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How the refrigerator got its hum

Failed machines

If the landscape of American social history is cluttered with the remains of failed communes and cooperatives, the landscape of American technical history is littered with the remains of abandoned machines. These are not the junked cars and used refrigerators that people leave along roadsides and in garbage dumps, but the rusting hulks of aborted ideas: patents that were never exploited (the patent record contains literally millions of them); test models that could not be manufactured at affordable prices; machines that had considerable potential but that were, for one reason or another, actively suppressed by the companies that had the license to manufacture them; devices that were put on the market but that never sold well and were soon abandoned. The publications of the Patent Office and the 'new patents' columns in technical magazines reveal that the ratio of 'failed' machines to successful ones is high, although no scholar has yet devised a formula by which it can actually be determined. Some nostalgia buffs have even become collectors of these 'rusting hulks,' filling scrapbooks with advertisements for bizarre devices and selling extant versions of them to one another at flea markets and antique shows.

The women's magazines of the nineteenth and twentieth centuries are filled with such aborted ideas: an ice-making machine driven by a small water wheel; a rocking chair that simultaneously propels a butter churn and a cradle; individual household incinerators; central vacuum-cleaning systems; sanitary toilets that do not use water; fireless cookers. There was a vast array of devices, some ludicrous but many, at least on the surface, very sensible. What resident of a drought-prone area today would not be grateful for a toilet that does not use water? How many energy-conscious housewives would be unwilling to try out a fireless cooker? In what city and town, plagued by erratic and expensive garbage pickup, would a householder not be pleased to be the first on the block to own a household incinerator? Why are these items either no longer on the market or not there at prices that most households can afford? Why do we have popcorn makers and electric can openers but not gas refrigerators or inexpensive

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central vacuum cleaners? If we can put a man on the moon, why have we been unable to pipe our garbage disposals into our compost heaps?

The answers to these questions are not simple: they involve economic decisions made by complex social institutions operating over long periods. In order to find out why a particular patent was not exploited, one must discover something about the Patent Office, something about the inventor, and something about potential consumers; in order to find out why a particular test model was never manufactured, one must learn about the technical problems involved, the decision-making procedures within the company that developed the test model, the state of the general economy, the availability of resources, and so forth. Yet if one wants to learn why our houses and our kitchens are constructed in certain ways but not in others – that is, why household work is shaped by certain constraints and not by others – then an exploration of the forces that cause some machines to 'fail' and others to 'succeed' may well be in order. One such case, which I shall here consider as an example of all the others, was the rivalry between the gas refrigerator (the machine that failed) and the electric refrigerator (the one that succeeded).

The refrigerator: gas versus electric

All mechanical refrigerators create low temperatures by controlling the vaporization and the condensation of a liquid, called a 'refrigerant'; when liquids vaporize they absorb heat and when they condense they release it, so that a liquid can remove heat from one place (the 'box' in a refrigerator) and transport it to another (in this instance, your kitchen). Virtually every refrigerator on the market in the United States today controls the condensation and the vaporization of its refrigerant by a special electric pump known as a 'compressor.' Compression is not, however, the only technique by which these two processes can be controlled. The simplest of the other techniques is 'absorption.' The gas refrigerator is an absorption refrigerator. Inside its walls, a refrigerant (ammonia, usually) is heated by a gas flame so as to vaporize; the ammonia gas then dissolves (or is absorbed into) a liquid (water, usually), and as it dissolves it simultaneously cools and condenses. The absorption of ammonia in water automatically alters the pressure in the closed system and thus keeps the refrigerant flowing, hence making it possible for heat to be absorbed in one place and released in another, just as it would be if the flow of the refrigerant were regulated by a compressor. The absorption refrigerator, consequently, does not require a motor – the crucial difference between the gas refrigerator and its electric cousin. Indeed, with the exception of either a timing device or a thermal switch (which turns the gas flame on and off so as to regulate the cycles of refrigeration), the gas refrigerator need have no moving parts at all, hence no parts that are likely to break or to make noise.

The basic designs for both compression and absorption machinery were perfected during the nineteenth century.[1] The phenomenon of latent heat (the heat absorbed when a liquid changes to a gas and released when the process is reversed) was discovered late in the eighteenth century and explored in great detail in the nineteenth because of its importance both in the new science of thermodynamics and in the new technologies of the steam engine. In those same decades, the need for mechanical refrigeration was growing as cities began to expand, both in Europe and in the United States, and ever larger quantities of food had to be preserved for longer periods of time as people continued to move farther from the places where it was grown. Between 1830 and 1880, dozens upon dozens of mechanical refrigerating machines were patented – machines that would make ice as well as machines that would cool large compartments without making ice. By the end of that period, the fundamental designs for large-scale compression and absorption installations had been perfected, largely through inventive and commercial trial and error. As a result of all this activity, manufactured ice became available throughout the southeastern United States by 1890 and throughout the northeast (where natural ice was more readily available through much of the year) by 1910. By 1890, nearly every brewery in the United States had purchased a refrigerating machine to remove the heat generated during the fermentation of beer and to cool the finished product while it aged and awaited transportation. Before the nineteenth century had turned into the twentieth, meat packers were using mechanical refrigeration in the handling and processing of meat, cold-storage warehouses had begun to appear in cities, icemen were carrying manufactured ice through the streets, and refrigerated transport (which utilized manufactured ice in railroad cars and refrigerating machines on ocean-going vessels) was becoming increasingly common and less expensive.

Operating a commercial refrigerator was an ambitious undertaking. Few machines weighed less than five tons, and a substantial number of them weighed from one hundred to two hundred tons. All the compression, and some of the absorption, machines required a source of mechanical power; and, as the electric motor was not yet perfected, this source was most commonly a steam engine (although hot-air engines and water turbines were occasionally used), which itself might weigh several dozen tons. As automatic controls were primitive, the machine was tended night and day by skilled operators, and each machine required a staff of even more skilled people to perform normal maintenance activities. Designing these machines was no simple task, since each one was built to unique specifications. By the turn of the century, a new profession had emerged: the refrigeration engineer – a person who could design and maintain refrigeration equipment. The American Society of Refrigerating Engineers was formed in 1904; and the Refrigerating Machinery Association, which represented the interests of manufacturers, one year earlier, in 1903.

None of this activity affected American households directly, even as late

as 1920. Indirectly, many Americans benefited from lower prices for ice and greater availability of fresh meat, poultry, dairy products, and eggs during the first two decades of the century, but mechanical refrigeration was not yet possible in the household itself. The technical obstacles to developing a domestic mechanical refrigerator were substantial: such a refrigerator would have to be small and light enough to fit somewhere in a household, automatic enough not to require constant supervision, reliable enough not to require constant servicing; and it would have to have a power source that could be operated by a totally unskilled worker. Ultimately, it would also have to be designed so that it could be mass-produced, and it would have to be safe: many of the refrigerants then in common use were either toxic or flammable, and 'ice-house' accidents were regularly highlighted in the newspapers. That a potential market existed was clear, for the use of ice and iceboxes in American households expanded drastically after 1880. In Philadelphia, Baltimore, and Chicago, over five times as much ice was consumed in 1914 as in 1880; and in New Orleans, the increase was thirteenfold; the dollar value of iceboxes manufactured in the United States more than doubled between 1909 and 1919.[2] In the early years (1910–20), neophyte manufacturers of domestic refrigerators had no difficulty finding investors willing to lend them money and large coporations willing to buy them out. Just before and after the First World War, the problems involved in initiating domestic refrigeration were technical, not financial or social, and appear to have been about as great for the absorption machine as for the compression one. Indeed, since, until about 1925, gas service was more widespread than electric service, one might guess that the absorption machine would have had the competitive edge.

The electric compression machine

The first domestic refrigerator actually to go into large-scale production, however, was a compression machine. The honor of being first seems to belong to A. H. Goss, then an executive of the General Motors Company; to E. J. Copeland, a purchasing agent for General Motors; and to Nathaniel B. Wales, a Harvard graduate who was an independent inventor.[3]* On

*In matters technological, the question of who was 'first' is difficult to resolve, initially because one must be careful to specify 'first at doing *what*,' and then because available accounts, embedded as they are in the history of extremely private enterprises, are frequently vague, often in conflict, and most commonly nonexistent. Most authorities say that the Kelvinator was the first successful domestic refrigerator, but they may do so only because, at some point, the Kelvinator Corporation donated one of its 'first' models to the Smithsonian. A reporter for *Air Conditioning and Refrigeration News* (then, *Air Conditioner, Heating and Refrigeration News*) asserted that the Isko Company (which was started 'by Fred Wolf with the backing of . . . Detroit capitalists') went into business in 1912, and that the Guardian Frigerator Company (which later became Frigidaire) was started in 1916, but provided no date for the commencement of manufacturing in either case.[4] Lacking more complete information, Kelvinator remains 'first.'

14 September 1914, Goss and Copeland contracted with Wales to do the development work on a domestic refrigeration machine. After creating several test models, Wales settled on a compression machine using sulfur dioxide as a refrigerant; he had originally worked on an absorption machine, but – for reasons that are unclear – those plans were dropped. On 13 May 1916, this enterprise was incorporated as Goss & Copeland Electro-Automatic Refrigerator Company; but a few months later, the name was changed to 'Kelvinator.' At this juncture, Wales left the enterprise. In 1917, Copeland developed a satisfactory automatic control device and a solution to the problem of gas leakage (sulfur dioxide is toxic); and in February 1918, the first Kelvinator refrigerators were sold.

The path that Goss and Copeland pioneered quickly became a beaten track. By 1923, when the officers of the General Electric Company decided to do a thorough study of the domestic refrigeration business, the mechanical engineer to whom they entrusted the job, A. R. Stevenson, was able to identify fifty-six companies that were already involved in the business.[5] Some of these, such as Kelvinator and its rival, Frigidaire (which had been founded in 1916 and purchased by General Motors in 1919), were heavily capitalized and had already produced several thousand refrigerators. Other companies had just entered the field and had only test models and/or faltering finances. In those early years, compression refrigerators dominated the field; and out of the fifty-six companies, only eight were yet either well financed or well on their way to large-scale production.

Yet, in 1923, even the compression domestic machine was still in its developmental stage: the machines on the market did not inspire every middling householder to reach immediately for a checkbook. They were, to start with, expensive: the price had fallen from its original peak; but in 1923, the cheapest still ran to $450 – not an inconsiderable sum at a time when most people earned less than $2,000 a year. Furthermore, refrigerators were difficult to run. Electric utilities estimated that, once every three months, they serviced the machines that they had sold: the tubes leaked; the compressors malfunctioned; the thermostats broke; and so did the motors.[6] All these early machines were, in addition, 'separated' machines – and water-cooled ones at that. The refrigerating machinery was sold separately from the refrigerating compartment, which might well have been simply the icebox that a family had previously used; the machinery could be set up in the basement, say, and the icebox put in the kitchen. The compressor had additional work to do, since the refrigerant had to be moved a considerable distance, but it must have been a relief to householders to have the noise, the oil, and the serviceman in some remote part of the house. Water cooling (the standard technique in large commercial installations) was not convenient in the home. The water pipes froze in some locales in the winter time (turning a refrigerator back into an icebox); or the water frequently leaked into parts of the machinery where excess humidity created excess problems. F. C. Pratt, a vice president of G.E. in

1923, forwarded Stevenson's report to Gerard Swope, president of the company, with the following warning:

> There reads through Mr. Stevenson's report the important fact that all existing practice carries a more than normal hazard of being revolutionized by inventions of a fundamental character. So many active minds throughout the country are being directed to the solution of these problems that it would be perhaps surprising if some such inventions did not materialize. The business is a rapidly evolving one, making real strides from the developmental to the commercial stage.[7]

Pratt was right, as it turned out. In the decade between 1923 and 1933, inventions that would profoundly alter the design of domestic refrigerators did, in fact, materialize; and, again as he predicted, they materialized in more than one quarter. In Sweden, for example, two young engineering students, Carl G. Munters and Baltzar von Platen, figured out how to design an absorption refrigerator that would run continuously and thus would not require expensive automatic controls; this machine (the Electrolux-Servel) went on the market in 1926. Engineers at Kelvinator and, later, at General Electric discovered techniques for dispensing with water as a cooling agent. In 1927, General Electric became the first manufacturer to make a hermetically sealed motor and to sell the box as an integral part of its refrigerating machinery. Within a year, other manufacturers followed suit and also began mass production of refrigerator boxes made from steel rather than from wood. In 1930, chemists at General Motors (which still owned Frigidaire) developed a series of artificial refrigerants (the Freons) that were neither toxic nor flammable; and in 1932, engineers at Servel designed an air-cooled absorption machine. By the middle years of the Depression, most of the fundamental innovations in domestic refrigeration design (with the exception of automatic defrosting, which came later) had been made.[8]

These innovations did not occur out of the blue. They were the end result of deliberate assignments given to a large number of highly trained (and highly paid) people, and of the equally deliberate expenditure of large sums of money not only to develop these ideas but to equip assembly lines that could realize them in production. The stakes were thought to be very high. The potential market for domestic refrigeration was enormous: by 1923, it was clear that every household in the United States was going to be equipped with either gas or electric service (and probably both in many places); and, thus, that if the price could be brought low enough, every household would become a potential customer for a refrigerator.[9] The potential revenues for the gas and electric utility companies would be even more enormous, since, unlike other household appliances, the refrigerator operates twenty-four hours a day. Thus, it is hardly surprising that the money and the time necessary to achieve these innovations was available – especially during the economically free-wheeling 1920s. Yet, to say that the stakes were high is also to say that the risks were great. Some manufacturers were going to succeed, and others were going to fail – and one of the failures would turn out to be the only manufacturer in a competitive position to keep the gas refrigerator on the market.

One of the manufacturers that succeeded, and whose success helped carry the compression refrigerator to dominance, was General Electric. By the 1920s, General Electric was an enormous corporation with vast resources and had its finger in almost every aspect of the electrical industry in the United States, from the design of large generating plants to the manufacture of light bulbs.[10] The refrigerator that General Electric introduced to the public in 1925 (called the 'Monitor Top' because the working parts were located in a circular box that sat on top of the refrigerating cabinet itself) was the product of almost fifteen years of developmental work on the part of General Electric employees. In 1911, G.E. had agreed to manufacture a commercial refrigerator for the Audiffren Company, which held the American rights to a patent owned by a French monk, the Abbé Audiffren. Sometime during 1917, engineers at the Fort Wayne, Indiana, plant (where the Audiffren was manufactured) began to build test models of a modified Audiffren design, suitable for use in the household. Immediately after the First World War, G.E. found itself in poor financial condition; in 1922, the company was reorganized, and Gerard Swope was brought in as president. Swope believed that General Electric was going to have to enter the consumer electric market and, to this end, instructed A. R. Stevenson, who was then head of the engineering laboratories in the company's main headquarters in Schenectady, to review the current state of the refrigerator business.[11]

Stevenson's report, a model of engineering and econometric skill, provides glimpses of the factors that influenced decision makers at G.E. The report contained everything from engineering tests on competing machines to projections of the potential market for refrigerators sold at various prices. Stevenson had been asked to recommend a course of action to the managers of the company, and he did so without equivocating. Was it worth entering the domestic refrigeration business at all? Certainly Yes, concluded Stevenson. If it did, should G.E. purchase one of the many small companies already in the field (No) or make cross-licensing arrangements (our motors for your compressors) with one of the larger companies (No). Should G.E. take advantage of the development work that had already been done at Fort Wayne and try to work with an Audiffren type of apparatus (Yes). Was it worth spending the time and money that would be required to switch from water to air cooling? Absolutely, said Stevenson, not just because water cooling was a problem for home owners, but also because General Electric had to worry about the interests of its most important customers – not the home owners but the electric utility companies:

> the electric power bill of the air cooled machine would be about $1.30 more in six months than the water cooled machine. . . . Since the General Electric Company is entering this field for the benefit of the central station [the utility company that is generating electricity] it would seem wise to exploit a machine in which the total revenue would accrue to the central station rather than partly to the water works.[12]

Stevenson understood that General Electric would be assuming a considerable risk if it entered the refrigerator business; but he believed the risk to be worth taking for a number of reasons: he believed that there was a good chance that G.E. would be first, that the company had the resources to sustain the initial losses, that after this initial period the profits would be great, and finally that 'widespread adoption [would] increase the revenue of the central stations, thus indirectly benefiting the General Electric Company.'[13] G.E. stood to gain, both coming and going, from developing a successful refrigerator.

The managers of G.E. must have agreed with Stevenson. During 1924, a group of engineers worked on developing an air-cooled model of the original Fort Wayne design. In the fall of 1925, limited production began, and the 'Monitor Top' was introduced to G.E.'s sales force and to the electric utility companies. During 1926, construction of an assembly line began (at a total cost of eighteen million dollars), and the design was modified again to allow for mass production. In 1927, a new department of the company was created to promote and market the machine; and within months of its establishment, the first mass-produced Monitor Tops had found their way into kitchens across the land. By 1929, fifty thousand Monitor Tops had been sold – a figure that may have been as surprising to the top management of General Electric (the company had anticipated sales of seven thousand to ten thousand per year) as it was to everyone else.[14]

General Electric stimulated sales of its refrigerators by means of outlandish advertising and public relations techniques. Franchised distributors were appointed in the major cities across the country and given exclusive rights to sell and service their territories. Rex Cole, in New York, was famous for constructing a neon sign that could be read three miles away, and for staging promotional parades. Judson Burns of Philadelphia had his new store designed in the shape of a Monitor Top. When G.E. introduced its first all-steel cabinets in 1929, a novel 'Pirate's Chest' sales campaign was broached:

> For some time previous to March 22 mysterious looking old iron-bound boxes closely resembling pirates' treasure chests had been on display in the windows of General Electric refrigerator dealers, with a sign saying that they would be opened on March 22. The night before, large door keys were hung on door knobs in the residential sections with an invitation to attend the opening the following morning.

> The event had been advertised in newspapers and through direct-by-mail literature. Many distributors and dealers arranged parties for the opening. A greater number provided radio programs. . . . In some cities the mayor was invited to open the box. In various stores, pirates swashbuckled inside and outside the sales rooms, and rode on floats with jazz bands.
>
> Promptly at 11 o'clock that morning, in the presence of crowds of onlookers, numbering from 200 to 800 each, the chests were unlocked and disclosed the new All-Steel G.E. Refrigerator.[15]

Special exhibition railroad cars toured the country, displaying refrigerators. Animated puppets danced in dealers' windows:

> The June ANIMATED Window Display dramatized the shortest 'short story' ever produced . . . and the action takes place in a realistic stage setting in the interior of the G-E refrigerator.
>
> Prologue: A BRIDE IN JUNE. Stage set consists of an illuminated cathedral interior during a wedding ceremony.
>
> Act I: A SERVANT IN SEPTEMBER. A revolving stage discloses a second illuminated set consisting of a wearied housewife in an old-fashioned kitchen without electrical conveniences.
>
> Act II: FREEDOM IN A G-E KITCHEN: The revolving stage shows a third set consisting of a glorified G-E Kitchen and the symbolical 'Freedom' figure [a vaguely-Grecian female with arms extended in a gesture of leaping joyousness].[16]

The millionth Monitor Top was presented to Henry Ford in a special radio broadcast in 1931, and another one was sent on a submarine voyage to the North Pole with Robert Ripley (the originator of 'Believe It or Not') in 1928. The most expensive media device of all was undertaken in 1935 – a film that told 'an interesting story in which comedy and romance are skillfully blended, all of which pivots on and revolves about the complete electric kitchen.' An anonymous publicist waxed ecstatic:

> It is of no avail to attempt to describe this picture, 'Three Women.' We can tell you that it is the most pretentious [*sic*], the most beautiful, the most effective commercial story ever told on the talking screen; that it is the first commercial Technicolor film ever made; that for gorgeous color and amazing realism it is on a par with outstanding examples of cinema artistry.[17]

The film ran for close to an hour and starred such Hollywood notables as Sheila Mannors and Hedda Hopper, Bert Roach and Johnny Mack Brown.

General Electric was not alone, either in these outlandish promotional schemes or in its effort to develop a successful compression refrigerator; the other major refrigerator manufacturers, just as anxious to attract consumer attention (especially during the straitened Depression years), were just as willing to spend money on advertising and promotion. The electric

utility companies, which were then in a most expansive and profitable phase of their history, cooperated in selling both refrigerators and the idea of mechanical refrigeration to their customers. By 1940 the market for household refrigerators was dominated by the four manufacturers of compression machines which had at their disposal the financial resources of enormous corporations: General Electric; Westinghouse, which began to manufacture refrigerators in 1930; Kelvinator, which was then owned by American Motors; and Frigidaire, which still belonged to General Motors.[18] Cross-licensing and mass-production techniques had made it possible for the manufacturers to lower their prices; installment plans and occasional price wars had made it possible for ever larger numbers of people to purchase refrigerators. Despite the Depression, and despite the still relatively high cost of refrigerators (when compared with other household appliances), roughly 45 percent of American homes were taking advantage of mechanical refrigeration by the time we entered the Second World War.[19]

The gas absorption machine

The manufacturers of gas absorption refrigerators were not idle during these years, but they lacked the large sums of money, the armies of skilled personnel, the competitive pressure, and the aggressive assistance of utility companies that the compression manufacturers had been able to command. When Stevenson surveyed the refrigeration business in 1923, he located eight prospective manufacturers of absorption refrigerators.[20] In the next several years, several of these went out of business – hardly surprising, since they had had little or no paid-in capital with which to work; the Common Sense Company, for example, was working with thirty thousand dollars in the same year in which Kelvinator had one million dollars.[21]

There seems to have been little question among knowledgeable people that the absorption refrigerator had the potential to be a superb machine for household use; and adjectives such as 'ingenious' and 'clever' were frequently appended to descriptions of gas refrigerators in the technical literature. 'Thousands of people have examined this machine, among them a large number of engineers; in fact, generally speaking, the more technical a person is, the greater is the appeal made by the machine,' wrote one commentator.[22] From the consumer's point of view, these refrigerators' chief advantages were that they were virtually silent (refrigerators with compressors once made a lot more noise than they do now – and they still hum noticeably); that, having few moving parts, they were potentially easy to maintain; and that operating costs could be kept fairly low, especially in locales where gas was cheaper than electricity. Stevenson's report on the Common Sense machine noted, for example:

> The salesman at the People's Gas Company in Chicago claims that they have sold about fifty of these machines. Some of them have been in service

> for two years, and he claims that they have no trouble or service calls. . . . Mr. Robertson of . . . [G.E.'s] Chicago office, says that this ice machine is different from any other that he has seen, in that it has no rotating parts, and the machine appears to be very simple to maintain.[23]

Yet the absorption machine, like the compression machine, was going to require expensive development and promotion before it could be made commercially successful; all the absorption machines that Stevenson located were water-cooled, and there was a public prejudice against the use of ammonia as a refrigerant. It remained to be seen whether anyone was going to undertake the developmental work, which would be both time consuming and expensive.

By 1926, when the American Gas Association met in Atlantic City for its annual convention, only three manufacturers of gas refrigerators remained in the field; and of these three, only one – Servel – would succeed in reaching the stage of mass production.[24] In the early 1920s, Servel (whose name stood for 'servant electricity') had been funded by a group of electric utility holding companies to manufacture and market compression refrigerators. But in 1925, it had purchased the American rights to the Swedish patents on the continuous absorption refrigerator, and had reorganized (with the injection of five million dollars from the financial interests that controlled the Consolidated Gas Company of New York) to devote itself principally to gas refrigeration.[25] Since it had a manufacturing plant already in existence when it purchased these new patents, it was able to commence production quickly; the Servel gas refrigerator went on the market in 1926 to the accompaniment of a good deal of publicity.

