Adrian Johns

INTELLECTUAL PROPERTY AND THE NATURE OF SCIENCE

The debate about the patenting of research is perhaps the most passionate now taking place about science and scientific culture. It is widely maintained that the expansion of patenting since about 1980 betrays a scientific tradition to which norms of universalism and common ownership of knowledge were central. This paper goes back to mid-twentieth century debates about science and intellectual property (IP) to argue that many of the norms we take as so central to science were themselves first articulated to critique patenting practices. In particular, it looks at how an economist (Arnold Plant), a scientist/philosopher (Michael Polanyi), and an information theorist (Norbert Wiener) responded to such practices. It especially focuses on the role of intellectual-property concerns in the making of Polanyi's philosophy of science, which it excavates through a reading of his unpublished papers. This reveals that the modern field of 'science studies' is indebted for some of its key concepts to an earlier generation of patent wars – an inheritance that exemplifies some of the strange ways in which the sociopolitical meanings of ideas can change from generation to generation. The point is not that present-day critics of scientific patenting are wrong, but that the very terms of the debate are more deeply-seated in the development of scientific culture than any of us has realized.

Keywords intellectual property; patenting; science studies; Michael Polanyi; Arnold Plant; Norbert Wiener

One of the more prominent convictions about science today is that, just as it has become global in scope, so it has obliterated long-honored boundaries between public and private, between interest and disinterest, and between academy and industry. In the United States, especially, not only does science now seem to vault over the gothic walls of academia with greater ease than ever before, such that privately funded research becomes more and more integrated with that conducted in the great public universities and laboratories, but this state of affairs is also widely reckoned to be distinctively new and morally consequential. What scientific research *is* is often taken to have changed radically. And intellectual property is central to this reshaping. For defenders of the trend, patents 'incentivize' inventive originality – and the increase in the number filed since about 1980 apparently testifies to their

Cultural Studies Vol. 20, Nos 2–3 March/May 2006, pp. 145–164
ISSN 0950-2386 print/ISSN 1466-4348 online
http://www.tandf.co.uk/journals DOI: 10.1080/09502380500495643

success in doing so. They thus seem to be both the measure and the motor of this change in the nature of science. For the antagonists, a 'rush' to patent epitomizes everything they see as corrupt and corrupting about the new nature of science.

Good evidence for the extent of this cultural change certainly exists. Daniel Kevles (2001) points out that guidelines for scientists at Harvard, for example, did a 180-degree flip in 1975. Where once it was an article of faith that Harvard scientists should *not* seek patents in their work, now it was expected that they *should*. What was once outlawed as breaching a fundamental norm of the scientific enterprise was henceforth recommended as a central element of good research practice. The emergent consensus is that much the same *volte face* has occurred broadly across the sciences. Thus Arti Rai and Rebecca Eisenberg (2003) argue that modern commercialized biomedicine departs historically from 'a very different tradition of open science'. In this tradition, they maintain, 'longstanding norms call[ed] for relatively unfettered access to fundamental knowledge developed by prior researchers'. Their contention is that this tradition has virtually died out, because proprietary conventions have extended 'upstream' from the commercial world into that of research proper. There is now virtually no headwater left uncontaminated. And this is by no means an unusual analysis: examples worded similarly could be multiplied *ad libitum* from the legal, policy, or science-studies literature.

What has died, it would not be unreasonable to infer, is something fundamental. After all, the 'norms' that are so often appealed to as characterizing the prior tradition of science are, virtually term for term, those articulated by Robert K. Merton in pioneering the sociology of science in the mid-twentieth century (Merton 1942): universalism, disinterestedness, organized skepticism, and 'communism'. All four are implicated, but it is the fourth that is most centrally so. By communism Merton meant the common achievement and ownership of research results, and of the knowledge to which they gave rise. With this norm in operation, the incentive to succeed in science was mainly one of honor, not economic reward, and priority disputes occurred in pursuit of reputation, not royalties. Progress itself, he implied, depended on the repudiation of trade secrecy that this norm enshrined in the scientific community. 'The communism of science', Merton therefore warned, 'is incompatible with the definition of technology as "private property" in a capitalistic economy' – and with contemporary uses of patents in particular (Merton 1942, p. 275). If this cluster of norms was previously definitive of science, then it almost seems as though the recent enthronement of intellectual property has led to something that certain 1990s scientists loudly predicted to be imminent in a rather different sense, namely the 'end of science' (e.g. Horgan 1996). We would be entitled to conclude that the end of science actually happened – it's just that everyone was too busy chasing the money to notice.

It almost goes without saying that this account fits very neatly with widely distributed understandings of modern economic currents. The increased use, scope, geographical reach, and value of patents are perhaps the most evident of all proxies for the process of globalization. When critics question that process, what they focus on, alongside environmental degradation, immiseration, and cultural homogenization, is the 'enclosure' of intellectual property behind such devices (Boyle 1996 is the canonical instance of this). In this sense, what is happening to science is paralleled by what is happening to other creative practices. But there is a difference. In the broader economy, globalization is often represented as replacing localism of various forms. In the sciences, something different is at stake. The new, market-oriented form of internationalism is seen as replacing an older – and, many think, nobler – form of internationalism. That older form was the ideal of 'pure science': an objective, ideologically neutral endeavor, yielding knowledge independent of the place of its creation (Livingstone 2003, pp. 1–16 gives an analysis of this ideal). It is this difference that lends critiques of patent-oriented scientific culture their peculiar bite. It turns their complaint from one of deterioration into one of betrayal. Critics tend to urge that we are subverting the ideals of science itself, by discarding its old, Mertonian aspiration to disinterest. The hallowed ethos of openness, communitarianism, and skepticism is said to have gone. What we are left with is an empty, commercialized simulacrum of a once-noble enterprise.

