference between the two genres may seem today, the movement from one to the other reflected important changes in the size and structure of the academic publishing market, the pace of communication, the role of peer review, and the very meaning of 'peer.'

Journals posed practical and legal problems not found in book publishing. They required special open-ended privileges which the authorities were extremely reluctant to issue (as they saw the periodical press as a close kin to politically dangerous journalism).⁶⁰ Journals also needed an abundant supply of referees to keep up with a fixed publication schedule (unlike books that were published less frequently and not according to a specific schedule). But unlike any other institution (except, perhaps, the universities of Cambridge and Oxford) the charters of the Society and the Académie gave them the power to put out specialized periodicals, and their institutional structure gave them access to plenty of in-house referees. This shows how close the emergence of the paradigmatic academic genre, the journal, was to the special legal instruments granted to early academies, as well as to their institutional resources.

A much more serious problem for early philosophical and scientific journals was the scarcity of publishable material. While the *Journal des Savants* and its emulators could find plenty of books from different disciplines to review, more specialized research journals could feed only on a limited number of producers. Until the 18th century, journal issues were very short and could be sustained only by extensive international feeding networks like those developed by the Royal Society. It may not be accidental that the Académie started its *Mémoires* only after 1699, that is, after its membership was enlarged and therefore better suited to sustain its journal.

The alternative, of course, was to publish work coming from outside the academies (work that, with characteristic solipsism, the Académie termed 'étranger'). In this regard, the social boundaries between academic insiders and outsiders seemed more important than national boundaries themselves.⁶¹ While both the Society and the Académie did end up publishing 'foreign' work, the practice opened up a dangerous can of worms. First of all, were the academies legally authorized to publish work done by non-academicians, or did their kings mean to give them the privilege to publish only their work? By publishing 'foreign' work, in fact, the academies could have put themselves in the position of royal censors and licensers for the whole field of natural philosophy. And even if the academies decided they had such a privilege, how could they trust authors who may not have been known to their members, or whose work they had not been able to supervise? These questions may sound surprising to modern ears as we assume that, at least in principle, the treatment of a manuscript should be independent from its place of origin and the circumstances of its authors. But early peer review was anything but 'blind,' and it was precisely by *not* being blind (that is, by being tied to specific authoritative institutions and their memberships) that peer review could emerge to begin with.

Equally surprising is the close relationship between the risks of publishing

'foreign' works and those of managing a correspondence. To correspond, in fact, was not a right but a privilege, and the early modern state wished it had the means to do a much better job controlling it. Correspondence could be about innocuous private matters, but it could also come close to the genre of illicit news sheets.⁶² And correspondence could pose further problems when it was not conducted by private citizens but by institutions bearing 'royal' in their title. Both the Académie des Sciences and the Royal Society, in fact, required explicit privileges to manage correspondence with non-academics and foreigners. The Society's royal charter stated that:

... full Power and Authority is granted unto them from time to time by letters under the hand of the President in the presence of the Council, to hold Correspondence and Intelligence with any Strangers, whether private Persons or Collegiate Societies or Corporations, without any Interruption or Molestation whatsoever: Provided that this Indulgence or Grant be extended to no further use than the particular Benefit and Interest of the Society in Matters Philosophical, Mathematical, and Mechanical.⁶³

Similarly, the Académie's 1699 statutes stipulated that:

The Académie shall entertain commerce with various savants, either from Paris and the provinces of the kingdom, or from foreign countries, with the purpose of being promptly informed about novelties concerning mathematics or natural philosophy.⁶⁴

References to 'interruption or molestation' in the Society's charter indicate that, without an explicit royal privilege, its correspondence may have been subjected to censorship. And even then, the Society's secretary's unpleasant stay in the Tower of London under charges of espionage shows that the legal risks of managing a philosophical correspondence were quite real.

The legalities surrounding academic correspondence show a family resemblance with those of book censorship. Both cases were seen by the state as potentially dangerous and in need of regulation. While the two academies were granted the privileges to correspond and to license and print books, these grants were limited to the narrow domain of natural philosophy and mathematics. At the same time, the presence of stipulations about correspondence in the statutes of both academies indicates that, as much as the state wished to control exchanges with the 'outside' (which included foreign nations as well as local savants who had not been screened by the academies or their royal sponsors), those exchanges were recognized to be necessary, not only unavoidable.

If one looks at the 'Règlement' (an amendment to the Académie's rules concerning correspondence issued by Louis XV in 1753), the analogy between the correspondence of a royal academy and state diplomacy becomes clear.⁶⁵ 'Correspondent' was a role, not just a person who sent a letter. One had to be a correspondent to correspond, the same way one had to be an accredited diplomat (not just a foreign visitor) to be received. After 1753, correspondents' qualifications had to be assessed and then ratified through a vote of the full

membership. The results ('correspondance accordée' or 'correspondance refusée') were finally entered in the academy's registers presumably to inform the secretary about whose letters were to be received and whose to be declined.⁶⁶ Peer review, in sum, was extended to both foreign texts and foreign people.

Paris: Toward the Exportation of Authority

When the Académie introduced the *Mémoires* in 1702, it included only academicians' essays. The 1699 statutes seemed clear: the king wished 'to facilitate the printing of the various works that academicians may produce,' not the works of 'étrangers.' Outsiders' work was included in the *Mémoires* only on rare occasions by having it paraphrased by the editor and inserted in the 'Histoire' section of the journal that was clearly demarcated from the section dedicated to the academicians' work.⁶⁷ Transformed in this manner, these texts could be seen as reports produced by the editor (an academician) and no longer as 'foreign' works. When, after 1720, the *Mémoires* included a few non-paraphrased essays by non-academicians, these were the winning essays of prize competitions put on by the Académie. The publication of outsiders' essays remained a touchy subject that was never explicitly solved, only displaced with the introduction of the *Mémoires des savants étrangers* in 1750 — a journal the Académie dedicated to the work of non-academicians. The publication of the new journal may have been synchronized with the king's extension (also in 1750) of the Académie's publishing scope through an unprecedented 'Privilege General' stating that the Académie was allowed to publish whatever it wished to (though, of course, within the boundaries of natural philosophy).⁶⁸

The Académie is a development of a separate journal for the work of outsiders, even though it had the legal power to include it in its standard *Mémoires* indicates that, by 1750, peer review had already incorporated other elements to its initial licensing role. It was no longer just about preserving the status of the institution and its royal patron. Its simultaneous publication and marginalization of 'foreign' work in a special new series of the *Mémoires* indicates that, thanks to the exceptional printing privileges it had initially obtained because of the relative irrelevance and scarcity of natural philosophical texts, the Académie was able, a few decades later, to turn itself into the scientific judge and publisher of other people's work. While at first it was allowed to act as its own censors, it later expanded its jurisdiction to whomever wished to 'submit' his or her work for publication in the Académie's journals.⁶⁹

As a result of this movement from inside to outside, the Académie's peer review began to shed its function as a legal licensing practice and assume the mantle of quality control. No longer focused exclusively on defending the corporate honor of the institution or the moral-political health of the state, peer review became part of a move toward 'exporting' the Académie's authority over the growing field structured around the many academies that had sprouted in all major European capitals and many provincial cities.⁷⁰ While this may appear to be a movement from a negative (filtering) to a positive

(endorsing) function, the difference between these two registers of peer review is better understood in terms of its changing scales of operation, from the exchanges made possible by a wider academic landscape, to the notions of value that developed around those exchanges.

