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What's in a Patent?

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In this essay, I am concerned with the kinds of accounts given of technical objects in patents, scientific literature, and company archives and in the relationships among the differing presentations of patents in these various sources. Numerous authors have pointed to the importance of patents in industrial science. In a notable turn of phrase, David Noble asserted that "Patents petrified the process of science, and the frozen fragments of genius became weapons in the armories of science-based industry."[1] Thomas Hughes (1983) has highlighted the fact that the research laboratory at General Electric was set up on the advice of the patent lawyer;[2] Reich (1985) has shown that in the Bell Company, industrial research was encouraged only when the strategy failed of buying up patents, then defending them in court.[3] Dennis and Bowker have identified the to-and-fro between patent lawyer and industrial laboratory as a key feature of industrial science.[4] In a ground-breaking essay, Cambrosio, Keating, and Mackenzie (1988, forthcoming) discussed the parallel between sociological and legal discourse about inventions and concluded that lawyers attacking patents draw on the same repertoire of analytical tools as the externalist sociologist.[5] I intend to develop this new perspective by looking at the ways in which patents are defended both in the courtroom and in the field. I will draw on the example of one company, Schlumberger, to discuss the relationship between the official version of history written into the patent and the actual use made of the patent.

In looking at patents as texts, I will concentrate on two features common to them all: they give internalist and Whig accounts of the development of the process or apparatus that they describe, and as legal instruments they attempt to impose that interpretation on the material world.[6] Now that within the history of science the ramparts of internalism and Whiggism have transmuted from stone to straw, *we* know that any account couched in these terms is necessarily false.

Further, all actors in the situations we describe (with the possible exception of the trial judge) themselves concede (outside of the courtroom) that such an account is false. And yet an immense amount of articulation work is done in an attempt to create a situation in which it can stand up in a court of law. We will be looking at this articulation work, and in particular at the question of who knows what about the patents and how this knowledge affects the success of the companies involved. To borrow a phrase from the sociological literature, we will be looking at the various awareness contexts within which patents function and at the articulation of the relationships among these contexts.[7]

To structure the analysis, I will draw an analogy between the process of defending a patent in the courtroom and that of defending a position within the discipline of history. The analogy is a natural one: these are among the only fields in which an "authorized" version of events (an historical occurrence or a scientific/technological discovery) is produced by the discussion of documents written to fit strict formal codes. The purpose of the comparison is to throw into relief three forms of relationship between "what actually happened" and what gets written about it. These three forms are: (i) the narrative's internal content and its immediate validity; (ii) the narrative's institutional setting, and how this reflects back on its truth; and (iii) the contribution of the narrative to making itself true. To illustrate my point, I will take the debate between Richard Tawney and Hugh Trevor-Roper about the origins of the English revolution.[8]

This debate provides a clear example of a structural framework that can, I believe, be fruitfully applied to the analysis of the defense of patents. The English revolution in 1640 saw the usurpation of the king's power by Parliament, in the form of Oliver Cromwell, and was the occasion of a famous series of debates (the Putney debates) about the nature of personal and religious liberty. Tawney gave a fairly classical Marxist account of this event: for him it was spearheaded by members of the "rising middle class" who were for the first time flexing their political muscles, using the language of universal freedom to pursue their own interests (as would the French revolutionaries later). For Trevor-Roper, the revolution was led by the "declining gentry," who were frightened of the increasing power of the bourgeoisie and took power in a desperate attempt to salvage their own privileges. Already, then, there is a clear parallel with a patent battle: two diametrically opposed accounts of a historical process clash in a public forum—academic journals in the one instance and the courtroom in the other.

The structural parallel that I want to develop relates to the three levels at which the debate played itself out: in journals, in institutions, and in the world at large. The first level is the internal one of the debate itself, involving the exchange of arguments in journals according to fairly strict rules of academic behavior. Archives were quarried for details of estate management and income; genealogies were drawn up to chart membership of the middle class and the nobility.[9] The equivalent level for the technical object is most clearly the courtroom battle about patents and the attached legal research, including the induction of expert witnesses. We shall see that here too competing histories were at stake and were being defended within a fairly strict framework. Most notably, both for the debate and for the patent battle there is an explicit belief that this cloistered, rule-driven activity can decide a historical truth. In both cases, many of the actors concerned recognize explicitly, outside of the academic presentation or the courtroom, that the debate is really decided elsewhere; and yet in both cases there is a vested interest on the part of actors in protesting that the show is all. This is the central aporia of this essay.

The second level at which the historical debate was played out was the institutional one. "Schools" were formed; journals favored one side or the other; a "Trevor-Roper" candidate could not get a job in a "Tawney" department. At this level, the appropriate weapons were not the telling argument or the subtle "mot" but the manipulation of research grants and access to publications—in short, occupation of the academic terrain. It was generally important that one's own side was bearing up respectably in the internal debate, but this debate was no longer crucial. The analogy with the patents process here is also clear. The company strategy is to occupy the terrain; and patents are a part of this process, but only one part. The patent must not only win in the courtroom, but the technical object attached to it must also impose itself in the real world. Control of the infrastructure was at stake. There is a figure/ground problem here. For those in the courtroom, the truth of the historical account was all, and the work of creating the infrastructure was background. For those outside, the historical account of the patent was in turn irrelevant background noise to the *real* focus of company activity. Strategies for imposing patents outside of the courtroom involve all kinds of different uses for them and sometimes ways to work around them. We shall see how companies could skirmish at this level and maintain a discourse about patents that denied this skirmishing.

