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From ciphers to confidentiality: secrecy, openness and priority in science

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Abstract. I make three related claims. First, certain seemingly secretive behaviours displayed by scientists and inventors are expression neither of socio-professional values nor of strategies for the maximization of the economic value of their knowledge. They are, instead, protective responses to unavoidable risks inherent in the process of publication and priority claiming. Scientists and inventors fear being scooped by direct competitors, but have also worried about people who publish their claims or determine their priority: journal editors or referees who may appropriate the claims in the manuscript they review or patent clerks who may claim or leak the inventions contained in the applications that cross their desks. Second, these protective responses point to the existence of an unavoidable moment of instability in any procedure aimed at establishing priority. Making things public is an inherently risky business and it is impossible, I argue, to ensure that priority may not be lost in the very process that is supposed to establish it. Third, I offer a brief archaeology of regimes and techniques of priority registration, showing the distinctly different definitions of priority developed by each system.

The temporality of secrecy and openness – the different ways in which time frames them as concepts – illuminates their mutual relationship as well as their fundamental link to priority.[1] It also explains certain seemingly secretive behaviours displayed by scientists and inventors, showing that they are, in fact, protective responses to risks inherent in the process of publication and priority claiming. Scientists and inventors fear being scooped by fellow practitioners, but also by people we would see as intermediaries in the publication of the practitioners' claims or in the determination of their priority: printers

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1 I take my argument to be complementary to and distinct from the work by Ludwick Fleck, Augustine Brannigan, Harry Collins and Simon Schaffer on the *a posteriori* construction of discoveries following from the closure of priority disputes. We all look at the instabilities inherent in the process of claiming discoveries, but while they concern themselves with the sociocultural stabilization and destabilization of the object of discovery and the identity of the discoverer, I look at unavoidable instabilities in any priority registration system – instabilities inherent in the process of making things public that would play out irrespective of a consensus about the identity of the object of discovery or the practices for its determination. Despite the obsolescence of their methodology, Robert Merton's essays remain fundamental to discussions of scientific priority: 'Priorities in scientific discovery: a chapter in the sociology of science', *American Sociological Review* (1957) 22, pp. 635–659; *idem*, 'Singletons and multiples in scientific discovery: a chapter in the sociology of science', *Proceedings of the American Philosophical Society* (1961) 105, pp. 470–486; and *idem*, 'Resistance to the systematic study of multiple discoveries in science', *European Journal of Sociology* (1963) 4, pp. 237–282.

who might pass on to a third party the knowledge inscribed in a manuscripts they typeset, journal editors or referees who may do the same with the articles they review, clerks who had the opportunity to claim or leak the inventions contained in the applications that cross their desks, or secretaries of scientific academies who might help themselves to a scientist's claim whose priority they were supposed to register. The evasive manoeuvres scientists and inventors developed to control these risks may appear 'secretive', but are not expressions of socio-professional values like, say, the secrecy commonly attributed to artisanal guilds or alchemists. They are not aimed at keeping knowledge secret but rather at putting it in the open and being rewarded for its novelty. As I hope to show with a few examples, such publication-related risks are not a mere problem but a sign of the inescapable predicament of the process of making knowledge public and of establishing its author's priority.

Losing priority on the way to publication

Prior to publishing the *Sidereus nuncius* in March 1610, Galileo carefully controlled the circulation of knowledge about the telescopic observations he was conducting – a delaying tactic that allowed him to produce stable claims for which he could get credit.[2] But the most dangerous step came after that, when he went to press. Ready to be made public, Galileo's discoveries were also potentially ready for the taking, which may explain why he delivered the book manuscript to his Venetian printer in several instalments.[3] He is said to have written some sections of the book while others were being printed, but he was probably also avoiding having all of his claims sitting in a print shop for weeks in a form that could be understood by anyone who could simply read. (Tycho Brahe's establishment of his own printing press on the island of Hven was likely aimed at controlling the same problem, not just the fear of seeing his texts edited, abridged or poorly printed.[4])

Galileo's worries may have been fuelled by personal experience. New evidence indicates that he did not develop his telescope independently (as he emphatically stated in his books and correspondence), but that he was given a full description of (or even material access to) such an instrument at the very beginning of his telescope-making programme.[5] This was not information he obtained by looking over the shoulders of prior telescope makers or by bribing their associates. It came directly to him from the person who, on behalf of the Venetian Senate, was evaluating a telescope submitted by a foreigner seeking a reward or a patent. (The examiner happened to be Paolo Sarpi, one of Galileo's closest friends.) Knowledge about the telescope was not directly

2 Mario Biagioli, *Galileo's Instruments of Credit*, Chicago: University of Chicago Press, 2006, pp. 77–134.

3 Owen Gingerich and Albert van Helden, 'From occhiale to printed page: the making of Galileo's *Sidereus nuncius*', *Journal for the History of Astronomy* (2003) 34, pp. 251–267.

4 Adrian Johns, *The Nature of the Book*, Chicago: University of Chicago Press, 1998, pp. 6–27.

5 Mario Biagioli, 'Venetian tech-transfer: how Galileo copied the telescope', in Albert van Helden, Sven Dupré, Rob van Gent and Huib Zuidervaart (eds.), *The Origins of the Telescope*, Amsterdam: Amsterdam University Press, 2011, pp. 203–230.

appropriated by a competitor (Galileo), but was rather leaked by an official in charge of examining the device to decide whether it deserved public reward – a role perhaps comparable to that of a modern patent examiner.

Move up about sixty years to Christiaan Huygens's announcement of the invention of the spring watch. We now take journals to be reliable registers of priority, but in 1675 Huygens thought otherwise. When he decided to publish a report of his revolutionary timekeeper in the *Philosophical Transactions* he did not send the full text but only an anagram to Henry Oldenburg – the editor of the journal and the secretary of the Royal Society of London, its de facto publisher. At first, all Oldenburg heard about Huygens's invention was: '*413537312343242 abcefilmnorstux*'.[6] Huygens followed up with a description of the watch only after receiving a letter from the editor acknowledging the receipt of the anagram and informing him that it had been shown 'to our common friends'.[7] An announcement of Huygens's invention was eventually printed in the *Philosophical Transactions* of 25 March 1675.

This procedure suggests that Huygens had only qualified trust in the journal's editor (the person supposed to make his claims public), in the standards of confidentiality of the Royal Society whose correspondence Oldenburg managed, and in the chain of communication linking Paris to London.[8] Huygens used with Oldenburg and the society the same cryptographic method he had previously used to communicate important discoveries (like Titan, a satellite of Saturn) to potentially competitive correspondents, or to publish a broadsheet in 1656 announcing his discovery of Saturn's ring.[9] Similarly, as shown by Rob Iliffe, Robert Hooke (who was to clash with Huygens over the invention of the spring watch) shared, in a rather more virulent form, Huygens's concerns about the confidentiality and impartiality of the institutional and editorial

6 Christiaan Huygens to Henry Oldenburg, 30 January 1675, in Christiaan Huygens, *Oeuvres complètes*, vol. 7, The Hague: Martinus Nijhoff, 1897, pp. 399–400. The solution to the anagram was later given as '*Axis circuli mobilis affixus in centro volutae ferreae*'.

7 On 12 February 1675 Oldenburg wrote to Huygens, '*Au reste, i'ay fait voir à nos amis communs l'Anagramme touchant votre nouvelle invention d'horologes. Ils m'ont tesmoigné leur grand desir d'en voir l'effect, et s'en promettent des nouvelles de votre bonté*.' Huygens, vol. 7, op. cit. (6), p. 416. Huygens sent Oldenburg a brief description of the watch on 20 February, informing him that he could use the relevant parts of his letter as an announcement to be published in the *Philosophical Transactions*. Huygens, vol. 7, op. cit. (6), pp. 422–424.