Peer review began to function more as a producer of academic value as a result of a fast-growing market for academic texts and people. In the 17th century, the Académie's publications were essentially autarkic products meant primarily for internal or royal consumption. They barely circulated. But things changed after 1699, first with the *Mémoires* and then with the *Mémoires des savants étrangers*. These publications were clearly meant to be consumed by distant audiences, but were also expected to elicit submissions from foreigners. Like correspondence, they were part of a circulation of texts and people in the academic market and they required constant and simultaneous boundary crossing and boundary maintenance.

Correspondence was treated as carefully as submissions from non-academicians because its otherness posed comparable dangers. It also offered comparable benefits. With the academic boom of the 18th century, academicians were almost in short supply, as shown by the bidding wars over star academicians between Paris, Berlin, and St. Petersburg. Recruitment, not only boundary keeping, became crucial to the survival and furthering of the new academic system. Correspondent status and publications became ingredients of the candidates' profiles and, like today's tenure requirements, they were spelled out in the academies' statutes.⁷¹ Peer review of foreign work or the selection of correspondents, therefore, was not just an attempt to export authority outside the Académie's walls, it was also part of a commonly understood market with cycles of exchanges, recruitment, and reproduction which the Académie could manage more independently without the near-total royal control it experienced in its early years.

The increasing differentiation of peer review from censorship, therefore, was linked to the development of an academic market that extended outside of the academies' walls, that is, outside of the royal apparatus. At the same time, the branching out of the Académie's peer review practices toward a broader market was made possible precisely by those exceptional tools — printing and licensing privileges — it had received from the state. The explicit codification of academic value in terms of publications and correspondence work marks another shift connected to the changing scale and structure of the academic market: the beginning of 'remote' evaluation. Correspondents or 'foreign' contributors were not in Paris, and could not interact at close quarters with the other academicians.⁷² Unlike the original academicians, who were mostly Parisian insiders known to and selected by royal officials, the members of later generations were more likely to come from different locations, and therefore to be evaluated, at least initially, more through their work and letters and less through, so to speak, their bodies. This might have been the beginning of the trend toward 'impersonal' review that culminated, 200 years later, in 'blind' review. However, it seems that the beginning of this trajectory may have been informed less by a desire for transparency and objectivity than by the constraints posed by an expanding market

that made the introduction of evaluation and recruitment at a distance necessary.

These trends were just emerging in the 18th century. As indicated by the Académie's differential handling of essays by non-academics, the 'republic of letters' was still far from being a multicentered and pluri-hierarchical system. The Académie's review system did not amount to what is now considered peer review in the basic sense that it assumed there was only one peer that counted and that peer was a corporate one: the Académie itself.

London: Living on 'Philosophical Commerce'

Although the Royal Society could legally publish the work of non-academics without having to tweek with its royal privilege, it seemed much more insecure about its authority and feared that its publication of 'foreign works' could be read as an endorsement. Unlike the Académie, the Royal Society saw passing judgment on work by outsiders a risk, not a sign of power. Besides sharing the Académie's concern with making sure not to print anything that would embarrass the king and jeopardize its printing privileges, the Royal Society had to confront an additional problem: it had neither the financial support nor the symbolic legitimation the Académie received from the French kings. A dilemma followed: the Royal Society was dependent on the size of its correspondence networks and the frequency and quality of its publications to make up for what it lacked in terms of royal legitimation, but its initial symbolic capital was relatively small to shoulder such a potentially dangerous course of action. Furthermore, because the Society's membership criteria were quite welcoming (at least to people with the appropriate social background), it would have been unwise to assume its members were as pre-disciplined and publication-safe as those of the Académie. Risk control, in fact, became the driving concern behind most of the Society's peer review and publication practices.

The Council even worried about the risks brought about by expressing opinions on manuscripts submitted for comment (not publication) by practitioners external to the Society. In a resolution predating the establishment of the *Transactions*, the Council stated that:

Upon the occasion of the manuscript philosophical books, which were presented to the society for their examination, and received a good character from those members who had read them, it was ordered, that no books presented to the censure of the society shall receive a public approbation for them; but only, if the society thinks fit to refer such books to one or more of the fellows, esteemed by them competent judges thereof, that the report made of the society by such fellows may be communicated to the authors of the books thus presented; and that it may be signed by one of the secretaries.⁷³

Similar concerns emerged during the Society's review of books it had commissioned from its own members (like Hooke's *Micrographia*). These books were

likely to be perceived as stemming from the Society and bearing its endorsement. Hooke was ordered to:

... give notice in the dedication of that work to the society that, though they have licensed it, yet they own no theory, nor will be thought to do so: and that several hypotheses and theories laid down by him therein, are not delivered as certainties, but as conjectures; and that he intends not at all to obtrude or expose them to the world as the opinion of the society.⁷⁴

The next day, Hooke wrote Boyle that although the text of the *Micrographia* was already printed, the completion and release of the book had been delayed for about a month because of 'the examination of them [the observations] by several members of the society; and the preface, which will be large, and has been stayed very long in the hands of some who were to read it.'⁷⁵ This defensive attitude was reflected in the Society's editorial interventions, which, in fact, were essentially 'subtractive.' They mostly took controversial passages out or toned down claims. They were about shielding the Society, not about maximizing the readers' or authors' benefit.

When it came to publish the *Philosophical Transactions*, the Society had to confront an additional problem on top of the already serious concerns about appearing to endorse the work of non-members: the Society did not produce sufficient material to sustain the publication of a journal. Its strength as an institution lay in its correspondence network, not in a productive or particularly competent membership. Paradoxically, the Society had plenty of 'foreign' material to sustain a journal, and turn itself into a clearinghouse for philosophical information, and a center for the registration of discoveries and priority claims, but these were precisely the kind of resources whose publication gave the Society serious headaches. The richness of its correspondence was, in a way, a sign of its institutional weakness. While the Académie had a limited correspondence because it kept fencing off people eager to put a foot in the door by becoming correspondents and thus inch toward membership (a membership that could carry a salary, substantial status, and several perks), the Society did not pay its members (actually it required membership fees from them) and therefore welcomed correspondents as a resource, not a potential drain.⁷⁶ But, for the same reason, the Society received many texts it could not publish without endangering its own fledgling reputation.

The solution to this 'publish foreign or perish' dilemma took the shape of an interesting hybrid. In March 1665, the Society authorized (actually, it mandated) the publication of the journal:

It was ordered ... that the *Philosophical Transactions* be composed by Mr Oldenburg, be printed the first Monday of every month, if he have sufficient matter for it; and that that tract be licensed by the council of the society, being first reviewed by some of the persons of the same.⁷⁷

Subsequently, the Society licensed all issues of the journal, had them printed by its official printer, and even ordered Oldenburg to insert in its issues some of the more interesting papers it received and read.⁷⁸ But despite such a well-

documented direct involvement, the Society had Oldenburg state in the journal's first issue that the *Transactions* were not the business of the Society but the editor's own enterprise ('onely the Gleanings of my private diversions in broken hours') — a statement not unlike the one it had ordered included in Hooke's *Micrographia*. This message was repeated in 1666 and then, under a different editor, in 1683.⁷⁹ It was only in March 1752, when a committee was appointed 'to consider the papers read before them, and select of them such as they should judge most proper for publication in future *Transactions*' that the Society claimed full responsibility for the journal.⁸⁰ What changed hands in 1752 was not only the official responsibility for the journal, but its ownership as well.