And there is a third level at which the Tawney/Trevor-Roper debate played out: that of history itself. Tawney's position was inserted in a world in which there was continual progress toward socialism, Trevor-Roper's in a world in which socialism could never be other than a chimera. Indeed, each was trying through their writing to contribute to these respective outcomes. Their debate was interventionist in the same way that patent battles are: if the story is told well enough, then there is a chance that it will become true before the ultimate tribunal—history itself. If there were another revolution in England, a classic proletarian one this time, then Tawney would come out institutionally triumphant whether or not he was winning in the journals or in academia; if socialism crumbled there, then Trevor-Roper would win the day. One position would win out, and the fact that it had won would make the historical account it gave of seventeenth-century England true—at least as far as later historians were concerned. To put it simply, the fact that Marxism appears to be dead in Britain is a problem for Tawney's position on the English revolution.

The analogy with patent here is that the patents qua official history only survive within a certain framework of the state of the art and the state of the industry. If they *survive* the institutional and court battles, they might either prove to have described how the industry was shaped or prove to have been outside of the mainstream. The historical debate and the patent battle both seek simultaneously to describe a past reality (and impose that description) and to create a present one (and impose that creation). This is another form of aporia, because the name of the game in the debate is for the right hand (the intellectual one bearing rapier) not to know what the left hand (bearing club) is doing.[10] Success in debate and sufficient control of the infrastructure while the world changes enough to make you right are both vital; but there are no public forums at which historians writing histories or companies producing technical innovations admit to attempting both simultaneously.

There are two works within the field of the history of science and technology that seek to deal with this kind of broad sweep of social change. The first is Michel Callon's highly original article on electrical cars,[11] which gives a clear instance of this type of implication of the technical object in the process of social creation (and the creation of sociological theory). The other is Shapin and Schaffer's work on the air pump, where at stake in the development of scientific protocol is the imposition of a social form wherein the science of oneself and one's opponent would become respectively true and false.[12]

We now have a focus and a structure, so I will turn to a presentation of the final player—the patents and their attached technical objects. I have described in detail elsewhere how Schlumberger's electrical logging operations worked;[13] here I will give a brief overview, which should be sufficient to our purposes. Schlumberger started out in the business of searching for deposits of metals and oil in the period after World War I. Their technology was electrical. Their canonical technique was to run a current between two point on the surface of the earth, chart the electrical field that was generated, and use a variety of mathematical tools and theoretical graphs to interpret that chart. A development of this approach furnished their second main tool: in this instance they did not generate a field but charted variations in the earth's own electrical field. These techniques met with limited success worldwide, being overshadowed quickly by the much more accurate seismic methods. Their use did serve to bring Schlumberger to the attention of the oil companies, though.

The breakthrough for Schlumberger came in 1927, when it was decided to try exactly the same electrical techniques *inside* well holes as they were being dug. Now, rather than having to tease information out of approximate data covering vast terrains, Schlumberger could deal with small electrical variations that operated over a few feet. Further, the competitor was no longer the seismic method (which at that stage was not finely enough tuned to operate in the well hole) but mechanical coring (sample taking), which was notoriously inefficient, slow, and expensive.

The method was incredibly successful in the search for oil. Schlumberger's first measurement served essentially to distinguish strata of high and low resistivity. Oil was particularly resistive, water layers were conductive. It served to *correlate* fields (charting the continuations of a known field in accord with geological data) and to indicate the water shut-off point (the lowest point to which you could drill without hitting water—clearly valuable information for maximizing a field's production). It was not enough, however, to indicate the presence of oil or gas. The problem was that granite was just as resistive as oil. The analogy of the measurement of the earth's own field made the difference, because spontaneous activity in the earth's field was high in porous layers and low in nonporous ones. The only resistive porous layers, so the story went, were oil-bearing. Of course this overview is, as we shall see, itself a fabrication. It is, however, a neat and easy one that gives some view of the official history of Schlumberger.

With everything in place, I will rephrase the question motivating this essay. I have asserted that patents necessarily proffer a Whig interpretation of history, and an internalist one. I assume that life (let alone oil) is not really like that and so ask: how is the space created such that these interpretations can, locally and temporarily, hold? Why is this space created? This question comes down to one of the central problems of this book: how and why is the boundary between "inside" and "outside" created in scientific work?

Patents and Publications

One thing that stands out from the trial is that Schlumberger tried at all costs to impose an internalist view of events, whereas its opponent, Halliburton, tried to impose an externalist account.[15] For Halliburton, Schlumberger's patents did not describe the technical object used in the field, which had been modified to meet local conditions. Further, the logs that were produced were not universal but specific to a time and a place. There was no inner truth attached to the patent; environment was all. To prove this, they demonstrated that a Schlumberger logger could not interpret a given log without knowing where it came from. Thus for Halliburton, Schlumberger's patents put a universalistic gloss on a local and limited method. Not content with denying the present status of the technical object described by the patent, Halliburton tried to insert it into a different history. They went back to the 1830s to find analogues of the method in the Cornish tin mines. They argued that there was no rupture between this method and Schlumberger's own.