8 Huygens's caution may have been fuelled by tensions generated by earlier priority conflicts with members of the Royal Society – disputes that may have led him to distrust the society's (and Oldenburg's) proclaimed impartiality. Rob Iliffe, '"In the warehouse": privacy, property and priority in the early Royal Society', *History of Science* (1992) 30, pp. 29–68, 35, 39–41.

9 Albert van Helden, 'Annulo cingitur: the solution of the problem of Saturn', *Journal for the History of Astronomy* (1974) 5, pp. 155–174, 156. The anagram for Saturn was: *aaaaaaacccccdeeeeeghiiiiiiillllmmnnnnnnnnnooooppqrrstttttuuuuu*, which coded for '*Annulo cingitur, tenui, plano, nusquam cohaerente ad eclipticam inclinato*' ('It is girded by a thin flat ring nowhere touching inclined to the ecliptic'). Huygens published it on 5 March 1656 at the end of his *De Saturni Luna observatio nova*, The Hague: Adrian Vlacq, 1656, and then solved it in his *Systema Saturnium*, The Hague: Vlacq, 1659, p. 47. He also disclosed it privately in a 28 March 1658 letter to Chapelain. The anagram for Titan was '*Admovere oculis distantia sidera nostris vvvvvvv ccc rr h n b q x*', which he sent to correspondents in 1655 (having discovered, as he claimed, the satellite on 25 March 1655). He then solved it in 1656 as '*Saturno luna sua circunducitur diebus sexdecim horis quatuor*' ('A moon revolves around Saturn in 16 days and 4 hours') in *De Saturni Luna*.

protocols that the Royal Society was putting in place to manage priority claims.[10] In the heat of the dispute with Huygens, Hooke communicated various of his discoveries as anagrams printed in an appendix to his 1675 *A Description of Helioscopes*, thus bypassing both the registering system of the Royal Society (which he had come to distrust), and the possibility of seeing his discoveries made 'public before publication' by porous print shops or talkative journal editors.[11]

Then, on 31 January, the day after sending the anagram of the invention to Oldenburg, Huygens visited Colbert (Louis XIV's chief minister) to show him a model of the watch and set in motion an application for a French patent. Huygens had good connections and managed to receive a twenty-year patent by 15 February – in less than two weeks.[12] It was only after being awarded the patent by Louis XIV that he sent a description of the invention to the Royal Society, mentioning that his watch was now protected by a French patent.[13] Had this happened today, we would assume that Huygens wanted to patent before publishing because doing otherwise would have made the invention public and thus unpatentable, but that was not at all the case in 1675.[14]

Huygens patented for priority, not property.[15] What mattered the most at this point in time was not the monopoly the patent conferred on the manufacture and sale of the invention (which applied only to France) but the royal time stamp (which reached well outside France's boundaries).[16] That was a time stamp Huygens could deploy not only with French artisans trying to produce and sell it without authorization, but also with philosophers (domestic or foreign alike) contesting his priority over the idea of using a spring rather than a swinging pendulum.

Huygens's dealings with the *Journal des Sçavans* shows that he was cautious when approaching all journals, not just a specific editor. Although he published the very first report of his invention in the 25 February issue of the *Journal*, he did so, even in that

10 Iliffe, op. cit. (8), pp. 46–50.

11 At the end of an Appendix that Hooke added at the last moment to respond to (or, perhaps, attack) Oldenburg and, indirectly, Huygens, we find a list of discoveries, four of which are given as anagrams: 'The true mathematical and mechanical form of all manner of Arches for Building' (*abcccddeeeeeefggiiiiiiiiillmmmmmnnnnnooprrsssttttttuuuuuuuux*), 'The true theory of elasticity or Springness' (*ceiiinosssttu*), 'A new Sort of Philosophical-Scales' (*cdeiinnoopsssttuu*), and 'A New Invention in Mechanicks of prodigious use' (*aaaabccddeeeeeegiiilmmmmnnooppqrrrrstttuuuuu. aeffhiiiinnlrrsstuu*). Robert Hooke, *A Description of Helioscopes and some other Instruments*, London: Martyn, 1676 (but published in October 1675), pp. 31–32. While he did eventually solve some anagrams, like the one related to 'Hooke's Law', he left the others encrypted.

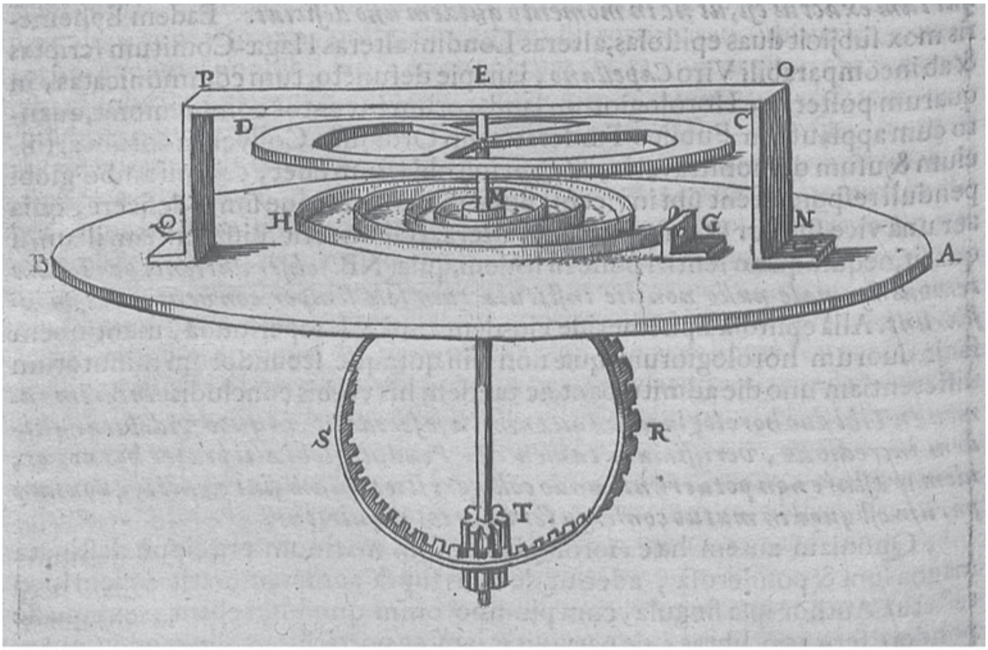
12 Huygens, vol. 7, op. cit. (6), pp. 419–420.

13 Huygens, vol. 7, op. cit. (6), p. 423.

14 Mario Biagioli, 'From print to patents: living on instruments in early modern Europe', *History of Science* (2006) 44, pp. 139–186, 157–158.

15 Similarly, in the introduction to his *Horologium oscillatorium*, The Hague: Vlacq, 1678, Huygens defended his priority over the pendulum clock by citing the Dutch patents he had been granted in 1657.

16 Determined to secure the international recognition of his inventorship, Huygens appeared less interested in the financial rewards associated with patenting, to the point of offering Oldenburg and the Royal Society an English patent for his watch under their name. Huygens, vol. 7, op. cit. (6), p. 424. Likewise he passed the 1657 Dutch patent on his first pendulum watch to the clockmaker Coster. Biagioli, op. cit. (14), p. 145 n. 53.



Quelle: Deutsche Fotothek

Figure 1. Engraving of Christiaan Huygens's balance spring published in the *Journal des Sçavans* of 25 February 1675. Photo by Johann Christoph Sturm (Deutsche Fotothek, file:df_tg_0003783), public domain, via Wikimedia Commons.

case, only after obtaining the Royal privilege.[17] As he wrote in his diary, on Monday 18 February 1675, 'In the evening I received my Privilege, which Monsieur Colbert has sent me. I gave the drawing of the flywheel to the engraver to put it in the *Journal des Sçavants*.'[18] Huygens is referring to the diagram that, together with its verbal description, constituted the core of his invention and was likely to render it reproducible by readers with some skill in horology (see Figure 1). It seems that he did not want this diagram lying around the engraver's and printer's workshops or the editor's office for any longer than necessary – a concern we have already seen reflected in Galileo's piecemeal printing of the *Sidereus nuncius*. They were both concerned about how one might lose priority as a result of trying to establish it through print.