These kinds of assumptions structure what is the most angry, even violent, debate to swirl around the sciences today (the only competition would be disputes over stem-cell research or evolution, but these are largely peculiar to the USA). The central assertion is that the intellectual 'commons' are being 'enclosed' and science itself corrupted. What I want to argue here is not that that line of attack is necessarily wrong – in some respects, at least, I think that it is not wrong at all – but that its premises are poorly understood. In particular, the image of proper science that it appeals to is itself a relic of earlier debates about the patenting of research knowledge and the enclosure of prior 'commons'. That is: it is not just that pure science never existed, but that the idea that it *could* exist is itself one we owe to debates about patenting and intellectual property. And this has consequences for how we understand and respond to today's furor.

I want to make this point by restoring to view arguments carried on in the mid-twentieth century about patenting and the nature of research. Those arguments, I think, formed part of the context for Merton's immensely influential discussions; but they are far less well-known today, and when recovered they place our own debates about science in an unfamiliar light. They concerned how the intellectual property system related to the nature of scientific creativity – and to creativity in general. Contributors ranged widely, and included the popular press and broadcasters. But I shall indicate the range

of views they held by selecting one representative from economics, one from science and the philosophy of science, and one from information theory. These were among the most visible protagonists of a debate about cultural property in the 1920s–50s that was in truth every bit as vehement, impassioned, and fundamental as our own. Their shared foci give a good impression of what, ultimately, seemed to be at stake for science in the emergence of an information economy policed by rules of intellectual property.

*

We tend to forget now that the legal conventions of intellectual property – and, indeed, intellectual property itself as a cultural category – are not that old, and have never been fully secure. In fact, the term 'intellectual property' came into currency at much the same time as the specialized creators of such property were first dubbed 'scientists' (Ross 1962, Bowler and Morus 2005). In this period – from the mid-1820s to the end of the nineteenth century – both science and intellectual property rose to occupy central places in modern industrial culture. The scientist – a term originally coined half-jokingly in the 1830s, and not universally adopted for decades – stood, many thought, for a technically expert figure devoted to specialized research, the quality of which was properly measured by new *discoveries* (Schaffer 1986, Cahan 2003, p. 4). By century's end, the physicist in his expensive, complex, and generally either state- or industry-sponsored laboratory was the prime instance of such a figure (Morus 2005, pp. 226–260). Perhaps unsurprisingly, the period saw a series of fierce public debates about the nature, extent, form, and impact of intellectual property with respect to this burgeoning world of science. These debates took place across Europe, and in some countries (notably the Netherlands) led to the abolition of patents altogether, on the grounds that they were monopolies and thus contrary to the public good (MacFie 1869, MacFie 1879–83, Machlup and Penrose 1950). But they were fiercest in the greatest industrial power of the time, Britain (Coulter 1990). There, the proponents and antagonists of intellectual property (both patents and copyrights) included many of the nation's most prominent scientists and engineers: John Stuart Mill, Herbert Spencer, David Brewster, Lyon Playfair, Isambard Kingdom Brunel, William Robert Grove, Huxley, Tyndall, Bessemer, and Siemens. Today's loud confrontations notwithstanding, it is this nineteenth century assault that remains the most powerful, broad-based, and influential ever mounted against intellectual property. It centered on whether scientific research were truly attributable to individual authors, and on whether, even if it were, giving those authors monopolistic rights could be justified. Many scientists and engineers (to say nothing of the wider public) thought not. Had the British campaign won – and it came within striking distance of doing so – then the world's most powerful nation, and its supreme

advocate of free trade, would have led the way into a future of science without property rules.

These exchanges mapped out for the first time what remains to this day our range of options relating to intellectual property. Those options extended all the way from the outright abolition of patents and copyrights to their confirmation as absolute 'rights'. The abolitionist case reflected laissez-faire strictures against 'monopolies' of all kinds; the absolutist embraced convictions about the intrinsic justice of rewarding labor, originality, and creative genius – and insisted on a sometimes utilitarian imperative to support creators for the sake of the common good. Between the two extremes stood many plans for mitigated property regimes. Some proposed compulsory license schemes, for example, which would permit others to make and distribute a creation, but compel them to pay a predetermined royalty when they did so. Others suggested some kind of state-appointed panel of experts to reward inventors for their creations. Every side laid claim to support from the nature of science, but represented that nature differently. Was the scientist a positivist revealer of God-given facts, a heroic discoverer possessed of unique genius, or an everyman laboring for meager rewards? Was he a loner at all – or was science properly a collective practice? At a time when the identity of the scientist itself remained ill-defined, these were not settled questions. And the clash over them invoked the most contested political conceptions of the age, including free trade, colonialism, and political reform.

These debates revived in the 1930s, spurred by the return of liberal economics. In the face of the Keynesian intervention championed by New Dealers in the US and both Tory and Labour administrations in Britain, a so-called 'neoliberal' movement grew up to resurrect laissez-faire doctrines. A major (yet now partly forgotten) part of this 'neoliberal' movement concerned matters of cultural policy – broadcasting regulation and cultural monopolies in particular. A prominent instance was the BBC, a *bête noire* of the neoliberals on which their fire was to be renewed after the war in perhaps the only case where, by virtue of the licensing of commercial television in the UK, their arguments seemed to have real and relatively immediate effects (Coase 1950, Plant 1951). Intellectual property proved a pivotal part of this campaign throughout. If monopolies were always bad, as neoliberals assumed they were, then on what basis could *information* monopolies be defended? Copyrights and patents were artificial (indeed, state-created) monopolies of this kind; so *should* they indeed be defended?