To formalize the differences between the two presentations, we can consider them along two axes. The first is that of rupture/continuity. Halliburton claimed that there was a rupture *now* between their methods and Schlumberger's (because what they are measuring is totally different); but that there was none *then* (between the past and the present). Inversely, Schlumberger claimed that there was a rupture then (marked by the original act of invention) but there is none *now* (when Halliburton is infringing on their patents). The second axis is that of the internal/external division. For Halliburton external factors apply *now*, because the patents conceal the actual, local truth of Schlumberger operations. For Schlumberger external factors applied *then*, before in a stroke of pure science Conrad Schlumberger formalized and rendered scientific the electrical treatment of the subsoil. This formal grid gives us a good idea of

the framework of the discussion. As in all foundational stories of new science, the externalist explanation works for everyone before the founder.[16] The grid only makes sense if both parties assume that there is positive value in a new method being distinct from past forms and an improvement on them, and being governed by merely internal factors. At stake in the trial would be the allocation of points of rupture (then or now) and of significance to the local (then or now): the judge would decide the "correct" Whig, internalist account of the process of invention.

Schlumberger's whole publication strategy was geared toward the production of this correct account. Let us begin by looking at the clear pressure on the company to publish something about their methods. The first pressure came from the very newness of the technique itself. In a textbook in 1940, Heiland noted that "Progress and development in most geophysical methods have been largely the result of preceding developments in geophysical science. In gravitational, magnetic and seismic methods field procedure and methods of observation are closely allied to those in pure geophysics. Electrical methods lacking this background have followed their own course of development."[17] Not only was there no well-developed theory that Schlumberger could wave a hand at, but the whole area was under suspicion: "The quack and the shyster seem to have a strong predilection for electrical vestments. Another unfortunate circumstance is that electrical trappings are, in the minds of many laymen, endowed with mystical power."[18] Gish's statement is amply borne out by developments both at the time (attempts to measure oil reserves in Russia from measurements of the electrical field in a laboratory in Paris)[19] and since (a recent electrical hoax proved expensive for the French government). So Schlumberger had to publish something to establish scientificity. Second, the domain itself was professionalizing, producing its own journals. One had to be visible within the nascent community, as Conrad Schlumberger reluctantly conceded in a letter to his American manager: "My brother Marcel and I received an invitation from the Secretary of the American Institute of Mining and Metallurgical Engineers asking me to apply for membership. Motive: publication by C. and M. Schlumberger in their periodical. The cost is $15 annually, plus $20 for initial membership. We would get their publications. I don't personally like these tapeworms very much, they are small but they spread—nor do I like these papers that pile up but no-one reads. Nevertheless if you think that it would be *useful* to join, please send the attached application

on. If not, toss it in the waste-paper basket."[20] A third pressure to publish was that this might help establish priority and probity in future court battles. Thus Conrad went to Washington to offer to do some relatively uneconomic work for the Geological Survey with the recompense that "Our work for the Geological Survey would be published, but on the other hand it would give us better standing in the United States. And I think there is going to be a lot of competition and we are going to have to go to court to defend our patents."[21] Cumulatively, then, there was sufficient pressure to force the reluctant company to publish something.

But what should they publish? The immediate tension here was between the desire for secrecy and the need to get into print. We have seen the force of the latter. Here is one indication of the former: "Geophysical methods are only useful insofar as they are secret, since these methods are not patentable."[22] The logic of this statement is that what you cannot patent, you do not publish.[23] Thus at the trial, there was a debate about whether or not Schlumberger's patent used direct or alternating current. One of the witnesses said he had no idea, and elaborated: "Schlumberger has been pretty careful not to give out any detailed information of what they do The articles that have been published in general arc practically reprints of the patent with additional examples of some practical log."[24] The patent itself was (in this case fortunately) ambiguous, and the written record deliberately so.[25] One tactic here was simply to be particularly careful about what one was writing. The American manager E. G. Léonardon summarized the internal discussion about publication just as Schlumberger was becoming successful: "I replied to you that some propaganda with a scientific air is possible, without our necessarily giving away all our secrets. There is no need for us to publish anything of high scientific integrity—they are already calling Mason's talk 'scholarly' in this country." He was not in favor of producing a pamphlet:

> The "pamphlet" is, in effect, a piece of propaganda and what is in it is necessarily considered to be advertising. We need to keep people in suspense, to show them that we are out there and that we can write sensibly about physics and mathematics. On occasion, so as not to give our competitors information, we'll have to be jesuitical and lie by omission. I think that we can say a lot without necessarily talking about resistivities and other specialities that we worked hard to get together. All we need to do is publish a few papers that don't have the degree of probity you are so concerned about. Naturally it would be difficult for you to sign them, but no-one is asking you to make that sacrifice.[26]

Rather simpler than tucking away what one wanted to keep secret and printing only what one could defend in a court of law was to request that others do the writing. Thus when De Golyer came to edit a textbook about prospecting, he agreed with Léonardon that rather than call on Schlumberger itself to write about its method, he would ask their customers to give their appreciation of what it was and how it worked.[27] These were the friendly accounts. Naturally, Schlumberger's competitors were also producing official texts—Sean Kelly, for example. Léonardon disapproved of sharing information with Kelly, who used Jakosky's method:

> There is also the way he treats us when he writes a technical article. Shall I mention the fact that in everything he has published during the last three years on the SP phenomenon, he has consistently emphasized the fact that this was an old discovery, dating from the early part of the last century. Also, in an article published on dam sites, he made references to everyone concerned with this important aspect of geophysical exploration except Schlumberger. At least, the slight indications given in this connection seem to imply that we were late-comers in this kind of investigation.[28]

Thus defense of current and future patents was central to Schlumberger's publication strategy. This overriding concern actually extended into internal correspondence too: the decision was made to conduct all correspondence in English (although most of the staff were French-speaking) so that in the event of future use of the records in a court case, they would have documentary evidence in the court's own language. Clearly this comes back reflexively on the historian, since the only material he or she often has at hand is archives that have been written with the official history in mind.