But trying to establish priority through a patent was not necessarily safer than relying on publication. Early modern patenting demanded very little in the way of a verbal or pictorial description of the invention, but it typically required the display of a working prototype or model. Moving to meet those expectations, Huygens had a model made of his spring watch, which he showed to Colbert. But the watchmaker he hired to build the

17 'Extrait d'une lettre de Mr Hugens a l'Auteur du Journal, touchant une nouvelle invention d'horloges tres-justes & portatives', *Journal des Sçavans*, 25 February 1675, pp. 68–70 (68 is misprinted as 64 in the text).

18 Huygens, vol. 7, op. cit. (6), p. 414.

model – Isaac Thuret – became so quickly involved with the development of Huygens's idea that, within days, he started considering himself at least a co-inventor of the device. And because of his own court networks – he was horloger ordinaire du roi and had good connections with Colbert's wife – Thuret was able, for a while, to challenge Huygens's own claims to the inventorship of the watch.[19]

As with the risks that leaky printers, editors and secretaries of academies posed to authors who sought priority and attribution through print or institutional forms of registration, an inventor's work became most vulnerable precisely when it reached the patent application phase. The various procedures we now collectively label 'peer review', and the various safeguards put in place against the appropriation of the author's work by its reviewers, are obviously a response to problems that were already quite evident to Hooke, Huygens and others. We also know that such safeguards took time to develop and remain difficult to maintain and monitor. Data recently assembled by the Office of Research Integrity at the National Institute for Health (NIH) show that the majority of cases of plagiarism in contemporary US biomedicine involve the peer-review process of manuscripts and grant applications, not printed publications.[20]

We find comparable patterns when we move from scientific publications to patents. Attempts by early modern inventors across Europe to file descriptions of their inventions only after receiving the privilege – a demand that was accepted by British patent law until about 1852 – reflect the distrust inventors had for the clerks who handled their applications or the courtiers who facilitated the granting of the patent from the crown.[21] These concerns were explicit in the 1737 French privilege for colour printing awarded to Jacob Le Blon, which was made contingent on his willingness to 'work and state all his secrets, and the practice of his art' in front of experts appointed by the king. Dániel Margócsy has shown that, to assuage the inventor's concerns, it was agreed that the experts 'will not be allowed to claim any part of the profit that could result from the implementation of the privilege'.[22]

While ultimately unsubstantiated, Hooke's claim that the secretary of the Royal Society may have tampered with the content and chronology of the claims that were submitted for registration at the Royal Society evidenced a plausible concern that was shared by both natural philosophers and inventors.[23] Modern readers may be surprised that in the same years when the Royal Society was casting itself as the most trustworthy international repository of priority claims it was also discussing, at its public meetings,

19 Huygens, vol. 7, op. cit. (6), pp. 399–435.

20 Alan Price, 'Cases of plagiarism handled by the United States Office of Research Integrity 1992–2005', *Plagiary: Cross-Disciplinary Studies in Plagiarism, Fabrication, and Falsification* (2006) 1, pp. 1–11.

21 A.A. Gomme, *Patents of Invention: Origin and Growth of the Patent System in Britain*, London: Longmans, 1946, p. 25. Similarly, the US 1790 Patent Act required the deposit of enabling specifications at the time of the grant of the patent, not the application. Edward Walterscheid, *To Promote the Progress of the Useful Arts: American Patent Law and Administration, 1787–1836*, Littleton: Rothman, 1998, p. 465.

22 Dániel Margócsy, 'Commercial visions: trading with representations of Nature in early modern Netherlands', PhD dissertation, Department of the History of Science, Harvard University, 2009, AAT 3365343, p. 220.

23 Steven Shapin, 'O Henry', *Isis* (1987) 78, pp. 417–424.

the contents of pending patent applications that Sir Robert Moray (a member of the society and a prominent courtier) happened to have taken home from work.[24]

Such problems continued in the nineteenth century. The 1836 US Patent Act ruled that Patent Office employees could not develop financial interests in the patents they were reviewing.[25] This did not reflect hypothetical fears. Between 1809 and 1811 the first director of the patent office, William Thornton, intimated to Robert Fulton (who had applied for patents for steam-driven paddle boats) that his applications would be rejected unless he agreed to enter into a partnership to exploit those patents with Thornton himself.[26] Similar issues are still alive today. The 2009 annual report issued by the Beijing office of the European Union Chamber of Commerce alleges that '[foreign] companies are losing vital classified information at various stages of business development, including project certification, environmental impact assessment, *patent filings*, marketing approvals, and registration . . . It is not uncommon for such proprietary knowledge to be leaked to Chinese competitors'.[27]

The temporality of openness and secrecy

A comparison of the three main knowledge genres represented in these examples – scientific publications, trade secrets and patents – shows that the movement from secrecy to openness is one between two regimes characterized by radically different relations to time. Trade secrets – an excellent example of secrecy – can be said to be 'timeless' not because they last forever or because their economic value does not change in time, but in the sense that time does not play a role in defining what a trade secret is.[28] Trade secret law requires you to do a diligent job at keeping them secret, but there is no registration system, no time stamp necessary to make them 'trade secrets'. We could say that trade secrets encounter time only at the end of their life. They do not have a known date of birth, but only a time of death – the moment when they become public. They exist and function *in* time but are not constituted in temporal terms.

At the opposite end of the secrecy–openness spectrum, scientific authorship involves the publication of knowledge claims and their placement in the public domain in exchange for professional recognition.[29] Modern patents are somewhere in the middle. As part of the patent application, the inventor must make the invention public through

24 Thomas Birch, *The History of the Royal Society of London*, vol. 1, London: A. Millar, 1756, p. 252.

25 Walterscheid, op. cit. (21), p. 498.

26 Kenneth W. Dobyns, *Patent Office Pony: A History of the Early Patent Office*, Fredericksburg: Sergeant Kirkland's Museum Press, 1994, pp. 52–57.

27 '2009 Position Paper', Section One: Executive summary, p. 13, at europeanchamber.com.cn, accessed 10 October 2010, added emphasis. For a discussion of the report see 'EU firms voice fears of trade secret "leakage" in China', *Euractive.com Newsletter*, www.euractiv.com/en/enterprise-jobs/eu-firms-voice-fears-trade-secret-leakage-china/article–185148. I found this reference in an anonymous manuscript I happened to review.

28 For an overview of US trade secrets law see Robert Merges, Peter Menell and Mark Lemley, *Intellectual Property in the New Technological Age*, New York: Aspen Publishers, 2006, pp. 33–113.

29 Mario Biagioli and Peter Galison (eds.), *Scientific Authorship: Credit and Intellectual Property in Science*, New York: Routledge, 2003.

its specification, but while the law makes public the knowledge disclosed in the specification, it prohibits unlicensed reproduction, use or sale of the invention until the patent expires.[30] Contrary to trade secrets, patents and scientific authorship are inherently temporal in the sense of necessarily having a beginning and an end. Hinging on novelty and priority, they are simply unthinkable without a time stamp marking their beginning.

As exemplified by trade secrets, patents and scientific authorship, secrecy and openness are not just two opposites, like up and down or negative and positive, but are rather distinguished by different relations to time. Secrecy exists in time but is alien to the logic of priority and of protection and rewards that start and end at specific points in time. Openness, instead, is entirely framed by temporality – sandwiched between the moment at which priority and novelty are established and, at the other end, the expiration either of the terms of protection or of the person of the author. Finally, openness is inherently linked to novelty – at least in the credit regimes of the technosciences. Only the new can be made public. What would it mean to make public something that is not new, something that is already public?