In Britain, a prime mover of this argument was Arnold Plant (1898–1978), an engineer-turned-economist based at the London School of Economics. Plant (1934a–b) reconstructed a forgotten tradition arguing that intellectual property suffered from all the flaws of monopolies: it elevated prices, provided an indiscriminate encouragement for unjustifiable undertakings, and was in many cases unnecessary (the frequent republication of

classics in many different formats providing abundant proof of this). Perhaps, Plant mused, copyright and patents should be abolished. After all, in the Renaissance printers had produced plenty of books despite the absence of copyright. A culture of reprinting – of 'piracy' – had come into existence then that he likened to that of knock-offs in the world of high fashion in the modern era. Clearly, Milan's fashion houses did not fold simply because high-street chains imitated their designs, and high-street chains in turn did not fold when cheap merchants imitated their imitations. What developed was a quality hierarchy with largely discrete markets. In the nineteenth century, Plant noted, something similar had obtained in transatlantic publishing, with many British authors receiving substantial incomes from American publishers despite the absence of international copyright. The need to be first in a market, coupled with the 'tacit understanding' among publishers that the books published by one 'should not be pirated by another', were sufficient bases for such transactions. Cheap publishers were relatively free to issue penny-dreadful versions with impunity. But if a respectable American rival violated this 'understanding' by publishing an edition for the same market, Plant remarked, then the big publishers would produce 'fighting editions' to undercut them, much as London bus companies in the 1930s used 'fighting buses' to combat pirate operators. (This aside, incidentally, referred to a real practice, part of a long-running, fascinating, but now utterly forgotten episode in metropolitan history; the pirate bus operators, like early pirate radio broadcasters, were often demobbed servicemen.) It was a conventional system resting on civility, not a rule-based one resting on statute law.

Plant's focus was not primarily on the sciences. But he did claim to find in scientific research a modern version of that Renaissance marketplace. Invention could clearly proceed apace without patents: in biology, for instance, plant breeding continued (an example that today seems very ironic); so did medical research (ditto). Besides, patents, where they were granted, created an artificial and false authorship; in reality, discoveries and inventions were far more collective products. Finally, patents created the real risk of blocking future discoveries that might transgress too many property rights. In all, Plant thought it perfectly possible to have a vibrant world of intellectual communication that would be governed by tacit norms, rather than by legally sanctioned monopolies. And for many authors – scientists in particular – he thought this world would actually be *preferable*. Such writers simply wanted their creations dispersed as widely as possible, and intellectual property actively hindered that dispersal.

Abolition being an unrealistic proposal, however, Plant returned to the nineteenth century debates and proposed an alternative called compulsory licensing, that had first been proposed back in 1837. The notion had often been condemned, most vehemently by Herbert Spencer (for whom, Plant rather acidly observed, students counted as 'fair game for monopolistic

authors'). It gained fresh notoriety when a self-proclaimed 'King of the Pirates', one Frederick Willetts, defended it publicly in testimony before parliament. Willetts had virtually brought down Britain's music publishing trade, and gave a bravura performance defending public access to music on a model of discrete markets for different qualities of republication (analogous to trains with first-, second- and third-class carriages). Willetts's bravado put him in jail, but in the end helped lead to compulsory licensing actually being passed into law (Johns 2002, Coover 1985). (Interestingly enough, in 2004 the media conglomerate Bertelsmann announced that it would adopt a very similar market-segmentation strategy in an attempt to counter CD piracy: Harding 2004). After 1911, copyrights in Britain became subject to a compulsory royalty for the last 25 years of their terms. Plant approved: here was a system that avoided the perils of authorial monopoly to the greatest possible extent. He proposed that the idea be generalized. The period of monopoly in patents and copyrights alike should be reduced drastically, to about five years, and the compulsory royalty principle correspondingly extended.

This preferred policy was not to come about – largely because the international standards developed in the Berne process never adopted such principles. (This was arguably an early instance of international harmonization acting not just to standardize intellectual property, but to define its nature.) But Plant's proposal stubbornly refused to go away. The reason for this was not that his arguments about patenting were, in themselves, new. They were not. But he was among the first to see scientific intellectual property as part of a larger – indeed, a *general* – issue. That issue was how to deal with cultural property in something that Plant forecast would soon become an information economy (Plant 1953). The fact that that forecast came true helps to explain why echoes of his argument continue to sound today – of which more later.

*

The principal advocate of these kinds of claims in the realm of science was the chemist and philosopher Michael Polanyi. Born in Budapest, Polanyi had worked in Haber's laboratory in Berlin until he was forced to leave when the Nazis came to power. He went to Manchester, where he continued his chemical researches. But he also became increasingly interested in accounting for the nature, success, and authority of science. It is this work that has proved lastingly influential – not least in providing some of the central principles of what is now called 'science studies' (Polanyi 1958; see also Scott and Moleski 2005 for general biographical details).

The mid-twentieth century saw a fierce debate about the nature of science. Its topics: (1) whether the nature of science did or should include some commitment to the public good; and (2) whether the state had a legitimate role planning the pursuit of science so as to serve that good. The argument that

it had clearly partook of the general political position that the state should guide socioeconomic developments. In Britain, where Polanyi now found himself, leading proponents of the line that research should be socially planned included J.D. Bernal, the molecular scientist, crystallographer, Marxist, and pacifist. Bernal argued for science as a socially engaged activity, and for the scientist as a figure *responsible* for what research created. Bernal's 'Cambridge Scientists' Anti War Group' promoted anti-Fascist causes throughout the 1930s, and in 1939 his own bestselling book *The Social Function of Science* tackled the apparent success of Nazi science. Bernal maintained that the natures of science and capitalism would eventually prove incompatible. What would replace them would have many of what he saw as the positive features of the early, hopeful Soviet revolution. Science would become truly harmonious with society.

As it became clear that the war would be won, the question arose with increasing urgency of what sort of society should be made by the victors. Realization dawned among opponents of planning that a 'threat' loomed of a Labour government. Figures like Plant himself, Friedrich Hayek, and Karl Popper entered the lists to counter it. The result was the appearance of Hayek's *Road to Serfdom* and Popper's *Open Society and its Enemies*, both of which were express warnings against a trend to totalitarianism that they saw in ambitions for state planning. Labour's victory nonetheless signaled the high point of planning, committed as Atlee's party was to nationalizing key industries and creating a socialized health service. After Atlee's victory, Hayek and his allies formed the Mont Pèlerin Society, a discussion group dedicated to preserving and advancing the arguments of these neoliberals in what they perceived to be a hostile world. Popper and Hayek were founder-members. So was Polanyi.