Ownership, in fact, was one of the few issues the Society could use to draw a line between itself and the journal. The journal was run by Oldenburg (and, later, his successors) who financed and profited (marginally) from it.⁸¹ For the rest, the *Transactions* were very much like one of the several books authored by members the Society licensed, published, and distributed through its printers. The phrasing of the Council's resolution related to 'ordering' Oldenburg to compose the *Transactions* is reminiscent of similar orders the Society had issued to its few paid members (usually Hooke) to write books to be published by the Society.⁸² This, I believe, offers a key to the puzzle of the Society's simultaneous involvement with, and disowning of, the *Transactions*.

The Society, eager to publish the journal but worried about the related dangers, may have engaged in a bit of creative taxonomy. It did not treat the *Transactions* as a journal that published essays by academicians and non-academicians alike, that is, a collection of individually authored essays. Rather it construed each issue of the *Transactions* as a book produced by one of its members, Oldenburg (who was therefore treated as an author, not an editor). Once the *Transactions* were framed as an internally produced book, the Society's responsibility in it could be no more or no less of what it had been in Hooke's *Micrographia*. Oldenburg could be construed as the author of each issue of the *Transactions* the way Hooke was the author of *Micrographia*. Both books were ordered, reviewed, licensed, and printed by the Society, and in both cases the Council did not shy away from occasionally telling the authors what they had to include in their books. At the same time, these books were the sole responsibility of their authors, not the Society.

In sum, the Society did and did not peer review the journal: it reviewed each issue of the *Transactions* as a book, but did not select each article through peer review.⁸³ As puzzling as this categorization of the *Transactions* might be to modern eyes (or to anyone who could gauge the Society's substantial involvement in the journal), it did fit the contemporary perception of journals as books authored by their editors (unlike today's journals, but like today's edited books).⁸⁴

What was being worked out here was not only the function of peer review and the relationship between an institution and its journal, but also the very role of the editor.⁸⁵ Such a fluidity affected also the status of the texts received by the Society and published in the very early *Transactions* — texts which would not easily fit modern categories such as 'journal article,' 'editorial,' and

'letter to the editor,' etc. For instance, were they submitted for publication or sent as letters to be read and discussed?⁸⁶ And if their authors thought of them as letters, were they meant for Oldenburg the secretary of the Society or Oldenburg the editor of the *Transactions*? How ethical was it to have Oldenburg use the Society's correspondence for something the Society declared to be his private business?⁸⁷ Clearly, the journal's editorial practices were also very much in the making. For the first two years the *Transactions* contained more copy based on Oldenburg's paraphrases of papers and letters sent to the Society (or articles already published in the *Journal des Savants*) than original authored pieces explicitly written and submitted for publication in the *Transactions* and presented without obvious abridgment. These different kinds of texts, however, were not demarcated in any systematic way. Anonymous reports (or reports rendered anonymous by Oldenburg) were not uncommon, as were anonymous book reviews. It seems that, because of the novelty of the publishing experiment, Oldenburg could only make up his editorial protocols as he went.

But as experimental or problematic as his practices may have been, they did make it easier for the Society to think of the *Transactions* as the secretary's business because much of the copy in the early issues had actually become Oldenburg's work by the time it was printed. By transforming bits and pieces of his incoming correspondence and personal communications into journal entries, Oldenburg had cast himself not just as an editor but also as an author. And Oldenburg's role as the author responsible for his books and 'sources' allowed the Society to take credit for the journal.

What Oldenburg was making up as he went was not just the function of the editor, but that of the secretary. We have seen how touchy a subject correspondence had been to the Académie des Sciences because of its liminal status at the boundaries of the institution — a position not unlike that of the editor. And correspondence was a serious matter for the Society too because, like publishing, it was a delicate privilege, not a right. But what is more interesting in this regard is the structural analogy between the editor and the secretary in their hybrid role at the boundary between the inside and the outside of the institution. For instance, by keeping the role of the secretary somewhat unclear (as unclear as the connection between the editorship of the *Transactions* and the Society), the Society managed to have his cake and eat it too. If everything went well, Oldenburg's correspondence was that of the Society. If there was a problem, then it was only his private venture. In sum, the Society used Oldenburg as a flexible buffer between itself and the outside — it simultaneously feared and needed. (Parenthetically, this is a pattern of musical chairs we still find today, and often, between publishers and editors, or between editors and referees of journals.)

Conclusion

The broad hypothesis I have outlined here is that peer review started as an early modern disciplinary technique closely related to book censorship. The slow differentiation of peer review from book censorship started in the second half of the 17th century, with the development of royal academies. In the 18th

century, peer review maintained its close family resemblance to the censorship system, but expanded its jurisdiction. It started out as an in-house disciplinary technique for specific academies, but was then extended to the evaluation of the work of non-academicians. In several ways, this transition was connected to the development of academic journals.⁸⁸

Unlike what we see today, all early journals were not just written by and for academics but were also institutionally tied to academies. It was because of the continuous link that connected royal academies, their journals, their statutory requirements for peer review, and their publishing privileges, that peer review became an inherent part of early academic journal publishing and, by extension, of the publication protocols of the 18th-century academy-based 'republic of letters.' The extension of the jurisdiction of academic review outside of the membership circle of early academies marks what, I believe, was a crucial point in the genealogy of what we now call peer review. It is the first step toward redefining 'peer' in non-local terms, as well as toward establishing a broad scientific publication system hinged on review by academic peers, not state censors.

Along with the institutionalization of scientific disciplines, peer review also moved from a filtering function (to stop 'unsuitable' books from being printed) to an editorial function (to intervene on texts to make them conform to disciplinary standards). The development of such parameters of quality or publishability did not represent a simple freeing of science from the legalities of censorship but the articulation of a new, more specialized and internalized kind of disciplining.