The other two manufacturers failed within a few years: they could neither compete with Servel nor sell the machines on which they held patents to any of the large corporations that might have had the resources to compete. The trials and tribulations of these small businesses are exemplified in the story of the SORCO refrigerator, which was one of the other two on display in Atlantic City in 1926.[26] SORCO was the creation of Stuart Otto, an engineer who had patented an absorption refrigerator in 1923. He owned a factory in Scranton, Pennsylvania, that produced dress forms for seamstresses, and persuaded twenty of the leading businessmen of Scranton to put up five thousand dollars apiece so that he could develop his machine and modify his factory to produce it. These early SORCO refrigerators were advertised in gas-industry periodicals ('Build Up Your Summer Load – and fill your daily valleys: Gas controlled entirely by time-switch to be set by your service man') and were sold to gas utility companies.[27] The results of the tests being more or less positive, Otto decided in the fall of 1926 that the time had come to attempt large-scale production:

> I was not able to raise the money from my stockholders when I informed them that $1,000,000 or more would be required. My only alternative was to buy out my stockholders. So I made an option agreement with them to pay them for their stock within a year. I then went about the country offering manufacturing companies non-exclusive licenses for the manufacture of my machines under our patents, of which some fifteen existed.
>
> I licensed Pathe Radio & Phonograph Co., Brooklyn, N.Y., Crocker Chair Company, Sheboygan, Wisconsin, Plymouth Radio & Phonograph Co., Plymouth, Wisconsin.
>
> Each of these companies paid me a cash down payment on signing of $25,000 and agreed to a guaranteed minimum of $35,000 per year royalty on a 5% of net sales, for 17 years.[28]

Otto had tried to interest General Electric and General Motors in his refrigerator. General Electric was, however, just about to bring out its own refrigerator; and General Motors had just purchased the patent rights on an English machine that utilized a solid rather than a liquid solvent.* Otto was trying to enter the national market with ludicrously small sums of money; the days in which David had any reasonable chance of succeeding against Goliath had long since passed. Within a few years, Otto was forced to acknowledge failure: 'Unfortunately . . . we were not financially able to carry the loads. After two years I managed to collect only a small portion of the accrued royalties.'[29]

Thus, Servel was essentially alone: from 1927 until 1956, (when it ceased production of refrigerators), it was the only major manufacturer of gas-absorption refrigerators in the United States. Never as highly capitalized as its competitors in the field of compression machinery (G.E., after all, had invested eighteen million dollars just in its production facilities in 1927, when Servel's entire assets amounted to not more than twelve million dollars), Servel had entered the market somewhat later than the other manufacturers and was never able to compete effectively. The gas utilities, notoriously conservative companies, were defending themselves against the encroachments of electricity and were not helpful; they complained that Servel was badly managed, that its refrigerators were more expensive than comparable electric machines, and that the lack of another manufacturer meant a lack of models with which to interest prospective customers.[30] Servel did not succeed in bringing out an air-cooled refrigerator until 1933, six or seven years after the electrics had done so; and by then the race was virtually lost. For all its virtues as a machine, the Servel, even in its peak years, never commanded more than 8 percent to 10 percent of the total market for mechanical refrigerators.[31]

The demise of the gas refrigerator was not the result of inherent deficiencies in the machine itself. The machine was not perfect when it was first brought on the market, but it was no less perfect than the compression machine, its rival. The latter succeeded for reasons that were

*This refrigerator, the Faraday, was marketed, on a limited basis, by G.M. in the mid-1930s; but, as it was water-cooled and very expensive, G.M. soon dropped it.

as much social and economic as technical; its development was encouraged by a few companies that could draw upon vast technical and financial resources. With the exception of Servel, none of the absorption manufacturers was ever able to finance the same level of development or promotion; and Servel never approached the capabilities of General Motors, General Electric, or Westinghouse. The compression refrigerator manufacturers came on the market earlier and innovated earlier, making it doubly difficult for competing devices to succeed. The fact that the electric utilities were in a period of growth and great profitability between 1920 and 1950, while the gas manufacturers and utility companies were defensive, conservative, and financially weak, cannot have helped matters either. If Stuart Otto had been able to obtain either capital or encouragement from the gas utilities, if Servel had been managed well enough to have innovated earlier, if either one of them had been able to command a chemical laboratory capable of discovering a new refrigerant, if there had been a sufficient number of gas-refrigerator manufacturers to have staged price wars, or license innovations to each other, or develop cooperative promotional schemes along with the gas-utility companies – well then, the vast majority of Americans might have absolutely silent and virtually indefatigable refrigerators in their kitchens. The machine that was 'best' from the point of view of the producer was not necessarily 'best' from the point of view of the consumer.

The profit motive and the alternative machine

The case of the gas refrigerator appears, in many particulars, to be structurally similar to the cases of many other aborted or abandoned devices intended for the household. There were, at one time, dozens of different kinds of washing machine: contraptions that simulated the action of a washboard; tubs with sieves that rotated inside fixed tubs filled with soapy water; tubs that rocked back and forth on a horizontal axis; motor-driven plungers that pounded the clothing inside a tub. All these washing machines yielded, during the 1920s and 1930s, to the agitator within the vertically rotated drum, because of the aggressive business practices of the Maytag Company which owned the rights to that design.[32] The central vacuum cleaner, which technical experts preferred, quickly lost ground to its noisier and more cumbersome portable competitor, in part because of the marketing techniques pioneered by door-to-door and store-demonstration salesmen employed by such firms as Hoover and Apex.[33]

Furthermore, many of the companies that pioneered successful household appliances had already developed a sound financial base manufacturing something else. Fedders, for example, made radiators for cars and airplanes before it made air conditioners; Regina made music boxes before it made vacuum cleaners; Maytag made farm implements; Sunbeam made scissors

and clippers for shearing sheep; Hoover made leather goods.[34] Alternatively, small companies with innovative ideas rarely succeeded unless they were purchased by, or made cooperative agreements with, much larger companies that had greater financial flexibility and the resources necessary to broach the national consumer market. Hotpoint belonged to General Electric, as did Edison Electric. Birdseye became part of General Foods; Norge, of Borg-Warner; Kelvinator, of American Motors. Bendix Home Appliances was a subsidiary of the Bendix Corporation, manufacturers of airplane parts. A larger corporation frequently purchased smaller ones or introduced new products when one (or several) of their old lines were failing. William C. Durant, of General Motors, for example, purchased Frigidaire because he wanted his salesmen to have something to sell when automobiles went off the consumer market during the First World War. Landers, Frary & Clark began to sell small appliances (under the name 'Universal') when their cutlery trade fell off. Westinghouse went into refrigerators as a cushion against the Depression. Maytag started making washing machines because of seasonal slacks in sales of farm machinery.[35]

By itself, the gas refrigerator would not have profoundly altered the dominant patterns of household work in the United States; but a reliable refrigerator, combined with a central vacuum-cleaning system, a household incinerator, a fireless cooker, a waterless toilet (otherwise known as an 'earth closet'), and individually owned fertilizer-manufacturing plants (otherwise known as 'garbage disposals that make compost') would certainly have gone a long way to altering patterns of household expenditure and of municipal services. We have compression, rather than absorption, refrigerators in the United States today not because one was technically better than the other, and not even because consumers preferred one machine (in the abstract) over the other, but because General Electric, General Motors, Kelvinator, and Westinghouse were very large, very powerful, very aggressive, and very resourceful companies, while Servel and SORCO were not. Consumer 'preference' can only be expressed for whatever is, in fact, available for purchase, and is always tempered by the price and convenience of the goods that are so available. At no time, in these terms, were refrigerators that ran on gas really competitive with those that ran on electric current.

In an economy such as ours in the United States, the first question that gets asked about a new device is not, Will it be good for the household – or even, Will householders buy it? but, rather, Can we manufacture it and sell it at a profit? Consumers do not get to choose among everything that they might like to have, but only among those things that manufacturers and financiers believe can be sold at a good profit. Profits are always the bottom line, and profits are partly compounded out of sales – but only partly. Profits are also compounded out of how much staff time has to be spent, whether a marketing arrangement is already in place, how easily manufacturing facilities can be converted, how reliably an item can be mass-produced – and similar considerations. General Electric became interested in refrigerators because it was experiencing financial difficulties after the First World War and needed to develop a new and different line of goods. G.E. decided to manufacture compression, rather than absorption, refrigerators because it stood to make more profits from exploiting its own designs and its own expertise than someone else's. Once having gone into the market for compression refrigerators, G.E. helped to improve that market, not just by its promotional efforts on its own behalf, but by the innovations that it could then sell to, or stimulate in, other manufacturers. And having done all that, G.E. helped to sound the death knell for the absorption machinery, since only a remarkable technical staff and a remarkable marketing staff, combined with an even more remarkable fluidity of capital, could have successfully competed with the likes of General Electric, Westinghouse, General Motors, and Kelvinator.

Notes

1 The account that follows is based upon Oscar Edward Anderson, Jr., *Refrigeration in America: A History of a New Technology and its Impact* (Princeton, N.J., 1953).
2 These figures come from U.S. Census Bureau data as quoted in Anderson, *Refrigeration* [1], pp. 114–115.
3 See 'Arnold H. Goss Ends His Life,' *Electric Refrigeration News* 25 (26 October 1938): 1, 2, 11. In addition to this article, my account of the origin of the Kelvinator is based on Anderson, *Refrigeration* [1], p. 195; obituary of Nathaniel B. Wales, *New York Times* (18 November 1974); J. W. Beckman, 'Copeland Tells Story of Household Refrigeration Development' *Air Conditioning, Heating and Refrigeration News* 6 (6 July 1932): 9–11; and Giedion, *Mechanization Takes Command* (New York, 1948), p. 602.
4 Beckman, 'Copeland Tells Story' [3].
5 Stevenson's report, 'Domestic Refrigerating Machines,' can be found, in its original typewritten form, in the Technical Data Library, General Electric Company, Schenectady, N.Y., Data File 1120. The original report was dated 17 August 1923, but many appendices were added in the ensuing five years, making a document that runs to several hundred pages. I was given access to it originally and will quote from it (citing it as (*DRM - GE*) through the kindness of Dr. George Wise, Corporate Research and Development, General Electric Company, Schenectady. The pagination in various sections of the report is not sequential. The complete list of companies and the report on their products is *DRM - GE*, vol. III.
6 *Electric Domestic Refrigeration, 1924,* a report of the Electric Domestic Refrigeration Committee, National Electric Light Association (New York, 1924), p. 2, table 1.
7 Letter, Francis C. Pratt to Gerard Swope, 17 August 1923, *DRM - GE* [5], p. 4.
8 Anderson, *Refrigeration* [1], chap. 11; 'Electrolux Inventors Receive Franklin Award,' *Gas Age* 70 (2 July 1932); 'Industry Pioneer Number,' *Air Conditioning, Heating and Refrigeration News* 19 (7 October 1936), passim.
9 See *Electric Domestic Refrigeration, 1924* [6], p. 2; and *The Facts About Gas Refrigeration Today,* American Gas Association (New York, 1933).
10 There is no scholarly history of General Electric; the best of the popular accounts is John Winthrop Hammond, *Men and Volts, The Story of General Electric* (Philadelphia, 1941),

the copyright on which was held by G.E. See also David G. Loth, *Swope of G.E.* (New York, 1958). On the history of G.E.'s refrigerator, see *DRM - GE* [5], Report 2, General Survey, Historical Introduction, pp. 1–2; and Report 1, Summary and Conclusions, Audiffren, pp. 16–19, and appendices 21 and 22.
11 See Loth, *Swope* [10], pp. 116–18; and letter from Pratt to Swope, 17 August 1923, *DRM - GE* [5].
12 *DRM - GE* [5], Report 1, Summary and Conclusions, Reasons for Exploitation, p. 24.
13 *DRM - GE* [5], Report 1, Summary and Conclusions, Reasons for Exploitation, p. 17.
14 'Outline History of the General Electric Household Refrigerator,' (typescript, Public Relations Dept. G.E., Schenectady, N.Y., 1970); 'G.E. Announces New Refrigerator,' *G.E. Monogram* (October 1925): 22; Ralph Roeder, 'General Electric Refrigerators' (typescript, Public Relations Dept., G.E., Schenectady N.Y., n.d.); and T. K. Quinn, *Giant Business, Threat to Democracy, The Autobiography of an Insider* (New York, 1956), chap. 8. Quinn was in charge of the refrigerator division of G.E. during the late 1920s and early 1930s.
15 'Door of All-Steel G.E. Refrigerator Slammed Shut 300,000 Times but Remains in Excellent Condition,' *G.E. Monogram* (April 1929): 25. *G.E. Monogram*, was an in-house magazine for G.E. employees.
16 'June Bride Animated Display,' *On the Top* 9 (June 1935): 23. *On the Top* was a newsmagazine of G.E.'s Specialty Appliance Department.
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18 For a summary of the refrigerators that were available in the late 1930s and their relative advantages and disadvantages, see John F. Wostrel and John G. Praetz, *Household Electric Refrigeration, Including Gas Absorption Systems* New York, 1938). For the relative market share of each manufacturer, see Frank Joseph Kottke, *Electrical Technology and the Public Interest* (Washington, D.C., 1944), pp. 168–70.
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20 *DRM - GE* [5], vol. III, appendices.
21 Ibid., especially appendices on 'Common Sense' and 'Kelvinator.'
22 H. B. Hull, *Household Refrigeration,* 3rd ed. (Chicago, 1927), p. 321.
23 *DRM - GE* [5], vol. III, appendix on 'Common Sense.'
24 'Survey of Gas Refrigerators.' *American Gas Journal* (2 April 1927), pp. 329–34.
25 This and subsequent summaries of the early history of Servel are based upon the following articles in the *New York Times:* 11 August 1925 (26:4); 22 December 1925 (28:2); 23 January 1926 (23:1); 17 March 1926 (32:2); 18 March 1926 (34:2); 15 October 1926 (34:2); 5 August 1927 (23:6); 3 January 1928 (36:2); 5 May 1928 (3:1); and 15 October 1929 (48:5); as well as upon the entries for Servel in *Moody's Manual of Investments* for 1928 and 1940.
26 The discussion that follows is based upon material in the *Stuart Otto Papers* (hereafter cited as SOP), Department of Manuscripts and University Archives, Cornell University (no. 2389); especially the typewritten documents, 'Household Refrigeration by Gas,' 26 June 1957 and 'Memorandum *re* Gas Refrigeration Corporation,' 19 June 1940.
27 Advertisers proof copy, *Gas-Age Record* [16 May 1925], *SOP* [26].
28 'Household Refrigeration by Gas,' pp. 1–2, *SOP* [26].
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30 The judgments made in this paragraph are based upon statements made by Stuart Otto

to various correspondents; see, for example, 'Report to United American Bosch Co., Spring, 1934,' typescript, *SOP* [26]. *The Facts About Gas Refrigeration Today* American Gas Association (New York, 1933), will give the reader some sense of the reluctance of gas utility companies to become actively involved in selling gas refrigerators.

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33 On the advantages of the central vacuum cleaner over the portable forms, see M. S. Cooley, *Vacuum Cleaning Systems* (New York, 1913), chap. 1. On the sales techniques of the portable vacuum cleaner manufacturers, see Frank G. Hoover, *Fabulous Dustpan: The Story of the Hoover Company* (New York, 1955); and Earl Lifshey, *The Housewares Story: A History of the American Housewares Industry* (Chicago, 1973), chap. 8.

34 The information in this sentence is derived from promotional material distributed by each of the companies mentioned; I am grateful to Richard Grant for helping me acquire these materials. See also Lifshey, *Housewares Story* [33], passim.

35 On Durant, see Mel Gustin, *Wild Billy: Wiliam C. Durant, Founder of General Motors* (Detroit, 1963), p. 187. On Landers, Frary and Clark, 'A History of Landers, Frary & Clark' (typescript) in *Dean S. Paden Collection* (no. 281), Baker Library, Harvard University. On Westinghouse, see 'Westinghouse Electric,' *Fortune* (February 1938), p. 45. On Maytag, see Jacob Swisher, 'The Evolution of Washday,' *Iowa Journal of History and Politics* 38 (1940): 39.

Moyra Doorly

A woman's place: Dolores Hayden on the 'grand domestic revolution'

In the last half of the 19th century and the first quarter of this one, there existed in the United States a remarkable school of feminist thought which tied together architecture and economics in a cogent social theory. The most basic cause of women's inequality, they argued, was the economic exploitation of women's labour by men. Women suffered from two of the fundamental characteristics of industrial capitalism: the physical separation of household space from public space and the economic separation of the domestic economy from the political economy.

These women – 'material feminists,' as they are dubbed in Dolores Hayden's classic study of their ideas – demanded a grand domestic revolution.* They wanted wages for housework. They set up new kinds of neighbourhood organisation – such as housewives' cooperatives which would undertake housework for payment. Most significant of all, they chivvied architects into exploring radical new types of building. They pushed architects and town planners into looking more intently at the effects of design on family life.

The central object of their campaigning was the need to socialise domestic work. They wanted all household labour and child care to become social labour, in home-like, nurturing neighbourhoods. They wanted neighbourhoods planned to provide laundry facilities, dining and cooking services and extensive child care facilities. In her book, *The Grand Domestic Revolution*, Dolores Hayden records their belief 'that women must create feminist homes with socialised housework and child care before they could become truly equal members of society.'

Two of the more influential women were Melusina Fay Peirce, and Charlotte Perkins Gilman. Melusina Fay Peirce laid out her proposals for cooperative housekeeping in 1868. She loved Cambridge, Massachusetts and after six years of marriage to a Harvard lecturer she described the 'costly and unnatural sacrifice' of her wider talents to 'the dusty drudgery of house ordering.' Her idea was that 'groups of 12–50 women would

**The Grand Domestic Revolution*, subtitled *A History of Feminist Designs for American Homes, Neighbourhoods And Cities*, by Dolores Hayden is published by the MIT Press.

The Social Shaping of Technology

How the refrigerator got its hum

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Noble

David F. Noble

Social choice in machine design: the case of automatically controlled machine tools

The technology: automatically controlled machine tools

The focus here is numerically controlled machine tools, a particular production technology of relatively recent vintage. According to many observers, the advent of this new technology has produced something of a revolution in manufacturing, a revolution which, among other things, is leading to increased concentration in the metalworking industry and to a reorganization of the production process in the direction of greater managerial control. These changes in the horizontal and vertical relations of production are seen to follow logically and inevitably from the introduction of the new technology. 'We will see some companies die, but I think we will see other companies grow very rapidly,' a sanguine president of Data Systems Corporation opined (Stephanz 1971). Less sanguine are the owners of the vast majority of the smaller metalworking firms which, in 1971, constituted 83 percent of the industry; they have been less able to adopt the new technology because of the very high initial expense of the hardware, and the overhead and difficulties associated with the software (ibid). In addition, within the larger, better endowed shops, where the technology has been introduced, another change in social relations has been taking place. Earl Lundgren, a sociologist who surveyed these shops in the late 1960s, observed a dramatic transfer of planning and control from the shop floor to the office (1969).

For the technological determinist, the story is pretty much told: numerical control leads to industrial concentration and greater managerial control over the production process. The social analyst, having identified the cause, has only to describe the inevitable effects. For the critical observer, however, the problem has merely been defined. This new technology was developed under the auspices of management within the large metalworking firms. Is it just a coincidence that the technology tends to strengthen the market position of these firms and enhance managerial authority in the shop? Why did this new technology take the form that it did, a form which seems to have rendered it accessible only to some firms, and why only this technology? Is there any other way to automate machine tools, a technology, for example, which would lend itself less to managerial control? To answer these questions, let us take a closer look at the technology.

A machine tool (for instance, a lathe or milling machine) is a machine used to cut away surplus material from a piece of metal in order to produce a part with the desired shape, size, and finish. Machine tools are really the guts of machine-based industry because they are the means whereby all machinery, including the machine tools themselves, are made. The machine tool has traditionally been operated by a machinist who transmits his skill and purpose to the machine by means of cranks, levers, and handles. Feedback is achieved through hands, ears, and eyes. Throughout the nineteenth century, technical advances in machining developed by innovative machinists built some intelligence into the machine tools themselves – automatic feeds, stops, throw-out dogs, mechanical cams – making them partially 'self-acting.' These mechanical devices relieved the machinist of certain manual tasks, but he retained control over the operation of the machine. Together with elaborate tooling – fixtures for holding the workpiece in the proper cutting position and jigs for guiding the path of the cutting tool – these design innovations made it possible for less skilled operators to use the machines to cut parts after they had been properly 'set up' by more skilled men; but the source of the intelligence was still the skilled machinist on the floor.

The 1930s and 1940s saw the development of tracer technology. Here patterns, or templates, were traced by a hydraulic or electronic sensing device which then conveyed the information to a cutting tool which reproduced the pattern in the workpiece. Tracer technology made possible elaborate contour cutting, but it was only a partial form of automation: for instance, different templates were needed for different surfaces on the same workpiece. With the war-spurred development of a whole host of new sensing and measuring devices, as well as precision servomotors which made possible the accurate control of mechanical motion, people began to think about the possibility of completely automating contour machining.

Automating a machine tool is different from automating, say, automotive manufacturing equipment, which is single-purpose, fixed automation, and cost-effective only if high demand makes possible a high product volume. Machine tools are general purpose, versatile machines, used primarily for small batch, low volume production of parts. The challenge of automating machine tools, then, was to render them self-acting while retaining their versatility. The solution was to develop a mechanism that translated electrical signals into machine motion and a medium (film, lines on paper, magnetic or punched paper tape, punched cards) on which the information could be stored and from which the signals could be reproduced.

The automating of machine tools, then, involves two separate processes. You need tape-reading and machine controls, a means of transmitting information from the medium to the machine to make the tables and cutting tool move as desired, and you need a means of getting the information on the medium, the tape, in the first place. The real challenge was the latter. Machine controls were just another step in a known direction, an extension of gunfire control technology developed during the war. The tape preparation was something new. The first viable solution was 'record-playback,' a system developed in 1946–1947 by General Electric, Gisholt, and a few smaller firms.[1] It involved having a machinist make a part while the motions of the machine under his command were recorded on magnetic tape. After the first piece was made, identical parts could be made automatically by playing back the tape and reproducing the machine motions. John Diebold, a management consultant and one of the first people to write about 'flexible automation,' heralded record-playback as 'no small achievement . . . it means that automatic operation of machine tools is possible for the job shop – normally the last place in which anyone would expect even partial automation' (1952:88). But record-playback enjoyed only a brief existence, for reasons we shall explore. (It was nevertheless immortalized as the inspiration for Kurt Vonnegut's *Player Piano*. Vonnegut was a publicist at GE at the time and saw the record-playback lathe which he describes in the novel.)

The second solution to the medium-preparation problem was 'numerical control' (N/C), a name coined by MIT engineers William Pease and James McDonough. Although some trace its history back to the Jacquard loom of 1804, N/C was in fact of more recent vintage; the brainchild of John Parsons, an air force subcontractor in Michigan who manufactured rotor blades for Sikorski and Bell helicopters. In 1949 Parsons successfully sold the air force on his ideas, and then contracted out most of the research work to the Servomechanisms Laboratory at MIT; three years later the first numerically controlled machine tool, a vertical milling machine, was demonstrated and widely publicized.