It takes time to construct a claim or an invention and bring it to the point at which it becomes a thing of recognizable novelty – something its author could receive credit or rights for. Knowledge and inventions, therefore, cannot start in the open but need to be moved into openness from a temporarily secretive state in which they developed. In this context openness is not a quality of a thing or person but rather the end of a trajectory – a transition from a regime that is 'timeless' to one that is thoroughly framed by time, origin, novelty, priority and, ultimately, expiration. That, it turns out, is a difficult boundary to cross without giving something away.

Points of singularity and the two-suitcase problem

More than a transfer, the movement from secrecy to openness in the modern technosciences is an exchange regulated by contract or contract-like arrangements. Scientific publications make your knowledge public in exchange for authorial credit and attribution. And modern patent law hinges on the so-called patent bargain: the public disclosure of the invention in the application in exchange for a temporary monopoly on that invention. Making things public means that knowledge changes hands or gets into more hands. But if the quid pro quo between disclosure and credit is clear enough, carrying it out is a surprisingly complicated business that has exercised the minds of practitioners and institutions since at least the early modern period.

The problem is substantially trickier than those scenes in gangster movies where the briefcase with the drugs has to be exchanged for one full of cash, with both sides realizing that there is no safe way to handle what ought be a perfectly synchronized mutual give-and-take without anybody ever managing to hold both briefcases at once. (Technically, the exchange could be managed through some escrow system, but who

30 Mario Biagioli, 'Patent republic: representing inventions, constructing rights and authors', *Social Research* (2006) 73, pp. 1129–1172.

would be the third party above the parts to handle the escrow?) In the technosciences the exchange is made even more complicated – radically so – by the fact that information is a non-rivalrous good. Because you can copy somebody's idea without physically taking that idea away from that person, the briefcase with the drugs does not need to change hands for the exchange (or appropriation) to take place. You can appropriate the invention or discovery by simply opening the briefcase, viewing its contents and remembering them. (Modern mathematicians have carefully studied this predicament, developing 'bit-commitment protocols', 'zero-knowledge proofs', and other algorithms to manage such exchanges.[31])

The problem would disappear if a scientist's claim could emerge as a fully formed article instantaneously printed in a journal with a date and author's name displayed on it without having to traverse intermediate stages that would make it porous to both outbound and inbound borrowings: conference presentations, grant applications, drafts circulated among colleagues and manuscripts submitted for publication and peer review. Typically, allegations of plagiarism are made possible by the time it takes to produce a claim and make it public (a period in which one can get to know what another author has been working on) or by the fact that some findings may not be published at all (thus making a possible independent rediscoverer liable to accusations that s/he has plagiarized it). An imaginary scenario in which widespread publication coincided with conception or discovery would both establish the author's priority and make accusations of plagiarism either impossible or at least resolvable.

In reality, given the impossibility of the instantaneity of conception and global publication (which even electronic publishing cannot achieve), the relationship between credit, authorship and the very content of a claim is bound to be unstable as the claim traverses the limbo between secrecy and openness on its way to priority – the stage at which it is no longer secret but not yet fully public either, articulated enough to be ready for publication and rewards but also most appropriable because of that.[32] Such a phase of heightened vulnerability, however, is not just unavoidable but in a sense necessary as it provides the space in which the work of registering and crediting (not to mention assessing, revising and editing) can take place. In a nutshell, the predicament of making things public is that it takes (and it needs to take) time to process claims and inventions into public inscriptions and yet the process would be safer for everybody involved if it were as close to instantaneous as possible.

31 A 'bit commitment' is defined as 'a protocol between two mistrusting parties, *Alice* and *Bob*, which is supposed to provide the following functionality: In a *commit phase*, Alice gives as input a value X (e.g., a bit) and Bob gets a confirmation that Alice has committed to a value (without learning the actual value of X). Later, in an *opening phase*, Alice can decide to reveal the value X to Bob. The functionality of a bit commitment protocol can be compared with that of a safe as follows: To commit to a value X, Alice writes X on a sheet of paper, locks the paper in the safe, and sends the safe to Bob while keeping the key. To open the commitment, Alice simply sends the key to Bob who opens the safe and reads the value of X'. See www.quantiki.org/wiki/Bit_commitment.

32 This would not apply to very difficult claims, whose obscurity could function almost as a form of natural encryption.

Making time safe

Prior to the development of scientific academies and patent offices, scientists, discoverers and inventors communicated and claimed priority for their work and findings through letters, personal visits, lectures, manuscripts, printed pamphlets, books, appendices to books, local privileges for books or inventions, and so on. All these practices were vulnerable to the leaks described above – risks that could be alleviated only by making publication as fast and wide-reaching as materially possible.

In addition to these practices (many of which are still used today) we also see the emergence, roughly between 1600 and 1850, of two more formalized systems of priority communication and registration, and at least one hybrid between the two. Rather than minimizing the chance of a leak by maximizing the speed of publication or reward, these systems accepted the fact that it did take considerable time to make a claim public or patent an invention and sought to make that period of time safe for the author. But if their goals were identical, their methods were radically different – perhaps even incommensurable.

One system emerged sometime before the establishment of scientific academies in the middle of the seventeenth century and, requiring some cryptographic skills, it was typically used by mathematicians. Its distinctive feature was to break down the process of making things public to a sequence of discrete interlocking steps, a bit like the way some European banks allow you to exit a branch only after briefly holding you between double doors, the one behind you locked and the front one opening up only after they decide that you are not walking out with excess cash. The best examples of this kind of priority registration system are the two-step ciphers (typically anagrams) used by Galileo, Huygens, Newton and Hooke to communicate their discoveries.

The first step was to register the existence of the claim by printing an anagram in a book or pamphlet or communicating it in letters to several key people, preferably spread over a wide geographical area.[33] With this step the author tried to obtain a time stamp not for the discovery but rather for the claim of having a discovery – a claim of having a claim – because at this stage the discovery itself was encrypted and thus inaccessible. By acknowledging receipt of the anagram, the recipients provided such a time stamp; that is, they witnessed the receipt of the author's claim of having a claim.[34] It was at this point, with a receipt in hand, that the author made public the solution of the cipher. In sum, this procedure first separated the content of the claim from its priority, time-stamped the 'outside' of the claim (the encrypted claim, not its actual content) and, finally, rejoined the content with its separately certified temporal origin.

As elegant as this scheme may be, an anagram (or any other short cipher) does not have a univocal relation with the claim it is supposed to register. This introduces an inescapable play that may allow the author to retroactively massage the cipher so as to

33 Paradoxically, a stronger time stamp (obtained by spreading the anagram far and wide) strengthens the claim, but it also puts it at higher risk of being appropriated as a result of being sent out to more people, thus increasing the probability that someone will crack the cipher.