When, from the end of the 18th century on, state censorship systems faded out with the absolutist regimes on which they hinged, peer review did not follow their demise. It lost its legal role within the licensing system of scientific publications, but it persisted unchallenged as a selection practice that eventually came to characterize the whole of academic and university science. By the 19th century, peer review had become so dispersed and decentered (like the scientific community it had become part of) that its logic no longer depended on a center of authority (like the absolutist state or the king) or on a handful of royal institutions. In my view, however, peer review had moved from being a disciplinary technique to becoming a discipline (a discipline that constituted academic disciplines) even before the collapse of the absolutist center and its police-like censorship practices. That process, I believe, had started earlier on, with the establishment of the royal academies and with the slow transformation of state disciplinary techniques into academic ones.⁸⁹ It is perhaps for this reason that peer review had very little difficulty shedding its negative symbolic connections to early modern absolutism to become, instead, the new symbol of the relationship between science and liberal societies.⁹⁰

If we take a *long-durée* perspective, the history of peer review marks a series of changes in the meaning of both 'peer' and 'review' — changes bordering on role reversals. The transition from disciplinary technique to internalized professional discipline, for instance, was accompanied by a sharp change in the meaning of 'peer' from royal subject to academic researcher. While it would have been easy to define 'peer' around 1700 in terms of membership in a given

academy, now the meaning of peer is highly situational, perhaps hopelessly so. Also, because of the narrow range of markers of professionalization available in the 17th century, academic membership tended to be the cause rather than the effect of one's designation as 'peer.' This has been largely reversed in modern scenarios. University training (usually the PhD) has become the requirement for admission into the basic degree of peerdom, and it is during this training process that the graduate student is disciplined into peer review. Today one gains membership into most professional associations simply by paying membership dues (which frequently coincide with the subscription to the society's journal), but unlike what we have seen in the 17th-century scenarios, membership in a modern professional society has no impact on one's ability to publish in its journal.⁹¹

We have also seen that early royal academies drew a sharp line between themselves and the rest. From their point of view (or that of their royal patrons) the 'rest' was not necessarily made up of 'peers.' Despite more or less sincere invocations of a 'republic of letters,' scientific publications were managed by the journals of a few academies which saw themselves as judges or managers, not as providers of publication 'services' to the 'community.' That changed slowly, but not as the result of a simple democratization of the scientific field, or with the end of absolutism. This transition did not reflect the end of the distinctly hierarchical structure of early modern science, but rather the multiplication of authoritative centers and the development of a pluri-hierarchical field.

The proliferation of more authoritative nodes within the community, the further specialization and multiplication of disciplines, the development of professional societies (as distinct from all-comprehensive academies) and their specialized journals, the emergence of universities as center of research (not just teaching), and the overall quantitative and geographical expansion (or explosion) of the scientific field led to a radical reshaping of the relationship between journals and institutions: By and large, journals have ceased to be the official organs of academies. In sum, we have moved from a scenario in which publishers and producers were the same people, housed in the same royal institution, who met once or twice a week and took turns at reviewing each other's work, to a situation in which a sharp division of labor (and often an institutional division too) has been introduced between producers, editors, reviewers, and publishers. Hierarchies, of course, have remained, but their taxonomies have become both more specialized and fragmented. If in the early modern period the competition may have been between one royal academy and that of another nation (each of them with their members, journals, laboratories, museums, and observatories, etc.), now the competition is between one journal and another, one laboratory and another, one university or department and another.

These patterns of expansion, dispersal, specialization, and fragmentation have deeply changed the definition and role of peer review as well. While it would be relatively easy to define what 17th-century book censorship was about, or how peer review functioned within a specific early modern academy, today peer review is the name we give to a remarkably broad range of review

and judgment practices we may not be able to list or describe exhaustively. Although it no longer licenses texts, the scope of peer review has vastly increased from scientific publications to other decision-making processes. At least in the public eye, peer review has maintained the aura of the imprimatur, though one that is now about technical accuracy, not legal approbation. Peer review now tends to be seen as a service for an international community, not for few specific institutions and the political authorities sponsoring them. Editorial boards of many scientific journals are selected so as to represent a cross-section of professional communities and, in some cases, the editorship rotates among different members of professional associations, making the journal 'travel' throughout the community.

Today the beneficiaries of peer review appear to be with the 'consumers' (readers, universities using peer-reviewed publications in promotion cases, and, ultimately, taxpayers) more than the "producers' (the journal, the publisher, and the institutions that may sponsor the journal, etc.). While today's referees tend to remain anonymous and receive no public credit (and, usually, no monetary remuneration), early modern referees increased their professional authority by yielding their reviewing and licensing powers. People understood that 'approuvé par l'Académie Royale des Sciences' meant being approved by a few well-known academicians. While the modern erasure of the referees' identity (and, often, of the author's name as well) is claimed to improve the objectivity of the review by making it 'blind,' it may be also read as an acknowledgment that referees are no longer cast as specific, identifiable judges residing in a few specific institutions, but as nameless voices of a geographically dispersed and multicentered scientific field.

A few journals are still connected to academies (and may still operate according to a model similar to that of the Académie's *Mémoires*), but more are privately owned and run according to a business model, that is, more as a 'service' than a 'tribunal.' The main priority of these modern journals is not the enhancement or protection of the reputation of their parent academy or of its royal sponsor but the maximization of their own symbolic capital — a capital that translates into 'impact factor,' higher subscriptions, and higher revenues from advertisements.⁹² Even the many journals that are owned by professional associations (as in the case of *Science* and the AAAS or other, more specialized journals) are still private enterprises (if corporate ones).

I believe that it is because the social system of science has become so complicated, unregulated, and dispersed both in terms of geography and disciplines that peer review (no matter its actual performance or the uniformity of its modalities and applications) has been elevated to a 'principle' — a unifying principle for a remarkably fragmented field.

I started this essay by saying how peer review is often invoked to differentiate science from other disciplines whose decisions are framed by market considerations, not disinterested judgment. Perhaps there are some useful analogies between the ways the concept of peer review functions within the discourse about the legitimation of science and scientific expertise, and the ways the notion of the market operates within liberal economies. The market is described as the space where private interests are played out with different

degrees of skill and with different outcomes. But when it is treated as an entity separate from the actions that economic actors perform in it, the market is also attributed certain impersonal features, self-regulating processes, and laws. It is these law-like and therefore 'disinterested' features of the market that are invoked to justify capitalistic economy by presenting it as a 'rational' system.

Peer review too seems to be treated in two very different ways. Like the market, peer review is widely recognized to be a space populated by intelligent judgment but also by low expertise, fraudulent behavior, hasty decisions, and excessive conservatism — the full range of behaviors attributed to economic actors as well. But then, as in the case of the market, peer review is also elevated to a different entity status — one that casts it as a disinterested principle. In this case, peer review becomes the principle through which science regulates itself (like a well-behaving market) through a series of rational judgments and decisions. In this incarnation, peer review (like the depersonalized market) is deployed as a powerful discursive tool for the legitimation of science and expertise. These analogies are not, I believe, accidental.

Notes

1. Zuckerman, Harriet and Robert Merton (1971) 'Patterns of Evaluation in Science: Institutionalization, Structure, and Function of the Referee System,' *Minerva*, 9: 66–100; Jasanoff, Sheila (1990) *The Fifth Branch: Science Advisers as Policymakers*, Cambridge, MA: Harvard University Press, chapter 4, 'Peer Review and Regulatory Science,' pp. 61–83; Chubin, Daryl and Edward Hackett (1990) *Peerless Science: Peer Review and US Science Policy*, Albany, NY: SUNY Press; Lock, Stephen (1985) *A Difficult Balance: Editorial Peer Review in Medicine*, London: Nuffield Provisional Hospitals Trust; Rennie, Drummond (ed.) *Peer Review in Scientific Publishing*, Chicago, IL: Council of Biology Editors.
2. However, the low-live status of peer review does not extend outside the boundaries of science. For instance, scientific peer review has not been given legal status in the court of law, as indicated by the Supreme Court findings in *Daubert v. Merrell Dow Pharmaceuticals Inc.*, 113 S. Ct. 2794 (1993).
3. Norman, Colin and Eliot Marshall (1986) 'Over a (Pork) Barrel: The Senate Rejects Peer Review,' *Science*, 233: 145–146; Malakoff, David (2001) 'White House Asks Community to Oppose Earmark Projects,' *Science*, 1293: 2364.
4. Disciplines like particle physics where practitioners tend to work at a few shared facilities in large but intra-dependent teams tend to rely quite heavily on internal peer review. In these cases, the peer review added by journals does not seem to add much to the reliability of publications. On these issues, see Biagioli, Mario (2000) 'Rights or Rewards: Changing Contexts and Definitions of Scientific Authorship,' *Journal of College and University Law*, 27: 99–103.
5. Tamber, Pritbal S. (2001) 'BioMed Central: Taking a Fresh Look at Scholarly