Polanyi differed radically from virtually all writers on science prior to Kuhn. Against those who maintained that scientific research proceeded by the observance of stipulatable rules, Polanyi insisted that it was far more a matter of skills. In practice, science proceeded by the inculcation of 'tacit knowledge', which is to say, by congeries of unspoken techniques, preferences, and even norms. It resembled a tradition, far more than a rational system. For that reason, he thought it unworkable *in principle* to plan research. (In that, he noted, he differed from Hayek: Hayek thought planning was tyrannical, Polanyi that it was impossible.) Research must be allowed to play out in a field of competition as open as possible, and no agency could ever hope to predict the winners. Planning led straight to Lysenkoism. Bernal and his allies, Polanyi thought, had 'surrendered' 'to a philosophy which denied the very existence of their intellectual pursuits' (Polanyi 1958/1974, p. 237). He had founded a Society for Freedom in Science with similar aims within the sciences to those of the Mont Pèlerin Society in economics and politics.

This background makes a paper that Polanyi published in 1944 all the more remarkable. 'Patent reform' appeared in the *Review of Economic Studies* that Autumn. Placed alongside his other papers, and those of his interlocutors, it seemed to mark a sharp departure. Polanyi argued that patents misrepresented creativity and corrupted research. Their distortions were great enough to outweigh the dangers of state intervention. The system could only be fixed by change so radical as to amount to its destruction – and this must be achieved by state intervention itself. For a start, Plant's argument for a compulsory licence system must be accepted. More than that, however: there would need to be some systemic alternative.

Polanyi conceded that monopolies had been widely accepted for 'pioneer' enterprises that could not yet survive commercially (see also Polanyi papers, box 29, folder 9, pp. 209–26, 1944). But he insisted that they remained problematic even for these. There was (according to his view of creativity) no rational algorithm that could predict which discoveries or inventions would succeed, and hence no objective accounting of risk. That meant that there was *no such thing* as 'commercially justified' investment in such pioneer ventures even with the patent system in place. In addition, no accounting was possible to decide whether the vast discrepancies in returns that backers experienced were equitable. And then there was the 'grave difficulty' with patents that 'the full benefit of knowledge is only reaped when its circulation is free'. Polanyi set great store by the conviction that monopolies made society worse off, not least, in this context, by restricting further research. Moreover, in recent years the harmful effects of patents had increased. They had been employed repeatedly to consolidate trade monopolies extending far beyond new inventions – glass manufacture in 1930s America being one example Polanyi cited. 'It cannot be doubted that patents are not infrequently being used today for the consolidation of the very kind of purely restrictive monopoly to which patents for inventions were originally meant to stand in definite contrast'. The system fettered industry. Even valid patents could be so lengthily challenged as to ruin their owner.

Why were patents so bad? Because, Polanyi argued, they sought 'a purpose which cannot be rationally achieved'. They tried 'to parcel up a stream of creative thought into a series of distinct claims, each of which is to constitute the basis of a separately owned monopoly. But the growth of human knowledge cannot be divided up into such sharply circumscribed phases'. In reality, 'invention and discovery' drew on 'the whole network of human knowledge'. Patents misrepresented these processes.

Yet, for all this, Polanyi acknowledged that without disclosure conventions of some kind the culture of further research would be destroyed. Craft secrecy – the likeliest alternative were there no formal system – generally led to stagnation, as evidenced by industries like brewing. So what was needed was radical reform. Polanyi proposed to replace much of the existing structure of

patent law with what he called 'a system of appropriate governmental action'. Given 'the nature of knowledge', he wrote, full publicity and free use was the optimum. *Any* proprietorship was 'both irrational and open to grave abuses'. Given this, Polanyi had a proposal. 'In order that inventions may be used freely by all, we must relieve inventors of the necessity of earning their rewards commercially and must grant them instead the right to be rewarded from the public purse'. Over a generation or so, the benefits of new inventions accrued to all, and therefore 'no great injustice would be committed against anyone if the rewards of inventors and their financiers were charged collectively to the public'. But would this not have the disadvantages of all other government intervention? Polanyi argued that it need not: the commercial function being undertaken was a narrow one, already imperfectly performed, so the target of doing better was not difficult to hit. Furthermore, he insisted that his proposal carried no implication of accepting 'the feasibility of a "planned economy"', since it did not involved government allocating resources between various productive institutions. It merely involved government streamlining a proper distribution that was already aspired to.

'This would practically amount to the abolition of patents', Polanyi announced. What would replace them was a government-directed reward to patentees in order to keep encouraging inventors and their backers. Tribunals of experts would be created, to inaugurate a *graded* system of attributions, where authorship was no longer seen as absolute and indivisible. Polanyi estimated (it is not clear on what basis) that a reward of 10–30 percent of the appraised economic benefit of a given invention for the prior year would be enough. Given the spur to innovation created by the new system, he reckoned that society could easily afford the one percent of national income this would demand in the USA.

But how to calculate these benefits? 'In order to avoid the danger of corruption and arbitrary oppression which is never far removed from the grant of Government subsidies', Polanyi insisted, the protocol of appraisal should be 'rigid'. In many cases it would be carried out solely by government agents. It would amount to a kind of perfect bureaucratic oversight of the field of industrial creativity. He forecast that this system 'would efface the last vestige of control which a patentee could exercise over his competitors and eliminate at the same time any serious interest on the part of the latter to infringe patents by an unlicensed use of the protected invention'. That is, monopoly and piracy would be eradicated at a stroke.

This was an astoundingly interventionist, technocratic proposal. It has no known counterpart throughout Polanyi's other writings. That Polanyi proposed it reflects a number of things worth remembering. First is the intensity of debate on creative properties and their public implications. Second is the connection of that debate with the debate about science and the public good. Third is Polanyi's own philosophy of originality/creativity itself, which

stressed its irreducibility to rules. For there is no indication whatsoever that Polanyi was anything but serious in making his suggestion.