34 If the anagram was printed, the author probably did not need to receive confirmation that somebody had read it. The press itself functioned as witness, so to speak.

make it look to refer to a different claim. One could, for instance, try to increase one's credit by connecting the cipher to a more important discovery that may not have been stabilized at the time the cipher was first issued. Conversely, one could revise its solution to cover up embarrassing developments, like having claimed a discovery that subsequently faded out of existence. Along the same lines, one could simply decide not to solve the cipher at all, as Hooke did with some of the anagrams he printed in 1675.[35]

The play mixes some risks in with the opportunities. For instance, if one of the addressees does not acknowledge having received the letter with the anagram but is skilled enough to quickly unscramble it and figure out its referent before it is publicly disclosed by the original author, he could easily run with the claim. In this case, the first author would have lost her claim not to a person who was already a competitor, but to one who had been given the opportunity to become a competitor by the author's very attempt to mobilize him to provide a time stamp for a priority claim she was trying to establish. (It is not altogether clear how legitimate or illegitimate that appropriation would be.)[36] Though we have no clear evidence of the occurrence of such scenarios (with the partial exception of John Wallis's attempt to hijack Huygens's discovery of the first satellite of Saturn) we need to remember that our database on the priority-related uses of anagrams is quite limited.[37]

Other creative uses of this technique are better documented. Anagrams used by scientists did not encode fully fledged descriptions of the claim, which introduced further play between ciphers and claims.[38] Furthermore, anagrams could encode short enigmas and metaphors requiring additional interpretation, like the one used by Galileo to communicate his discovery of the phases of Venus in 1610. The anagram read, '*Haec immatura a me iam frustra leguntur o y*' ('These immature ones are now read in vain by me o y'), but, when unscrambled, it yielded, '*Cynthiae figuras aemulatur mater amorum*', ('The mother of loves [Venus] imitates the shape of Cynthia').[39] There is no mention of phases here, only an analogy between the appearances of two mythological

35 On Hooke see note 10 above.

36 It would be inaccurate to cast a recipient who might have managed to crack the cipher and publish its content as his own as a plagiarist – at least not in the standard sense of the term. That person would not have sought to steal anything from the first discoverer prior to receiving the cipher – a cipher s/he had not requested. Furthermore, when people receive unsolicited ciphers they typically try to figure out what they are about. Not only is one implicitly challenged to crack the cipher, but having been sent a cipher meant that the sender did not quite trust the recipient. And if the sender did not trust the recipient to begin with, it is not clear why the sender could be surprised if the recipient were to behave in an untrustworthy manner.

37 E.W. Maunder, 'The discovery of Titan', *The Observatory* (1889) 12(147), pp. 146–150; and W.T. Lynn, 'The discovery of Titan', *The Observatory* (1889) 12(148), pp. 181–182; *idem*, 'The first discovery of a satellite of Saturn', *The Athenaeum* (1888) 3171, pp. 165–166.

38 Writing in 1847, after the era of anagrams had come to a close, David Brewster argued, 'There are many discoveries and inventions which could neither be properly represented nor satisfactorily reproduced by the transposition of any considerable number of letters. The omission or the addition of a letter might alter or destroy the meaning of the whole, and by thus throwing discord among a mob of letters might occasion that very breach of the peace which the anagram was intended to prevent'. [David Brewster], 'Mr Adams and M. Le Verrier's researches respecting the new planet Neptune', *North British Review* (1847) 7, pp. 207–246, 243.

39 Galileo sent the anagram to Giuliano de' Medici on 11 December 1610. Galileo Galilei, *Le opere di Galileo Galilei* (hereafter *GO*) (ed. Antonio Favaro), 20 vols., Florence: Barbera, 1890–1909, vol. 10, p. 483, and unscrambled it on 1 January 1611. *GO*, op. cit., vol. 11, p. 12.

figures – an analogy that, using knowledge of both astronomy and mythology, would have to be interpreted in a specific way to reach Galileo's referent after the anagram was unscrambled.

In some cases deciphering an anagram could yield multiple syntactically correct sentences that were, however, unconnected to the author's claim. Still in 1610, Galileo communicated his discovery of the irregular shape of Saturn as '*smaismrmilmepoetaleumibunenugttauiras*' (which he later solved as '*Altissimum planetam tergeminum observavi*' – 'I have observed the highest planet tri-form').[40] But Kepler transposed it (in a remarkably clunky verse) as an announcement of a discovery Galileo never made. Probably projecting his own cosmological interests on Galileo's cipher, Kepler believed it encoded the observation of two satellites of Mars: '*Salve umbistineum geminatum Martia proles*', or 'Hail, double shield, children of Mars'.[41] This kind of play could help the claimant by derailing curious correspondents, but it could also allow him/her to change the referent of the anagram later on, to make it fit better the shape of the claim as it had evolved (or disappeared) in the intervening period.[42] (The brevity of anagrams facilitated such *a posteriori* reinterpretations – either expansive or defensive – by offering short and therefore partial descriptions of the discovery.)

Kepler's misreading of the Saturn cipher shows how a recipient could attach an anagram to an object it was not meant to code for, while Galileo's anagram of the phases of Venus is a good example of the possible defensive uses of the technique. There are, however, other differences worth noting. Galileo's anagram for Saturn was utterly meaningless in its scrambled form – *smaismrmilmepoetaleumibunenugttauiras* – while the one he used for the phases of Venus was an actual sentence: 'These immature ones are now read in vain by me – o y'. This sentence, and its intelligibility, reflected Galileo's predicament at the time he wrote it: he was still observing the changing appearance of Venus ('these immature ones'), and had not yet mapped the full sequence and specific pattern of the phases. The difference between the meaningless anagram for Saturn and the mildly representational anagram for Venus may therefore be related to the fact that while in the case of Saturn Galileo was communicating something he had already observed, in the case of Venus he was trying to register the fact that he was still

40 The anagram was sent in August (*GO*, op. cit. (39), vol. 10, p. 420) and was solved on 13 November 1610 (*GO*, op. cit. (39), vol. 10, p. 474). We do not have Galileo's letter with the original anagram, which we know only in the reproduction of it that Kepler gave in the introduction of his *Dioptrice*. Although Galileo solved the anagram only in November, he was already sure of (what he took to be) the three-bodied Saturn by 30 July. *GO*, op. cit. (39), vol. 10, p. 410.

41 Johannes Kepler, *Narratio de observatis a se quatuor Iovis satellibus erronibus* . . ., Frankfurt: Zachariae Palthenii, 1611, in *GO*, op. cit. (39), vol. 3, Part 1, p. 185. His account of how he came up with this 'solution' is somewhat bizarre, as he claimed to have ordered the string of letters contained in Galileo's anagram that way just as a mnemonic device, and that it was this odd Latin verse that gave him the idea of observing to see if Mars had satellites. But given that, in the 1610 *Dissertatio*, Kepler had already presented the hypothesis that Mars could have two satellites, it would seem that he read Galileo's anagram according to his own guess from a few months earlier.

42 Published in his *Narratio*, Kepler's rendition of Galileo's anagram then prodded other mathematicians (like Harriot) to come up with further 'centrifugal' solutions. John North, *The Universal Frame*, London: Hambledon Press, 1989, pp. 119–120.

developing a claim that may or may not have panned out. In other words, he was trying to obtain a pre-dated birth certificate for a claim that was still emerging.

The Saturn anagram had one specific solution at the time it was written – Saturn appears three-bodied – but Galileo's Venus cipher was literally double- or perhaps triple-faced. Had somebody claimed the discovery of the phases of Venus right after the dissemination of the anagram Galileo could have rescued his priority by unscrambling the anagram to mean, 'Venus imitates the shapes of the Moon'. But if he had not completed the discovery by the date on which he sent out the anagram (as seems to have been the case), then the unscrambling of the anagram in response to somebody else's discovery claim would have given him an earlier-than-deserved priority on the phases of Venus.[43] Conversely, had the phases failed to stabilize, Galileo could have simply said that, as intimated by the literal reading of the anagram, he had just been observing something 'immature' that never grew into anything.

Without venturing further into the intriguing world of cryptography, it is important to realize at least that, as problematic as it may be, the possibility of retroactively attaching new or different objects (or no object at all) to a previously deposited anagram (or of effectively pre-dating a discovery) is an unintended consequence of an attempt to solve the 'two-briefcase problem' by expanding the time frame of the exchange. The cryptographic and semantic play is made possible precisely by the space that is opened up between the content of the claim and its time of birth – the necessary *décalage* that is at the core of such multi-step schemes. Making time safe is also what allows for the discoverer to engage in 'time travel' – the *a posteriori* revisiting of her claims. In turn, the *décalage* between content and time of birth was introduced (or perhaps had to be introduced) because of the impossibility of instantaneous dissemination of the claim, and the problems that followed from its relatively slow publication. The play that ensues is not, therefore, a problem but rather a predicament that puts into relief the unavoidable singularity in the transition from secrecy to openness – a singularity that may be managed but never fully controlled.[44]

Interlude: between ciphers and modern priority

The establishment of scientific academies in the second half of the seventeenth century had a profound long-term impact on protocols of priority registration. Academies advertised their registers as reliable ledgers of discoveries and offered their journals as channels through which priority claims could be made public. But trust in these new

43 Weirdly, if Galileo had not completed the observations by the time the hypothetical competitor announced the discovery of the phases of Venus, then Galileo's disclosure of the cipher to reclaim priority would have amounted, in a sense, to a well-covered-up case of plagiarism. Galileo, in fact, would have used the pre-existent cipher to claim as his a discovery he had not yet completed.