Publishing,' *Science Editor*, 24(4): 121. For empirical studies see Mahoney, Michael (1977) 'Publication Prejudices: An Experimental Study of Confirmatory Bias in the Peer Review System,' *Cognitive Therapy and Research*, 1: 161–175; Peters, Douglas and Stephen Ceci (1982) 'Peer Review Practices of Psychological Journals: The Fate of Published Articles Submitted Again,' *Behavioral and Brain Sciences*, 5: 187–195; *Journal of the American Medical Association*, 280(3) (special issue on peer review); Cole, Stephen, Leonard Rubin and Jonathan Cole (1978) *Peer Review in the National Science Foundation*, Washington, DC: National Academy of Sciences. For a sustained critique of peer review see Roy, Rustum (1982) 'Peer Review of Proposals — Rationale, Practice, and Performance,' *Bulletin of Science, Technology, and Society*, 2: 402–422.

6. Editorial (2001), 'Bad Peer Reviewers,' *Nature*, 413, September 13, 93.
7. Agnew, Bruce (1999) 'NIH Eyes Sweeping Reform of Peer Review,' *Science*, 286: 1074–1076; Alberts, Bruce *et al.* (1999), 'Proposed Changes for HHF's Center for Scientific Review,' *Science*, 285: 666–667. The evidence about Nobel prize work rejected by peer-reviewed funding is anecdotal in the case of science, but more systematic in economics (Gans, Joshua and George Sheperd (1994) 'How the Mighty Are Fallen: Rejected Classic Articles by Leading Economists,' *Journal of Economic Perspectives*, 8: 165–180).
8. Most of the literature on the topic has been produced since the 1980s and has focused almost exclusively on US science in the post-World War II period. It is distinctly policy-oriented. Merton and Zuckerman are the exception as their work focuses on the refereeing system in general and on how it ties in with Merton's model of science as a normative system (Merton, 'The Normative Structure of Science,' reprinted in Merton (1973) *The Sociology of Science*, Chicago, IL: University of Chicago Press, 267–278). Occasionally, historians have looked at peer review in earlier periods as an aspect of the history of scientific publications (Johns, Adrian (1998) *The Nature of the Book*, Chicago, IL: University of Chicago Press; Frasca-Spada, Marina and Nicholas Jardine (eds) (2000) *Books and the Sciences in History*, Cambridge: Cambridge University Press; Bazerman, Charles (1988) *Shaping Written Knowledge*, Madison, WI: University of Wisconsin Press, especially 128–150; Kronick, David (1976) *A History of Scientific and Technical Periodicals*, Metuchen, NJ: Scarecrow Press; Meadows, A.J. (ed.) (1980) *Development of Science Publishing in Europe*, Amsterdam: Elsevier).
9. For an overview of early modern censorship in relation to philosophy, see Grendler, Paul (1988) 'Printing and Censorship,' in Quentin Skinner and Eckhard Kessler (eds) *The Cambridge History of Renaissance Philosophy*, Cambridge: Cambridge University Press, especially 41–51.
10. The logic of peer review, however, predates royal academies and can be seen at play in earlier private academies and religious institutions — institutions that had *not* received special royal dispensations about publishing. For instance, when the Roman Accademia dei Lincei (a private academy) decided to publish and endorse Galileo's Assayer in 1623, it circulated the manuscript among several of its top members to read and

edit it before it was submitted to the censor for the imprimatur. Then, from the late 16th century, Jesuit authors were required to submit their manuscripts to a review committee to receive permission to publish through a procedure called *censurae librorum* (Baldini, Ugo (1985) 'Una Fonte Poco Utilizzata per la Storia Intellettuale: Le 'Censurae Librorum' e 'Opinionum' Nell' Antica Compagnia di Gesù', *Annali dell'Istituto Storico Italo-Germanico di Trento*, 11: 32–55). As the review was aimed at shielding the reputation of the Society (and, indirectly, of the church), referees assessed the theological and philosophical orthodoxy of the manuscripts, but also their relevance, originality, and technical quality. It was a hybrid kind of review that brought together traditional religious censorship and what we would now call scientific peer review. Although Jesuit authors still needed to seek imprimaturs for their manuscripts even after they had passed internal review, it is clear that their internal *censurae librorum* were modeled after traditional book censorship norms.

11. *Histoire de l'Académie Royale des Sciences Année 1699 avec Mémoires de Mathématique ...*, Paris: Jean Boudot, 1702.
12. The chronological priority of science, however, is quite limited in the French case. In 1701, the Académie Royale des Inscriptions et Belles Lettres gained exactly the same printing privileges that the Académie Royale des Sciences had obtained two years earlier, in 1699 (Aucoc, Leon (1889) *L'institut de France: Lois, Statuts et Règlements Concernant les Anciennes Académies et l'Institut, de 1635 à 1889*, Paris: Imprimerie Nationale, 56, 58–59, 89, 92). Some aspects of peer review predate these examples as many renaissance and baroque academies did review and debate the work of their members (sometime before it was published). However, I do not discuss these examples here because such protocols did not have a legal function, that is, they did not possess the power of book licensing.
13. Rotondò (1963), 'Nuovi Documenti per la Storia dell'Indice dei Libri Proibiti (1572–1638)', *Rinascimento*, Series II, 3: 157; Infelise, Mario (1994) 'La Censure dans les Pays Méditerranéens, 1600–1750,' in Hans Bots and Françoise Waquet (eds) *Commercium Litterarium: Forms of Communication in the Republic of Letters, 1600–1750*, Amsterdam: APA, 261–279.
14. On licensing in England up to 1650 see Greg, W.W. (1956) *Some Aspects and Problems of London Publishing between 1550 and 1650*, Oxford: Clarendon Press, 41–62. On the licensing of early presses at Oxford and Cambridge see Johnson, John and Strickland Gibson (1946) *Print and Privilege at Oxford to the Year 1700*, Oxford: Oxford University Press. A comprehensive map of the licensing system in late 18th-century France can be found in Hesse, Carla (1991) *Publishing and Cultural Politics in Revolutionary Paris, 1789–1810*, Berkeley, CA: University of California Press, 14. The university's role was stronger from the 16th to the early 17th century. On March 18, 1521, Francis I had ordered 'that any new works should be examined and approved by representatives of the University of Paris, and in particular that any concerning the Christian faith or the interpretation of Holy Scripture should be passed as orthodox by the Faculty of Theology before being put on sale' (Armstrong, Elizabeth (1990) *Before Copyright: The French*

Book-privilege System, 1498–1526, Cambridge: Cambridge University Press, 100). The role of the university declined, however, after 1623 with the development of royal censors. Similarly, the French clergy maintained direct licensing control only over theological books, which, in any case, also required the state censors' authorization (Roche, Daniel (1988) *Les Republi-cains des Lettres*, Paris: Fayard, 31). The censorship system of the Habsburg Empire also relied on universities and religious institutions as licensors (Schroder, Thomas (2001) 'The Origins of the German Press,' in Brendan Dooley and Sabrina Baron (eds) *The Politics of Information in Early Modern Europe*, New York: Routledge, 135).