Schlumberger needed to publish to gain respectability and to establish their own version of the history of prospecting; if they said too much, they might jeopardize their patents either by giving up secrets or by specifying the method to such an extent that someone could invent around it. Accordingly, scientific articles and textbook references would be as far as possible inscribed within the account that the patents gave of their technical objects or would be written by others with knowledge of only the public face of the company. Either way, they would necessarily accept the framework that the patents themselves imposed: that the technical objects they protected were indeed black boxes at rupture with the local, and that they constituted a marked progress on past methods.

We can now begin to see how the initial analogy with defending a historical position applies. In each case, there emerges a generally

accepted account about "what actually happened." In each case, production of this account is in much the same way severed from its own institutional roots—the preservation of the integrity of scientific/technical knowledge or of academic debate. That is, there is no reference in the accepted account to the variety of ways in which the account got imposed—each party claims to be constructing a discourse of pure truth (though perhaps accusing the others of being impure). There is general agreement about where the high ground is, and we will see how this is constructed in the next section.

Patents and Company Strategy

We have seen that priority was a key issue in determining the validity of a patent. Priority itself was subtly negotiated behind the scenes. One wanted some of it, but not too much. As explained by Léonardon in a memo in 1936, the company could have traced their invention back to 1921, "but in a discussion concerning prior art it would be detrimental to our interests to go back as far as 1921 and take the risk of reducing accordingly the life of our resistivity patent 1,819,923". On the other hand, Schlumberger could use the six-year hiatus between a *precursor* the idea (the surface analogue) to prove that inventive genius was required.[29] However, this later date needed some protection. Therefore Schlumberger acquired the Schlichter patent 1,826,961. It was of limited scope and was never used for petrol, but it did antedate Schlumberger's by a few weeks and involve measurements of conductivity down a drill hole. The company had been advised that the fact of Schlumberger's prior invention was sufficient: "As stated above, it is easy to prove the Schlumberger invention as far back as 1927. However, we did not know if Schlichter could still anteriorize this date. On the contrary, having pooled our interests, we gained the good will of the Schlichter interests and had communication of all the Schlichter early experiments." After the acquisition they also acquired Schlichter's records and learned that April 1928 was the date of the first test.[30] Thus when there was no public trial of priority in the form of a patent battle, there was often a rewriting of history behind the scene: the Whiggish account was as carefully constructed as the object it defended.[31]

Another way in which Schlumberger had to defend its patents behind the scenes was from parasitic attacks. Faussemagne's described one such attack: "The only competition we had at that time was from J. J. Jakosky. He was a professor at the University of

California and his tactic was to examine the patents and see how he could get around them, then to develop some apparatus as cheaply as he could and then sell the patent for the process that the original company had forgotten to take out a patent for."[32] The solution was the same as for Schlichter, but this time constructing the boundary around the patent rather than its priority:

> I met Jakosky in 1936 in a field in Louisiana. At that time after we had done our electrical logging, he ran a log with a truck that he had prepared. Jakosky's principle was to measure resistivity with an alternating current and monoelectrodes. His graphs were really disastrous—they were inclined and had no baseline; but he touched them up so that they looked like Schlumberger logs. He was a bit of a pest, and we bought out his equipment.[33]

This parasitic form is a special case of the process of "inventing around" others' patents, which has been studied by Hughes and others.[34]

Let us assume that a given company has patents with a safe boundary in time and space. What do they do with them, and how does this reflect on the printed account of the technical object they are using? Schlumberger was involved in its early years in two major patent battles, with much the same result in each instance. The first was with the Lane Wells company, which did mechanical coring of oil wells. This battle was in one sense similar to that with Halliburton: both Halliburton and Lane Wells were encroaching on Schlumberger territory, one from the direction of oil-well casing and the other from the direction of perforation. The central office in Paris wrote to Léonardon about the threat from Lane Wells:

> It should not be forgotten that it is only electrical logging that has rendered the use of the "single column" and of perforation possible. Therefore it would be illogical, and at the same time extremely upsetting, if our organization could not make anything out of this new activity whose success we are largely responsible for. Finally, if we let our competitors occupy the perforation market, they will get themselves a lot of trucks with electrical equipments and other surface installations, and will be naturally tempted to trespass on our own domain and to do electrical logging themselves.

With respect to Lane Wells, headquarters recommended that Schlumberger study their patents on perforation and quickly get an "explosive perforator," using the same principles as Delamare-Mozi, Moza, and Lane Wells, but with as many differences as possible on points of detail from Lane Wells.[35] Just as Schlumberger tried to

work around Lane Wells's patent, Lane Wells tried to work around theirs:

"Wells thought that he was going to eat us, even if we made a very expensive arrangement with him because he had his hands on a Swedish patent that was very complicated using alternating currents. With his lack of education in electrical matters, he thought that it was better because it was more complicated. This is more or less the same as what happened before with the Germans, who had wanted to do electrical logging. It often happens. More complex things give more information, but mixed up—and you have to know how to sort it out."[36]

Thus Schlumberger decided that rather than go into a patent battle, which they might lose because of the possible anteriority of the Swedish patent, they could fight in the oil field: "In the meantime, we do not agree to enter into a direct battle in the United States on the patents issue.... The fight with Lane Wells in the States should be carried out with the Fuse Perforator, that is to say 'technically.'"[37]

Looking back, one of Schlumberger's early heads saw this as the right decision:

The Lane Wells trial was very positive for Schlumberger, in the sense that it freed us from the sword of Damocles hanging over us in the form of perforation. Schlumberger was still doubtful about starting up perforation in the US without the license for the Lane Wells patent, but Schlumberger's main goal in starting the trial was to prevent or reduce the danger of Lane Wells taking a large part of the logging market. Put simply, Schlumberger's attitude was: it is better to make things more difficult for ourselves with respect to perforation by being forced to pay a license provided we can make it more difficult for Lane Wells to get into the logging market.[38]

The choice was a happy one in terms of future patent battles, since (as noted at the time): "Finally, we shake off the image that we are trying to set up a monopoly, which American courts abhor, and we get official recognition of our patents."[39]

There were, then, two main ways in which the companies involved strove to maintain the possibility of a Whig, internalist account of their inventions. Firstly, they shored up the historical account and the boundaries around the technical objects by buying up patents. This cleared away cases of "real" priority and parasitic attacks. Secondly, when the real world became too messy for such an account to possibly stand up, they settled out of court and fought "technically." In neither case was the purity of the patent's history a self-sufficient objective. As for the academic journals of our analogy with

an historical debate, if you wanted to carry on the battle in court, you had to be able to produce such an account. And just as for our analogy, what happened in court was not necessarily the determining factor of success: the account had to be *respectable*, but it made little difference if it was actually *right*. Thus for the case that did go to court, against Halliburton, Schlumberger ultimately lost the case but gained custom and consolidated their de facto at the same time as they lost their de jure monopoly. Henri Doll indicated this in an interview in 1973, when he pointed out that Standard Oil had given Halliburton their logging patent.

> The best proof that Standard set up the whole thing to attack our monopoly and not so much to get cheaper logs was that as soon as the Schlumberger patents were decreed to be valueless, Standard gave its business back to Schlumberger so that it would be done better, knowing that Schlumberger could not charge too high a price or sit on their monopoly and say: it's not worth bothering ourselves about doing research.[40]

He concluded, "If I had been in their position I'd have done exactly the same thing—that is to say attacking the monopoly and setting up a competition which could remain latent."[41]

Thus when we look at company strategy with respect to patents, we find that there is no belief on the part of the actors in the independence either of the patent (its history and boundaries are actively constructed) or of the patent trial (which is seen in the Halliburton instance as an integral part of a strategy aimed elsewhere). It is no accident that an untenable Whig, internalist account appears in textbooks and scientific articles: a deliberate filtration process goes on behind the scenes, the result of which is that such an account constitutes the public face of the company.

Again there is a clear analogy with the process of historical debate. For an argument to be accepted by the courts or pass the peer review process, it needs to take an accepted form—to go through a filtering process. This means in particular that reference to any mediation between the "facts of the matter" and the company's/author's personal position is systematically excluded (James Clifford gives a particularly lucid account of this kind of exclusion for shoring up the authority of the participant observe).[42] It is not that Tawney and Trevor-Roper would not recognize the personal and political roots of their controversy; it is just that they knew very well that they would not get published if they made reference to it in their arguments. As a result, their debate was carried out with remarkable vituperation, but with all the animus vested in legitimated forms of

disagreement about the interpretation of evidence. For both patents and the historical debate, this exclusion is not a given; it is the end point of the process of deciding where and how to fight the battle. The filtration process is much the same in the two instances: for Schlumberger and for the professional historian, "irrelevant data" includes any account of company/personal motivation or of other battle sites. In both cases a world that no one really believes in (with real truths, arguments being decided purely on their merits) is underwriting a "legal fiction." We will now see what purposes that fiction can serve for the protagonists.

Patents and Oil Fields

The picture that we see emerging from the preceding sections is that although none of the actors believe that technical objects are simple enough to present to the world a clear rupture with other objects and a single moment of discovery, all of them believe that it is worth creating this impression locally and temporarily. In the last section we saw that they made the effort; we will now try to model *why* they do so. We will be looking at the third level of the analogy with the historical debate: the way in which the historical process itself can validate or render irrelevant the historical view that is taken.

Everything revolved around the issue of timing: the complex of relations around the state of the industry, the development of petroleum geology and geophysics and the kind of oil reserves being discovered. The overall process was admirably described in 1937 by J. H. M. A. Thomeer: "Electrical coring and modern oil field exploitation have deeply influenced each other, have grown up together and are now so intimately linked up that separation would be impossible without serious damage to both."[43] In a sense, this was the real motivation for defending patents (and thus blocking competitors for a time). Thomeer is referring here to the fact that Schlumberger influenced the drilling method used, the drillers' mud that circulated in the well, the ways in which fields were exploited, and the use of other prospecting methods. Schlumberger aimed to *survive* long enough in the field so that they could build up the necessary expertise and methods to win, and so that they could transform the field so that they had to win. They needed to "geophysicize" geologists sufficiently: "we had a lot of education and penetration work to do, particularly with geologists"[44]; at the same time as geologists strove to "geologize" the industry: "This conquest of the industry by geology has been not unlike the process of metasomatism, to borrow an

appropriate if somewhat imposing term from the hard-rock geologists. Metasomatism is that important process in ore deposition whereby the invading solution, although it leaves the outward form or body of the host rock unchanged, nevertheless entirely transforms its intrinsic character, replacing the original internal constituents, molecule by molecule, with substitutes of its own selection. So has geology reformed the business of producing oil."[45] Comparison can be made here with the steel industry, whose "chemicization" Thomas Misa (this volume) brings out, again in conjunction with the implantation of a new technology.