44 Modern electronic publication technologies can make the delivery of a claim almost instantaneous and almost global. But even those publication models that do not involve the peer review of the work (and thus avoid the possibility of appropriation by reviewers) cannot change the time needed to produce the claim to begin with. They cannot, therefore, eliminate the porous predicament in which the claim grows prior to publication.

institutions did not grow overnight, keeping ciphers in business until about the end of the seventeenth century. The coexistence of old protocols and new institutions led to the emergence of hybrid protocols of registration like Huygens's use of ciphers to communicate his work to journals and academies, or his reliance on patents as time stamps. Other examples could be Hooke's inclusion of anagrams of his inventions as appendices to books licensed and printed by the Royal Society – a practice that communicated mistrust of the academy's practices of priority registration but also an appreciation of its publications as a more effective vector for the communication of ciphers than private correspondence. Similarly, Newton decided not to publish his discovery of the calculus in the pages of the *Philosophical Transactions*, but sent it to Oldenburg, its editor, as a cipher in a 1677 letter. That he asked Oldenburg to forward the cipher to Leibniz suggests that while Newton did not wish to share the content of his discovery with either the society or the readers of its journal, he still wanted to use the society's secretary as a witness to the time at which the cipher was communicated to Leibniz.[45]

The most important and long-lasting of the hybrids between ciphers and institution-based priority systems was, however, the sealed note.[46] Over seventeen thousand of them were deposited at the Académie des sciences in Paris between 1735 and 1983.[47] Materially speaking, sealed notes and anagrams do not appear to share much more than a reliance on paper, but their logic was virtually identical. The sealed note functioned like a cipher in the sense that when a scientist deposited a sealed claim with the secretary of an academy, the claim was inaccessible – made unreadable not by encryption but by the physical wrapping and seal. And as in anagram-based protocols, the scientist who deposited a sealed note received a time stamp for a claim of having a claim, not for the claim itself. Similarly, it was up to the scientist to decide whether and when to make the claim public.[48]

45 Isaac Newton, *The Correspondence of Isaac Newton*, 7 vols. (ed. H.W. Turnbull *et al.*), Cambridge: Cambridge University Press, 1959–1977, vol. 2, pp. 110–129. Newton's approach resembles Huygens's decision to communicate the invention of the spring watch in a cipher to Oldenburg, except that in this case Newton asked Oldenburg not to publish in the journal but to forward it to Leibniz in a private letter. That letter was forwarded to Leibniz on 2 May 1677. Henry Oldenburg, *The Correspondence of Henry Oldenburg*, vol. 13 (ed. and tr. Rupert Hall and Marie Boas Hall), London: Taylor & Francis, 1986, pp. 267–269. Once unscrambled, the anagram read: '*Data aequatione quotcunque fluentes quantitates involvente, fluxiones invenire et vice versa*' – 'Given an equation involving any number of fluent quantities to find the fluxions, and vice versa.' On the exchange, see A. Rupert Hall, *Philosophers at War: The Quarrel between Newton and Leibniz*, Cambridge: Cambridge University Press, 1980, pp. 62–69.

46 Another hybrid was the short-lived attempts by academies to standardize the use of anagrams and their length to register and communicate discoveries discussed in Iliffe, op. cit. (8), pp. 35–36. There is a clear trend toward descriptive anagrams in Huygens and Hooke, in contrast with the more metaphorical ones used by Galileo.

47 The Royal Society of London started using them in February 1668. Iliffe, op. cit. (8), p. 35. Their adoption by the Académie des sciences in Paris is discussed in Pierre Berthon, 'Les plis cachetés de l'Academie des Sciences', *Revue d'histoire des sciences* (1986) 39, pp. 71–78; and Stewart Saunders, 'The archives of the Academie des Sciences', *French Historical Studies* (1978) 10, pp. 696–702.

48 While it was up to the author to unseal or unscramble the claim, it was understood that if one took too long to do that, s/he would effectively relinquish priority. (I owe this point to an anonymous *BJHS* referee).

As suggested by the many *plis cachetés* that remain sealed in the archives of the Académie des sciences, one could claim a very early priority based on work in progress and then decline to unseal the note if the claim did not pan out as expected or if, in luckier cases, nobody contested the priority of that claim when it was eventually made public in print.[49] Furthermore, as with ciphers, the difference between the short note sealed and deposited with an academy and the much longer text the scientist may publish later on created room for *a posteriori* semantic manoeuvres. While the note and the article or book that followed it were cast as being about same claim, the system gave some leeway in how one could craft that relation of 'identity'. (Parenthetically, these practices are not limited to science but find an analogue in the so-called 'patent caveats' allowed in the US until 1909, or the 'provisional applications' now allowed in most countries – shortened applications that grant the applicants a temporary priority time stamp while allowing them to file the patent claims later, or perhaps never.[50])

Ciphers and sealed notes shared a similar function – they were both instantiations of 'bit-commitment protocols'. However, while the former relied on encryption in order to function in a pre-institutional environment in which trust was a rare and fragile commodity, the latter relied on institutional practices of deposit and safekeeping – practices that were made possible by (and in turn sustained) the trust that practitioners were developing in their academies.[51] The virtually complete transition from ciphers to sealed notes by the beginning of the eighteenth century marks the first step in a trajectory that reframed the 'two-briefcase problem' as something to be addressed through institutional protocols rather than encryption techniques.[52]

In 1860 the Académie des sciences claimed the right to open unclaimed notes after one hundred years. Berthon, op. cit. (47), p. 72.

49 Despite the fact that in 1860 the Académie des sciences claimed the right to open unclaimed notes after one hundred years, many of them remain unsealed in the archives. Berthon, op. cit. (47), p. 72. Unlike earlier cipher-based systems, the sealed note had a supplemental relation to publication. If you sent out a cipher, people expected you to solve it at some point, but that expectation did not apply to the sealed note. The sealed note was not necessarily the first step in the process of making claims public (as I think many or most of the early ciphers were), but rather an 'insurance policy' on the priority of one's claim – an insurance one hoped never to use.

50 Like modern provisional applications, caveats described the invention but did not spell out the claims, thus creating some leeway in which they could be written later on. Neither caveats nor temporary applications involve examination, thus making them function only as evidence of priority, like an early modern sealed note, or an anagram. Provisional applications are not published, thus making them function like sealed notes. Ian Cockburn, 'A provisional application – an important tool in the right hands', at www.wipo.int/sme/en/documents/prov_application.html.

51 The recipients of early anagrams did not need to be competent in the discipline related to the claim. Their role was that of time-stampers, not evaluators. Unlike the members of academies who gathered together to testify experiments as part of their 'form of life', being a witness to a priority claim delivered in an anagram was not a voluntary act. The former may be seen as colleagues, but the latter should not. The recipients of the anagrams were turned into witnesses by the very fact of receiving a letter containing a cipher.

52 Based on a preliminary search, I have found no evidence of the continuing use of ciphers for priority purposes in the eighteenth century. I therefore tend to agree with David Brewster's remarks, written in 1847: 'The disadvantages of the Anagram as a secret receptacle for scientific truth, must have been long ago perceived; and we believe, it has been seldom, if ever, used in the last or the present century'. [Brewster], op. cit. (38), p. 242.