Record-playback was, in reality, a multiplier of skill, simply a means of obtaining repeatability. The intelligence of production still came from the machinist who made the tape by producing the first part. Numerical control, however, was based upon an entirely different philosophy of manufacturing. The specifications for a part – the information contained in an engineering blueprint – are first broken down into a mathematical representation of the part, then into a mathematical description of the desired path of the cutting tool along up to five axes, and finally into hundreds or thousands of discrete instructions, translated for economy into a numerical code, which is read and translated into electrical signals for the machine controls. The N/C tape, in short, is a means of formally circumventing the role of the machinist as the source of the intelligence of production. This new approach to machining was heralded by the National Commission on Technology, Automation, and Economic Progress as 'probably the most significant development in manufacturing since the introduction of the moving assembly line' (Lynn *et al.* 1966:89).

Choice in design: horizontal relations of production

This short history of the automation of machine tools describes the evolution of new technology as if it were simply a technical, and thus logical, development. Hence it tells us very little about why the technology took the form that it did, why N/C was developed while record-playback was not, or why N/C as it was designed proved difficult for the metalworking industry as a whole to absorb. Answers to questions such as these require a closer look at the social context in which the N/C technology was developed. In this section we will look at the ways in which the design of the N/C technology reflected the horizontal relations of production, those between firms. In the following section, we will explore why N/C was chosen over record-playback by looking at the vertical relations of production, those between labor and management.

To begin with, we must examine the nature of the machine-tool industry itself. This tiny industry which produces capital goods for the nation's manufacturers is a boom or bust industry that is very sensitive to fluctuations in the business cycle, experiencing an exaggerated impact of good times – when everybody buys new equipment – and bad times – when nobody buys. Moreover, there is an emphasis on the production of 'special' machines, essentially custom-made for users. These two factors explain much of the cost of machine tools: manufacturers devote their attention to the requirements of the larger users so that they can cash in on the demand for high-performance specialized machinery, which is very expensive due to high labor costs and the relatively inefficient low-volume production methods (see Rosenberg 1963; Wagoner 1968; Brown and Rosenberg 1961; Melman 1959). The development of N/C exaggerated these tendencies. John Parsons conceived of the new technology while trying to figure out a way of cutting the difficult contours of helicopter rotor blade templates to close tolerances; since he was using a computer to calculate the points for drilling holes (which were then filed together to make the contour) he began to think of having the computer control the actual positioning of the drill itself. He extended this idea to three-axis milling when he examined the specification for a wing panel for a new combat fighter. The new high-performance, high-speed aircraft demanded a great deal of difficult and expensive machining to produce airfoils (wing surfaces, jet engine blades), integrally stiffened wing sections for greater tensile strength and less weight, and variable thickness skins. Parsons took his idea, christened 'Cardomatic' after the IBM cards he used, to Wright Patterson Air Force Base and convinced people at the Air Material Command that the air force should underwrite the development of this potent new technology. When Parsons got the contract, he subcontracted with MIT's Servomechanism Laboratory, which had experience in gunfire control systems.[2] Between the signing of the initial contract in 1949 and 1959, when the air force ceased its formal

support for the development of software, the military spent at least $62 million on the research, development, and transfer of N/C. Up until 1953, the air force and MIT mounted a large campaign to interest machine-tool builders and the aircraft industry in the new technology, but only one company, Giddings and Lewis, was sufficiently interested to put their own money into it. Then, in 1955, N/C promoters succeeded in having the specifications in the Air Material Command budget allocation for the stockpiling of machine tools changed from tracer-controlled machines to N/C machines. At that time, the only fully N/C machine in existence was in the Servomechanism Lab. The air force undertook to pay for the purchase, installation, and maintenance of over 100 N/C machines in factories of prime subcontractors; the contractors, aircraft manufacturers, and their suppliers would also be paid to learn to use the new technology. In short, the air force created a market for N/C. Not surprisingly, machine-tool builders got into action, and research and development expenditure in the industry multiplied eight-fold between 1951 and 1957.

The point is that what made N/C possible – massive air force support – also helped determine the shape the technology would take. While criteria for the design of machinery normally includes cost to the user, here this was not a major consideration; machine-tool builders were simply competing to meet performance and 'competence' specifications for government-funded users in the aircraft industry. They had little concern with cost effectiveness and absolutely no incentive to produce less expensive machinery for the commercial market.

But the development of the machinery itself is only part of the story; there was also the separate evolution of the software. Here, too, air force requirements dictated the shape of the technology. At the outset, no one fully appreciated the difficulty of getting the intelligence of production on tape, least of all the MIT engineers on the N/C project, few of whom had had any machining experience before becoming involved in the project. Although they were primarily control engineers and mathematicians, they had sufficient hubris to believe that they could readily synthesize the skill of a machinist. It did not take them long to discover their error. Once it was clear that tape preparation was the stumbling block to N/C's economic viability, programming became the major focus of the project. The first programs were prepared manually, a tedious, time-consuming operation performed by graduate students, but thereafter efforts were made to enlist the aid of Whirlwind, MIT's first digital computer. The earliest programs were essentially subroutines for particular geometric surfaces which were compiled by an executive program. In 1956, after MIT had received another air force contract for software development, a young engineer and mathematician named Douglas Ross came up with a new approach to programming. Rather than treating each separate problem with a separate subroutine, the new system, called APT (Automatically Programmed Tools), was essentially a skeleton program – a 'systematized solution,' as it was called – for moving a cutting tool through space; this skeleton was to be 'fleshed out' for every particular application. The APT system was flexible and fundamental; equally important, it met air force specifications that the language must have a capacity for up to five-axis control. The air force loved APT because of its flexibility; it seemed to allow for rapid mobilization, for rapid design change, and for interchangeability between machines within a plant, between users and vendors, and between contractors and subcontractors throughout the country (presumably of 'strategic importance' in case of enemy attack). With these ends in mind, the air force pushed for standardization of the APT system and the Air Material Command cooperated with the Aircraft Industries Association Committee on Numerical Control to make APT the industry standard, the machine tool and control manufacturers followed suit, developing 'postprocessors' to adapt each particular system for use with APT.

Before long the APT computer language had become the industry standard, despite initial resistance within aircraft company plants. Many of these companies had developed their own languages to program their N/C equipment, and these in-house languages, while less flexible than APT, were nevertheless proven, relatively simple to use, and suited to the needs of the company. APT was something else entirely. For all its advantages – indeed, because of them – the APT system had decided disadvantages. The more fundamental a system is, the more cumbersome it is, and the more complex it is, the more skilled a programmer must be, and the bigger a computer must be to handle the larger amount of information. In addition, the greater the amount of information, the greater the chance for error. But initial resistance was overcome by higher level management, who had come to believe it necessary to learn how to use the new system 'for business reasons' (cost-plus contracts with the air force). The exclusive use of APT was enforced. Thus began what Douglas Ross himself has described as 'the tremendous turmoil of practicalities of the APT system development'; the system remained 'erratic and unreliable,' and a major headache for the aircraft industry for a long time.

The standardization of APT, at the behest of the air force, had two other interrelated consequences. First, it inhibited for a decade the development of alternative, simpler languages, such as the strictly numerical language NUFORM (created by A. S. Thomas, Inc.), which might have rendered contour programming more accessible to smaller shops. Second, it forced those who ventured into N/C into a dependence on those who controlled the development of APT,[3] on large computers and mathematically sophisticated programmers. The aircraft companies, for all their headaches, could afford to grapple with APT because of the air force subsidy, but commercial users were not so lucky. Companies that wanted military contracts were compelled to adopt the APT system, and those who could not afford the system, with its training requirements, its computer demands, and its headaches, were thus deprived of government jobs. The point here

is that the software system which became the de facto standard in industry had been designed with a user, the air force, in mind. As Ross explained, 'the universal factor throughout the design process is the economics involved. The advantage to be derived from a given aspect of the language must be balanced against the difficulties in incorporating that aspect into a complete and working system' (Ross 1978:13). APT served the air force and the aircraft industry well, but at the expense of less endowed competitors.

Choice in design: vertical relations of production

Thus far we have talked only about the form of N/C, its hardware and software, and how these reflected the horizontal relations of production. But what about the precursor to N/C, record-playback? Here was a technology that was apparently perfectly suited to the small shop: tapes could be prepared by recording the motions of a machine tool, guided by a machinist or a tracer template, without programmers, mathematics, languages, or computers.[4] Yet this technology was abandoned in favor of N/C by the aircraft industry and by the control manufacturers. Small firms never saw it. The Gisholt system, designed by Hans Trechsel to be fully accessible to machinists on the floor, was shelved once that company was bought by Giddings and Lewis, one of the major N/C manufacturers. The GE record-playback system was never really marketed since demonstrations of the system for potential customers in the machine-tool and aircraft companies elicited little enthusiasm. Giddings and Lewis did in fact purchase a record-playback control for a large profile 'skin mill' at Lockheed but switched over to a modified N/C System before regular production got underway. GE's magnetic tape control system, the most popular system in the 1950s and 1960s, was initially described in sales literature as having a 'record-playback option,' but mention of this feature soon disappeared from the manuals, even though the system retained the record-playback capacity.[5]

Why was there so little interest in this technology? The answer to this question is complicated. First, air force performance specifications for four- and five-axis machining of complex parts, often out of difficult materials, were simply beyond the capacity of either record-playback or manual methods. In terms of expected cost reductions, moreover, neither of these methods appeared to make possible as much of a reduction in the manufacturing and storage costs of jigs, fixtures, and templates as did N/C. Along the same lines, N/C also promised to reduce more significantly the labor costs for toolmakers, machinists, and patternmakers. And, of course, the very large air force subsidization of N/C technology lured most manufacturers and users to where the action was. Yet there were still other, less practical, reasons for the adoption of N/C and the abandonment of record-playback, reasons that have more to do with the ideology of engineering than with economic calculations. However useful as a production technology, record-playback was considered quaint from the start, especially with the advent of N/C. N/C was always more than a technology for cutting metals, especially in the eyes of its MIT designers, who knew little about metalcutting: it was a symbol of the computer age, of mathematical elegance, of power, order, and predictability, of continuous flow, of remote control, of the automatic factory. Record-playback, on the other hand, however much it represented a significant advance on manual methods, retained a vestige of traditional human skills; as such, in the eyes of the future (and engineers always confuse the present and the future) it was obsolete.

The drive for total automation which N/C represented, like the drive to substitute capital for labor, is not always altogether rational. This is not to say that the profit motive is insignificant – hardly. But economic explanations are not the whole story, especially in cases where ample government financing renders cost-minimization less of an imperative. Here the ideology of control emerges most clearly as a motivating force, an ideology in which the distrust of the human agency is paramount, in which human judgment is construed as 'human error.' But this ideology is itself a reflection of something else: the reality of the capitalist mode of production. The distrust of human beings by engineers is a manifestation of capital's distrust of labor. The elimination of human error and uncertainty is the engineering expression of capital's attempt to minimize its dependence upon labor by increasing its control over production. The ideology of engineering, in short, mirrors the antagonistic social relations of capitalist production. Insofar as the design of machinery, like machine tools, is informed by this ideology, it reflects the social relations of production.[6] Here we will emphasize this aspect of the explanation – why N/C was developed and record-playback was not – primarily because it is the aspect most often left out of such stories.

Ever since the nineteenth century, labor-intensive machine shops have been a bastion of skilled labor and the locus of considerable shop-floor struggle. Frederick Taylor introduced his system of scientific management in part to try to put a stop to what he called 'systematic soldiering' (now called 'pacing'). Workers practiced pacing for many reasons: to keep some time for themselves, to exercise authority over their own work, to avoid killing 'gravy' piece-rate jobs by overproducing and risking a rate cut, to stretch out available work for fear of layoffs, to exercise their creativity and ingenuity in order to 'make out' on 'stinkers' (poorly rated jobs), and, of course, to express hostility to management (see articles by Roy; Mathewson 1969). Aside from collective cooperation and labor-prescribed norms of behavior, the chief vehicle available to machinists for achieving shop-floor control over production was their control over the machines. Machining is not a handicraft skill but a machine-based skill; the possession

of this skill, together with control over the speeds, feeds, and motions of the machines, enables machinists alone to produce finished parts to tolerance (Montgomery 1976b). But the very same skills and shop-floor control that made production possible also make pacing possible. Taylor therefore tried to eliminate soldiering by changing the process of production itself, transferring skills from the hands of machinists to the handbooks of management; this, he thought, would enable management, not labor, to prescribe the details of production tasks. He was not altogether successful. For one thing, there is still no absolute science of metalcutting and methods engineers, time-study people, and Method Time Measurement (MTM) specialists – however much they may have changed the formal processes of machine-shop practice – have not succeeded in putting a stop to shop-floor control over production.[7]

Thus, when sociologist Donald Roy went to work in a machine shop in the 1940s, he found pacing alive and well. He recounts an incident that demonstrates how traditional patterns of authority rather than scientific management still reigned supreme:

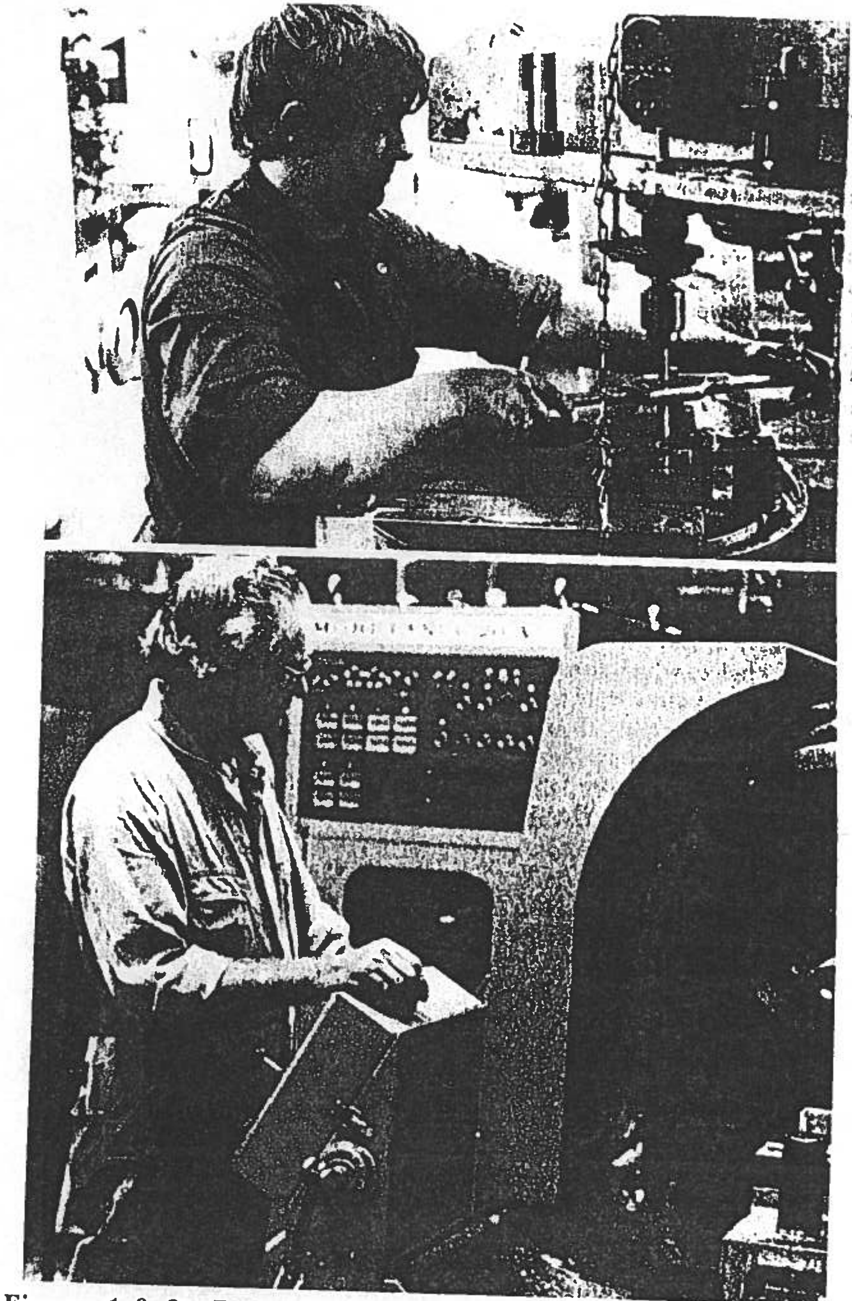
> 'I want 25 or 30 of those by 11 o'clock,' Steve the superintendent said sharply, a couple of minutes after the 7:15 whistle blew. I [Roy] smiled at him agreeably. 'I mean it,' said Steve, half smiling himself, as McCann and Smith, who were standing near us, laughed aloud. Steve had to grin in spite of himself and walked away. 'What he wants and what he is going to get are two different things,' said McCann. (1953:513)

Thirty years later, sociologist Michael Burawoy returned to the same shop and concluded, in his own study of shop-floor relations, that 'in a machine shop, the nature of the relationship of workers to their machines rules out coercion as a means of extracting surplus' (1976).

This was the larger context in which the automation of machine tools took place; it should be seen, therefore, as a further managerial attempt to wrest control over production from the shop-floor work force. As Peter Drucker once observed, 'What is today called automation is conceptually a logical extension of Taylor's scientific management' (1967:26). Thus it is not surprising that when Parsons began to develop his N/C 'Cardomatic' system, he took care not to tell the union (the UAW) in his shop in Traverse City about his exciting new venture. At GE (Schenectady), a decade of work-stoppages over layoffs, rate cuts, speed-ups, and the replacement of machinists with less skilled apprentices and women during the war, culminated in 1946 in the biggest strike in the company's history, led by machinists in the United Electrical Workers (UE) and bitterly opposed by the GE Engineers' Association. GE's machine-tool automation project, launched by these engineers soon afterward, was secret, and although the project had strong management support, publicist Vonnegut recalled, with characteristic understatement, that 'they wanted no publicity this time.'[8]

During the first decade of machine-tool automation development, the aircraft industry – the major user of automatic machine tools – also experienced serious labor trouble as the machinists and auto workers competed to organize the plants. The postwar depression had created discontent among workers faced with layoffs, company claims of inability to pay, and massive downward reclassifications (Allen and Schneider 1956). Major strikes took place at Boeing, Bell Aircraft (Parsons' prime contractor), McDonnell Douglas, Wright Aeronautical, GE (Evandale) (jet engines), North American Aviation, and Republic Aircraft. It is not difficult, then, to explain the popularity among management and technical men of a November 1946 *Fortune* article entitled 'Machines Without Men.' Surveying the technological fruits of the war (sensing and measuring devices, servomechanisms, computers, etc.), two Canadian physicists promised that 'these devices are not subject to any human limitations. They do not mind working around the clock. They never feel hunger or fatigue. They are always satisfied with working conditions, and never demand higher wages based on the company's ability to pay.' In short, 'they cause much less trouble than humans doing comparable work' (Leaver and Brown 1946:203).

One of the people who was inspired by this article was Lowell Holmes, the young electrical engineer who directed the GE automation project. However, in record-playback, he developed a system for replacing machinists that ultimately retained machinist and shop-floor control over production because of the method of tape preparation.[9] This 'defect' was recognized immediately by those who attended the demonstration of the system; they showed little interest in the technology. 'Give us something that will do what we say, not what we do,' one of them said. The defects of record-playback were conceptual, not technical; the system simply did not meet the needs of the larger firms for managerial control over production. N/C did. 'Managers like N/C because it means they can sit in their offices, write down what they want, and give it to someone and say, "do it,"' the chief GE consulting engineer on both the record-playback and N/C projects explained. 'With N/C there is no need to get your hands dirty, or argue' (personal interview). Another consulting engineer, head of the Industrial Applications Group which served as intermediary between the research department and sales department at GE (Schenectady) and a key figure in the development of both technologies, explained the shift from record-playback to N/C: 'Look, with record-playback the control of the machine remains with the machinist – control of feeds, speeds, number of cuts, output; with N/C there is a shift of control to management. Management is no longer dependent upon the operator and can thus optimize the use of their machines. With N/C, control over the process is placed firmly in the hands of management – and why shouldn't we have it?' (personal interview). It is no wonder that at GE, N/C was often referred to as a management system, not as a technology of cutting metals.



Figures 1 & 2 *From skilled craftworker to buttonpusher?*
(Photo by permission of the Australian Metal Workers Union).

Numerical control dovetailed nicely with larger efforts to computerize company operations, which also entailed concentrating the intelligence of manufacturing in a centralized office. In the intensely anti-Communist 1950s, moreover, as one former machine-tool design engineer has suggested, N/C looked like a solution to security problems, enabling management to remove blueprints from the floor so that subversives and spies couldn't get their hands on them. N/C also appeared to minimize the need for costly tooling and it made possible the cutting of complex shapes that defied manual and tracer methods, and reduced actual chip-cutting time. Equally important, however, N/C replaced problematic time-study methods with 'tape time' – using the time it takes to run a cycle as the base for calculating rates – replaced troublesome skilled machinists with more tractable 'button-pushers,' and eliminated once and for all the problem of pacing. If, with hindsight, N/C seems to have led to organizational changes in the factory, changes which enhanced managerial control over production, it is because the technology was chosen, in part, for just that purpose.[10]

Reality on the shop floor

Although the evolution of a technology follows from the social choices that inform it, choices which mirror the social relations of production, it would be an error to assume that in having exposed the choices, we can simply deduce the rest of reality from them. Reality cannot be extrapolated from the intentions that underlie the technology any more than from the technology itself.[11] Desire is not identical to satisfaction. . . .

The introduction of N/C was not uneventful, especially in plants where the machinists' unions had a long history. Work stoppages and strikes over rates for the new machines were common in the 1960s, as they still are today. There are also less overt indications that management dreams of automatic machinery and a docile, disciplined work force but they have tended to remain just that. Here we will examine briefly three of management's expectations: the use of 'tape time' to set rates; the deskilling of machine operators; and the elimination of pacing. . . .

[In the extract chosen, only the second of these points is discussed.]

In reality, N/C machines do not run by themselves – as the United Electrical Workers argued in its 1960 *Guide to Automation*, the new equipment, like the old, requires a spectrum of manual intervention and careful attention to detail, depending upon the machine, the product, and so on.

The deskilling of machine operators has, on the whole, not taken place as expected, for two reasons. First, as mentioned earlier, the assigning of labor grades and thus rates to the new machinery was, and is, a hotly contested and unresolved issue in union shops. Second, in union and nonunion shops alike, the determination of skill requirements for N/C must take into account the actual degree of automation and reliability of the machinery. Management has thus had to have people on the machines who know what they are doing simply because the machines (and programming) are not totally reliable; they do not run by themselves and produce good finished parts. Also, the machinery is still very expensive (even without microprocessors) and thus so is a machine smash-up. Hence, while it is true that many manufacturers initially tried to put unskilled people on the new equipment, they rather quickly saw their error and upgraded the classification. (In some places the most skilled people were put on the N/C machines and given a premium but the lower formal classifications were retained, presumably in the hope that someday the skill requirements would actually drop to match the classification – and the union would be decertified.) The point is that the intelligence of production has neither been built entirely into the machinery nor been taken off the shop floor. It remains in the possession of the work force.

Notes

1 The discussion of the record-playback technology is based upon extensive interviews and correspondence with the engineers who participated in the projects at General Electric (Schenectady) and Gisholt (Madison, Wisconsin), and the trade journal and technical literature.

2 This brief history of the origins of N/C is based upon interviews with Parsons and MIT personnel, as well as the use of Parsons' personal files and the project records of the Servomechanism Laboratory.