Looking back on the process, Henri Doll recognized that neither of their two revolutionary methods really did what they said they would: "But then again what does absolute scale mean? The log that you take essentially measures the invaded zone [the zone that has been invaded by the driller's mud]. Now maybe that means something, but after all if you really want to know whether there is water or oil with your resistivity measurements we now know that there is not so much difference between a petrol layer and a water one, unless you want to look behind the invaded zone—which neither of our measurements did."[46] But these methods that did not really work were effective in combination with local experience: "It is clear that ample experience and thorough knowledge of local conditions in a given field are essential factors for a reliable interpretation of Schlumberger logs, the same factors, therefore, as are required for any other method of studying the productive prospects of reservoir rocks. Correct interpretation of the meaning of oil smell, chloroform cuts, sand appearance etc observed on core samples, also is largely a matter of local experience."[47] Or, as Gish (a Schlumberger rival) wrote in an article on electrical methods: "In the present state of the art, success depends to a considerable extent upon familiarity with the method and apparatus, and this is not gained in a day."[48]

This process of building up prior knowledge and expertise (dealing with the messy and the local) went hand in hand with the process of building up the patent (creating purity), as the following samples from internal correspondence show: "When we know the region better, our information will be more sure and precise than that of the geologists, and we will save on many feet of useless forage;"[49] "Petrol, petrol, that is all that interests the companies. Ah, if we knew a procedure for detecting it, they wouldn't find us too expensive. They are always asking me: 'Can you distinguish a petrol-bearing layer from an aqueous one?' The answer is delicate: yes we can, providing you first tell us if there is any sand there at all;"[50] or again the report

that in the San Joaquin Valley, "certainly more than in Los Angeles, the operators have to depend on our interpretations since our logs are not so evident by themselves. The small operator in the San Joaquin Valley cannot very well take the risk of a Geoanalyzer survey, which would be valueless to him since the Geoanalyzer engineers can not have the large experience we have already gathered. As for the majors, they are with a few exceptions still using entirely Schlumberger."[51]

What is interesting here is the indication of a double process. In the first process, Schlumberger was defending their patent by claiming that it gave the correct historical account of the development of electrical logging. In the second process, they were changing the nature of well digging so that electrical logging was the only possible adjunct to the drilling process. Thus they were in a messy way creating the hegemony that they already claimed was the correct account—and their claim that this was the correct account helped them create the hegemony. The analogy with the Tawney/Trevor-Roper case is twofold. First, imagine the reception of Tawney's work being such that a number of his readers change allegiance and fight in the class war. His "legal fiction" would have stood up well enough at the time to motivate others to make it true. Second, imagine Tawney participating in an insurrection in England. In so doing he would have been struggling to impose his account of history, and if his struggle succeeded, then his history would prove to have been correct—institutionally and therefore in the history books. In his case, Marxism would be at both the beginning and end of the process. These two counterfactuals illustrate the general point that historical truths change with time, and that historical accounts feed recursively into this change. In Schlumberger's case, an internalist history is at the beginning and the end—at the beginning in terms of the patent and its defense, and at the end because the world of oil will have been sufficiently altered that Schlumberger is inevitable.

Conclusion

Schlumberger engineer Martin recalled in an interview that, "in 1933, Conrad Schlumberger said: 'Basically, what makes this business of electrical logging a difficult one is that we want to go down into other people's holes.' This was certainly true at the time, but a few years later I would say that the companies were desperate to offer us their holes."[52] The sexual metaphor expresses a historical truth: Schlumberger did become inevitable. We have seen that they

did so in part by losing a patent battle, in which they tried to defend two patents that did not really work. To explain this paradoxical success, we have seen that there were things going on in at least three different places (the courtroom, the company, the oil field) and at three different rhythms, and that the *timing* of these three processes was at least as important as the "correctness" of the science or the history within any one of them.

We can now return to the two accounts of similar relationships that I referred to in the introduction: Callon's and Shapin and Schaffer's. Callon's actors are unconsciously backing two radically different sociologies; Shapin and Schaffer's are consciously developing their political philosophy in the laboratory. In our case there was no conflict—conscious or otherwise—between the types of historical account that the actors sought to impose. Schlumberger worked away at both ends for a Whiggish, internalist account of their own inventions; the achievement of such an account would be a mark of their victory and would constitute an entry in the history books. We are dealing with the process by which accounts that scientists and technologists give of their own disciplines/crafts are always willing to be externalist about the other and internalist about themselves. It is not just that the internalist explanation is what they "believe" or what will prove to have been true if they succeed. If it works, it also means that they have changed the world sufficiently that it becomes true. Changing the world in this instance means changing the nature of geophysics, the practice of drilling an oil well, and so on—each case is different.

It should be noted, however, that these changes are not limited to changing institutions. In our case, it meant changing that part of the world that oil companies dealt with, and so changing the world of which they were phenomenologically aware. Thus, for example, in the early years there was a popular superstition against faults: "The absurdity, as now we understand it to be, was commonly expressed in the once-familiar words: 'The country appears too much broken up.'"[53] Faulted country was avoided, and where there was an oil field, faults were not part of the model.

We had to work as quickly as possible to show that, contrary to what was the received opinion of all Gulf Coast geologists that there was generalized lenticularity and no faults, that both existed. When I say that the general theory was that there was no possibility of correlation in the Gulf Coast, that was an absolute theory that all geologists in the region held and when I went to visit Monsieur Thomson of Pure Oil shortly after my arrival, I saw a magnificent model of a field with all the productive layers being

lenticular. They worked from the wells they were producing from and got lost somewhere in nature.[54]

Faults were good news for Schlumberger: "A fault seems to exist running East-West, the south side being the higher and the one where up to now large production has been found. Besides the major fault, minor unconformities and some lenticularity prevail in the area, and the transition from oil to grey sand takes place sometimes on very short distances. This condition will make the use of electrical logging in this new field entirely systematic."[55] Thus that bit of the world that is faulted was ignored and unmanageable before Schlumberger and was sought after and manageable after.