The corporate remaking of priority

The next step was much more radical. If ciphers and sealed notes were two different techniques to manage the same notion of priority, the nineteenth century redefined priority altogether: from first-to-discover to first-to-print.[53]

Like older anagram-based schemes, modern priority registration systems use protocols for time-stamping claims, but do not accept claims that are either sealed or encrypted. Modern scientists send full manuscripts, not ciphers, to journals. To prevent leaks and appropriations as the now 'naked' claim makes its way through the evaluation and publication process, confidentiality requirements are imposed on those who access and process the claim. (For example, referees are typically told to destroy the grant proposal or the manuscript they have reviewed and never to use any of the knowledge it presented.) As with the cryptographic two-step schemes discussed above, confidentiality-based methods aim at ensuring that a scientist's priority is not compromised during the very publication process that is supposed to establish it. However, historical evidence suggests that, from the author's point of view, confidentiality may not be as secure as a cipher. Also, confidentiality-based protocols of registration are more effective in limiting the scientists' ability to retrospectively modify their priority claims. Because modern priority registration and review, like that provided by journals, requires the claim to be disclosed in full (sometimes even with the supporting data), it effectively eliminates the possibility of 'rewriting history' or 'keeping it sealed' that was instead enabled by the play inherent in cryptographic bit-commitment protocols and by the discretion the author could exercise in unsealing his note. If cipher-based systems tended to favour the author, confidentiality-based systems seem to have a built-in bias in favour of the institution in charge of registration and review.

The history of the move from ciphers to confidentiality-based protocols still needs to be fleshed out. The shift, however, does not seem to have been driven by performance concerns about older registration protocols as much as by a change in the 'form of life' of the scientific field and by the new role that priority assumed within that new professional economy. The modern practice of registering priority by sending a full claim inscribed in a non-encrypted manuscript to a journal conveys more than trust in the confidentiality protocols with which the journal is to process that text. It indicates that (unlike Galileo, Hooke, Newton or Huygens) modern scientists expect (and need) more than a simple time stamp from the people or institutions that register their priority.

Ciphers helped to establish scientists' priority, but did not reward them for it. Confidentiality-based peer-review systems, instead, do other things with (and to) the scientists' claim in addition to registering their time of arrival. Peer review attaches value to the claims while the editorial interventions 'socialize' them by making them conform to disciplinary conventions. (Similarly, the review of a patent application – if successful – grants the inventor intellectual property rights, not just a diploma of priority.)

53 There was a similar trend in patent law where the right to patent was eventually attributed to the person to first file a patent application, but who may or may not have been the first to come up with the invention. The US was the only important exception to the 'first-to-file' rule, but that changed in 2011 with the reform of the patent code that brought the US into alignment with all other countries.

Inventions and scientific claims are not simply time-stamped but become intellectual property or works through which scientists may obtain jobs and grants. Unlike their cipher-loving early modern ancestors who protected their claims as carefully as they could, modern scientists may be willing to give colleagues (who may turn out to be potential competitors) full access to their hopefully soon-to-be-published work simply because they must do that in order to participate in the game called science. If submitting a manuscript for publication may be seen as a 'gift', it surely is a compulsory one.[54]

This shift cannot be pinned to a moment or place, but is nicely epitomized in the mid-nineteenth-century debate over the discovery of Neptune recently analysed by Alex Csiszar.[55] What matters for us about this dispute is not whether the discoverer of Neptune was the British John Couch Adams (as unsuccessfully argued by David Brewster) or the French Urbain Leverrier (as successfully championed by François Arago), but rather the radically different notions of priority mobilized in support of the two contenders.[56] The prediction of Neptune's existence seems to be a case of independent discovery, as neither Adams nor Leverrier accused the other of plagiarism. Adams came up with an approximation of the orbit of Neptune before Leverrier did – a prediction he communicated in person and in letters to a number of colleagues in Cambridge and elsewhere in England. Leverrier started later than Adams but was able to produce a more accurate prediction of Neptune's orbit, which greatly facilitated its corroboration by the Berlin Observatory in September 1846.

The empirical corroboration of Neptune's existence gave Leverrier great international visibility, but one could still argue that Adams deserved priority for the prediction of Neptune's existence, no matter whose prediction the German astronomers had followed to detect the new planet.[57] That was not, however, how things played out. Leverrier emerged the winner from a dispute that did not hinge so much on who discovered what or when, but rather on how 'publication' was to be defined; that is, on the steps one needed to take to be certified a discoverer. Adams had communicated his predictions quite broadly in a fashion that would have easily amounted to publication by traditional early modern standards. Leverrier, too, had communicated his predictions verbally to the Académie des sciences in Paris but, unlike Adams, he also printed them in August 1846. That was the difference that made a difference.

In championing Leverrier's claim, Arago stated that priority could only be granted to claims published through print – a position that had less to do with the technical features of the medium than with the quid pro quo enabled by a specific use of the medium. Printing an article in an academic venue establishes priority not so much by providing a

54 Warren O. Hagstrom, 'Gift giving as an organizing principle in science', in Barry Barnes and David Edge (eds.), *Science in Context: Readings in the Sociology of Science*, Cambridge, MA: MIT Press, 1982, pp. 21–34.

55 Alex Csiszar, 'Broken pieces of fact: the periodical press and the search for scientific order in nineteenth-century France and Britain', PhD dissertation, Harvard University, 2010, AAT 3435324, Chapter 4, 'The literature search and the machinery of scientific periodicals'.

56 Nick Kollestrom's 'Neptune's discovery: the British case for co-prediction', at www.dioi.org/kn/neptune/index.htm, is an excellent resource for both the documents and the historiography of the dispute.

57 If one construed 'discovery' as the act of seeing Neptune, then Galle was the discoverer, not Leverrier or Adams.

reliable machine-made time stamp, but rather by producing priority credit for the discoverer as a reward for making a relevant finding available to many colleagues. In earlier periods, publication meant just that: to make something public to some people by using a variety of means that could (but did not need to) include print. By the mid-nineteenth century, instead, publication came to mean 'publication to the world'. This was not because printing presses and shipping got so fast as to be able to spread print all over the world, but because priority had become the result of a gift exchange: give something to the scientific community – the 'world' – and the community will recognize your priority back.

Believing that the way Adams had communicated his work did not amount to publication, Arago found his priority claims utterly irrelevant:

> The public owes nothing to him from whom they have learned nothing, and who has not rendered them any service . . . Mr Adams has no right to figure in the history of the discovery of the new planet [Neptune], neither by a detailed citation, nor even by the slightest allusion.[58]

In the new academic regime described by Arago, depositing a sealed note, an anagram or a personal verbal communication would not do because you are effectively not giving anything to your colleagues.[59] Arago's claim was well aligned with French post-Revolutionary patent law (which he most likely knew): inventors are entitled to patents, but only in exchange for making their inventions public in the patent application.[60]

58 Arago quoted in [Brewster], op. cit. (38), p. 229. Though less sanguine than Arago, Biot took the same position: 'The laurel which you [Adams] have been the first to deserve has been merited also by another, who has carried it off before you had the courage to seize it. The discovery belongs to him, who proclaimed and published it to all, while you reserved the secret to yourself.' Biot cited in [Brewster], op. cit. (38), p. 230.