15. Moreover, these privileges were not given as grants (which could be easily withdrawn), but were part of the founding charters of these institutions. To the best of my knowledge, the only legally comparable case is that of the 1534 royal charter to the University of Cambridge (which, however, could be interpreted as a simple extension to printing of much older university prerogatives concerning the copying and distribution of manuscripts). The interpretation of the Cambridge charter, however, was less than transparent and was in fact challenged (unsuccessfully) by the London Stationers. Oxford did not seem to have a comparable charter, but used the Cambridge example to claim printing prerogatives for itself (Johnson and Gibson, 5–7). On Oldenburg as a licensor for the London Company of Stationers see Johns, 242–244. On Fontenelle as royal censor in Paris see Goldgar, Anne (1992) 'The Absolutism of Taste: Journalists as Censors in 18th-century Paris,' in Robin Myers and Michael Harris (eds) *Censorship and the Control of Print in England and France, 1600–1910*, Winchester: St Paul's Bibliographies, 100.
16. It is doubtful that a technical journal like the *Mémoires* could have survived if it had not been the subsidized publication it was, but as a for-profit venture. They operated in a kind of 'royal vanity press' economy. Even the less technical *Philosophical Transactions* provided marginal income to its editor, Henri Oldenburg, who complained to Robert Boyle on September 24, 1667 that: 'Mr Martyn deals very mercenarily with me; for knowing that others will hardly undertake the printing of those papers, now so many of ym have been printed by severall, wch renders it difficult for me to compleat them, without redeeming the interest of others; he knowing this, I say, hath constrain'd me to abate him the rate yet lower, so that, after ye proportion, he allows now, I shall hardly bring it to 30 lb. a year' (Hall, Rupert and Marie Boas Hall (eds) (1966) *The Correspondence of Henry Oldenburg*, Madison, WI: University of Wisconsin Press, Volume III, 480). The pattern has persisted, *mutatis mutandis*, till today. The *Times Literary Supplement* or the *New York Review of Books* manage to operate in the market environment, but scholarly journals are largely supported by institutional subscriptions, and several scientific journals require page fees from the authors or their institutions.
17. Furthermore, the members of the Académies des Sciences seemed particularly mild compared to those of other royal academies. The statutes of the Académie Française (dedicated to the canonization of proper French

language, poetry, and eloquence) included a number of articles about the need to control unacceptable behavior (Aucoc, XXXVI, articles 13 and 11). Similarly, the statutes of the Académie Royale de Peinture include several articles legislating polite behavior, the avoidance of parties and noisy banquets, and other seemingly inappropriate behaviors — articles that are not found in the Académie des Sciences' statutes (Aucoc, CIX: 'Il ne s'y proposera de faire aucun festin ni banquet, soit pour la réception de ceux qui seront juges dignes d'être du corps de l'Académie, ou pour quelque autre prétexte que ce puisse être; au contraire l'ivrognerie, la débauche et le jeu en seront rigoureusement bannis, et l'argent qui se recevra des amendes pecuniaries').

18. Goldgar, 90, citing the work of Catherine Blagonet.
19. Goldgar, 98–104. It is almost funny to see how closely the relationship between early modern censors and authors maps on that of academic and referees today: censors who try to gain the authors' favors by writing positive, book-review-style censorship reports to be published with the book (and with the censor's name attached to them), and, alternatively, censors who try to keep their anonymity when they turned down manuscripts. Then, like today's referees, censors were not paid (Goldgar, 100–101, 104).
20. I would add that while the state could appear to be the official sponsor of an academy (and therefore to be directly behind its publications), it could have appeared inappropriate and politically damaging for the state to be officially attached to a more literary journal like the *Journal des Scavans*. The use of editors and journals as 'fronts' for the state is clear in the case of the *Scavans*, but also, earlier on, with Renaudot and his various periodicals (see note 26 below). The difference here may have to do with the fact that while scientific claims were seen as 'objective' (and therefore not susceptible to influence), texts that reported political news and book reviews were seen as inherently 'soft' and conditionable. In this case, sponsorship could be seen as irrelevant in the first case, but damaging in the latter.
21. Cocheris, Hippolyte (1860) *Histoire du Journal des Savants*, Paris: Durant, VIII–XI.
22. Cocheris, XII.
23. Hahn, Roger (1971) *Anatomy of a Scientific Institution*, Berkeley, CA: University of California Press, 64.
24. Goldgar, 90.
25. On Fontenelle as book reviews editor of the *Journal* see Hahn, 64. On Fontenelle as censor see Goldgar, 100, footnote 68.
26. Solomon, Howard M. (1972) *Public Welfare, Science, and Propaganda in Seventeenth Century France*, Princeton, NJ: Princeton University Press, 100–122.
27. The *Acta Eruditorum* received printing privileges from both the Habsburg emperor and the elector of Saxony. On subsidies see Laeven, Hub (1990) *The Acta Eruditorum under the Editorship of Otto Mencke*, Amsterdam: APA-Holland University Press, 119–123. On dedication to the House of Wettin, see *ibid.*, 43–44.

28. Gardair, Jean-Michel (1984) *Le 'Giornale de' Letterati' de Rome, 1668–1681*, Florence: Olschki, 59.
29. Gardair, 60.
30. Gardair, 79–82.
31. Laeven, 16–17.
32. Laeven, 18.
33. Biagioli, Mario (1996) 'Etiquette, Interdependence, and Sociability in Seventeenth-century Science,' *Critical Inquiry*, 22: 193–238.
34. Article XXIV: 'La principale fonction de l'Académie sera de travailler avec tout le soin et toute la diligence possibles à donner des règles certaines à notre langue et à la rendre pure, éloquente et capable de traiter les arts et les sciences' (Aucoc, XXXVIII).
35. Aucoc, XXXIX, article XXVI.
36. Although the Académie Française did not have the same printing privileges of the Académie des Sciences or of Inscriptions et Belles Lettres, it dealt with matters (the codification of French language and eloquence) that were both contentious and about which many people could claim competence. So the need to establish consensus was reflected in the remarkable fastidiousness of the review rules — rules that tried to address also many of the problems we still witness today: confidentiality, protection against plagiarism, politeness, referees' qualifications and overload, and the presses' undue interventions in the authors' work (see Aucoc, 34–43).
37. This was an unusual review scenario. As we have seen, peer review runs into conceptual contradictions when it is asked to judge (based on present expertise) the best future course for scientific work (a course that may end up destabilizing present knowledge and expertise). But the case of the Académie Française was one in which 'progress' was not the issue ('perfection' was). The primary concern was canon formation based on the expressions of the French language that were already available. In this sense, the kind of review employed by the Académie Française was similar to a legal judgment, that is, the assessment of a given case against an established body of doctrine or canon. This kind of judgment is conceptually different from what we call peer review.
38. Aucoc, XXXVIII, article XXI.
39. Hahn, 27.
40. Cited in Hahn, 28–29.
41. Aucoc, XCII.
42. 'Any work read before the company and approved by the membership was automatically invested with this legal sanction, without further ado' (Hahn, 60). The Abbé Bignon ran both institutions from 1699 to 1715 (Le Brun, J. (1975) 'Censure Preventive et Literature Religieuse en France au Début du XVIIIe Siècle,' *Revue d'Histoire de l'Eglise en France*, 61: 204–205). It is not clear, however, how the statutes were interpreted by the authorities in charge of the book business. For instance, the first publication of the Académie after the statutes (in 1701) did not include any 'approbation' by the censors, but the two later ones (in 1703) did. Those reviews were later dropped. It seems that the Académie's review of the statutes (and their