3 The air force funded development of APT was centered initially at MIT. In 1961 the effort was shifted to the Illinois Institute of Technology Research Institute (IITRI) where it has been carried on under the direction of a consortium composed of the air force, the Aircraft Industries Association (AIA), and major manufacturers of machine tools and electronic controls. Membership in the consortium has always been expensive, beyond the financial means of the vast majority of firms in the metalworking industry. APT system use, therefore, has tended to be restricted to those who enjoyed privileged access to information about the system's development. Moreover, the APT system has been treated as proprietary information within user plants; programmers have had to sign out for manuals and have been forbidden from taking them home or talking about their contents with people outside the company.

4 Technically, record-playback was as reliable as N/C, if not more so – since all the programming was done at the machine, errors could be eliminated during the programming process, before production began. Moreover, it could be used to reproduce parts to within a tolerance of a thousandth of an inch, just like N/C. (It is a common mistake to assume that if an N/C control system generates discrete pulses corresponding to increments of half a thousandth, the machine can produce parts to within the same tolerances. In reality, the limits of accuracy are set by the machine itself – not to mention the weather – rather than by the electrical signals.)

5 This history is based upon interviews with Hans Trechsel, designer of Gisholt's 'Factrol' system, and interviews and correspondence with participating engineering and sales

personnel at GE (Schenectady), as well as articles in various engineering and trade journals.

6 It could be argued that control in the capitalist mode of production is not an independent factor (a manifestation of class conflict), but merely a means to an economic end (the accumulation of capital). Technology introduced to increase managerial control over the work force and eliminate pacing is, in this view, introduced simply to increase profits. Such reductionism, which collapses control and class questions into economistic ones, renders impossible any explanation of technological development in terms of social relations or any careful distinction between productive technology which directly increases output per person-hour and technology which does so only indirectly by reducing worker resistance or restriction of output. Finally, it makes it hard to distinguish a technology that reduces pacing from a gun in the service of union-busting company agents; both investments ultimately have the same effect and the economic results look the same on the balance sheet. As Jeremy Brecher reminds us, 'The critical historian must go behind the economic category of cost-minimization to discover the social relations that it embodies (and conceals)' (1978).

7 The setting of rates on jobs in machine shops is still more of a guess than a scientific determination. This fact is not lost on machinists, as their typical descriptions of the methods-men suggests: 'They ask their wives, they don't know; they ask their children, they don't know; so they ask their friends.' Of course, this apparent and acknowledged lack of scientific certainty comes into play during bargaining sessions over rates, when 'fairness' and power, not science, determine the outcome.

8 Kurt Vonnegut, letter to author, February 1977.

9 The fact that record-playback lends itself to shop-floor control of production more readily than N/C is borne out by a study of N/C in the United Kingdom done by Erik Christiansen in 1968. Only in those cases where record-playback or plugboard controls were in use (he found six British-made record-playback jig borers) did the machinist keep the same pay scale as with conventional equipment and retain control over the entire machining process. In Christiansen's words, record-playback (and plugboard programming) 'mean that the shop floor retains control of the work cycle through the skill of the man who first programmed the machine' (1968:27, 31).

10 The cost effectiveness of N/C depends upon many factors, including training costs, programming costs, computer costs, and the like, beyond mere time saved in actual chip-cutting or reduction in direct labor costs. The MIT staff who conducted the early studies on the economics of N/C focused on the savings in cutting time and waxed eloquent about the new revolution. At the same time, however, they warned that the key to the economic viability of N/C was a reduction in programming (software) costs. Machine-tool company salesmen were not disposed to emphasize these potential drawbacks, though, and numerous users went bankrupt because they believed what they were told. In the early days, however, most users were buffered against such tragedy by state subsidy. Today, potential users are somewhat more cautious, and machine-tool builders are more restrained in their advertising, tempering their promise of economic success with qualifiers about proper use, the right lot and batch size, sufficient training, etc.

For the independent investigator, it is extremely difficult to assess the economic viability of such a technology. There are many reasons for this. First, the data is rarely available or accessible. Whatever the motivation – technical fascination, keeping up with competitors, etc. – the purchase of new capital equipment must be justified in economic terms. But justifications are not too difficult to come by if the item is desired enough by the right people. They are self-interested anticipations and thus usually optimistic ones. More important, firms rarely conduct postaudits on their purchases, to see if their justifications were warranted. Nobody wants to document his errors and if the machinery is fixed in its foundation, that is where it will stay, whatever a postaudit reveals; you learn to live with it. The point here is that the economics of capital equipment is not nearly so tidy as economists would sometimes have us believe. The invisible hand has to do quite a bit of sweeping up after the fact.

If the data does exist, it is very difficult to get a hold of. Companies have a proprietary interest in the information and are wary about disclosing it for fear of revealing (and thus jeopardizing) their position vis à vis labor unions (wages), competitors (prices), and government (regulations and taxes). Moreover, the data, if it were accessible, is not all tabulated and in a drawer somewhere. It is distributed among departments, with separate budgets, and the costs to one are the hidden costs to the others. Also, there is every reason to believe that the data that does exist is self-serving information provided by each operating unit to enhance its position in the firm. And, finally, there is the tricky question of how 'viability' is defined in the first place. Sometimes, machines make money for a company whether they were used productively or not.

The purpose of this aside is to emphasize the fact that 'bottom-line' explanations for complex historical developments, like the introduction of new capital equipment, are never in themselves sufficient, nor necessarily to be trusted. If a company wants to introduce something new, it must justify it in terms of making a profit. This is not to say, however, that profit making was its real (or, if so, its only) motive or that a profit was ever made. In the case of automation, steps are taken less out of careful calculation than on the faith that it is always good to replace labor with capital, a faith kindled deep in the soul of manufacturing engineers and managers (as economist Michael Piore, among others, has shown. See, for example, Piore 1968). Thus, automation is driven forward, not simply by the profit motive, but by the ideology of automation itself, which reflects the social relations of production.

11 This is an error that Braverman tended to make in discussing N/C.

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11

Cynthia Cockburn

The material of male power

A skilled craftsman may be no more than a worker in relation to capital, but seen from within the working class he has been a king among men and lord of his household. As a high earner he preferred to see himself as the sole breadwinner, supporter of wife and children. As artisan he defined the unskilled workman as someone of inferior status, and would 'scarcely count him a brother and certainly not an equal' (Berg, 1979: 121). For any socialist movement concerned with unity in the working class, the skilled craftsman is therefore a problem. For anyone concerned with the relationship of class and gender, and with the foundations of male power, skilled men provide a fertile field for study.

Compositors in the printing trade are an artisan group that have long defeated the attempts of capital to weaken the tight grip on the labour process from which their strength derives. Now their occupation is undergoing a dramatic technological change initiated by employers. Introduction of the new computerized technology of photocomposition represents an attack on what remains of their control over their occupation and wipes out many of the aspects of the work which have served as criteria by which 'hot metal' composition for printing has been defined as a manual skill and a man's craft.[1]

In this paper I look in some detail at the compositors' crisis, what has given rise to it and what it may lead to in future. Trying to understand it has led me to ask questions in the context of socialist-feminist theory. These I discuss first, as preface to an account of key moments in the compositors' craft history. I then isolate the themes of *skill* and *technology* for further analysis, and conclude with the suggestion that there may be more to male power than 'patriarchal' relations.

Producing class and gender

The first difficulty I have encountered in socialist-feminist theory is one that is widely recognized: the problem of bringing into a single focus our experience of both class and gender. Our attempts to ally the Marxist theory of capitalism with the feminist theory of 'patriarchy' have till now been unsatisfactory to us (Hartmann, 1979a).

Foray . Hilaire Perez

THE ECONOMICS OF OPEN TECHNOLOGY: COLLECTIVE ORGANIZATION AND INDIVIDUAL CLAIMS IN THE " FABRIQUE LYONNAISE " DURING THE OLD REGIME

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prepared for the Conference in honor of Paul A.David,
Turin (Italy), May 2000

warning: first draft

INTRODUCTION

What we call "open knowledge" is a system in which the principles of rapid disclosure of new knowledge are predominant, and in which a number of procedures facilitate and reinforce the circulation not only of codified knowledge but also of practical knowledge and research tools. It is not pure chance that in this world new knowledge is codified and carefully systematized in order to facilitate its transmission and discussion. But in this world particular attention is also paid to the reproduction of knowledge, that is, to learning. It is not because knowledge flows freely – in the form of manuals and codified instructions – that it is necessarily reconstituted from one place to the next. It is also necessary to create and maintain relationships between "masters and apprentices", either in the context of work communities or in that of formal processes of teaching practical knowledge.

11 - From open science....

The economic analysis of open knowledge has been particularly developed in the field of scientific research thanks to the seminal works of Dasgupta and David (1994) and David (1998 and 1999). The approach of the so called " new economics of science " provides the great advantage to make two important arguments for theoretical analysis as well as policy implication in the field of the economics of knowledge:

- Firstly, knowledge openness and sharing behaviours do not only express some kinds of ethics or moral attitude (although ethical conviction plays certainly a role). Knowledge openness is viewed, above all, as a mechanism generating economic efficiency that people in certain circumstances are willing to implement and maintain in order to be a player of a positive sum game. In fact, knowledge openness which entails rapid and complete distribution facilitates coordination between agents, reduces risks of duplication between research projects, functions as a sort of " quality assurance "in so far as disclosed results can be reproduced and verified by other members of the community and, above all, by propagating knowledge within a heterogeneous population of researchers and entrepreneurs, increases the probability of later discoveries and inventions and decreases the risk of this knowledge falling into the hands of agents incapable of exploiting its potential (David & Foray, 1995).[1]

- Secondly, open knowledge does not mean the absence of any individual incentives. In the case of open science an ingenious mechanism comes into play, consisting of the granting of moral property rights which are not concretized in exclusivity rights (in other words, they are compatible with the complete disclosure norm). It is the priority rule which identifies the author of the discovery as soon as s/he publishes and which thus determines the constitution of "reputation capital", a decisive element when it comes to obtaining grants. This mechanism creates contexts of races (or tournaments), while ensuring that results are disclosed. It is a remarkable device since it allows for the creation of private assets, a form of intellectual property, resulting from the very act of foregoing exclusive ownership of the knowledge concerned. Here the need to be identified and recognized as "the one who discovered" forces people to release new knowledge quickly and completely. In this sense the priority rule is a highly effective device that offers non-market incentives to the production of public goods [Dasgupta & David, 1994; Callon & Foray, 1997]. This form of organization is particularly efficient for it ensures the rapid and complete diffusion of new knowledge, while preserving a certain level of incentive. [2]

[1] In economic term, since the marginal cost of use of knowledge is nil, maximum efficiency in its use implies that there is no restriction to access and that the price of use is equal to 0. Knowledge should be a "free" good; that is the condition for optimum use of a non-rival good.

[2] - These "good properties" have recently been modelled by David [1998], who shows how the disclosure norm positively influences the cognitive performance of the system under consideration. David models stochastic interactions in a group of rational researchers individually engaged in a continuous process of experimental observation, information exchange and revision of choices in relation to locally constituted majorities. This modelling is then used to link up micro-behaviours (being open, being closed) and macro-performances. Simulations suggest that the social norm of openness, which influences micro-behaviours, favours free entry into knowledge networks and in so doing prevents them from closing in on themselves too quickly and excluding different opinions. David shows that a system situated beyond the critical openness threshold ensures confrontation of ideas and provides a mechanism which guarantees the production of consensus and preserves the diversity of opinions. The capacity to collectively produce scientific statements while preserving a degree of diversity of opinions and arguments is thus an important feature in an open research network, and standards of disclosure and openness appear to be decisive in the cognitive performances of the network. The advantage of such an approach is that it produces formal results, derived from the mathematical theory of percolation, on the basis of which more political reflection can be envisaged:

Of course, the ideal world of openness described here does not exclude the possibility of bending or departing from the rules. On the contrary, the tournament contexts created by the priority rule, as well as the size of related rewards, tend to encourage bad conduct. The notion of "open science" is therefore based on an ideal never achieved (in other words, there will always be many cases of various degrees of retention). It is nevertheless still part of the "scientific culture" and as such influences researchers' behaviour. It is a type of prescriptive norm which, all things considered, facilitates the formation of cooperative networks[3].

12 ...to open technology

We have discussed "open science" because it is probably the organization of science that is closest to this standard of openness. Yet in the past there have been numerous cases of "open technology", albeit limited in time and space. Historically, most situations of openness were linked to a specific territory: Lyons in the case of the circulation of techniques and inventions relating to the silk industry [Hilaire Perez, 1994] and Lancashire in the case of collective invention in the metallurgical industry [Allen, 1983]. More recent cases are those of emerging industries such as virtual reality (Swann, 1999) or financial software (Crede and Steinmueller, 1999).

The historical analysis of open technology - and the particular case of the " fabrique lyonnaise " to which this paper is devoted - allows to draw a parallel with the economics of open science:

- Firstly, the existence of a collective ethics greatly matters (see below: § 323).
- Secondly, the efficiency properties of systems of open technology are rather similar to the efficiency of open science: in both cases this is a similar way to increase the performance of a system of invention in making the existing stock of knowledge more socially useful by improving transfer, transformation and access to the existing innovations.

- the size of the network is important (the smaller the network, the greater the risk of it rapidly becoming trapped in one of those "absorbing states", i.e. in a situation of complete agreement of all agents, from which it is difficult to collectively withdraw);
- the network can tolerate certain shortcomings and divergence compared to the openness norm. In other words, the same cognitive performance is guaranteed as long as the network is above a certain critical threshold. Cooperative behaviour can emerge and be maintained without everyone complying perfectly with the openness standard.

[3] - In this sense we cannot follow Latour (1987) who has come to portray the Mertonian norms as apologetic ideology, self-serving myths about cooperative, dis-interested science. In Dasgupta and David (1994) it is argued that the norms are prescriptive, and that beliefs that are instilled in scientists as part of the " culture of science " have effect on their behavior -- making it easier to form cooperative networks where it is in their mutual interest (and that of society at large) to organize research cooperatively.

- Thirdly, similar collective belief to be part of a positive sum game plays a key role as well. Such a common knowledge that open technology is a positive sum game is particularly effective and " has a force " in the case of Lyon since the place of Lyon is engaged in an international competition with London and the inventors know well that the prosperity of the local system to which they belong directly influences their own individual prosperity.

- Fourthly, both the collective ethics and the common knowledge about the efficiency of open technology are not enough to sustain a system based on the free dissemination of knowledge. There is also a need for some kinds of mechanisms aiming at rewarding inventors without granting exclusivity rights. Particular mechanisms were designed to reward inventors who accept to disclose their knowledge and to actively participate to the reproduction of that knowledge (teaching). The setting up of a rewarding fund, the double process of examination of inventions as well as the system of financial bonuses rewarded to those who accept not only to disclose but also to teach their knowledge are institutional mechanisms which make the system quite effective. The " collective fabrique " appears, however, very fragile and quite vulnerable to individual claims, frustrations, hopes of individuals.

This is probably the main argument who would like to discuss: beyond the beauty of systems of collective invention and the nice economic performance such systems can produce, the dimension of individual incentives remains decisive and calls for great institutional mechanisms to give credit to the inventors without granting them with some kinds of exclusivity. A kind of mechanism Dasgupta and David have explored in the case of open science and which remains quite uncertain yet in the case of open technology although the case of the " fabrique lyonnaise " provides some ideas about it[4]..

2 - INVENTION AND INSTITUTION DURING THE OLD REGIME IN FRANCE

The historical meaning of invention is being more and more distinguished from the myths of the origins for explaining technical change in craft and industry : the glory of pionners leading the

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[4] - As defined in a recent research project (Foray & Steinmueller, 1999) the type of open knowledge we are dealing with in this paper is different from the collusive and explicit forms of collective invention (such as high technology consortiums) which require explicit coordination mechanisms as well as the formalization of agreements on both the distribution of tasks and the attribution of results. Moreover collusive forms delimit semi-private areas for the circulation and pooling of knowledge, which may in some cases be less open than informal networks we are studying here. The main difference between these two types of collective enterprise deals with the mode of production of knowledge. In the cases studied here, trading or sharing concerns knowledge that is already available. The participants do not participate in a coordinated research project; they trade or share existing technical data. This is an incremental

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2 - INVENTION AND INSTITUTION DURING THE OLD REGIME IN FRANCE

The historical meaning of invention is being more and more distinguished from the myths of the origins for explaining technical change in craft and industry : the glory of pionners leading the way thanks to inventions disseminating in the whole economy has lost its virtue for understanding what invention and innovation meant in history. This mythology has, in fact, become a historical field per se.

Authors rather focus on the sketchy nature of invention, a notion that was already expressed by actors in the past, at least in modern Europe. During the Renaissance and then in the Enlightenment, theaters of machines and Diderot and d'Alembert's Encyclopédie presented invention like re-discovering, re-combining and re-using existing devices, materials or patterns. Learning, imitating and excelling in one's craft were the best ways to invent. There existed a method, an "art" of invention, as Luisa Dolza and Hélène Vérin wrote. Moreover, the Encyclopédie emphasised that inventions were reaching efficiency only when they were put in practice by users amending them and contriving further devices. Improving was central.

The reality seemed in tune with these literary and theoretical approaches. In craft world, partnerships, kinship and subcontracting played an important part in innovating. In scientific world, experimental proofs, such in academic societies, were also leading to improvements and to contriving new tools for testing inventions. Invention was then a collective and cumulative process; it dealt with borrowing, inheriting, collaborating and sharing devices and know-how.

During the Old Regime, in France, the institutional framework matched these innovative processes. The individual dimensions of the English patents for instance were not yet established ; private benefit deriving from inventions was balanced by the efficiency of a whole set of collective institutions, ranging from societies for encouragement (also very strong in England) to

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governmental and local systems of rewards. The protection of inventors was based on very sophisticated procedures ; depositing, collecting, disclosing, transmitting knowledge were the basis of any official grant. Inventors had to deserve the grant and to make the invention a common wealth. They never managed innovation on ther own. Invention was a collective concern, even a civic concern, and it was embedded in open technology policies. As "the cultural matrix, the speech communities and the priorities placed upon knowledge and learning are now seen to have provided new dimensions of creativity", the collective dimensions of invention are highlightened by historians.

Nevertheless, such collective meanings were always entangled with individualistic trends. One of the most striking feature of the last historical studies in innovation is the interplay of cooperative processes and individual strategies, ranging from free-riding enterprises to identitary claim for inventive genious. Evaluating successfull flexible and decentralized economies, from the Lyonnaise silk to British engineering or Oyonnax plastic, Charles F. Sabel and Jonathan Zeitlin (1997) have explained that the burst and spread out of innovation was much helped by *"a system of collective tutelage which monitored the fluid exchanges among private parties without intruding into them"*. In such economies, actors would not oppose cooperation and opportunism, neither distinguish politics and economy, nor traditional and modern regulations. The whole set of regulations provided a key for coordinating the different units, for diffusing new knowledge and helping actors to innovate and take opportunities in unstable and uncertain markets. The question of the "governance" was then a central issue.

We would like to stress and enhance these issues in two ways. First, with the example of innovation processes in the Lyonnaise silk industry, we argue that corporation, municipality and central government actually promoted collective practices of economic reformation. The ancient ethos of the community was re-used and combined with other institutional models based upon cooperation, such as the provincial academy and the enlightened central administration represented by the intendant de la généralité. Open technology in Lyon was a success because there was a general agreement about the meaning of invention amongst elites in Old Regime France.

Second, whereas this cooperation strenghtened uncertain ventures and helped innovation in a balanced way, the interplay had also to deal with disruption. As Simona Cerutti has shown, solidarity and institutional homogeneity in corporate world produced heterogeneity, that is hierarchies and strategies of self-distinction. Indeed, the cooperative policy in Lyon brought two paradoxal consequences : it sharpened competition and conflicts in corporate world and it fostered a strong individualistic self-conscienciousness. In Old Regime, invention was a clue to understand the meanings, the representations and the practices of work.

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process based on the dissemination and reuse of knowledge available within a group of firms. In the case of collusive and explicit forms of colelctive invention, the actors engage in operations of knowledge production.

3 - COMMUNITY, OPEN TECHNOLOGY AND INVENTION IN THE XVIIITH CENTURY LYON SILK INDUSTRY

What did individuality mean for eighteenth-century inventors living in the corporate town of Lyon and working within the Grande Fabrique ? What did mean collective ethos for them when they were innovating ? How did these two sets of value combine or oppose ? How did local economic elites cope with individualism growing amongst inventive artisans ?

31 - Historiography

There already exist some answers to these questions thanks to historical studies ranging from Justin Godart and Charles Ballot to Maurice Garden, Carlo Poni (1998) and Alain Cottereau (1997), the last two authors having collaborated with Sabel and Zeitlin.

Poni, for instance, echoing Garden, brings a first set of explanations : the Grande Fabrique was a highly turbulent world, submitted to harsh tensions between merchants and independant masters, and to free-riding enterprises of drawers and merchants for launching new patterns fitting the consumers' taste. Though the author mentions the rich negociations and "creative cooperation" which took place between merchants, drawers, liseuses and weavers, he mostly stresses individual appropriation of drawings, thefts and secrecy, he explains how technical inventions originated from these opportunistic strategies based upon novelty and fashion, and how powerfull merchants did infringe guild regulations. The corporation could then be considered as an old set of rules, grounded on quality products and associated ethos, not dynamic enough to suit the new structure of the fashion market.

Quite different are Cottereau's arguments when he confronts Spitalfields to Lyon at the beginning of the XIXth century. In London, individualism, secrecy and patents put a break to the diffusion of innovation, whereas in Lyon, "local regulations put major innovations directly into the public domain of the manufacture" thanks to equipment credits and to "coherent common policy" involving municipality, learned societies, Chamber of Commerce and Prud'hommes. This model was the continuation of a system grounded in the Old Regime, when the Grande Fabrique was so strong. Lyon was a "collective manufacture" were coordination was based upon "communicative action" and this pattern reached its highest degree after the French Revolution. Instead of describing the growing power of merchants in Lyon and the victory of liberalism against corporations, like in classic historical essays, Cotterau insist on the development of negociations and bargain between weavers and merchants, especially thanks to an industrial tribunal, the Conseil de Prud'hommes

(1806). Mass production and deregulation were not the fate of Western industry ; even, these could obstruct inventivitylike in London were Jacquard's Loom did not disseminate.

As a consequence, the diffusion of new information (either drawings or inventions) is presented very differently in both articles : in the former, it derived from new networks and coordinations, internal to merchant companies, especially between consumers, drawers and merchants, in the latter it rested upon institutional cooperation provided by "municipalism" originating in traditions.

We propose a third analysis based on archives which were not central in these studies : the letters and reports relating to eighteenth-century Lyonnais inventors' claims for grants and privilèges which were treated both in Lyon and in Paris by the Bureau du Commerce. We argue that collaboration in XVIIIth century silk industry was even much stronger than Cottereau found it in XIXth century, because very ancient patrimonial policy was re-invested by new enlightened ideals and practices ; this agreement between artisans, merchants and Parisan elites was essential. "Municpalism" was embedded in a broader context and this coordination between different instititional scales was the key for success.

At the mean time, conflicts, opportunism and individualistic claims were not always balanced by "communicative action" in Lyon ; on the contrary, though paradoxal, the cooperative framework fostered private dynamics and disruption, as we shall see later on.