But there is more to be said here, about where Polanyi's account of science came from. It is well known that in the 1930s and '40s he was active in opposing movements for the planning of science. What is less familiar is the internal struggle he waged with himself to articulate his convictions. During the war years, in fact, Polanyi struggled with a number of book drafts, the remains of which are to be found among his papers today. The successive drafts – each of which incorporated sections from its predecessors – reveal an intellectual progress in which the issue of patenting turns out to be rather pivotal.

Briefly, Polanyi's first projected book was to be called *Science*, and was directed squarely against the 'planning of science' movement. He worked on this for three years, in 1940–43. Then he abandoned it, and moved on to another text provisionally called *The Scientific Method in Society*. This too he abandoned for yet another work, titled *The autonomy of science*, which presented science in terms of a sweeping three-stages view of history extending back centuries. This then transmuted into what might seem a radically different work entitled *Economic Planning*. Finally, this in turn gave way to a volume named *Full employment in theory and practice*. And this last did appear in print, as *Full employment and free trade* (1945). Only much later did fragments of the other texts resurface in *Personal knowledge* and Polanyi's other philosophy of science works. (This reconstruction traces materials in the Polanyi papers, box 29, folder 1, pp. 5–8, and folders 11–12.)

Looking at these successive drafts, it becomes clear that throughout his extended labors Polanyi's argument about patents formed one of their few common threads. It reappeared repeatedly in each new project. Even the full-employment work ended with it, insisting that a moral form of free trade was what was needed for low unemployment to be sustained, and that this would principally involve the ending of legal protections – which meant patents in particular. Why this centrality, though, and to what consequence?

A major reason for it was that Polanyi believed that the ideology of free trade itself had originated in opposition to patent monopolies under Elizabeth I and James I. So patents had an important historical role in the foundation of what he took to be the ideology of economic liberty. But more immediately relevant was the fact that in Polanyi's time patenting was widely held to be the guarantor of internationalism and objectivity in industrial scientific research. The pooling of patents created research combines extending across industries and nations, within which relatively free communication (an intellectual marketplace, ideally) might arise. In chemistry and electrical engineering, especially, the world leaders were all patent-pooling commercial labs of this kind: Bell, GE, Phillips, Osram. It was all being hailed as a brave new kind of science – a planned enterprise, overseen by rational managers, carried out

collectively, and to public benefit. Patents defined its field of operation. They were thus the lynchpin of a system in which planned science seemed an achieved and admirable *fact*. Polanyi therefore *had* to attack the system at this point. Patents were the thin end of the wedge for planned science on a state-wide scale.

Of course, Polanyi thought that this kind of planned enterprise was in fact an impossibility. In large part, this conviction came from this own experience in scientific research. He himself had found at ICI that he had been constrained from speaking openly to others even in the same factory. The widespread perception that patent pools created ringfenced spaces for the free communication of research was, he insisted, an illusion: large-scale combines of this kind were merely monopolistic. In the sequence of his drafts we see how he worked up from this kind of personal experience to his well-known views on the nature of science. And it was the need to tackle the role of patents in apparently making a planned science possible that compelled him to address the nature of creativity.

In the big combines, he insisted, real originality in fact came from individuals (like himself at ICI) – often ones who had had their main ideas first, and were only hired into the combine *after* they had made their discoveries. This was because no rational firm could accord an inventor the leeway needed for all the failures necessary to produce a success. These large groups, he believed, thus mistook the nature of real science, which was irreducibly individual and a matter of trial-and-error. And *why* did combines betray science? For the same reason a scientific paper was truly a creative act: because 'there is no mechanical, safe rule to discovery'. '*Very Important*', he noted at a crucial point: 'Usual argument: Invention, progress, is logical, determined, therefore foreseeable. While in reality: *because* it follows the evolution of inherent logic in steps, *each of which is a maximum step of human intelligence*, therefore unforeseeable' (Polanyi papers, box 29, folder 5, unpaginated, Polanyi's emphases). In other words – and this is worth noting because of the uses to which Polanyi's philosophy was later put – it was not that the process was inherently anarchic or irrational, but that each step, while itself logical, took one to the limits of predictability at the moment of its being taken; so no human agency could hope to predict the results of several such steps taken one after another. And here one book manuscript ended abruptly. If the treatment of data could be centrally guided, he later added, 'This is not research but surveying; no originality'. Science had above all to be a matter of originality.

The *Scientific method in society* draft then took this point further. The book concentrated on the role of different kinds of *secrecy* in science. Polanyi identified two kinds, created by governments and companies respectively. Both kinds weakened knowledge, because structures of secrecy in practice preserved strata of social subordination, and therefore constricted the

marketplace of ideas. With respect to governmental secrecy, Polanyi planned to propose that secret military research be made illegal under international law. But in fact he believed that the corporate kind of secrecy was more damaging to the general welfare. This was where patents came in. They were the corporate world's equivalent to military secrecy. This was perhaps a counterintuitive case to make, for then, as now, defenders of patents would often point out that they represented a bargain of revelation for protection, and that in that sense they guarded *against* secrecy. But Polanyi's point seems to have been that this rule fell by the wayside in its application. In practice, in mid-twentieth century industrial capitalism, patents were invariably made into tools for cartel-building, and the boundaries of cartels were in the first instance social, or managerial, not legal. It is an interesting argument to consider today, incidentally, given that Peter Galison (2004) has recently found that modern military secrecy traces its own genealogy back to corporate trade secrets of the period. But that is a subject for another paper. In the meantime, Polanyi's own concern was more immediately practical, as well as more philosophical. Given that patents had this nature, how should society best deal with them?