later amendments) suggest that the granting of imprimaturs to manuscripts reviewed by the Académie was a bureaucratic matter that did not require any additional review. As stated by article 46, the king simply had privileges sent directly to the printer selected by the Académie. If, however, one reads 'privileges' as literally meaning 'privileges' (not imprimaturs and privileges), then the story is different.

43. *Histoire de l'Académie Royale des Sciences Année 1699 avec Mémoires de Mathématique ...*, Paris: Jean Boudot, 1702. Boudot is presented as 'Imprimeur de l'Académie Royale des Sciences,' a title he was granted by the Académie (according to article 46 of the statutes) on March 18, 1699, that is, right after the statutes were issued.
44. Birch, Thomas (1756) *The History of the Royal Society of London*, London, reprinted New York: Johnson, 1968, Volume 1, 347. Additionally, on December 19: 'It was ordered that all those of the society who should print any books of a philosophical nature by order of the society be desired to own themselves in the title page fellows of the society' (389). On April 18, 1667, the Council petitioned the king for a modification of the charter so as to have 'a power to be granted to the president alone to license such books as are published by any fellow of the society' (Birch, II, 168). I have not been able to find whether this petition was accepted. Interestingly, the petition mentions books by fellows, not by external savants.
45. It is interesting, however, that some of the most prestigious members, like Robert Boyle, tended to publish their work outside of the Society's system, possibly because they did not want to be seen as submitting themselves to the authority of that institution.
46. Miller, Edwin Haviland (1959) *The Professional Writer in Elizabethan England*, Cambridge, MA: Harvard University Press, 185. Royal control over the publication of books started in 1538 (through the Privy Council). Before then, book censorship had been in the hands of the church (*ibid.*, 183–184). For the Stationers' role in censorship and book licensing, see Johns, 187–265.
47. Adrian Johns suggests that the Society's licensing system was loosely modeled after that of the Stationers (Johns, 494–495).
48. Sprat, Thomas (1667) *History of the Royal Society*, London: Martyn, 141–142.
49. Birch, Volume 1, 323–324, 328, 347, 366, 389. On the Society's relationship to its printers see Rostenberg, Leona (1965) 'The New Science: John Martyn, Printer to the Royal Society,' in *Literary, Political, Scientific, Religious & Legal Publishing, Printing & Bookselling in England, 1551–1700: Twelve Studies*, New York: Burt Franklin, Volume II, 237–273.
50. Birch, Volume 1, 347, 490; Volume 3, 16, 51, 123, 141, 156, 160, 176, 179, 195, 219, 222, 223, 224. In a few cases, the Society considered the licensing of books by non-members (see Birch, Volume 3, 414, 417), or by deceased English philosophers (see Johns, 495).
51. The detailed narrative is in Nehemiah Grew's preface to his *Anatomy of Plants*, reprinted London: Rowling, 1682.
52. Birch, Volume 1, 442.
53. Birch, Volume 1, 347; Volume 3, 56, 58, 88, 192, 473–474.

54. Birch, Volume 1, 397.
55. There were a few exceptions. On August 24, 1664, 'There was read a paper of Mr Hooke's concerning petrifications, designed by him as part of his microscopical book, then in the press. The society approved of the modesty used in his assertions, but advised him to omit what he had delivered concerning the ends of such petrifications' (Birch, Volume 1, 463).
56. What I mean by 'good quality' is that, when the Society decided to publish a text, it usually expressed a strong appreciation (sometimes even excitement) for it. However, that does not mean that the Society would have been able to spell out what 'good quality' meant according to specific, explicit parameters.
57. Birch's collection shows numerous instances in which a text is read at meetings, only to become 'dead letter.'
58. The Society did not license Sprat, Thomas (1667) *History of the Royal Society of London*, London: Martyn. Although it was basically an in-house publication, it was a book of history, not natural philosophy.
59. Such limited ambitions, I believe, reflected the limited conditions of possibility for specific review parameters. If the staggering variety of topics and, to modern eyes, staggering range of quality one finds in the early publications of the Society (including the journal it started in 1665) can be taken as a reasonable sample of what was produced in natural philosophy at that time (and of how broad the definition of the field could be), it follows that, with a few exceptions, it would have been impossible to develop specific peer review guidelines.
60. An overview of the history of periodicals and newspapers in various European countries is provided by Dooley, Brendan and Sabrina Baron (eds) (2001) *The Politics of Information in Early Modern Europe*, New York: Routledge.
61. This is significant because the Académie was explicitly open to foreign nationals. So the term 'foreign' was meant to refer only to non-academics, not foreign nationals.
62. While in France the genre of the printed newsletter (with regular or *ad hoc* publication schedules) emerged in the 17th century, it continued to share the market with 'agents of information' who distributed handwritten compilations of news (Vittu, Jean Pierre, 'Instruments of Political Information in France,' in Dooley and Baron, 163). But the profiles of well placed savants or secretaries of scientific academies who developed and maintained extensive correspondence networks were not very different from those 'agents of informations.' For instance, the philosophical correspondence of Oldenburg or Peiresc was replete with political and military news.
63. Sprat, 143.
64. Aucoc, LXXXVIII, article XXVII.
65. Aucoc, XCVIII-C.
66. Being a correspondent granted special privileges. If the correspondent decided to travel to Paris, he was given access to the Académie's meetings for up to a year. But the title could be lost by not sending 'useful'

correspondence to the Académie for more than three years, unless the silence was the result of illness or other constraints outside of the correspondent's control (Aucoc, C).