32 - The process of collective invention in Lyon

321 - Lyonnaises inventions

Lyon was the second French town, with 143 000 inhabitants (1789) and 25% were working in the silk industry (35 000). This huge sector was fostering an important internal and foreign trade for luxuous silk cloth. The domination of French silk industry was based on changing patterns according to taste and fashion and on researches either to realize new stitches or to set up (to "read") more easily the drawings on the looms and to make the rich cloth, broché, as quickly as possible (setting up the pattern on the loom would take 25 days). Some inventions aimed to program the patterns on the loom and to select warp threads (like Jean-Philippe Falcon's), others were intended to change quickly parts of the ropes, to reduce their number (Philippe de Lasalle's movable "sample") or to ease the pulling of the ropes linked to the threads (Jacques Vaucanson's hooks). First, the cost of draw-girls (auxiliairies), was a growing burden for guild families as Daryl Hafter explained. At the mid-century, these girls, who came from near provinces, were also very scarce. Not least, they were much despised by masters ; the strategies of the merchants to use them as a threat to the prestige of weaving did enhance masters' hostility against these girls. Second, the speed and synchronisation of the work became the core of inventions at the end of the century as the taste moved from brocades (heavy silk cloth with complicated patterns in gold and silver threads and many shuttles) to façonnés (lighter cloth which could be weft with smaller number of shuttles). This product, especially small façonnés, was the basis for successfull researches in suppressing the pulling of ropes. Jacquard loom (rewarded in 1804), which combined Falcon's program and Vaucanson's hooks, was intended for façonnés.

Nearly all Lyonnaises inventions which were addressed to the commerce departmement in Paris from 1700 to 1789 were related to the silk industry (181 in 265, for Lyon), and more precisely to weaving (116) (generally new devices of looms either for brochés or façonnés) and they occured mostly after 1730. Lyonnais artisans also represented a high proportion of inventors applying to the government: there were 170 inventors fom Lyon, in a total of 875 inventors addressing the administration of commerce (420 provincial ones), and 105 of them were working in craft trade. Inventors members of the Grande Fabrique were 73 and only 12 of them were large merchants. Though we do not exactly know the sociology of Lyonnais drawers, Poni (1997) wrote they were closely linked to merchants (putting out entrepreneurs) ("marchands fabricants"), the head of the Fabrique (only 70 ; 120 to 180 according to D. Hafter). The inventors' profile was quite different: they were rather independant masters ("maîtres marchands"), precisely that category which expected much of the cohesion within the community and which was facing the merchants' growing pressures (there were 700 independant masters but 8 000 hired masters).

322 - Institutions promoting technology openness

This innovative context was sustained by local institutions, traditionally involved in the management of innovation, since the XVIth century, by the means of local monopolies granted in ordonnances consulaires and financial rewards. In the XVIIIth century, monopolies became very sparse, and there was a rewarding fund officially established, the Caisse du droit des étoffes étrangères, created in 1711 (from a tax upon foreign silk) and intended to promote industry since 1725. From 1752, the intendant was at its head but the procedure involved corporative, municipal and academic institutions: there was even a double institutional network providing a complex procedure of enquiry. The invention was examined, in parallel, by the intendant and a member of the Académie de Lyon on one side, and by the provost of merchants (prevôt des marchands, representing the municipality) and the guild inspectors (maîtres-gardes) on the other side.

It was a model and a kind of laboratory for the enlightened government as it was based on a dynamic relationship between patrimonial ideals and emulation, and it actually enhanced social networks and cohesion.

The rewarded inventions were deposited in the Fabrique's office, close to the guild's chapel (église des Jacobins). There, some inventions would have a practical utility : inventors would teach their technics to others and the deposited inventions could be integrated to the traditional procedure for master piece (chef d'œuvre). In 1744, as the merchants elaborated a new Réglement général for the Fabrique, the writers, under direction of the inventive academician, silk inspector and inventor Jacques Vaucanson, decided to promote the new invented loom contrived by Jean-Philippe Falcon (1742) : they made compulsory that, amongst the looms used for the making of master pieces, one be a Falcon's model, and whereas masters could only run four looms each, they would be allowed to work with a fifth one let it be Falcon's ("un Falcon").

Let's take the example of Michel Berthet, who, inspired by Falcon, invented a loom for easing the work of the draw-girls (an essential matter in the Lyonnaise silk industry) : in 1760, the intendant de la Michodière agreed with the academician de Goiffon to grant him 1 000 pounds : 600 pounds and then, the rest of the sum, if he taught the maîtres-gardes and if four of his loom did exist in other houses than his. In 1765, for an improvement, the prévôt des marchands proposed 1 500 pounds in exchange of the secret and if some looms be set up in town. De Goiffon agreed for 300 pounds if the secret was explained at the first request of the maîtres-gardes and of all desiring masters. The intendant compelled him to deposit a model and a description at the Fabrique's office. The grants were not only rewarding the presumed economic utility of inventions ; they were indexed on the efforts of the inventor for sharing his knowledge within the whole community.

Thus, secrecy was actively opposed. There were few monopolies for invention in Lyon : nine affairs ended with a " privilège exclusif ", concerning 7 inventors. And 7 of these patents were granted before 1750, 3 outside the Fabrique, and 3 were only prorogations. The Lyonnaises elites preferred to invest in innovation, to make inventions a common wealth, and this was not only a fancy ideal, as the rewards were often bonuses indexed on the spreading of the inventions within the town. For instance, in 1760, Ringuet presented a new loom for broacades which imitated paintings and embroidery ; he was granted a 300 pounds ("livres tournois") bonus for the 10 first looms set up, 200 pounds for the next 10 looms and 100 pounds for the 100 next ones during 10 years. He was very successfull : as soon as 1760, he had set up the first 10 looms ; in 1762, the next 10 and even 17 more; in 1763, 47 others and in 1764, 85. Thus Rinhuet had even overpassed the quota (169 instead of 120 and in less than 10 years). He was payed for all the looms, even the ones which were not planned in the grant (19 900 pounds instead of 15 000). Such bonuses did combine corporate controls and the recognition of the power of the market. Each inventor was incited to be a dynamic actor collaborating to the public good and the official credit (financial and symbolic) of the invention was involving the choices and decisions of the common users. It was very characteristic of French expertise to combine the judgement of authority with judgements of facts.

Such a compromise between collective aims and private initiatives was fundamental in the ideal of concord and harmony that the Lyonnaises elites (corporated and academic) cherished at the mid-century. For instance, some inventions had to be sold at an official price. When the inventor de Barme was granted a 600 pounds pension in 1750, he was compelled to sell his silk reel (dévidoir) 360 pounds to the weavers. Whereas some inventors were very much rewarded (Falcon and Philippe de Lasalle), most of them were offered very similar sums, like Berthet's. It was a conscious policy, as the Lyonnais academician de Goiffon explained it in 1760 when he was in charge of the inventor Jacques Roche's affair: he pleaded for a grant that "*would be proportionated to the rewards already offered to inventors who must not be incited to jealousy*".

New technics should not bring tensions nor disorders but, on the contrary, they should cement the social cohesion within emulation. It was true in the way the Lyonnais fund was collectively run. The management of the Caisse was based on a contradictory proof procedure, contrived for getting the more information about any invention, so as to reduce uncertainties and secure the public investment. Since a long time, the French government was expert in examining and judging inventions because technical innovation was always considered as dealing with state legitimacy, not only with private business. This became common practice with the growing utilitarian concern and then, as academicians were challenged by other experts. But the procedure in Lyon was unique because it actually institutionalized the plurality of judgments as method of governance.

This double procedure of judgement meant stimulating exchanges between the central and local elites, compelled to negociate the rewards (they often contradict) and to mobilise their own networks. The system of the bonuses was also fostering contacts between maîtres-gardes and artisans as there were many visits in the wokshops of the town for quantifying the spread out of the new looms. When there was a disagreement, the networks could be denounced. The academician de Goiffon, who despised the maîtres-gardes' technical culture, would not trust them because, he argued, they were too close to the inventors and could be personnaly interested in a new project. Rejecting Farcy's invention in 1766, he wrote : "*the inventor fearing imitators, explains himself only through enigmatical panegyrics of his discovery, and the maîtres-gardes find it worthwhile to keep the same words*". Beyond the classical academic incrimination of trade secrets, we must stress how the legal procedure enhanced solidarities and exchanges in the town and within the corporate world, as will shall develop.

Such a collective institutional framework was efficient, as we shall develop, because Lyonnaises elites were not isolated. Their approach of inventions was articulated to a broader national disposal and to main streams in governmental policies.

Eighteenth-century inventors had to deal with an important governmental service called the Bureau du Commerce. Since its foundation (1722), it was deeply involved in reforming the whole economy and his concern grew under the aegis of liberal administrators like Vincent de Gournay and, most important, Daniel Charles Trudaine and his son who directed the whole commercial administration from 1749 to 1777. Liberalism in their mind meant that the economy had its own dynamic, based on private interests and the interdependance of factors, like a clock. Freeing initiatives would strenghthen social bounds and exchanges, would lead to social harmony and, if tensions might appear, they woud be crushed by the regulation of the State, like a finger on a weighing-scale. Much hope of reform was put in inventions as it was thought that even a slight improvement could bring out huge effects because of this chain between the different trades. Collaboration and diffusion were fundamental, even more than private benefit ; social cohesion was the only stake and inventions were considered as good means to reach this aim.

It had two consequences at least, which illustrate how much the Lyonnaise policy was echoed at the highest level in State. First, technical projects and reformation of the economy were involving a whole range of different actors. Either learned experts or practical ones would be required for their advices, they would meet, debate, and judge inventions with respect to their culture and to their uses, as producers or consumers. In that perspective, common utility would be the result of every one's needs, and technics would be a public and political concern. Experiments were truely moments of negociating technics utility and then, a kind of metaphor of the whole enlightened project of concord through progress.

Second, rethinking patrimoniality was central. The private appropriation of inventions which was permitted by monopolies was strongly rejected as, since the beginning of the century, exclusive privileges were often used to keep secrets within families or to foster financial transactions. A royal decree of 1762 set up that people could no longer inherit of a privilege, but had to deserve it. Moreover, monopolies became very few at the mid-century and monetary rewards grew up. This meant a close relationship between inventors and the State, based on merit and service to the State, especially by diffusing new knowledge. Liberals and encyclopedists were eager to suppress any obstacle to the free circulation of knowledge, in a revived Baconian approach.

Such a policy in the mid-century relied on widely shared ideals and practices, like in Lyon (but also, for instance, in provincial academies and new founded promoting societies). Both logics, corporate and liberal, were paradoxally uniting in the paradigm of open technology. The double procedure for grants in Lyon, involving the intendant and local rulers did illustrate that consensus.

<u>323 - Ethics: Philippe de Lasalle</u>

Some inventors were emblematic of this common concern, mixing different logics. The best example is Philippe de Lasalle's cursus (1723-1804). De Lasalle was very famous in the XVIIIth century, in France and abroad, and he was largely rewarded by the Grande Fabrique and the city of Lyon (122 000 pounds). The Lyonnaises elite cherished him but he devoted to the progress of the whole community. Enlightened administrators like Trudaine's son and Turgot, writers like Voltaire, were friends of his. He belonged to the republic of arts and letters as well as to the economic world. What he did and what he thought derived from general ideals and principles he was eager to realize.

He began learning drawing at some painters' and became a draughtsman and a merchant ("marchand-fabricant"). He was rewarded from 1758 by a pension for excelling in halftones for flower patterns (he also imitated tiger fur in silk cloth and he innovated by printing silk cloth like calicos. Soon after, in 1760, he was asked to teach drawing in the Fabrique and his pension was enlarged. Ten years later, his inventions for accelerating the changing of patterns on the looms (reversible loom and movable "semple") majored his pension and he gained a bonus for spreading his looms. After creating machine-tools for the better diffusion of his looms, he was granted 6000 pounds in 1778. According to he administrators and to de Lasalle himself, this pension meant that artistic creativity, technical invention and transmitting knowledge were closely bound. Collaborating and imitating were the main principles every where and the only ways to progress. Art and invention rested on a cumulative process, on methods, rules, devices, lines and colours to be learnt side by side to the master, to the teacher, to the contriver or to nature itself, and he had created a garden in South of France for sending his best pupil to train in drawing flowers. For de Lasalle, there was no genious without copying :

"Vous n'ignorez point que l'art s'acquiert par l'émulation, et les grands exemples ; le travail et mes observations sur les ouvrages de ceux qui se sont distingués dans la carrière que je suis ont seuls formé mes talents; plus d'ardeur encore à mériter la protection que vous leur accordez peut leur procurer un jour cette célébrité qui offre des modèles à imiter et excite d'autres génies qui la surpassent: ainsi parmi nous dès qu'un morceau frappant est sorti de la main d'un artiste habile il est levé et porté sous les yeux de chaque concurrent qui cherche les moyens de se le procurer et fournit souvent par son caractère ou la mode de la saison ou l'exemple d'un beau sujet. Lorsque j'eus traité en 1756 une peau de tigre travaillée avec un peu d'art sur un fond d'or, on vit éclore dans chaque fabrique des desseins pleins de goût représentant diverses fourrures; il en fut de même en d'autres temps lorsque j'introduisis des paysages, oiseaux et personnages".

Neither would de Lasalle condemn the theft of patterns or inventions; the aim was the circulating of knowledge and the progress of qualifications which could result. He was even pleased when his printed silk cloth was copied and his workers seduced by rivals :

"...plus de 20 de mes confrères occupent des pinceleuses (pinceauteuses) et séduisent journellement les miennes à mesure qu'elles se forment et en obtiennent les couleurs et même mes propres desseins; ce dont je m'afflige faiblement si cet évènement sert à prouver que tout préjugé contre les genres nouveaux sont en pure perte pour le commerce général et particulier".

In a very phenomenological way, all means would be good if diffusion was at stake : teaching, imitating, stealing and, not least, deeds and free offers. Several times, de Lasalle gave inventions and taught his new device without asking any counterpart. Charles Ballot was telling that "he let to rent some looms, provided freely sets of ropes to workers ane even gave them half of his bonus". In 1760, de Lassalle was granted a 200 pounds bonus for each pupil he would teach provided, but he refused and preferred to offer freely all his knowledge : "*il paraît...qu'il abandonne la gratification de 200 livres par chaque élève au nombre de six et même plus qu'il se propose de former en ne laissant rien ignorer de ce qu'une longue expérience lui a appris*". From 1777, the Bureau du Commerce organised public experiments in the Tuileries. As it was a success, the government decided to offer 80 looms to Parisian weavers (the word is "donnés", given, which meant a huge sum as each loom was estimated to 1 000 pounds). De Lasalle himself proposed to one of the weavers, called Renouard, to give him two looms if Renouard was ready to show them every time the government would ask him.

After the Revolution, de Lasalle still wanted to diffuse his devices ; he was granted two rooms in the Grand-Collège in 1801, and he tried to explain his devices through comparative observation of two looms, one he had invented, the other of common use. He was taking part to a broader tendency, the teachning of innovation which was developing for instance in the Conservatoire des Arts et Métiers (1794) and which was experimenting since the Old Regime new ways of transmitting technical knowledge : experts were teaching workers out of the traditional frame of the guilds (which had been successfully re-used in Lyon) but it was not theorical nor science based. It was a "pédagogie intermédiaire" fuelled by public demonstration run by technical expert. The maîtres-gardes in Lyon were actually aware of this innovation as they stated, in an encyclopedist wording, that "here the eye can judge, swiftly, what the mind takes a long time to grasp even in the clearest and the most methodical reports".

De Lasalle, artist, merchant, technician, set a bridge from the enlightened hopes in inventions and the new uses of displays, shows and visits to the civic purpose of technology, the foundation of museums and the commercialisation of the "pleasures of the imagination" ; the red line was self-improvement for citizens, building up a national patrimony and exercising sense and sensibility.

The analogy with Jacquard is clear, both because the Jacquard loom was contrived from the whole range of invented looms during the XVIIIth century (programs, suppressing the pulling of ropes, lighter cloth but still sophisticated patterns according to fashion) and because the way the Jacquard looms diffused was similar to de Lasalle's, through a collective and patrimonial town policy which was economically efficient.

4 - THE ECONOMIC PERFORMANCE OF THE " FABRIQUE LYONNAISE "

There are different ways of evaluating the efficiency of the open technology policies in Lyon. For the XIXth century, A. Cottereau faced a massive diffusion of the Jacquard loom in Lyon (20 000 ones existed at the mid-century) and he compared this success to failure in London (Spitalfields) were sweated industry, specialization and private strategies obstructed its dissemination. The spread out of inventions in the XVIIIth century is not so dramatic but archives reveal the channels through which new looms disseminated and even, how the process of technical creativity was helped by the collective dimension of economic and social life in Lyon.

In his article, Alain Cottereau explains that London and Lyon silk trade had the same basis : there were 12 000 looms in London in 1815 and 14 500 in Lyon, the weavers were mostly dependant on merchants who paid them a tarif but the evolution was quite pooles apart. Though London could compete with Lyon between 1790 and 1810, because revolutionary crisis disrupted production foı a while, London silk industry began to decline when in France, Jacquard's loom favored a renewal of sophisticated and varied silk Lyonnaise fabrics. In London, only 5000 looms could be found out in 1853; in Lyon, there were30 000 ones (and 30 000 more in rural areas outside the town). Before World War I, the French production was was much higher and most of it was exported, when England was importing its silk fabrics for home consumption.

What favored Lyon, and what London missed, was flexibility of production in international markets, especially the capacity to offer many new samples twice a year and to change patterns very quickly. Lyons merchants could order samples and fabrics to many workshops (designers', weavers' or dyers') and, in a reverse way, the workshops heads could deal with several manufacturers and change if necessary. The "organizational mobility" provided flexibility, polyvalence and autonomy of all agents. This "economy of variety" was echoing a tradition based on the mobilisation of "technical and human resources", on skill, reputation and self-esteem. In London, both merchants and workers were specialised in one type of fabric and the result was to reduce skill and "variety of experience", hence efficiency. Although in Lyon, in a few days, 5 000 looms could be mobilised for making a new fabric, only 500 could be in London. Lyon was still an industry of commission, waiting for orders, and using samples ; London had become an industry of speculation, based on anticipation and stocks. The difference between both manufactures increased after 1810 when deregulation developped in London, for instance as the Spitalfields Acts of 1773 containing legal prices lists were repealed in 1826.

One of the main difference between Lyon and London was the way credit circulated between merchants and workshops heads. In London, one workshop head would deal with only one merchant. Most of all, indebted weavers were bound to his creditors. As the workshop heads possessed their tools and equipment, the merchants were offering advances to weavers (fabrics were payed only after weaving), with low rates interests, but they prohibited the workers to deal with anyone else. They wanted to profit individually from their investment in the weaver's looms. The only solution for the weavers was to borrow (at high rates) and reimburse immediately the merchant, but very few could afford this. Weavers would not be tempted to acquire new invented looms.

In Lyon, a weaver could work for different merchants, even if he was indebted to one because his creditor would benefit from a deduction from the weaver's future pay. Then, when one merchant was investing in one workshop, he was also investing in favor of other merchants and, in a reverse way, he would be paid back by the profits the new merchants would realize thanks to that equipement. Though this credit regulations were set up since the 1770's, they were actually institutionalized under Napoleon, with the Conseil de prud'hommes (1806). For Cottereau, "*responsability for financing of workshop equipement (took) on a collective dimension : the manufacturers ... (were) collectively beneficiaries of the equipement and collectively responsible fror reimbursement*". This collective frame permitted the weavers to get more freedom and to "bargain" with their employers. "Speech communities" were no fancy in Lyon. Moreover, the pattern of investment in equipement had also favored "a small collective manufacture" for building looms ; numerous workshops of mechanics, locksmiths etc were operating in symbiosis with the polyvalent weavers, themselves being able to contrive looms and even to sell them. "*The regulation of equipment credits produced a collective solidarity over loans and created a collective responsability for the quality of the machinery while preventing anyone from trying to get exclusive rights to use*".

41 - The diffusion of Jacquard loom

These different settings between Lyon and London must be reminded when evaluating the fate of the Jacquard's loom in both countries. In London, Jacquard did not spread and generally speaking, there were not many inventions in london silk industry (Cottereau even speaks of the "backwardness of all British handlooms"). As for the Jacquard loom, its introduction "gave rise to a frantic race" between important manufacturers ; one G. Wilson succeeded and took a patent in 1821. "The new every man for himself of the companies allowed Wilson to keep the secret of the machine"and he did not sell the invention nor new built looms. Cotterau does not mention any use of licence ,though they existed in the cotton industry which was the model referred to by silk manufacturers. Wilson thought the Jacquard loom would standardize and concentrate work, like Arkwright's inventions did in cotton spinning.

On the contrary, in Lyon, the Jacquard loom was aiming at maintaining skill and autonomy of the weavers ; indeed, one had to train during one or two years before using a Jacquard loom. The invention was fitting the Lyonnaise silk industry. There was immediate spreading of the new loom and the "mental mobilization" it entailed did result in several usefull improvements. Local institutions were reinforcing this collective pattern. The municipality, following the Ancien Régime tradition, kept on rewarding inventions to "put them into the public domain"; then, in Lyon, "great technical innovations were treated as true communal goods". According to Cottereau, Jacquard agreed to give up his rights to patents "and left the fruits of his art to the community". This policy was sustained by learned societies, by the Chamber of Commerce and by the Prud'hommes tribunal. This last one played again a very important part : it set up calibers for the looms components, which were adapted to each type of fabric, so that technically, the Lyonnaise polyvalence was maintained. It also permitted to keep fair contracts between mechanics and weavers. Jacquard's invention could then be improved by other loom-builders who made hundreds of them although Jacquard only built up 57 looms and he had to pay damages to weavers when his looms did not fit. On the contrary, the Prud'hommes facilitated "credits for the most efficient looms".

42 - Other examples of innovation diffusion

Jacquard's example was quite impressive because the diffusion was massive ; the credit for equipment might have been essential. In XVIIIth century, workshops were not so numerous and part of the weavers were still independant from merchants and were able to run their business without such credits networks. Nevertheless, it is possible to know how efficient was the patrimonial policy of Lyonnaise elites. Even, archives tell a little bit more than the XIXth century ones about the channels of the diffusion.

Actually, thanks to the reports established by the maîtres gardes for the bonuses, we have very precise information of the diffusion of several new looms : Falcon's, Berthet's, Fleury Dardois' and Barbier's. Falcon's looms were quite numerous in town : 40 were working 1765, 100 in 1773 (out of 14 000 looms in town), according to Charles Ballot, and one rich merchant in 1786 had got 15 of them. The spread out was not impressive as it will be for Jacquard's, but the impact of the policy was much important for creating networks of diffusion and for developing technical creativity.

Dardois' new loom was rewarded 300 pounds in 1776 and 24 pounds for each of the 25 first ones set up in Lyon. In 1777, the maîtres gardes recorded 7 looms, in 1778 6 more and in

1779, 15 others (then 28, more than expected). The inspectors precisely noted the names and addresses of the masters who set up such looms (see document and map) : 6 were to be found in parishes near Grollée street were Fleury Dardois' workshop stood (in the old center of the peninsula), 12 in his street (4 of them in the same house, in 2 different dwellings). We also know that Berthet's looms were first built up at his 5 sons' and at his son-in-law's, Barthélémy Charles. Fleury Familial links, neighbourhood and kinship in the house, in the street and the nearest parishes were stimulated by the bonus system. Social bounds, cohesion which was so much cherished by local and central elites as the main stake of invention were actually strenghthening and this social impact was helping diffusion of the inventions. We must add that the bonuses were also enhancing kinship within the whole town, thanks to relationships between members of the guild : Dardois' looms were to be found far away from the inventor's workshop as 10 looms were set up in the northern part of the Peninsula (Saint-Vincent slopes and western side of the Saône). These networks were even more important : Dardois presented the certificates signed by a huge cohort of 91 masters and merchants ready to support him (he even printed them). Such a proportion was quite unsual (the maximum was reached by Philibert Saigne with 100 certificates), but it was revealing how inventions were involving the whole community and tightening bounds in a very practical way.