59 Csiszar, op. cit. (55), p. 29.

60 *Loi Relative aux Decouvertes utiles, & aux moyens d'en assurer la proprieté à ceux qui seront reconnus en etre les Auteurs. Donnée à Paris, le 7 Janvier 1791*, Paris: Imprimerie Royale, 1791. See also Biagioli, op. cit. (30), pp. 1131, 1134–1138. Brewster too invoked patent law, claiming that, in the absence of an international agreement about how to handle priority claims in science, one should look at the law regulating inventions. [Brewster], op. cit. (38), p. 237. Contarary to Arago, who compared Adams to an inventor who kept his invention secret, Brewster stated that Adams's disclosure of his discovery met the legal definition of public invention: 'The disclosure of an invention to only one person is not held in law to be publication, but the disclosure of it to two persons has been so held, and the patent subsequently obtained was reduced . . . The principle of law, therefore, on which these decisions rest, is, that an invention or discovery, communicated to more than one person, or placed within the view or knowledge of the public, even though they have not seen or known it, is published to such an extent, that no future inventor or discoverer can claim any right of a beneficial character. It extends even further than this: the public are held to be so thoroughly in possession of it, that the very original inventor or discoverer cannot afterwards take out a patent, because every patent right is granted as a compensation for a secret not in previous possession of the community. Now, in the case of Mr Adams, his discovery was known to various persons in Cambridge, and was freely communicated to two public functionaries, for the very purpose of giving to the public the benefit of his discovery'. [Brewster], op. cit. (38), p. 238. However, after establishing to his satisfaction that Adams's disclosure amounted to publication according to patent-law standards, Brewster proceed to say that, in effect, it did not matter anyway: 'but supposing that Mr Adams had communicated his discovery as a secret to Mr Challis and the Astronomer-Royal only . . . his claim to be the theoretical discoverer of the new planet became an established truth'. [Brewster], op. cit. (38), p. 233). Or: 'Had [Adams] even kept it secret, or embalmed it, according both to French and English custom, in the folds of a secret packet . . . would still have been the same.' [Brewster], op. cit. (38), p. 217. That, however, would have turned Adams's claim into a trade secret, not a 'publication' according to patent law.

Like an inventor who does not patent his/her innovation but keeps it as a trade secret only to see it later patented by somebody else who happens to reinvent it independently, a scientist who does not print her claim should not get upset if somebody else is later declared, and rewarded as, the discoverer when she publishes that same claim. Furthermore, just two years prior to the Adams–Leverrier dispute, on 5 July 1844, French patent law was amended to rule out the possibility of claiming priority of invention based on sealed notes deposited with a learned society.[61] In the worlds of both scientists and inventors, priority was about carrying out a contract involving public disclosure.

Brewster's position could not have been more different. To him, priority was 'a feature in space – an event in time', which could not be erased by any subsequent claim or action by anyone: 'What is done is done.'[62] If Arago was the voice of institutionalized science, Brewster defended Adams's priority over Leverrier's by invoking older 'individualistic' protocols of priority registration, citing Galileo's and Huygens's ciphers among his examples: 'In those palmy days of mathematical discovery, the doctrine of fixing dates by publication to the world was absolutely unknown, and would have been universally rejected.'[63] Although Adams had not printed his discovery, Brewster claimed he had made it public anyway through personal communication to key scientists. But, in any case, that should not have mattered:

> Had [Adams] even kept it secret, or embalmed it, according both to French and English custom, in the folds of a secret packet, intrusted to the private keeping of a credible witness, or deposited it in the archives of an academic body, his merit as the first discoverer . . . would still have been the same.[64]

A scientist has priority the moment s/he discovers something new. Brewster recognizes that the fact of the discovery needs to be registered through social protocols, but that does not imply acceptance of the whole 'social contract' promoted by Arago.[65] To Brewster, Arago and others are aggressively promoting 'new methods of publication' and 'a modern law' of priority registration that were advantageous to large and powerful institutions like the Parisian Académie – institutions that would implement and control such practices through their stranglehold on the venues of academic publications.[66]

What worried Brewster the most was not just the ethics or politics of such a power grab, but its technical consequences. Two concerns stood out. One was that, if priority became tied only to publication in an academic journal, practitioners in remote locales would be greatly disadvantaged by lack of access and connections. Furthermore,

Brewster's double standards about what publication means in patent law and science may indicate how much he is still operating within early modern concepts of priority.

61 Berthon, op. cit. (47), p. 72.

62 [Brewster], op. cit. (38), p. 233.

63 [Brewster], op. cit. (38), pp. 239–241, 242.

64 [Brewster], op. cit. (38), p. 217. On Adams's publication see pp. 232–233.

65 The method is up to the discoverer because s/he is not seeking rewards for it from an institution.

66 [Brewster], op. cit. (38), pp. 235, 237. In particular Brewster argues that the 'new law' is being imposed by Arago (and the Paris Académie) without any legislative process or ratification by any other academic authority.

academies could deny scientists priority by simply refusing to publish their papers.[67] We could say, articulating Brewster's point to an extent he did not pursue, that the new system folds the registration of *priority* together with something utterly different: a judgement about the *quality* of the claim. Priority ceases to be a chronological fact to become a judgement about the claim itself, not just its timing.

Brewster's second concern had to do with what he perceived as a push toward too-early publication. He liked the practice of filing sealed notes – 'the most efficacious of all methods' – because it allowed scientists to register a priority claim without disclosing the preliminary result, and then keep working at articulating it until it was ready to be published.[68] A more partial or gradual publication ultimately produced better, more 'mature' claims. This was the opposite of what Arago advocated: scientists should print a claim only once and for all, without relying on other forms of pre-registration or local publication. (A first-to-print definition of priority would in fact render illegitimate all non-printed, non-open forms of registration.) According to Brewster, that was a recipe for disaster because, forced to print too early in order to claim priority in the only way they were now allowed to, scientists might not only publish prematurely, but could also end up losing the very priority they were publishing to secure:

> If priority of publication is to carry off the laurel from priority of invention or discovery, the philosopher must rush upon the world with his first conceptions – frequently the germs of great discoveries; and if the secret thus thrown to the wind does light upon good soil, the harvest will pass into an alien granary, should the seed have escaped from the grubs of science, or the parasitic monads that pick the brains of philosophers.[69]

The 'new law' of publication could make it possible for well-funded scientists (that is, the members of powerful state academies) to pick up the embryonic findings that another priority-anxious scientist felt compelled to 'publish to the world', and then turn them into a bigger discovery which could eclipse the original one – a bigger discovery that could have belonged to the first scientist had she not felt compelled to publish too early.[70] Brewster's worry was in line with the scientists' concerns with appropriation by peer reviewers or patent examiners mentioned earlier.[71] From that point of view, Arago's approach to the problem of priority registration may look less a new solution than a replay of older problems.

Conclusion

Adams lost and Leverrier won, but that does not mean that Brewster's views on the relationship between priority and publication were any worse (or better) than Arago's. Both of them (as well as the proponents of previous techniques) articulated in different

67 [Brewster], op. cit. (38), p. 236.

68 [Brewster], op. cit. (38), p. 243.

69 [Brewster], op. cit. (38), p. 235.

70 Because of their location and status, more provincial or amateur practitioners were likely to have their work rejected by those who controlled the major journals – people 'whose pleasure and duty it is to verify and pursue' their claims – thus rendering priority registration more difficult. [Brewster], op. cit. (38), p. 236.

71 [Brewster], op. cit. (38), p. 236.

ways the transition from 'secrecy' to publication, but did not (and could not) achieve a perfect solution for the two-briefcase problem. They simply presented different rules for playing the game we call 'priority registration'.

The rules of the game will probably change again, but not as a direct effect of electronic publication itself. Current electronic journals may not use print for their output, but tend to function like traditional periodicals in all other aspects, including the link between priority registration and peer review. (If an essay is rejected, its priority remains undocumented.) It could be, however, that the way digital communication is increasing the diffusion and use of work in progress and preprints (like arXiv.org in physics or ssrn.org in the social sciences) may erode the role of peer-reviewed journals as the canonical venue for academic science. If we were to accept that 'to print' was equivalent to uploading one's work (reviewed for format but not for content) in open institutional library-like depositories, then the meaning of 'priority' would surely become something quite different from what both Arago and Brewster wanted it to be.