Polanyi seems to have been aware not only of Plant's arguments (he cited him explicitly, e.g. in Polanyi papers, box 29, folder 10, #8, p. 4), but of the Victorian debates that Plant was laboring to revive. He therefore began by recommending a compulsory licensing scheme; but then he went further. The best way to bring discoveries to public use, he conceded, was to vest development decisions in inventors and their sponsors, and the readiest way to do this was to retain *some* principle of property. But Polanyi insisted that inventors should retain their autonomy as individuals. To this end, he argued that they should be rewarded from the public purse, at levels commensurate with the benefits generated by their inventions – benefits to be calculated by his governmental bureaucracy. This would entail creating an extensive social machinery of measurement, of course, but Polanyi insisted that at least it need be no more imprecise than the existing patent regime. And the open patents he advocated would be readily divisible and combinable, allowing for the further invention of ambitious new devices without the huge and tortuous negotiations seen in existing technological industries, radio being an excellent example. Not only would his suggestions break down the walls of the big research combines; they would also permit individual originality to flourish apace. To that end, Polanyi also advocated that the government should subsidize the publication of scientific books, expand libraries, and relax copyright laws. The citizenry should be free to buy cheap reprints after a short period, in the interest of scientific progress.

In recommending radical reform, Polanyi had more than the scientific researcher in his sights. He wanted to set in train a transformation in large-scale technological industry. An important role for government's investment in the new tribunals that he envisaged was thus not just to eradicate restrictive

practices, but actively to inculcate what he called new 'habits of publicity' in the commercial world. Once established, he thought, these habits would become part of the normative structure of business itself. 'It will be considered as a feature of decency and dignity in industrial life', Polanyi predicted, 'to let everyone benefit freely from knowledge which is obtained in the firms' research laboratories'. In short, what lay behind his view of science was a sustained and agonized engagement with issues of planning in both private and public sectors of the economy – and patenting was the key to them. Although all that appeared in print at the time was one isolated paper, which fell silently from the press, for Polanyi himself the topic was of critical importance. He remained adamant that scientists would find that his scheme, if adopted, would 'pervasively refresh the intellectual atmosphere in which individual scientists spend their lives'. That was what it was for: to let science in capitalist industrial society remain what he thought it was.

*

My third example, Norbert Wiener, stands apart, and I shall refer to him more briefly. Wiener's major stalking ground was the United States, and he is known today primarily as the inventor of the notion of cybernetics, the science of control in humans and machines. He is further renowned as one of the architects of 'information culture' – as one of the people who made it possible to conceive of 'information' as a discrete entity in terms of which societies could be analysed and economies developed. This makes his contribution to the debate over intellectual property of unusual interest when considered in the light of arguments like Plant's and Polanyi's.

Wiener (1894–1964) was by his own profession a prodigy, receiving a PhD from Harvard at the age of 19. He worked after that achievement in various American and European universities (Cornell, Columbia, Cambridge, Göttingen, and Copenhagen) before settling at MIT (see Conway and Siegelman 2005 for biographical details). During World War II he worked on technical problems to do with the aiming systems of anti-aircraft guns, and this experience was one that helped lead him to his argument for the importance of control and communication in general (Mindell 2002). He came to argue for the generality of this perspective, maintaining that the science of control and communication was grounded in a general need to combat the entropy law of thermodynamics. He sought the key to this science in feedback mechanisms, described in complex statistical formulae. The result was a sometimes highly technical science christened *cybernetics* in his book of that name (Wiener 1948). Its central pillar was a theory of communication devoted to distinguishing 'signal' from 'noise' in networks, which would later become central to all modern telecommunications. After that, Wiener devoted himself largely to diversifying the influence of this notion that made 'the theory of

messages' central to accounts of living and nonliving nature alike. His work, along with that of interlocutors Claude Shannon and Warren Weaver and others in the 'cybernetics group', is commonly identified as foundational to the very possibility of an information age (Heims 1991).

Yet Wiener did not, in the end, pursue this work as far as he wanted to. A principal reason for this was that he became convinced that the true scope and impact of information were being obstructed by protocols of intellectual ownership. In part this rested on personal experience: in the 1930s he and Yuk-Wing Lee (1904–89) patented a network design, and sold the patent to Bell, only to see the company fail to develop the invention and also prevent others from using it for the patent's 17-year term. He also lodged a speculative proposal for a digital computer with the war authorities in the 1940s, only to see it too neglected. So he had fallen afoul of both of Polanyi's types of secrecy. But it was also a perspective of principle, that intellectual property conventions impeded the development and flow of information in the great network that was modern society. In other words, intellectual property was a massive ally of entropy and a massive enemy of information in Wiener's technical sense of the term.

For years Wiener sought to publicize this conviction. One result was a book entitled *Invention: the care and feeding of ideas* (Wiener 1993). It was commissioned as a study of the philosophy of invention. Written largely in 1954, the work in fact deals with history and sociology more than philosophy as such. It uses the history of inventing to mount an argument for the relative importance of individuals and 'environment' in creativity. Wiener's case is that innovation comes from the ease of interaction between scholars and craftsmen, and that this is conditioned by society, so that prior to the Renaissance (for example) Asian cultures were more successful than European because they facilitated deeper exchanges of this kind. Yet he also maintained – and in the end this was the point that he made more forcefully – that it had to be individuals who capitalized on this environment. His volume became an extended defense of the lone, Romantic inventor against three modern enemies: the McCarthyite enforcement of political orthodoxy; the increasing corporatization of invention since Edison's invention of the industrial research laboratory; and above all the iniquities of intellectual property law. They culminated together in the unholy phenomenon of 'megabuck science'.

Megabuck science, Wiener felt, transcended political ideology and geographical location. It was the activity of research carried out in the manner of a factory, *planned* (and here he resembled Polanyi) by managers and restricted in its scope to particular prescribed tasks. The problem with it was that it created channels of communication that either kept possibilities secret or – which was almost as bad – subdivided them into discrete problems that would be addressed by separate groups of researchers. In other words, big science killed invention because it was not in accord with Wiener's own

science of information. Information was more process than substance, he insisted. It flowed through a network rather than accumulating in some kind of static reservoir. To assume otherwise was the basic fallacy involved in both the state's creation of major research laboratories to hoard secret knowledge and the corporation's parallel development of laboratories to pursue planned research resulting in patents. The patent system was thus a kind of 'jamming' in the channels of communication essential to the welfare of society (Wiener 1954, pp. 128, 131).