They could also extend networks outside the guild. Collective emulation led to cross over traditional boundaries that separated the different corporate trades. These limits were fundamental for the identity of each trade but, the growing fabrication of looms led to major changes. Because the common pattern in Lyon was already "multivalent weaver-mechanics, making fabrics and marketing this or that technical process invented" (Cotterea), locksmiths, joiners, combers, lathe-turners, were getting more and more involved with weavers for contriving inventions. In 1785, Dardois presented some more 5 certificates and one came from a joiner who wrote he had built a loom "à la Dardois" in 1781 because of the command made by a master of the Fabrique.

These networks were the basis for the pattern of innovation in Lyon. Inventive artisans, either weavers or not, were quickly informed of new devices ; they watched working new looms, listened to weavers, talked with maîtres-gardes, they worked on rewarded looms and contrived improvements to them. Invention within open technology was breading invention. For instance, Falcon had been granted 300 pounds bonus for each loom until 60 set up in town. Archives keep the records of the first looms built up in 1764 : 7 buyers (out of 9) were living in rue Pierre Scize, near La Chana, where silk workers were numerous in the XVIIIth century and one of them was Berthet. Already in 1759, Berthet had presented an improvement of Falcon's 1742 loom. In 1765, he said he had improved the new Falcon's loom he had just acquired.

There were many other examples. Vaucanson's cylinder for programation was inspired by the numerous Falcon's first looms with paper boards passing round a prism (1742). In a similar way, one of the 91 certificates of Dardois was signed by Rivet ; the same year (1777), he also presented a loom for façonnés without tireuses (and then moved to Paris). For the building of his second loom, Falcon had called a weaver, Allard who then improved the loom in 1763. Echoing to this, Barbier successfully amended Falcon's 1764 loom in 1765 as the maîtres-gardes explained and his loom was preferred by the authorities to Falcon's. Then, it was de Lasalle who contrived his first loom, in 1767, from improvements made to Barbier's. Jacquard's invention was much improved by a mecanician from Privas, Breton. Moreover, inventors like Falcon and de Lasalle kept improving their own devices. One invention was never definitive but always evolving and these improvements were encouraged by the muncipality which, for instance, blamed Jacquard's desinterest for amending his own loom.

There were also more latent circulations of technical devices. For instance, Vaucanson's loom which were contrived between 1747 and 1750 had been forgotten in Lyon (historians don't agree on the reason why) but it was re-discovered by Jacquard who even re-built up one model of it for the Conservatoire des Arts et Métiers (created in 1794). Collecting and sharing had to deal with buried memories as well as with conscious processes (and Vaucanson's numerous machines were the basis of the collections of the Conservatoire des Arts et Métiers).

There was a kind of kinship between all these inventions, echoing the pattern of social and political life in Lyon. Actually, Cottereau has found out an essay witten in 1863 describing the networks between the new invented looms in Lyon : "The most conving proof that these successive inventions were borrowed from one another is that a jacquard card in use today may be applied both to Vaucanson's planchette with needles and to Falcon's, and the match is so good that Falcon's initial matrix must have fixed dimensions". According to Cottereau, the effects "were comparable to what could easily have been the case today if computer systems had been standardized from the start and made cumulatively compatible as they progressed", even if contrived by several different firms.

Then, collaboration and open technology in Lyon was highly efficient for the spreading of inventions, for sharing technical innovative culture and for helping autonomy and research in craftwork. Maybe, like in Swiss watchmaking, this flexible model had fostered a "professional elite" of "indefatigable researchers, skilled inventors and artisans, "artists", who often devoted more time to resaerch than to their own business". Fame, excellence, perfomance were these inventors' aims. But what was the boundary with self strategies ? Although Cottereau describes an equilibrium in the Lyon industry, conflicts and private interests were very harsh. In a paradoxal way, collective innovation did usher in a disruption of community ethos ; it did foster a burst of opportunism, an instrumentalization of corporate rules, especially by merchants, and, most of all, claims for priority and posterity amongst inventors.

5 - COMPETITION AND INDIVIDUAL CLAIMS

51 - Conflicts and self-pride in Lyon

There were two kinds of conflicts deriving from these close relationships, either horizontal, between masters, in the same guild or not, as we already mentioned, or vertical, opposing apprentices to masters, or individual inventors to the merchants and to administrative authorities.

511 - Priority disputes for rewards

First, the spreading of inventions in the Lyonnais workshops could easily foster imitations and improvements but, also priority disputes for rewards. Rivalry between Barbier and Falcon was famous in Lyon. Improving, imitating, stealing were very close and Falcon did not bear this copying, on the contrary of de Lasalle. The relationships between masters belonging to different guilds were also often mentionned in the reports because of quarrels. For instance, the two weavers Buisson and Chambeau competed on that ground for adaptating Kay's flying shuttle to the silk industry. They first had met thanks to a lathe-tuner from Switzerland called Hildebrand who assisted Chambeau. They also worked together with Conte (a lathe-turner, in Grollée street) and Catin, a joiner and Chambeau's neighbour. When they became enemies, Chambeau asked Catin to copy pieces of the loom and to carry them at a cabinet-maker's, Francfort, to make up his theft. In other affairs, sub-contracted mechanics pretended that they had invented new devices or that they were copied and their workers corrupted (Couturier), and controversies could rise, even if authorities would never be harsh for imitators.

So the lack of monopolies does not mean that the notion of property was ignored in Lyon. Buisson and Hildebrand claimed they had "property rights on the invention". The maîtres-gardes considered than Louis Jean-Baptiste Duon, competing with Hennequin and with Farcy, was the first inventor and the "owner" of the disputed invention.

The authorities had not set their mind very firmly on that question. When Condurier asked for a reward, in 1764, the maîtres-gardes agreed for 800 pounds if the inventor would disclose his secret, but the first alderman wrote that "*Natural Equity order should let him keep the making of his new invented cloth for his own profit so that the reward could be granted without submitting to any condition*". The intendant Baillon was not so generous and he reminded that "*it was not natural that he (the inventor) would be rewarded before he opened knowledge*". What meant "natural" for the alderman and for the intendant was poles appart. Both were right as they did refer to different principles, either collective sharing of innovation as a common wealth, or private benefits secured by secrecy. Though paradoxal in Lyon, this latter argument was not isolated, at least outside administration; it was even more frequently echoed by inventors as merchants' pressure grew at the end of the century.

512 - Tensions between apprentices and masters, masters and merchants

Often mingling with horizontal conflicts, vertical ones seemed more violent. They were manyfold, as some of them would oppose workers to masters, others masters to merchants or individual inventors to the merchant elite.

The former could develop between an apprentice and a master. Inventing could enhance the possibilities of a worker to pretend to a recognition and an autonomy in the workshop. For instance, Barbier whom we mentioned was a worker at the merchant master's Bondafonds. When Barbier presented his new loom in 1765, he was rewarded, not his master. Then Bonafonds died and his widow took over. She argued that Barbier had been recruited by Bonafonds only to build up the loom. But, for keeping he precious worker, the widow had to promise him he would get half the grant if she got one and at the end, the worker was rewarded, not the widow master.

But these vertical tensions could also develop on a more general level, between masters and merchants and this raised questions about the economic and social meaning of invention when groups were struggling in the corporate world. The situation is well represented by Fleury Dardois who wrote a pamphlet in 1775 for warning the Bureau du Commerce how his invention could be exploited by the merchants and widen the gap between them and the weavers, mostly salaried and dependant in the 1770's as Maurice Garden explained (kind of "verlag-system"). Fleury Dardois was fearing that merchants would take the opportunity of his new invented loom (easing the pulling of ropes) to lower the tariff allowed to the weavers. He was then asking Turgot to set up an official tariff as :

"...oui, Monseigneur, la mechanique est bonne,... mais ... les marchands voudront s'en prévaloir comme c'est toujours leur sistême; que diront-ils à l'ouvrier ? ils diront vous avés la un métier qui donne beaucoup d'aisance et de facilité, qui donne beaucoup moins de peine, d'embaras, et qui vous évite beaucoup de frais et de dépense que vous étiés obligés de faire auparavant pour monter un métier; ... car ces MM. parlent en Roi à l'ouvrier ... et il est bien juste ajouteront-ils encore, de vous diminuer aussi la moitié ou un quart (plus ou moins) sur le prix de la façon ...

Que diront les ouvriers de leur coté. ils refuseront d'employer la Mechanique...ils diront qu'ils préferent leurs métiers tels quils sont à toute l'utilité et les avantages de la Mechanique pour ne pas voir diminuer et reduire presque a rien le prix des façons, pour éviter de fournir des pretextes aux Marchands de les véxer encore d'avantage ...; plusieurs en murmurent d'avance, ... ".

What natural order was producing in the market place, was not harmony, and inventions could actually disrupt concord, more than cement solidarity. Such a conflicting natural order of society was not a hindrance to progress for Turgot. The balance between private interest and the "duty of justice" recommended by Boisguilbert, in the wake of Locke, was much endangered.
Actually, this was not a new situation within the Fabrique. Conflicts between independant masters and merchants were harsh all along the century and inventions did cristallize the problems as early as the mid-century, after the merchants' victory of 1744.

513 - Frustration: Jean Pierre Falcon

At that time, Falcon had invented his first loom, inspired from Bouchon's. He belonged to the wealthy members of the Fabrique ; he came from a bourgeois family (150 000 pounds annual rent), he was bound to be a merchant and he learnt the trade at a very famous master's, Jean Revel, a designer of a new stylistic pattern. In 1735, he made a beautiful coat and became a master and a merchant without paying any fee nor being apprentice. He received a grant and entered a partnership with Bouchon, also a wealthy member of the Fabrique. Except Philippe de Lasalle, he was the inventor most rewarded in Lyon ; when he lived, he received 108 384 pounds, that is 52 194 pounds from the Fabrique and 56 190 pounds from the town council (the caisse) and he was well-known in Lyon for that reason (his widow and his daughter still received 24 000 pounds). Never a mere master would obtain so much money. The Fabrique was actually rewarding the inventive merchant and was setting him as a model for the whole community (we remind his loom was actively spread out) ; the Fabrique was forging an elite of innovators (Bouchon, Falcon, de Lasalle).

Falcon became a target for masters competing the merchants' strenghthening power. In 1737, as masters had overcome merchants, they stopped paying Falcon who had to wait 1744 merchants' victory to get his money. During the next troubles of 1754, the opponents to the elite took argument of disappointments in the use of Falcon's loom to prompt workers to criticize the head of the Fabrique. Then, Buisson managed to make 12 weavers sign against Falcon. This "cabal" was successfull, Falcon was no longer payed for a time and he lost his dwelling. Solidarity here rather meant factions serving private interests', which mingled with struggle against the elites.

But, there was an unexpected development of the Falcon's affair which helps to grasp the importance of the conflicts with merchants for ascertaining inventors' claims to rights, honor and glory.

Though Falcon was celebrated by the Fabrique and the town council, his daughter kept disturbing the autorities long after his death in 1765 because she thought that the Lyonnais administrators had not treated well enough her father. It was not mere whimsycall fancy. On the contrary, her argument and her violence were efficient in the bursting out of inventors' natural right as the paradoxal outcome of the collective meaning of invention during the century.

Falcon's daughter claimed her father had been humiliated because he had been compelled to invent. He had received 6 000 pounds in 1737 and a pension to begin researches and after the loom was achieved (1742), Falcon was refused the huge reward he wished (20 000 pounds) because Trudaine thought that such a sum would no more stimulate him. She added that her father had to teach workers and to show his devices to all foreigners passing through Lyon . He would have earnt more money if he had worked as a merchant master (*"his superior genious could have granted him a huge fortune in trading, but he would have worked for his sole benefit"*) ; his belonging to the elite should have credited him much more (*"my father never was brought up as a worker"*). For her, *"whereas his talent and his genius should have secured him and family fortune, they have occasioned their ruin"*.

At the end of the century, according to Falcon's daughter, enlightened collaboration actually meant dependance, shackles and humiliation. This could be related to the growing pressure of merchants. Fleury Dardois also argued that the authorities had asked him to invent his loom within one month for 144 pounds ; he was only given 72 pounds ad the other 72 pounds were promised. As one maître-garde wanted to know his secret, he contrived a device for concealing the mechanism but he was "seduced" by the maîtres-gardes. He wrote in anger that *"they persist ... keeping the worker under their claws, to want him to depend on them as if they had sovereign power and even stronger : a... despotic power... Tyranny ! "*. He added he was *"illtreatred, insulted when he asked for his money" "reduced as a beggar" "because the guild wanted to deprive (frustrer) the artist from the reward and the merit of his work" ; "was it possible to treat so badly men so useful ... to humanity"*.

The question was rooted in economic and social struggle developing in the Fabrique, but it had also to deal with identity self-consciousness growing in craft trades. In both discourses, the question of pride and honor was central ; inventors were re-using traditional craft discourse on honor for asserting new claims, the rights of genious to unquestioned recognition. Inventors, who experienced being rewarded by the town and the State for their talent, thought they were somewhat exceptional. The cooperative framework, backing poor inventors, rewarding meriting ones, even glorifying some of them, instituted as models for the whole community, all this did encourage artisans' choices, competing desires, mobilities and individualistic self-conscienciousness. Invention had to deal with free-riding responsible subjects. In Lyon, such an emancipation was not easy to be tolerated by merchants at the end of the century, when the gap widened between them and the weavers. Different conflicts did occur as inventors became conscious that the collective ethos could be used as a stratagem for depriving them of any right upon their creations. This was essential for the bursting of natural right arguments in France.

This could express in very material forms. The father's honor in the words of Falcon's daughter rested upon a capacity to earn his living thanks to his genious creations. The question of money was central in her discourse and it was not hazard. Living of one's inventions, like living of one's books of paintings, was the basis of the emergence of the social, cultural and juridical identity of inventors, authors or artists. The market pressures weighing upon the creators' shoulders were as instrumental as the academic rigid judgments in fostering the claims for rights and freedom. The Lyonnais silk merchants were denounced (and Trudaine too) because, through the rewards, especially the complex system of bonuses, they were linking inventors to their market strategies and preventing them of developing their own initiatives.

There was a very similar problem repeating in the XIXth century for Jacquard and his new invented loom. Whereas A. Cottereau explains how the invention became property of the town and did quickly spread out, Pierre Cayez has stressed the conflicts between Jacquard and the municipality which compelled him to stay in Lyon and which feared so much that he would sell the invention to competitors. In 1814, as Jacquard had left Lyon, the police was urged to take him back and to check if he had transmitted his invention to rivals. But Jacquard has become a main figure of the mythic history of inventors ; at least he won the judgement of posterity. Falcon's daughter had still to fight for that.

Pride, as expressed by Falcon's daughter, was grounded on a familial and patriarcal sense of patrimoniality. Actually, the daughter and her husband had worked a lot on Falcon's looms. That was not unsual, as familal networks were essential in the transmitting of knowledge within the corporate world. The Fabrique was encouraging this spreading, often associated to apprenticeship, so that it was common for autorities to deal with inventors for two generations, for instance with revertible pensions (as Falcon's and de Lasalle's). Familial appropriation was enhanced in the name of utility and public good, though the new meaning of patrimoniality was not a familial one. The ambiguity was patent in Falcon's case as he had sent his own daughter to Paris for requirig the 20 000 pounds and, most of all, he planned that this money would provide dowry for a daughter and permit her to set up. Moreover, the pride of fathers and of their wives or daughters was emphasised in the guild during the whole century, as auxiliairies were thought to threaten masters' work, as D. Hafter has shown. This complex situation did enhance the memory of the inventor, of his name as a father. It was one more paradox of open technology. It fostered a desire of posterity, in the name of the father ; self-consciouness was deriving from a sort of cult of ancestor's genious, of the spirit of the family.

This pattern was not so far from the worship of scientists and inventors, which developped for instance in the Encyclopédie(though praising collective improvement), with names quoted as gallery of portraits, as a new lineage for humanity. Some inventors had to be reminded like fathers ; they belonged to a new built collectiveand selective memory, to myths.

52 - Incentive structures in systems of open knowledge: discussion

521 - The basic ingredients of systems of open knowledge

As clearly shown in both cases of open science and open technology a critical factor deals with the emergence and re-enforcement of a common knowledge that openness increases the general performance of the system and that diffusing its own knowledge contributes to a positive sum game. Such a collective belief is particularly strong in cases of localized systems of open knowledge which are competing with other systems (Lyon against London). Collective ethics plays also a great role. However this is not enough as clearly demonstrated by Dasgupta and David in their analysis of open science. There is also a need for a mechanism to give credit to the inventors without creating exclusivity rights. The ingenious mechanism of priority rule which determines the constitution of reputation capital plays this role. In the case of the " fabrique lyonnaise ", a financial reward is attributed to inventors who accept to diffuse their knowledge and bonuses are given if the inventor actively take part to the adoption of his technology by others. The great system of bonus shows how well the conditions for an efficient reproduction of the knowledge once created were understood: Michel Berthet received 600 pounds for his invention plus 400 pounds if he taught his knowledge and if four of his loom did exist in various other places.

In both cases - open science and open technology - the reward system introduces competition and increases the risk of disputes.Then the force of ethics as well as the effectiveness of the common knowledge about the efficiency of the system come into the front to mitigate individual misconducts and frustrations.

522 - The mistery of Linux

Linux is the new example of a technological community based on openness, without being territorially limited. It is a computer operating system inspired by Unix, delivered free-of-charge with the source code (the series of instructions that forms the programme before its compilation). The fact of giving the user access to the source code makes it possible to generate gigantic effects of learning-by-using, in other words, to fully exploit a fantastic amount of distributed intelligence. Thousands of users reveal problems and thousands of programmers find out how to eliminate them. According to the terms of the Free Software Foundation, everyone can use the code and amend it, provided they inform the organization of the change so that it can be checked and assessed. We have here the "good properties" of knowledge distribution and systems of open knowledge: only with the fast and large-scale circulation of knowledge can we benefit from the unique potential of a very large number of skilled individuals. In a way, the billions of dollars spent by Microsoft to maintain huge teams of researchers seems very expensive compared to Linux's capacity for "bringing together and exploiting the IQs of thousands of users in the four corners of the Internet" [Alper, 1999].

The case of Linux provides a new insight into our exploration of open technology. Open knowledge does not mean the absence of legal rules. There is a necessary " legal equipment " to protect the free nature of knowledge from private appropriation. In the case of Linux the general public licensing (GPL) makes it impossible to privately appropriate some improvements which could be introduced into the operating system.

Such an analysis, however, leaves one question open. While it is clear that there is a strong collective belief among the Linux community about the efficiency of the " bazaar model ", and that ethics plays a great role in the Linux enterprise as well , we do not see any mechanism designed to give credit to great software developers without creating exclusivity. This is probably due to the particular features of the division of labor in this kind of creative process (Arora and Gambardella, 1994): division of labour is so deep that it makes it difficult to individualise inventors and creators. However, this impossibility to reward individual efforts raises great concerns about the stability and durability of openness in this particular case.

CONCLUSION

The Lyonnaise silk industry was provided with institutions which played a major part in the building up of a policy for innovation and of a legal frame for inventing. There were two reasons for that. Lyonnaises institutions managed to combine emulation and cooperation and this was fundamental in the enlightened administrators's mind. Lyon was thus a central piece in the reforming and encyclopedic attempt of the mid-century. Corporate and municipal traditions were not combining with liberal reforms. The result was effective diffusion, though it was much more impressive in Jacquard's time, as foreign markets were more active too. The impact was rather one of cumulative and collective invention which benefited to Jacquard (Cayez called his loom "Vaucanson-Falcon-Breton").

But, the lack of monopolies, coupled with the collective principle and practices were also perceived as means for dispossessing inventors and clushing down their desires of autonomy. The situation was more conflictual than ever as the rewarding of meriting inventors and the praising of their initiatives had fostered desires of social, economic and cultural upwarding (which did occur for some workers) and the feeling that inventors deserved public recognition and ever-lasting memory. They were heroes, overpassing human laws, and this was the basis for claiming a natural right. The experience of feeling singular was originating in a collective process. The corporate and industrious town was thus a privilegied place for the elaboration of the inventors' identity, still an improver and already a heroe (amnesia and memory).

Thus, this study opens new research avenues - historical as well as analytical - about these classes of mechanisms allowing to give credit to individual inventors while supporting strongly the disclosure as well as the reproduction of knowledge; those mechanisms which strictly govern the solidity and stability of open systems, as Paul David has it so clearly demonstrated in the case of science and academic research.

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World of Possibilities.

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In summary, the review team's report struck a positive note by characterizing the Joint Venture personnel as experienced and competent and the project as part of "a significant trend of transferring management of publicly funded projects to private enterprises," a trend that has become "increasingly popular for megaprojects, because it allows state and federal agencies to bolster their resident technical and professional resources with the highly specialized expertise from the private sector."[102] In the epilogue, we, too, shall portray the Central Artery/Tunnel Project as manifesting a future trend—toward an open postmodern style of coping with complexity.

VI Networking: *ARPANET*

Computing may someday be organized as a public utility just as the telephone system is a public utility. . . . The computer utility could become the basis of a new and important industry.

John McCarthy (1961)

The nationwide, real-time, interactive, computer-based information network, the ARPANET, became the first of an increasing number of information networks that spread across the United States and beyond after 1969. Soon interconnected, these networks formed the Internet, to which millions of computer users are connected today. Funded by the Advanced Research Projects Agency (ARPA), a U.S. Defense Department agency, and developed by university research centers, the ARPANET research and development project began in 1969. It culminated in 1972 with a public demonstration of a small network that interconnected computers, primarily at university sites. The last of the technological systems whose creation we shall consider suggests the characteristics of future project management and engineering.

The history of ARPANET provides a memorable, salient example of the manner in which ARPA, using a light touch, funded and managed the rapid development of high-risk, high-payoff computer projects, especially in the 1960s and 1970s. The ARPANET project also provides an outstanding example of federal government funding of academic scientists and engineers intent upon nurturing a new field of knowledge and practice, in this case a computer network. While the presumed military threat from the Soviet Union during the Cold War seemed to justify the military-funded SAGE and Atlas projects, the justification for military funding of the ARPANET project is not so obvious. Military needs can be discerned in the background of the ARPANET picture; the foreground contains scientists and engineers motivated by the excitement of problem solving and the satisfaction of advancing a burgeoning field of computer communications.

The academics who were developing the net were often directing university computer research centers populated not only by fellow faculty members but also by aspiring graduate students; the progenitors of ARPANET contributed substantially to the buildup of computer science departments at major research universities. ARPA funding created a body of knowledge, a set of techniques, and, as Professor Leonard Kleinrock, one of the principal developers of the network, adds, "a cadre of talent" that proved to be "very significant for the United States."[1]

Through the history of ARPANET, we shall discover government funding playing a critical role in one of the opening acts of the so-called computer and information age. The part played by the government, namely the military, in this historical technological transformation raises a perplexing question: is government funding needed to maintain the revolutionary development of computing and is government funding needed to generate other technological revolutions in the future?

In following the history of Atlas, we learned much about the way in which a government agency, the Western Development Division of the Air Force (WDD), acted not only as funder but also as manager of a research and development project. In both the Atlas and the ARPANET projects, a diverse set of nongovernment organizations acting as contractors carried out the bulk of the research and development. A prime function of WDD and ARPA was to schedule and coordinate the activities of the heterogeneous set of contractors. In both cases, the government as project manager granted the contractors considerable freedom to fulfill their responsibilities as long they met the specifications required for their particular component of the overarching system. This avoidance of micromanagement

allows us to speak of a light touch on the part of the government project managers. WDD, as we have seen, and ARPA, as we will see, assumed hands-on roles only when schedules and specifications were not being met.