In general, then, the geophysicists sought to change the world and to change the industry so that their emergent method could develop and become true. It is possible to read the story in realist terms: Schlumberger had discovered a method that on development fit better with the facts than any other. This is structurally not so different from a social determinist position, which would argue that Schlumberger had discovered a (social) method that on development fit better with the society of the oil industry than any other.[56] It is also possible to read it in constructionist terms: Schlumberger built up the truth of their method using all the tools at their disposal—from resistivity meters to rhetoric to rationalism. My own feeling is that the constructionist reading does not go far enough in recognizing that both physical and social reality exerted a definite influence: Schlumberger's work was significantly shaped by both, even as it was shaping. The realist/social determinist account clearly goes too far the other way.[57] What we are left with is a situation in which "nature" and "society" are both emergent realities that are constructed by their components at the same time as their components construct them.

There is a two-part strategy to making things more real within this emergent reality. First, an appropriate space is created by manipulating awareness contexts. Official debate and institutional battle are kept sufficiently unaware of each other that a series of publications and secondary institutions can build up in blissful ignorance of the messy side to such an extent that the two levels become, to borrow a somewhat weary phrase, semi-autonomous.[58] There is an inside and an outside—the former occupied by company engineers, strategists, and scientists, and the latter by the public face of science. Bruno Latour's mask of Janus, with one face before the fact is created and another after it is accepted, comes to mind.[59] Here we have shown

that in industrial science, this "before" and "after" occupy separate spaces; they become "inside" and "outside." Once the externalist explanation has been sufficiently constrained that only the patent holders believe it (and then only in private), then the boundary between inside and outside is in place.[60]

There is, then, no need to maintain this space forever. The second part of the strategy involves maintaining it only *long enough* that social and physical reality will alter.[61] This is what we characterized as the issue of timing. If Schlumberger survived for long enough in the patent battle, then whether or not they won that battle, they would win out historically, because they would have created the breathing space within which to impose their reality on the oil field. And that is exactly what they did.

Notes

This essay was made possible by a generous grant from the Fondation les Treilles. I would like to thank Leigh Star for her incisive comments.

1. Noble 1977, 111.

2. Hughes 1983.

3. Reich 1985.

4. Dennis 1987; Bowker 1989.

5. Cambrosio, Keating, and Mackenzie, forthcoming; cf. also Pinch, Ashmore, and Mulkay (this volume).

6. I am using classical definitions of "Whig" and "internalist." Whig history refers to an account of uninterrupted progress (as opposed to a discontinous/revolutionary or a nonprogressivist account); internalist history refers to a belief that scientific/technological change can be explained in its own terms (without an "externalist" reference to its social context).

7. For a development of this concept, see Strauss 1978 and Glaser and Strauss 1966.

8. For Trevor-Roper, see Trevor-Roper 1951 and 1953. For Tawney, see Tawney 1960. For an account of the debate, see Russell 1973 and Stone 1972. Stone asserted, "There have been few more brutally savage attacks in academic journals than that in which H. R. Trevor-Roper demonstrated the exaggerations and inconsistencies in my first article."

9. See, for example, Stone 1973.

10. Smith (1974 : 260) gives a good general account of the process by which the historical account is formed in rupture with historical reality:

> We begin at what actually happened and return via the social organization to 'what actually happened'. ... The two social organizations, of the production of the account and of the reading of the account, are distinguished in the first place because at the point at which the account is put into its final form it enters what I shall call 'document time'. This is that crucial point at which much if not every trace of what has gone into the making of that account is obliterated and what remains is only the text which aims at being read as 'what actually happened.'

11. Callon 1986b.

12. Schaffer and Shapin 1985.

13. Bowker 1987 and 1988. For a good technical history, see Allaud and Martin 1976.

14. Bowker 1987.

15. Compare Misa (this volume), who points out that participants in his dispute were ever ready to identify the "interests" of the others.

16. Cf. Lyell's *Principles of Geology*, for example, wherein all manner of social and ideological motives are imputed to everyone before Lyell himself.

17. Heiland 1940, 623.

18. Gish 1947 (1932), 498–499.

19. Box Pechelbronn, Folder 1922. [The early years of the Schlumberger archives are partially housed at the Ecole des Mines, Paris. They are divided into folders within subject boxes, and I annotate them accordingly.]

20. Box USA 1926–1933, Folder 1930, 5/8/30, Conrad Schlumberger to E. G. Léonardon.

21. Box USA 1926–1933, Folder 1926, 3/11/26, Conrad Schlumberger to Paris.

22. Box Pechelbronn, Folder 1925, Letter to Conrad Schlumberger, 6/6/25.

23. Compare the problem of secrecy as tied to classified publications. The story of the computer expert who was not allowed to read his own paper has reached the proportions of academic myth. In general, even though Hughes and others have highlighted the problem of secrecy in industrial science, it has not received systematic attention—in large part because the interesting documents are secret.

24. *Schlumberger* v *Halliburton*, vol. 13, 1205.

25. For the value of ambiguous patents, see the story of Diesel as recounted by Thomas (1987). Diesel had to defend a theory of his engine that he knew to be incorrect because it was enshrined in an overly precise first patent.