Practically, the system had created a forest of competing claims. While paying lip service to the individual inventor, it had reduced him to a figurehead for corporate interests capable of paying for the legal expertise to negotiate that forest. And, perhaps above all, it subjected judgments about the creative process to judges unacquainted with what that process truly was like.

Invention never in fact appeared in Wiener's own lifetime, and was only rediscovered and published relatively recently. That Wiener laid it aside is, paradoxically, an indication of the importance he attached to this argument about intellectual property and the corruption of science. He abandoned it in order to write, of all things, a novel about patenting. The novel *was* published, and is called *The Tempter* (Wiener 1959). It is a fictionalized account of the travails of a cussed English electrical engineer named Oliver Heaviside, a contemporary of James Clerk Maxwell (and in fact the inventor of what we now think of as Maxwell's equations). Heaviside in Wiener's view represented the truly noble inventor whose ideas were appropriated by the soulless denizens of industrial research. Wiener told his editor that 'the story is really a treatment in fictional form of my ideas on invention in the modern world' (Wiener 1993, p. xiii).

Heaviside's story was one that had captured Wiener's attention much earlier (others have also succumbed, among them Nahin in his 1988 biography of Heaviside). In 1941 he had even tried to persuade Orson Welles to make a film based on it (Wiener 1993, p. xiii). That never came about, but in *Invention* he devoted several sections to his own outraged telling of the tale. According to Wiener, what happened was this. Heaviside was one of a number of mathematical engineers at the close of the nineteenth century working on a problem of signal distortion in long telegraphy cables that was crucial to their use in major communication systems. A loner, who, as Wiener put it, 'was born poor, lived poor, and died poor', Heaviside lived outside the world of elite academia and industry. In Wiener's eye this made him 'sincere, courageous, and incorruptible' (Wiener 1993, p. 70). He stole time on a Post Office line to test his heterodox theories of inductance, on which the distortionless lines necessary for telephony might be based. He published the results in a book that initially sold poorly, but then was 'pirated' three times (once in China) and in this form became an essential foundation to twentieth century communications science.

Heaviside himself never sought to patent his notions. The fundamental work was old enough by the time that its significance was widely acknowledged that no patent could have been lodged. So AT&T sought a way to modify the work to claim a new right. It recruited Michael Pupin, a Columbia engineer, who as an outsider to the company would have credibility. Pupin furnished a qualification to Heaviside's work, essentially making explicit elements implicit in Heaviside's original papers. He patented this, effectively gaining the right to implement Heaviside's insights. The company then paid him $500,000 for the right. It did try to pacify Heaviside, but he declined all offers that did not recognize his sole credit as author of the inventions – the one thing the company could not yield without abandoning its investment. He died in noble poverty. Pupin himself, meanwhile, issued a series of ever more self-aggrandizing accounts of his role as heroic discoverer, culminating in a popular work entitled *From immigrant to inventor*. Wiener read this book, which enjoyed a great vogue among American adolescents, not as an inspirational success story, but as a massive exercise in assuaging guilt. 'For those who read between the lines', he concluded, '*From immigrant to inventor* is not a typical American success story, but a cry from Hell'. *The Tempter* took this conceit and made it into a fiction. It was a novel that, in Wiener's words, 'joined the story of Prometheus with the story of Marlowe's Dr. Faustus' (Wiener 1993, p. 76). And the patent system provided the mechanism – the cosmos, as it were, of pride, sin, and redemption – that structured Faustus's temptation and downfall.

*

Fast-forward to today. This may not quite be hell, but nor are we entirely out of it. We find that we are legatees of these mid-century debates. We inherit the terms of their arguments, forged at the onset of the information age, when both science and intellectual property were changing quite dramatically. If we think 'information wants to be free', as Wark asserts in this issue and as others have claimed elsewhere, then that is a sentiment which (if not worded in exactly the same way) was widely endorsed by critics like Polanyi and Plant. And the definition of research that we now think is being lost is also one articulated in those years, by Merton in his studies of the normative structure of science and the role of intellectual priority in it (Merton 1942, p. 275, Merton 1957). Those studies have typically been seen as motivated primarily by the need to counter totalitarian claims, as indeed they were. But, as Merton himself acknowledged, they also appeared within the context of a deeply felt debate about the relation between science and intellectual property that is now all but forgotten.

Is there, then, a route by which the arguments of mid-century have descended to inform those of today? If so, it is not an evident or

straightforward one. The sheer political disjuncture involved is remarkable enough: from the libertarian right in the 1940s to the skeptical left today. But there are some strange conjunctions and disjunctions that result from such a descent. Not least, the communities that set greatest store today on being the lineal descendents of Plant and his allies – namely, the globalization protagonists at the WTO and WIPO – are staunch upholders of strong intellectual property. They remain strangely silent on this aspect of their ancestry. On the other hand, Polanyi's formulation of science in terms of tacit knowledge was picked up by sociologists in the 1980s and made central to their own arguments for creating a new sociology of scientific knowledge (Collins 1985 is the *locus classicus*). Here the opposite situation obtains. The emergent discipline of 'science studies' would not be what it is without this appropriation, yet nowadays the field is replete with shocked claims that the patent system misrepresents science. Since Polanyi honed his own account with a view to attacking a patents regime, we should perhaps not be so surprised to find that a sociology of science that emphasizes craft, tradition, and tacit knowledge is just as inhospitable to the modern successor of that regime.