The major difference between the Atlas and the ARPANET projects was the much larger size of Atlas and this project's use of a systems engineering organization, Ramo-Wooldridge Corporation. Atlas involved seventeen principal contractors and hundreds of subcontractors, while ARPANET had less than half the number of principal contractors and a handful of subcontractors. These differences in size resulted in some contrasting managerial techniques. ARPANET's informal organizational structure and the spontaneity of its problem-solving approach suggest the engineering and management style of small groups working within the overarching structure of a large project like Atlas.[2] Another difference is that ARPANET was initially built on top of another major system, the Bell telephone system, which provided the communications network.[3]

]]]]]] Military Interest in Command and Control

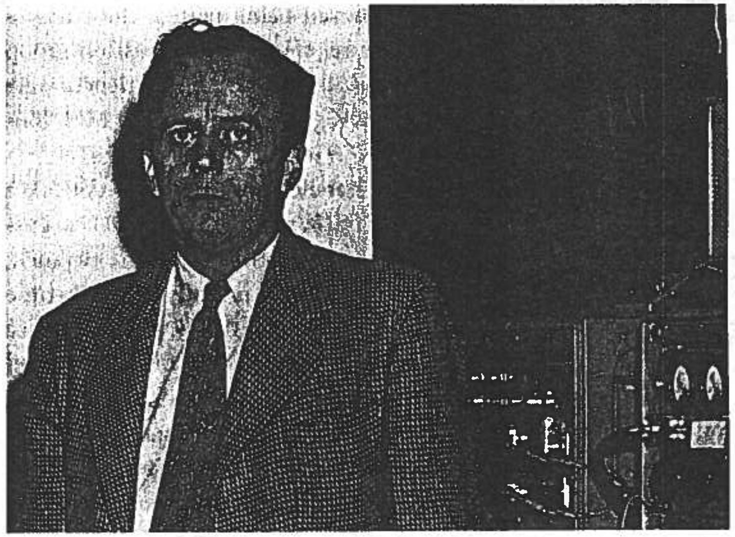
The interactions and evolution of events, institutions, and people that culminated in the deployment of the ARPANET can be traced back to the American military's interest in fostering the development of command and control techniques, sometimes called command, control, and communication (C3). We encountered command and control in our history of the SAGE air defense systems and in the introduction of the Whirlwind computer into this system. We also noted that the MITRE Corporation developed a number of command and control systems. The Whirlwind made possible real-time information processing, or the display of information about the movements of aircraft as these occurred. With the Whirlwind, people could interact with the information being processed and displayed on the computer screen; operators could select and highlight desired real-time information from amid the mass of other information being fed into the computer from radar and other sensors. Operators could also interact with the computer memory by asking for the display of information stored there. Military commanders used the information processed and displayed to make decisions and give commands intended to control the air defense situation.

As Air Force interest in computer-dependent command and control increased in the late 1950s, the Defense Department turned to ARPA for the cultivation of computer development. (Later the name changed to DARPA, Defense Advanced Research Projects Agency, but ARPA will be used throughout this chapter.) The Defense Department established ARPA in 1958 in response to the post-Sputnik concern in the United States that our military research and development in particular and our engineering and science in general were falling behind that in the Soviet Union. This organization had the responsibility of funding cutting-edge research and development. ARPA programs focused on problems associated, for instance, with ballistic missiles (reentry)—a particularly intractable problem, as we have seen—on ballistic missile defense, and on nuclear test detection. By the late 1960s, computer-related projects funded by ARPA, including ARPANET, cost in the vicinity of $30 million annually.

In 1959, in response to the military's interest in command and control, ARPA awarded a $6 million contract to the System Development Corporation (SDC) to do conceptual and operational studies related to command and control, including the interaction of the operators and the computers. SDC had developed complex software for SAGE computers, and, in so doing, had trained hundreds of programmers and helped launch the computer-programming community in the United States.

]]]]] J.C.R. Licklider

Joseph Carl Robnett Licklider transformed the ARPA command and control program into an encompassing endeavor that greatly stimulated the development of computers and information systems not only for the military but for academia and industry as well. The



JOSEPH CARL ROBNETT LICKLIDER AT SYMPOSIUM ON ENGINEERING APPLICATIONS OF SPEECH ANALYSIS AND SYNTHESIS, MIT, 1953. *(Courtesy MIT Museum)*

first head of the Information Processing Techniques Office (IPTO) of ARPA from 1962 to 1964, Licklider had a powerful vision of interactive, time-sharing, and networked computers. With a Ph.D. in psychology from the University of Rochester in 1942, he did research in the 1940s in a psychoacoustic laboratory and taught as a lecturer in the psychology department at Harvard University. During World War II, the psychoacoustic laboratory studied the maintenance of vital human communications under the noisy and mobile conditions of combat.[4] After the war, Licklider moved to MIT to head an acoustics laboratory there and to fill an appointment as associate professor in the electrical engineering department. He also served as a research associate in the MIT Research Laboratory of Electronics, noted for its espousal of interdisciplinary research. Licklider gained a full exposure to the MIT style of research in the 1950s, one intensified by his participation in Project Charles, the summer study group that helped define the SAGE Project. He said that he "fell in love with the [MIT] summer study process."[5] He also enormously enjoyed being part of the circle of scientists and engineers who met regularly in a discussion group presided over by Norbert Wiener. Licklider's MIT experiences were rounded off by his appointment as a group leader at the MIT Lincoln Laboratory.

In 1957, he moved to Bolt Beranek and Newman (BBN), a high-technology firm founded in 1948, to establish a psychoacoustics laboratory there. MIT professors Richard Bolt, Leo L. Beranek, and Robert B. Newman had founded BBN initially as a part-time venture to do architectural acoustics. Staffed by a preponderance of Harvard and MIT scientists and engineers, the firm specialized in solving acoustics problems amenable to statistical analysis techniques that involved computers. The move from acoustics through statistics to computers helps explain BBN's leading role in the computer field by the late 1960s. Heading the department of psychoacoustics, engineering psychology, and information systems research at BBN, Licklider focused his research on computers by investigating the interaction, the diagrammatic representations, and the simulation of complex systems, and by supporting the development of computer time sharing by BBN. Time sharing, which provided access for a number of users to a single mainframe computer, paved the way for the networking of a number of computers.

Licklider's success in furthering the development of computer and information systems, however, lies not so much in his cultivation of research and development as in his role as a visionary who inspired the problem choices and research and development activities of numerous computer and information pioneers. These, in turn, became members of a network that he helped establish and cultivate. His imaginative reasoning and programmatic thinking are impressively laid out in a seminal paper, "Man-Computer Symbiosis," published in 1960. This paper is a clarion call urging that computers be thought of as means of enhancing human thought and communication, not simply as arithmetic, or calculation, engines as they mostly were at the time.[6] In this essay, he calls for a partnership—"very close coupling"—between humans and electronic computers

that would facilitate thinking, decision making, and the control of complex situations. He does not dispute the claim of his colleagues in artificial intelligence that in the distant future computers might dominate cerebration, but he thinks that in the near future, perhaps the next ten to twenty years, a symbiotic partnership would evolve instead. These years, he anticipates, "should be intellectually the most creative and exciting in the history of mankind."[7] Such a vivid pronouncement helps us sense the enthusiasm he generated among the converted.

Licklider opened his "Man-Computer Symbiosis" with this analogy:

> The fig tree is pollinated only by the insect Blastophaga grossorum. The larva of the insect lives in the ovary of the fig tree, and there it gets its food. The tree and the insect are thus heavily interdependent: the tree cannot reproduce without the insect; the insect cannot eat without the tree; together, they constitute not only a viable but a productive and thriving partnership. This cooperative "living together in . . . intimate association or even close union of two dissimilar organisms" is called symbiosis.[8]

Man-computer symbiosis, he adds, is a subclass of man-machine systems. Other human-machine systems use machines as extensions of humans. Still others deploy humans to extend machines, to perform functions, for instance, that cannot yet be automated. By contrast, man-computer symbiosis depends on an interactive partnership of man and machine.

He describes how a partnership would change the way computers would be used, but first he points out the laborious, time-consuming way in which they were then being used. To solve a problem using a computer, the researcher had first to formulate the problem; then he or she had to turn to a professional programmer to program the problem for the computer; after this, the problem written in computer language was submitted to the operators of a centrally housed computer who placed the program in a queue to be run as computer time became available; and, finally, the computer processed the information, then printed out the results. This procedure was known as the "batch process."

Instead of this process, Licklider believed that problems in the future "would be easier to solve, and they could be solved faster, through an intuitively guided trial-and-error procedure in which the computer cooperated, turning up flaws in the reasoning or revealing unexpected turns in the solution."[9] Many problems, he adds, could not only be solved, but also formulated, with the aid of a computer.

Despite his call for a symbiotic relationship, Licklider, unlike contemporary artificial intelligence enthusiasts, relegated the computer to tasks mostly clerical or mechanical. He had found by self-observation that he gave most of his so-called thinking time to clerical or mechanical activities involving searching, calculating, plotting, transforming, and determining the logical consequences of hypotheses, and otherwise preparing the ground for his occasional—but essential—insights and decisions. All but the insights and decisions he wanted to turn over to his computer partner.

Licklider the psychologist drew a memorable comparison between a human and a computer. Humans are "noisy, narrow-band devices" but with many simultaneously active channels. Computer machines are fast and accurate, but they perform only a few elementary operations at a time. Humans are flexible, while computers are single-minded. Humans speak redundant languages while computers use a language with only two elementary symbols. To him, these contrasts suggest complementarity and symbiotic cooperation.

To fulfill his vision, Licklider calls for further development of time-sharing systems whereby a costly large-scale computer divides its time among many users. He also wants a "thinking center" with functions analogous to present-day libraries but with greatly enhanced information storage and retrieval. Then he imagines

> a network of such centers, connected to one another by side-band communications lines and to individual users by leased-wire services. . . . The cost of the gigantic memories and the sophisticated programs would then be divided by the number of users.[10]

Licklider thus anticipated two major research and development programs of ARPA that in fact did develop over the next decade: time sharing and computer information networks.

]]]]] The Licklider Network

While Licklider is remembered as a highly original thinker, others in the Cambridge computing community shared his commitment to time sharing and interactive computing. After he moved to ARPA, he shaped a developing field not only because he generated intellectual excitement but also because he cultivated a network of a dozen of so pioneer computer scientists and engineers. Like him, most of them had gained experience with computers in the 1950s when they worked in institutions located in the Boston-Cambridge area. This environment provided a supportive context for early computer research and development in much the same way that the Los Angeles/Cal Tech environment had fostered early aerospace development.[11]

Many in Licklider's network had taken degrees in MIT's electrical engineering department, served as research assistants and associates in MIT's Digital Computer Laboratory, and/or held research posts in the computer division and groups at MIT's Lincoln Laboratory.[12] These facilities provided them access to several of the few large-scale research computers then available, including the early successors to the Whirlwind computer.

There were extensive shared institutional affiliations between the MIT/Lincoln Lab network and ARPA. Licklider, Ivan Sutherland, and Lawrence Roberts, three of the first four directors of the Information Processing Techniques Office of ARPA, had been MIT graduate students or faculty members and also researchers at Lincoln Laboratory. Leonard Kleinrock, whom we shall find playing a major role in ARPANET history, was an MIT classmate of Sutherland and Roberts.

At ARPA, Licklider moved into a strategic position that enabled him to champion his network of computer pioneers.[13] Under him and his immediate successor, IPTO became the country's leading source of research funds for computer development. He concentrated on selecting and funding research and development projects nominally categorized as military command and control–related but more precisely characterized as interactive computing, or man-machine symbiosis—his deepest commitment.

"In Cambridge everybody was excited about making it [interactive computing] exist," but so many developmental problems still remained that Licklider began searching for the best universities in which to fund interdisciplinary centers dedicated primarily to solving its problems. He had earlier selected graduate student assistants by canvassing outstanding universities and choosing the students who scored highest on the Miller analogy test. He believed that any who scored above 85 on the test should be hired, because a person with a gift for seeing analogies was sure to be "very good for something." Now he looked for those he considered to be the brightest computer scientists and engineers in these same universities. Asked how he identified the outstanding universities and people, he judged the question naïve. For him, finding outstanding people "is a kind of networking. You learn to trust certain people, and they expand your acquaintance. And the best people are in the best universities, which one knows by reputation."

Licklider obtained proposals from MIT, Carnegie-Mellon, the University of Utah, Stanford, UCLA, and the University of California–Berkeley, where he intended to fund centers of excellence. He identified principal investigators for projects by "going around and talking to people." He "got proposals out of" them rather than suggesting proposals to them. As a government funder, he perceived his role as responding to suggestions, not defining projects. A number of the suggestions came from alumni of MIT and Lincoln Laboratory who had fanned out to new computer professorships in research universities. He believed he could relate to these persons on a basis of trust rather than through a bureaucratic funding structure decorated with red tape. Even though he solicited ideas and proposals from computer scientists and engineers, Licklider's own overarching vision influenced the suggestions that they made. In

his travels to centers of computer activity and to conferences, he deliberately talked about interactive computing and even about the possibility of a future network of computers. "We would get our gang together," Licklider recalls, "and there would be lots of discussion, and we would stay up late at night, and maybe drink a little alcohol and such." Conversation—and a little alcohol—often provoked responsive ideas, requests for funds, and gradually a loosely linked network of contracts—not unrelated to interactive computing. A system builder, Licklider conceived of the contracts as components from which coherent and systematic research and development programs that transcend individual universities could be fashioned.

His vision—his systems—not only responded to military needs, but also paved the high road to the future of nonmilitary computer engineering and science. He found that if he conceptualized his goals on a high level of abstraction—and if his principal investigators followed suit in similarly stating their objectives in their contract proposals—then "what the military needs is what the businessman needs is what the scientist needs." Their needs appear different only after the level of abstraction is reduced to specific tasks.

))))) Time Sharing: MIT's Project MAC

The evolution in the computer field from a single computer serving a single user at a central location to time sharing, which provided access for a number of users with individual terminals to a single computer, is analogous to the evolution of electric light and power systems from isolated generating plants that supply electricity to a single household or commercial establishment to central generation stations that supply electricity by distribution lines to a number of consumers. The analogy is especially apt when applied to a time-sharing system with individual terminals connected by a telephone network to a central computer.[14]

Licklider considered time sharing a necessary preliminary to interactive computing. He argued that the introduction of time sharing in place of the current dependence on batch processing would greatly improve command and control systems. Needless to say, he saw time sharing as enhancing computing in the civil sector as well.

Licklider generously funded Project MAC (multiple-access computer or machine-aided cognition), a large, innovative time-sharing project at MIT. MAC not only provided increased and more convenient use of large mainframe computers but also became a major stepping-stone toward the networking of mainframe computers.

Licklider and Robert Fano, an MIT electrical engineering professor, conceived of Project MAC in 1962. Instead of seeking funding for a single MIT laboratory to develop time sharing, Fano submitted a multimillion-dollar proposal in January 1963 that would allow him to distribute funds among a number of MIT research centers that wanted to participate in the development of time sharing.

In his two-page request for funds, Fano argues:

> . . . the nation is facing many urgent information processing problems both military and civilian, such as those involved in command and control and information storage and retrieval. The number of people skilled in the techniques of information processing (not just programming) is insufficient to attack these problems both effectively and speedily.[15]

Project MAC, with an initial funding of $2.2 million, became operational in 1963. A year later the system served about two hundred users from a number of academic departments. At any one time, about thirty researchers could each use one of the hundred consoles, or teletype terminals, on campus or in faculty homes connected by telephone lines to an IBM mainframe computer. Despite a number of problems, such as the loss of centrally stored data, the installation and growing prominence of MAC encouraged a number of profit-motivated computer-utility ventures.[16] MIT professor John McCarthy, an early conceptualizer of time sharing, suggested

by analogy the potential of commercialized time sharing. In a 1961 lecture, he predicted:

> If computers of the kind I have advocated [time sharing] become the computers of the future then computing may someday be organized as a public utility just as the telephone system is a public utility. . . . The computer utility could become the basis of a new and important industry.[17]

Commercial ventures began connecting paying customers to central mainframe computers possessing large stores of information and calculating power. In 1965, computer utilities and their stock issues became the "hottest new talk of the trade," but within several years serious difficulties in developing the needed software led to the bursting of the bubble and the folding of a number of companies that had sprung up to provide computer access for organizations and individuals. By 1970, computer utilities had become one of the "computer myths of the 1960s," thus providing one more example of a long history of false starts and dashed hopes in newly established, rapidly developing and changing fields of technology.[18] Several decades later, however, computer utilities flourished as hardware and software evolved.

On the other hand, Project MAC proved to be a fruitful learning experience for the research community using time sharing to access an interactive computer. Fano enthusiastically asserted that

> the availability of the MAC Computer System and of support for on-line interactive research resulted in a sudden explosion of computer research on the MIT campus. . . . On-line research with substantive external goals ranged from the development of problem-oriented languages in civil engineering to social systems analysis, from molecular model building to library information retrieval, from speech analysis to plasma physics, and from mathematical analysis to industrial dynamics.[19]

Time sharing suggested to a number of computer scientists the next critical problem to address on the advancing computer research and development front. Robert W. Taylor, successor to Licklider and Sutherland as head of IPTO, saw in a flash in 1966 the possibility of expanding the interactive community of computer users when he observed and speculated about the presence of three terminals in his Pentagon office. Each of these were connected by long-distance telephone lines to a time-sharing computer at an ARPA-funded research site. Because he often wished to communicate simultaneously with all three and because he shared Licklider's community-creating instincts, he decided that the three time-sharing computers should be interconnected to form a network.[20]

Like Licklider, Taylor possessed a vision of the future of computer communication, even though he, without hands-on computer experience, had to depend on others for the technical competence to fulfill the vision. Seeing that each of the various ARPA-funded sites "was digitally isolated from the other one," he decided "to build metacommunities by connecting them."

Precedent existed for such a network. Tom Marill, a psychologist who had studied under Licklider and who had established a small time-sharing utility, proposed to ARPA that he interconnect a Lincoln Laboratory computer with one in Santa Monica. ARPA counterproposed that Marill carry out the project under the aegis of Lincoln and that a young Lincoln researcher, Lawrence Roberts, later the ARPA administrator in charge of the ARPANET project, be in charge. Connected by Western Union lines, the computers exchanged messages, though the network was low in reliability and response time. Yet, Marill reported, he could "foresee no obstacle that a reasonable amount of diligence cannot be expected to overcome."[21]

]]]]]] Launching ARPANET

Licklider could take satisfaction in seeing Project MAC launched before he relinquished the reins of IPTO in 1964 to return to BBN. He had decided, however, that the time was not yet ripe for networking, time-sharing computers. He bequeathed this challenge to

his successors. The successor IPTO heads carried on in his tradition of choosing and funding centers and projects. Sutherland, a Ph.D. in electrical engineering from MIT, followed Licklider (1964–66); Taylor, with an M.A. in psychology from the University of Texas, was next in line (1966–69)—we should recall that Licklider was also trained in psychology; Roberts, another MIT Ph.D. in electrical engineering followed (1969–73); then Licklider returned for another stint as IPTO head in 1974.[22]

Though Taylor came to ARPA from NASA, where he had been a program officer, the others came to ARPA for two- or three-year terms while they were on leave from a university or a research laboratory. Their circumscribed time at ARPA and their backgrounds contradict the oft-heard criticism that government funding of technology and science projects necessarily places control in the hands of time-serving, unimaginative bureaucrats. No bloated organization, IPTO remained lean, with a small staff of two or three assisting the head. IPTO's funding of a few large-figure projects rather than of many small ones reduced the administrative chores. The prospect of administering funding on a large scale seems to have been one attraction that brought capable administrators to IPTO.

Congress supported IPTO generously. Licklider and his successors learned from other ARPA administrators techniques best suited to approaching Congress. These included requesting funds for projects that had already demonstrated notable achievements; start-up funds for a project often came under the umbrella of a related project already under way.

IPTO heads found funding the development of a computer network congenial because it promised more than a 10 percent improvement in the state of the art; it was two or three years ahead of industry's achievements in the field; and it was a large-budget project likely to absorb a million dollars or more. During the first decade or so of the project, these funding criteria tended to be the norm at ARPA, especially at the IPTO.

IPTO initiated the ARPANET project in 1966 after IPTO head Taylor brought pressure on Lincoln Laboratory, an ARPA contractor, to release Roberts to preside as project manager over development of a computer network to interconnect time-sharing computers at the seventeen ARPA-funded academic, industrial, and government computer centers around the country. An analogy with the earlier history of electric power system development again becomes appropriate. Decades earlier, electric utility managers had begun to interconnect their central stations by transmission lines, thus forming regional power systems. The interconnections provided substantial economic advantages because a central station overloaded at any particular time could draw power from central stations in the regional system that were not overloaded. This raised the so-called load factor, or capacity utilization, of all the stations. A computer network could similarly allow sharing of resources to meet demand.

Roberts reluctantly left Lincoln, even though he had experienced difficulty in transferring his inventions and discoveries from the laboratory into the field. He recalls that their highly innovative work on networking computers remained largely unapplied. He believed that as an ARPA administrator he could act as a gatekeeper to spread ideas among university and industrial contractors. Military applications concerned him only remotely, even though he knew that ARPA projects had to interest the military.[23] ARPA also lured him because of his interest in interconnecting computers, an interest initiated by the same 1962 conference on the future of computing that had sparked Licklider's and Fano's interest in time sharing. Roberts remembers Licklider and "a bunch of people from MIT" talking late into the night about the future. A conversation about the need of researchers using computers in far-flung locations to be able to exchange data and software directly left a lasting impression on him.

Following Licklider's managerial style, Roberts and Taylor turned to their principal investigators, the computer scientists and engineers who presided over the ARPA-funded centers, for network algorithms, specifications, and means for performance evaluations. At an annual general meeting of principal investigators convened in Michigan in 1967, networking became the topic of

discussion.[24] From that meeting, Roberts carried away several inventive concepts. Wesley Clark, an IPTO principal investigator at Washington University in St. Louis who had worked earlier on Project Whirlwind and SAGE at Lincoln Laboratories, suggested that host, or mainframe, computers should be interconnected not directly but through small interface computers that would provide a link between each host computer and the interconnecting network. Such an arrangement would permit host computers with different characteristics to connect through interface computers to a common communication network made up of telephone lines.[25] The role of the interface computers can be compared to that of the transformers, motor generator sets, and frequency changers that allow electric generating stations of different characteristics to connect to a transmission grid with its common voltage and frequency characteristics.[26] An alternative would have been to require all computers coming on the net to have standardized characteristics, but ARPA and its principal investigators preferred diversity.

While the principal investigators eagerly discussed the technical problems of creating a computer network, they proved far less enthusiastic once they had to face its funding requirements. ARPA saw networking as a means to reduce the principal investigators' need for funds to increase computer capacity and to support independent software development. Instead of purchasing increased capacity, ARPA wanted the seventeen computer centers to share capacity in a fashion similar to that employed by interconnected electric-generating stations. An overloaded station at any particular time, for example, would use the capacity of one underloaded at that period. At other times, the load circumstances of the stations might be reversed and the exchange would be in the opposite direction. This improved, as we have noted, each station's load factor.

The principal investigators initially preferred to have computer capacity completely under their local control and to develop their own software locally. After the net began operation, they began to see the advantages of resource-sharing. "It was only a couple years after they had gotten on it," Roberts remembers, "that they started raving about how they could now share research, and jointly publish papers, and do other things that they could never do before."