26. Box USA 1926–1931, Folder 1927, 23/11/27, Léonardon to Paris. Cf. the remark by Léonardon that he liked a conference memo by Charrin, but that it gave away too much and should not be distributed too widely. He proposed a copy of his English-language pamphlet: "whose commercial and non-scientific aspect will displease you." (Box USA 1926–1931, Folder 1927, Léonardon 27/6/27).

27. Box USA 1934–1938, Folder 1934, E.G. Léonardon, 3/8/34.

28. Box USA, Folder 1934–1938, E.G. Léonardon to Paris Office, 23/1 2/36.

29. Box Historique, Folder Operations, Léonardon, 8/9/36.

30. Box Historique, Folder Operations, Léonardon, 8/9/36.

31. Misa (this volume) also points to the variability of the category of "real priority."

32. Box Interviews 1, Interview Faussemagne. [The interviews I cite were carried out in the 1970s, largely by Martin in the preparation of his and Allaud's book or by A. Gruner-Schlumberger, daughter of Conrad Schlumberger (one founder of the firm).]

33. Box Interviews 1, Interview Faussemagne, p. 15.

34. See Hughes 1983 and Reich 1985 for example.

35. Box USA 1934–1938, Folder 1934, 14/6/35 Paris to E. G. Léonardon.

36. Box Procès, Folder Interview with H. G. Doll.

37. Box USA 1934–1938, Folder 1936, 29/5/36, SPE to Léonardon.

38. Box Procès, Folder Interview with H. G. Doll.

39. Box Procès, Folder Echange de Lettres à propos de Halliburton.

40. Box Procès, Folder Interview H. G. Doll.

41. Box Procès, Folder Interview H. G. Doll.

42. James Clifford, "On Ethnographic Authority," Representations, 1, 2, 1983.

43. Typescript by J. H. M. A. Thomeer of Bataafsche, "The Application of Schlumberger Electrical Logs in Oilfield Operations," in Box USA 1934–1937, Folder 1937.

44. Box Interviews 3, Interview Mathieu.

45. Pratt 1940, 1211

46. Box Procès, Folder Interview with H. G. Doll.

47. Typescript by J. H. M. A. Thomeer of Bataafsche, "The Application of Schlumberger Electrical Logs in Oilfield Operations," j in Box USA 1934–1937, Folder 1937, p. 6.

48. Gish, 1947 (1932), 502.

49. Box USA 1926–33, folder 1929, 11/12/29, Gallois to Henquet.

50. Box USA 1926–33, folder 1929, 17/11/29, note by Deschâtre.

51. Box Notes Techniques 1930–1938, "California Progress Report for the months of October, November and December 1936," p. 25.

52. Box Interviews 3, Interview Martin.

53. Clapp, 1929, 686.

54. Box Interviews 3, Interview Mathieu.

55. Box Progress Reports 1936–1944, "California Progress Report for the months of October, November, and December, 1936," p. 10.

56. This comes out clearly in our analogy with the Tawney/Trevor-Roper debate; since one way in which their positions are developed in the real world is by changing the structure of society and thence the craft of historian—so in this case "realism" is conflated with "social determinism."

57. Latour 1987.

58. To put it another way, we can refer again to Smith's (1974) analysis, wherein two different social worlds are created: that of the *author* (Schlumberger here) for whom the whole process is open and messy and that of the *reader* (the courtroom, the science student) for whom the process is closed and clean.

59. Latour 1987.

60. Pinch, Mulkay, and Ashmore (this volume) elucidate a similar coexistence of different technological rhetorics—a "strong program" for one audience and a "weak program" for another. In their case, the former rhetoric appeals to economists and government ideologues, the latter to administrators. Thus even when these rhetorics

cohabit the same text, the one group will come away with one inside story and the other with an opposed one. That ambiguity is deliberately fostered in order to be able to black box the technology—and once it is black-boxed, the strong program will doubtless become the dominant rhetoric.

61. Compare here Law and Callon's fruitful notion of negotiation space, which operates similarly in a slightly different context (Law and Callon this volume and Callon and Law 1989).

3

The Social Construction of Fluorescent Lighting, Or How an Artifact Was Invented in Its Diffusion Stage

Wiebe E. Bijker

Technology is assumed to be designed, developed, and produced by engineers.[1] They are at the drawing boards and behind the laboratory benches; they apply for patents, model the prototype, and test in the pilot plant; they show the newly born artifact to the press and, if lucky, they figure prominently in the glossy photographs of stories about heroic inventors. Once these engineers have produced the technology, it is passed on to the sales people, the managers, the trade, and, finally, to the users. Engineers design technology, managers produce it, salespeople sell it, tradespeople distribute it, users use it. Alas, this neat and orderly image of technical development, so pervasive in all but the most recent technology studies, is not only too simple—it is wrong.

This chapter has two aims. First, I want to show that the application of a linear stage model of technical development is detrimental to understanding the development of technical artifacts. Rather, no stages can be distinguished. I will demonstrate how the modern fluorescent lamp was designed during what commonly would have been called its "diffusion stage." If the fluorescent lamp is considered a static artifact, forever fixed and unchanging since it left the General Electric laboratories on April 21, 1938, it is difficult to understand what actually happened and the original lamp's relation to the present fluorescent lamp. Instead, I will analyze, from a social-constructivist perspective, the fluorescent lamp as something that was continually reshaped and redesigned by the various social groups involved.[2] The second aim of this chapter is to provide an illustration of the possibilities of integrating the social-shaping and the social-impact perspectives on technology.[3]

Part of the development of the fluorescent lamp is described in detail, using the social constructivist approach (SCOT).[4] In the SCOT descriptive model, *relevant social groups* are the key starting