If there is a moral, it is perhaps this. When we talk about intellectual property, we are necessarily historians for the duration. We have to be, because the very concept involves notions of origination and authorship that demand excavation of the past and narrative explanation of its relation to the present. This is as true of scientists determining priority disputes as it is of lawyers contending for royalties for their clients, not to mention academics trying to understand what either group thinks it is doing. But the history that is typically done is rather cramped, not to say essentialist: it involves the identification of chains of precedent, or the articulation of norms or rules that can then be taken to have been instantiated or violated in a given case. What we learn from looking at these older debates is that history itself is more penetrating than that. It has conditioned *how* we think of intellectual property and science, as well as *what* we think of them. To see this, we need to look both more broadly at the extent to which the current culture of intellectual property is a historical product, and more narrowly at how it was in fact built, fought over, established, and maintained. Whenever we look at intellectual property, then, we are always perforce historians; but we might be well advised to reconsider what kind of historians we really ought to be.

References

Bowler, P. J. & Morus, I. R. (2005) *Making Modern Science: A Historical Survey*, University of Chicago Press, Chicago.

Boyle, J. (1996) *Shamans, software, and spleens: law and the construction of the information society*, Harvard University Press, Cambridge, MA.

Cahan, D. (ed.) (2003) *From natural philosophy to the sciences: writing the history of nineteenth-century science*, University of Chicago Press, Chicago.

Coase, R. H. (1950) *British broadcasting: a study in monopoly*, Longmans, Green, London, New York.

Collins, H. (1985) *Changing order: replication and induction in scientific life*, Sage, London.

Conway, F. & Seigelman, J. (2005) *Dark hero of the information age: in search of Norbert Wiener, the father of cybernetics*, Basic Books, New York.

Coover, J. (1985) *Music publishing, copyright and piracy in Victorian England*, Mansell, London and New York.

Coulter, M. (1990) *Property in ideas: the patent question in mid-Victorian Britain*, Thomas Jefferson University Press, Kirksville, Mo.

Galison, P. (2004) 'Removing knowledge', *Critical Inquiry*, vol. 31, no. 1, pp. 229–243.

Harding, L. (2004) 'You've heard of the flights, but would you buy a no-frills CD?' *The Guardian*, 5 July, p. 3.

Heims, S. (1991) *The cybernetics group*, MIT Press, Cambridge, MA.

Horgan, J. (1996) *The end of science: facing the limits of knowledge in the twilight of the scientific age*, Addison-Wesley, Reading, MA.

Johns, A. (2002) 'Pop music pirate hunters', *Daedalus*, vol. 131, no. 2, pp. 67–77.

Kevles, D. (2001) 'Principles, Property Rights, and Profits: Historical Reflections on University/Industry Tensions', *Accountability in Research*, vol. 8, no. 4, pp. 293–307.

Livingstone, D. N. (2003) *Putting science in its place: Geographies of scientific knowledge*, University of Chicago Press, Chicago, IL.

MacFie, R. A. (1869) *Recent discussions on the abolition of patents for inventions*, Longmans, Green, Reader, and Dyer, London.

—— (1879–83) *Copyright and patents for inventions*, 2 volumes, T. & T. Clark, New York, Scribner and Welford *et al*., Edinburgh.

Machlup, F. & Penrose, E. T. (1950) 'The patent controversy in the nineteenth century', *Journal of economic history*, vol. 10, no. 1, pp. 1–29.

Merton, R. K. (1942/1973) 'The normative structure of science', reproduced in Merton, R. K., *The sociology of science: theoretical and empirical investigations*, University of Chicago Press, Chicago, IL, pp. 267–278.

—— (1957/1973) 'Priorities in scientific discovery', reproduced in Merton, R. K., *The sociology of science: theoretical and empirical investigations*, University of Chicago Press, Chicago, IL, pp. 286–324.

Mindell, D. A. (2002) *Between human and machine: feedback, control, and computing before cybernetics*, Johns Hopkins University Press, Baltimore, MD.

Morus, I. R. (2005) *When physics became king*, University of Chicago Press, Chicago, IL.

Nahin, P. J. (1988) *Oliver Heaviside: the life, work, and times of an electrical genius of the Victorian age*, Johns Hopkins University Press, Baltimore.

Plant Sir A. (1934a) 'The economic aspects of copyright in books', reproduced in Plant, Sir A. (1974), pp. 57–86.
—— (1934b) 'The economic theory concerning patents for invention', reproduced in Plant, Sir A. (1974), pp. 35–55.
—— (1951) 'Property in programmes', *BBC Quarterly*, vol. 6, no. 1, pp. 18–24.
—— (1953) 'The new commerce in ideas and intellectual property', reproduced in Plant, Sir A. (1974), pp. 87–116.
—— (1974) *Selected economic essays and addresses*, RKP for the Institute of Economic Affairs, London.
Polanyi, M. (n.d.) Unpublished papers. Special Collections Research Center, Regenstein Library, University of Chicago.
—— (1941) 'The growth of thought in society', *Economica*, new series, vol. 8, no. 32, pp. 428–56.
—— (1944) 'Patent reform', *Review of Economic Studies*, vol. 11, no. 2, pp. 61–76.
—— (1958) *Personal Knowledge: towards a post-critical philosophy*, reproduced 1974, University of Chicago Press, Chicago, IL.
—— (1962) 'The Republic of Science', *Minerva*, vol. 1, no. 1, pp. 54–73.
Rai, Arti, K. & Eisenberg, R. S. (2003) 'Bayh-Dole reform and the progress of biomedicine', *Law and contemporary problems*, vol. 66, nos 1–2, pp. 289–314.
Ross, S. (1962) *Scientist*: the story of a word', *Annals of Science*, vol. 18, pp. 65–85.
Schaffer, S. J. (1986) Scientific Discoveries and the End of Natural Philosophy', *Social Studies of Science*, vol. 16, no. 3, pp. 387–420.
Scott, W. T. & Moleski, M. X. (2005) *Michael Polanyi: scientist and philosopher*, Oxford University Press, Oxford.
Wiener, N. (1948) *Cybernetics: or, Control and communication in the animal and the machine*, Wiley, New York.
—— (1954) *The human use of human beings: Cybernetics and society*, Houghton Mifflin, Boston.
—— (1959) *The Tempter*, Random House, New York.
—— (1993) *Invention: the care and feeding of ideas*, MIT Press, Cambridge, MA.