67. Hahn, 61.
68. The 'Privilège Général pour l'impression des mémoires et autres ouvrages des academiciens' allowed the Académie 'de faire imprimer par tel imprimeur qu'ils voudront choisir toutes les recherches ou observations journalieres ou relations annuelles de tout ce qui aura été fait dans les assemblées de ladite Académie royale des sciences, les ouvrages, mémoires ou traites de chacun des particuliers qui la composent et generalement tout ce que ladite Académie voudra faire paroître, apres avoir fait examiner lesdites ouvrages et jugé qu'ils sont dignes de l'impression' (Aucoc, XCVI). Evidently, the Académie interpreted '... et generalment tout ce que ladite Académie voudra faire paroître' as including 'foreign' works. The content of the 'Privilège Général,' however, was not completely new. Similar expansive wording of the privilege can be found in the specific privileges included, for instance, in the *Mémoires* of 1703 and 1704.
69. This applies not only to publications but also to prize competitions, or to the Académie's review of patent applications (Hahn, 21–24, 61–62 (and footnote 9), 66–71).
70. McClellan, James (1985) *Science Reorganized: Scientific Societies in the Eighteenth Century*, Princeton, NJ: Princeton University Press.
71. Articles pertaining to correspondence are found in both the statutes of the Académie des Sciences and the Académie des Inscriptions et Belles Lettres (that is, of history, philology, and archaeology). The two academies were established almost at the same time (1699 and 1701) and their statutes shared many identical articles, including all those related to reviewing, publishing, and membership qualifications. On the relationship between correspondent status and membership, the 1699 statutes of the Académie des Sciences stated that 'L'Académie aura soin d'entretenir commerce avec les divers savants... et dans les elections pour remplir les placs d'academiciens, elle donnera beaucoup de preference aux savants qui auront été les plus exacts a cetté espece de commerce' (Aucoc, LXXXVIII, article XXVII). The Académie des Inscriptions had an identical article in its 1701 statute (article XXV, Aucoc, LX). The status of correspondent was eagerly sought if we find articles spelling out what kind of privileges should *not* be accorded to correspondents. For instance, article VI of the 1750 statutes of the same academy states that: 'L'Académie pourra cependant, à l'exemple de ce qui s'est toujours pratiqué dans l'Académie royale des sciences, delivrer des lettres de simple correspondance, qui ne donneront à ceux qui les obtiendront, ni le titre d'academicien, ni meme le droit de séance dans les assemblée (Aucoc, LXVI, LXXVI for a related article in the 1786 statute). The pressure from correspondents may have mounted if we find, in 1753, an amendment to the statutes which deals exclusively with the regulation of correspondence (Aucoc, XCVIII-C). This 'Règlement' spelled out what the proper definition and qualifications of a correspondent were, what protocols had to be followed to nominate and

- vote on correspondents, the circumstances under which the status of correspondent would lapse, and what privileges the correspondents were to receive. On the relations between publications and membership eligibility, see Aucoc, LXXXVI, article XIII, and XCIV. On publications and monetary rewards, see Aucoc, XCII, article XLVII.
72. Literally, correspondents were defined in the statutes as practitioners who lived at least 10 leagues away from Paris (Aucoc, XCIX, article II).
 73. Birch, Volume 1, 106 (August 27, 1662).
 74. Birch, Volume 1, 490–491. Hooke followed these instructions verbatim in the dedication of the book to the Royal Society (Hooke, Robert (1665) *Micrographia*, London: Martyn).
 75. Hooke to Boyle, November 24, 1664, reprinted in Birch, Volume 1, 490).
 76. Aucoc, XCVIII-C.
 77. Birch, Volume 2, 18.
 78. Records of the licensing of the *Transactions* are in Birch, Volume 2, 18, 169, 176; Volume 3, 94, 141. Records of the Council's decision to insert specific essays in the *Transactions* are in Birch, Volume 3, 9, 10, 132. The Council's order to Oldenburg to publish the journal was not an isolated event. After Oldenburg's death, the Council stated 'That Mr Hooke be desired to publish ... a sheet or two every fortnight of such philosophical matters, as he shall meet with from his correspondents; not making use of any thing contained in the Register-books of the Society without the leave of the council and author' (Birch, Volume 2, 491).
 79. The number 12, May 2, 1666 issue informs the reader that 'Whereas 'tis taken notice of that several persons persuade themselves that these *Philosophical Transactions* are published by the Royal Society, notwithstanding many circumstances to be met with in the already published ones that import the contrary; the writer thereof hath thought fit expressly here to declare, that the persuasion, if there be any such indeed, is a mere mistake' (213–214). The 1683 announcement is cited in Katzen, May F. (1980) 'The Changing Appearance of Research Journals in Science and Technology,' in A.J. Meadows (ed.) *Development of Science Publishing in Europe*, Amsterdam: Elsevier, 185.
 80. This statement was published in the 'Advertisement' of Volume 47 for 1751–52. On this transition, see Katzen, 184, 199–200.
 81. At that time Oldenburg was not paid by the Society for his job as secretary. The stipend came only in 1669. The *Transactions*, therefore, may have been a way to provide him with an income at no cost to the Society.
 82. Birch, Volume 1, 397: 'It was ordered that Mr Hooke produce at every meeting of the Society one of his microscopical discourses, in order for their being printed by order of the society.' See also Birch, Volume 3, 501.
 83. Things changed in the 1690s when 'the Council issued an order enjoining five members to assist the Clerk in drawing up the *Transactions*' (Katzen, 185).
 84. Kronick, David (1962) *A History of Scientific and Technical Periodicals*, New York: Scarecrow Press, 72–73.
 85. Shapin, Steven (1987) 'O Henri,' *Isis*, 78: 417–424; Iliffe, Rob (1999) 'Author-

- mongering: The 'Editor' between Producer and Consumer,' in Anne Bermingham and John Brewer (eds) *The Consumption of Culture, 1600–1800*, London: Routledge, 166–192; Johns, 497–499, 521–532.
86. For sure, the material included in the first issues was not submitted for publication, as correspondents could not have known of Oldenburg's intentions to publish a journal. Later on, selected and published older correspondence dates from well before 1665. It also appears that Oldenburg hatched his plan very quickly after having heard of the publication of the *Journal des Scavans*. The first issue of the *Transactions*, in fact, was published only six weeks after he had presented the *Journal des Scavans* to the Council (Birch, Volume 2, 6, January 11, 1665).
 87. The need to make sure that publication would not displease the author was addressed, but there is little evidence (except in a few cases, one of them being Newton's letter on the nature of light and colors) that these concerns were addressed systematically.
 88. In the case of the Royal Society, this transition took place within a few years. The Académie des Sciences, instead, took a few decades.
 89. Perhaps the transition from disciplinary techniques to discipline that Foucault situates, broadly speaking, at the end of the *ancien régime*, may have had different timeframes for different kinds of subjects. As a hypothesis, I suggest that the subjects (the physicians and the penologists, etc.) who developed the discourses about the disciplining of their subjects (the inmate and the patient, etc.) had become 'disciplined' at an earlier stage, that is, during the *ancien régime* itself. The epistemes of academics may have changed around 1800, but their subject positions may have changed little since the *ancien régime*. By the time the *ancien régime* had collapsed, the logic of the social system of science (and the role of peer review within it) had already been established.
 90. Though I have not traced the linguistic history of 'peer review' it would not surprise me if, as a term, peer review emerged only in the 19th century, possibly as a way to rewrite an early modern, absolutist practice into a new 'democratic' language.
 91. This does not apply to early-modern-style academies like the National Academy of Science, where membership is a powerful factor in gaining access to publication in the academy's journal.
 92. A recent debate in the pages of *Nature* regarding impact factors shows their widespread currency, their unreliability, and their close monitoring by both scientists and journals alike ('Errors in Citation Statistics,' *Nature*, January 10, 2002, 415: 101).