Within a few years, the concept of a computer network had become well enough established that in a 1970 article Roberts defined one as

> a set of autonomous, independent computer systems, interconnected so as to permit interactive resource sharing between any pair of systems. . . . The goal of the computer network is for each computer to make every local resource available to any computer in the net in such a way that any program available to local users can be used remotely. . . . The resources that can be shared in this way include software and data, as well as hardware.[27]

Independent computer "systems" referred to the time-sharing feature already in place at the various computer resource centers. Roberts foresaw that just as time-shared computer systems allowed hundreds of users to share hardware and software, networks interconnecting dozens of time-sharing systems would permit resource-sharing by thousands. He did not predict that within a generation the number would be in the millions and that the network would become a commercial enterprise as well as a research facility.

]]]]] Inventing ARPANET: Packet Switching

Besides the decision to develop small interface computers for the ARPANET, Taylor, Roberts, and their principal investigators decided to use packet switching. This technique had originally been proposed by Paul Baran, a researcher in the computer sciences department of RAND's mathematics division. Like other researchers who had earned a level of credibility from their RAND colleagues, Baran had considerable freedom in choosing his research problems. He decided that he could make the greatest contribution by tackling one aspect of a pressing Cold War problem upon which a number of RAND researchers were working. They hoped to find ways of assuring the survival of a retaliatory strike force following a devas-

tating attack. With a reasonable possibility of such a survival and retaliation, a first strike by either the Soviet Union or the United States became less likely. If there was no possibility of retaliation, either power might launch a peremptory strike, if the other gave evidence of preparing to launch an attack.

Baran chose to work on the problem of creating a communications network that could survive the strike and then command and control a retaliatory missile response. Not only did he believe that his experience with digital computers prepared him for such a task, but he also hoped that a secure communications network would alter the black-and-white choices of the Cold War adversaries to a gray—a light gray.[28]

After several false starts, he focused on the development of a "distributed adaptive message block network" system that he and several colleagues had generally defined by 1962 and then described in detail in a series of reports published in 1964.[29] "Distributed" refers to a noncentralized, nonhierarchical system in which the network transmission and reception nodes have a peer relationship. (By contrast, a hierarchical or centralized communication network has a single point of control from which connections extend to the various nodes. A single explosion could destroy the point of control.) The routing of messages is "adaptive" because they can be directed along different routes in the interconnected system to reach a particular node or destination. Each node is programmed to sense the availability of open connections or routing; if one route between nodes is loaded, then the node will automatically instruct message blocks to flow through alternative connections. "Message blocks" refers to the small packets into which messages are broken. The blocks are reassembled at their destination.

Baran later observed that it is relatively easy "to propose a global concept. It is far more difficult to provide enough details to overcome the hurdles raised by those that say 'It ain't gonna work.' " Stringent criticism came from the engineers in RAND's communications department who designed analog systems as well as from those engineers at AT&T who had long experience in designing and operating analog long-line telephone systems.

Baran found the AT&T headquarters people to whom he made his presentation gentlemanly—"Talk politely to them and they would invariably talk politely back to you"—but resolutely committed to analog systems and dismissive of digital ones. AT&T presided over a "monolithic" and "totally integrated" system requiring that any technological addition to the system had to fit in with existing equipment; it accepted evolutionary technological change, but rejected revolutionary change. Baran took as characteristic of the corporate culture a remark made to him by one AT&T engineer after an exasperating session: "First, it can't possibly work, and if it did, damned if we are going to allow creation of a competitor to ourselves." Later the creators of the ARPANET found AT&T unenthusiastic about providing the telephone connections they needed.

Even though RAND formally recommended to the Air Force that the distributed system be developed and despite the recommendation to proceed made by a 1966 Air Force evaluation review committee organized by MITRE, the system was not deployed until ARPA adopted and adapted it for the ARPANET several years later. Baran believes that the Defense Department did not have the in-house technical competence to develop the system.

Later reflecting about his contribution, Baran said:

> The process of technological developments is like building a cathedral Over the course of several hundred years, new people come along and each lays down a block on top of the old foundations, each saying, "I built a cathedral." Next month another block is placed atop the previous one. Then comes along an historian who asks, "Well, who built the cathedral?" Peter added some stones here, and Paul added a few more. If you are not careful, you can con yourself into believing that you did *the* most important part. But the reality is that each contribution has to follow onto previous work. Everything is tied to everything else.

Baran's thoughts about the difficulty historians have in dealing with simultaneity of discovery and invention and about their cumulative nature are prescient. Historians may well decide that

Leonard Kleinrock—not Baran—through his research and publications introduced concepts later adapted for and embodied in the ARPANET as packet switching and distributed routing of data. When a Ph.D. candidate at MIT, Kleinrock did a dissertation, published in 1964, in which he analyzed the effectiveness of "time slicing," which anticipated packet switching, and of distributed control in data networks. Larry Roberts and others who designed the ARPANET were more familiar with Kleinrock's ideas than with Baran's.[30]

To add another stone to the "cathedral," at about the same time Donald Davies, at the National Physical Laboratory (NPL) in Teddington, England, also conceived of a version of message-block transmission that he called "packet switching." Without knowledge of Baran's earlier work, he sought to reduce the cost of using telephone lines as linkages for time-sharing computers. In the spring of 1966, Davies organized a seminar at the National Physical Laboratory where he presented his scheme for computer network communications. He referred then to message packets rather than message blocks. Financially constrained, Davies and associates at NPL then built a one-node network using packet switching, but on a much smaller scale than the ARPANET.

Baran, Kleinrock, and Davies constructed their systems on long-standing foundations built of the prior solid work of other pioneers. Telegraph stations had stored entire messages when lines were overloaded, forwarding them as the lines became free, much in the way that packet-switching nodes held and transmitted message blocks or packets. In the early 1960s, the military used a digital store-and-forward message-switching system called AUTODIN.[31]

)))))) Inventing ARPANET: IMP

The problem of interconnecting host computers of different manufacture and design was discussed at the annual principal investigators meeting in 1967. At that meeting, Wesley Clark suggested as a solution to the heterogeneity problem the placing of small interface, or gateway, minicomputers between host computers and the network. The host computers would see the interface computers as "black boxes" for providing an interface to the telephone line–linked network. The research centers would not need to be involved with, or even understand, the functioning of the "black-boxed" gateways, except insofar as it was necessary to design a hardware or software connection to them from the host computer. The separating and black-boxing of functional responsibilities became known as "layering." In this instance, the host computers constituted one layer and the gateway computers another. Layering reduced the complexity with which designers and operators had to contend.[32]

In 1967, Roberts found that his principal investigators judged the gateway scheme technically feasible, so he looked for an organization to design and build the projected gateway computers, which he called "interface message processors" (IMPs). Others referred to them as "packet switches" because they routed the message packets among alternative links interconnecting IMPs. In the summer of 1968, IPTO circulated to 140 potential bidders a "request for proposals" for the design and construction of a physical network including the IMPs and their software.[33] Drawn up by Roberts and principal investigators, the RFP provided a general conceptual design for the IMPs and also specified criteria that would be used to select the winning bidder. These included the understanding of the technical problems, the availability of experienced personnel, the performance characteristics of the small computer to be used as the IMP, and the general quality and commitment of the submitting firm to the project.[34] The RFP called initially for a four-node network to be deployed in nine months and a system capable in the long run of incorporating nineteen IMPs. Specifications called for IMPS to break messages into packets, to provide buffers to store packets, to route the message packets, and to monitor the network's flow of traffic.

Raytheon, Bunker-Ramo, Jacobi Systems, and Bolt Beranek and Newman, in addition to eight other firms, responded. In January 1969, IPTO awarded the contract to BBN, a small, entrepreneurial Cambridge, Massachusetts, research firm that Licklider served as vice president and whose early history we have summarized. BBN had previously received ARPA contracts and been designated one of the node sites on the projected computer network, but the IPTO contract amounted to an unusually large one for this organization of only several hundred people.[35]

BBN enjoyed a reputation as highly innovative, noted for its research and development. BBN proposed to depend on the nearby Honeywell company to provide a minicomputer that would be redesigned as an IMP and then to manufacture a number of IMPs. Because BBN researchers enjoyed considerable latitude in choosing their problems, the firm attracted outstanding young MIT and Harvard graduates who found front-edge research coupled with freedom from teaching a stimulating combination. In addition, generous contracts had provided the company excellent research and test facilities. In the eyes of IPTO, BBN's extensive experience in designing time-sharing networks also enhanced its credentials. The firm had established MEDINET, a medical system designed in collaboration with General Electric.

Robert Kahn, later an IPTO head but then an associate of the BBN team that developed the IMP, offered the following characterization:

> BBN was a kind of hybrid version of Harvard and MIT in the sense that most of the people there were either faculty or former faculty at either Harvard or MIT. If you've ever spent any time at either of those places, you would know what a unique kind of organization BBN was. A lot of the students at those places spent time at BBN. It was kind of like a super hyped-up version of the union of the two, except that you didn't have to worry about classes and teaching. You could just focus on research. It was sort of the cognac of the research business, very distilled. The culture at BBN at the time was to do interesting things and move on to the next interesting thing. There was more incentive to come up with interesting ideas and explore them than to try to capitalize on them once they had been developed.[36]

BBN organized a small team of five or six persons to design the interface message processor. The resulting set of design specifications embodied a number of inventive solutions to network problems. Future historians fully aware of the remarkable development of the worldwide Internet following hard upon the path-breaking ARPANET may some day compare the inventive success of the small BBN group to the achievements of Thomas Edison and his small band of associates who invented an electric lighting system.

Engineers and scientists with MIT and Lincoln Laboratory backgrounds dominated the BBN team. Frank Heart headed the team that drew up the proposal for ARPA and then developed the hardware and software. After obtaining a master's degree in electrical engineering from MIT, Heart had worked on the Whirlwind computer, then had been a researcher and group leader at Lincoln Laboratory for fifteen years before moving to BBN in 1966. William Crowther, who had the responsibility for developing the IMP software, had a master's degree in physics from MIT; he had been a researcher at Lincoln Laboratory for ten years before he went to BBN. Severo Ornstein, designer of the IMP hardware, had a Harvard undergraduate degree in geology and had learned about computers in the field, having served as a researcher at Lincoln Laboratory for seven years. David Walden, who assisted Crowther with the software, after taking an undergraduate degree in math from San Francisco State College, then did research at Lincoln Laboratory for three years. Kahn contributed to the team as an authority on communications theory and as a designer of systems architecture. Earlier, he had earned a Ph.D. in electrical engineering from Princeton University before becoming an MIT faculty member in electrical engineering and a researcher on MIT's Project MAC.

Heart characterizes his team's endeavor as "a labor of love"; members of the team refer to their work as "fun." He encouraged team members to think holistically about the project and to reach decisions by consensus. Only as a last resort did he employ his authority as team leader.[37] Walden remembers that "mostly what happens is you sit in a room and argue about it until you all agree about what the right answer is."[38] Heart's style, like that at the IPTO office at the time, involved finding "bright" people interested in the problem at hand and giving them free rein.[39] The BBN culture called for macro-, not micro-, management.

Many of the engineers and scientists engaged in the ARPANET project began their professional careers during the turbulent counterculture 1960s. They became enthusiastic advocates of consensus over hierarchical management. They stressed the meritocratic nature of their problem-solving communities, which were also populated mainly by keen and dedicated scientists and engineers. The work characteristics of several of the BBN team members reinforce the supposition that the counterculture values of the 1960s influenced their behavior. Heart on one occasion expressed concern that Crowther would wear his sneakers to a high-level meeting at ARPA headquarters in Washington, D.C., which in fact he did—but without disturbing the tenor of the proceedings. Ornstein, who took part in antiwar demonstrations, only half-jokingly threatened to pin a resistance button on the colonel with whom the BBN team was negotiating.[40] In their recollections, members of the team generally describe their relations with the military as remote, despite their being a military agency and the ARPANET's having originated with military needs. Ornstein recalls that the team members felt "insulated" from the military.

Heart's team had offices side by side so that they could engage in countless informal meetings to discuss design problems. They kept informal working memoranda called "the IMP Guys' notes." (In time, the BBN team began to refer to itself as the IMP Guys.) When a breakthrough occurred, Walden remembers, "We'd run in and say, 'Look, I got this running. Somebody come and type on the teletype. . . . This is exciting. Something is cycling.' "[41]

The designers of software and hardware closely interacted. Ornstein, who had principal responsibility for developing the hardware, remembers solving problems by spending countless hours late into the night at his home with Kahn, the expert on systems architecture. Kahn had previously had little experience with designing hardware, but his omnivorous interest in all aspects of the IMP project led him to question Ornstein incessantly, thereby learning in depth from

THE ORIGINAL IMP GUYS: TRUETT THATCH, BILL BARTELL (HONEYWELL), DAVE WALDEN, JIM GEISMAN, BOB KAHN, FRANK HEART, BEN BARKER (BEHIND HEART), MARTY THORPE, WILL CROWTHER, SEVERO ORNSTEIN, BERNIE COSELL (NOT PICTURED, AND HAWLEY RISING (NOT PICTURED). *(Courtesy of Frank Heart)*

him. Ornstein himself worked closely with Crowther, the software expert, whom he found to be "a brilliant programmer . . . [who] thoroughly understood machine language code, the kind of code that you have to whittle down."[42]

Walden recalls that designing the IMP was essentially a problem in engineering design rather than the application of theory:

> It becomes an engineering problem as opposed to a theory problem. . . . We had to send these bits down the wire: how do you put a header on the front; how do you put a trailer on the back. There was a theory of how you put error correcting codes on it. Bob Kahn knew that theory and told us what it was. There were some constraints: this is the way that the 303 (or the 301 or

> whatever the Bell modem is) has to be interfaced to, but after that it was all pretty pragmatic. Not lots of theory coming from someplace else.[43]

The group designing the IMP did not rely heavily on information, or communications, theory. The BBN team, like the engineers and scientists who designed the heat shields for the Atlas and Titan missiles, could find little theory relevant for guiding their empirical thrusts, explaining their empirical successes, or rationalizing their empirical designs. Walden recalls that what was later "taught in courses in communications about networks and protocols and all of that, I would say we were mainly . . . inventing it—the academic analysis tended to come later."

Kahn, who had studied information theory and worked at Bell Laboratories, where Claude Shannon, the leading theoretician, had developed his concepts, often questioned the engineering empirical, or go-ahead, approach of other members of the team, some of whom always had their heads "right down in the bits." He applied theoretical analysis insofar as he could in designing simulations of message traffic flows predicted for the network. His associates found that he moved along the mountaintops of theory, but often did not have the patience to explain the details of an idea that he was championing. Expecting them to grasp his ideas, he might say, "Don't you see it? It is all there."[44] Because his approach differed sharply from theirs, some of the other members of the team despaired of his ever becoming a "computer person" despite his learning from Ornstein. They felt that he would "never come to understand the problems looking at them his way."[45] In time, the team found that had they heeded some of the criticisms from Kahn and others of a like-minded theoretical bent, it would have saved them some missteps. Furthermore, the team could have drawn more heavily on prior network theory published by Kleinrock and others. Kleinrock believes that the BBN team concentrated too much on obtaining an IMP design that would work and not sufficiently on developing a network that would perform well under various constraints.[46]

During the nine months that elapsed between the awarding of the contract and the delivering of the first IMP, Ornstein and his assistants concentrated on adapting a Honeywell 516 computer for use as an IMP. Crowther and his associates wrote the software for it. Ornstein found working with the Honeywell to adapt its 516 computer for use as an IMP trying. He had assumed that the 516 was a mature machine, but he discovered "bugs" in it and had to "diddle" with it extensively. He characterized the Honeywell engineers with whom he dealt as being "industrial strength," not "research strength" people. Most of the time, Honeywell sent "cabbages instead of computers"; the company not only delivered machines behind schedule but ones that did not work. Ornstein had to become "quite nasty at times and beat on the table." Yet Honeywell shaped up, producing special hardware under great pressure.[47]

Crowther strove for software that would enable the IMP to route the packets among the alternative routes to their destinations in such a way as to minimize cost and time of transmission and to optimally utilize the capacity of the distributed network. To do this, the software provided each IMP information continuously about the state of traffic, or information, flows throughout the network. Furthermore, when a route was not instantly available, the software placed message packets in waiting queues, or buffers. Crowther, who considers designing a routing algorithm a "fun" thing to do, tells us about his approach:

> If given a complex system and an algorithm, like a routing algorithm, I tend to be pretty good at visualizing the thing and seeing what will happen and what some of the bad cases are. So there were a lot of mental things like that. When you came up with one that looked pretty good, then you'd try it and see whether or not it worked.[48]

Shortly, we shall consider the testing done to "see whether or not it worked."

LEONARD KLEINROCK, AN ARPANET PIONEER. *(Courtesy of Leonard Kleinrock)*

The Network Working Group

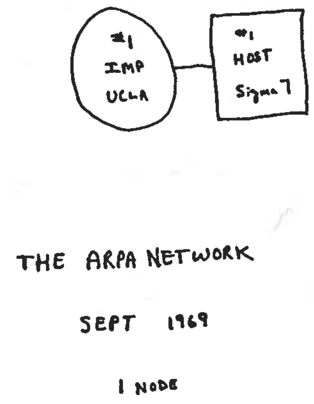
The BBN team delivered the first IMP in September 1969 to Leonard Kleinrock's computer research center at the University of California–Los Angeles. The ARPA office had chosen this site as the initial node on the four-node experimental ARPANET. In short order, Walden, the junior software designer, delivered three more to the University of California–Santa Barbara, the Stanford Research Institute (SRI), and the University of Utah.

Kleinrock, one of ARPA's well-funded principal investigators, had a group of about forty working in his center: secretaries, programmers, managers, faculty, and graduate students. He had studied in MIT's electrical engineering department, then served on the Lincoln Laboratory research staff at the same time as Roberts. They, with Sutherland, who preceded Roberts as head of IPTO, stood their final MIT Ph.D. theses defenses together in 1959. Though they worked on independent projects, all three of them used Lincoln's TX-2 computer. One of the most complex computers available at the time, a successor to the Whirlwind computer, TX-2 was a device on which many pioneers in the computer field learned computing, hands-on. Kleinrock used the TX-2 to simulate computer networks, an experience that prepared him for his contribution to the development of the ARPANET. In 1963, he left the Lincoln Laboratory to take a professorship at UCLA where he discovered that "I love teaching, I love research, I love the environment."[49]

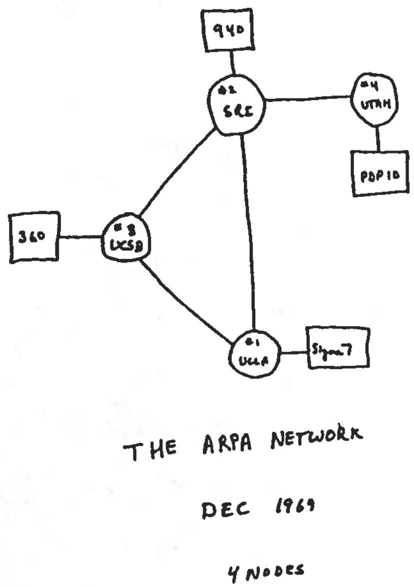
When Kleinrock watched the first IMP being wheeled into his computer research center, he and his team faced the daunting tasks of installing both hardware and software that would allow his center's host computer to "talk to," or "interface with," its IMP. The team could expect some help from Kahn, of the BBN team, who had spent weeks drawing general specifications for the connection between a host and its IMP.[50] While BBN took full responsibility for connections and communications among IMPs, Kleinrock's graduate students had the responsibility for developing the host-to-host protocol that would enable the UCLA host computer to communicate with host computers at other sites, or nodes, on the ARPANET.[51]

Writing the host-to-host "protocol" program proved to be one of the most difficult software problems encountered in deploying ARPANET. Traditionally, protocol refers to an agreement among diplomats concerning the etiquette and precedence that will facilitate communication, deliberation, and negotiation. Analogously, in the world of computer networks, protocol, besides designating agreed-upon format, syntax, and semantics of messages, decides upon the signaling information. This information is appended to a message that directs the movement of the message from sender to recipient. In similar fashion, the address on a conventional letter provides the signal generating the activity that results in the delivery of the message by the post office.[52]

In 1969, Kleinrock and other principal investigators set up the Network Working Group (NWG), a committee that included graduate students, to decide upon protocols for the ARPANET, especially those to interconnect host computers. Roberts assigned



THE ARPA NETWORK, SEPTEMBER 1969. *(Courtesy Computer Museum History Center)*

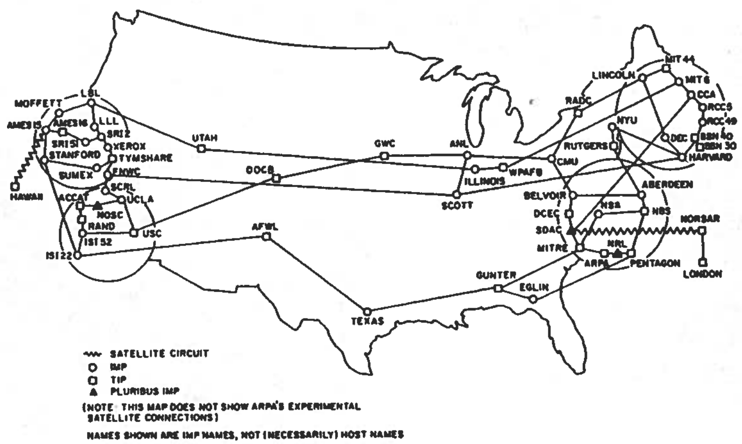


THE ARPA NETWORK, DECEMBER 1969. *(Courtesy Computer Museum History Center)* the protocol problem to a committee because he believed that ingenious ideas were dispersed among the members of the various ARPA-funded computer research centers and because he wanted all the potential sites on the ARPANET to have a stake in the project. A disproportionate number of graduate students from the computer research centers served on the NWG. In retrospect, this is not surprising because they were solving many of the computer problems that their professors had previously identified and defined.

A UCLA graduate student at the time, Vinton Cerf, who later became a major figure in the computing field, recalls:

> We were just rank amateurs, and we were expecting that some authority would finally come along and say, "Here's how we are going to do it." And nobody ever came along, so we were sort of tentatively feeling our way into how we could go about getting the software up and running. In the longer term, Larry Roberts was very insistent that this intrepid bunch of graduate students—not just at UCLA but at other sites like MIT, and Utah, and SRI, and UC Santa Barbara—get their rear ends in gear and actually make decisions about the protocols and get them instantiated and get them running in all the operating systems.[53]

Another UCLA graduate student and member of the UCLA Computer Club, which was made up of young computer enthusiasts, Steve Crocker became de facto head of the Network Working Group and its spokesperson at meetings held mostly at ARPANET sites on the East and West coasts. Crocker also initiated the taking of minutes of the meetings. These he circulated with requests for comments (RFC). Through the minutes, other notes, and the responses to RFCs, the Network Working Group community accumulated a large store of experience-based information about the processes of designing protocols. Roberts did not insist that the committee solve problems in a particular way, only that the members avoid the academic tendency to haggle over fine points and, instead, to reach by consensus decisions that could be tested empir-



THE ARPANET, JUNE 1977. *(Courtesy Computer Museum History Center)*

ically. Alexander McKenzie, who took part in NWG deliberations as the BBN representative, suggests the management style of Roberts and ARPA:

> The program managers at ARPA didn't really have the time to tell people how to do it. They really much more aimed at funding people who were smart enough and self-motivated enough to recognize what the problems were and go solve them. There was, in my experience, zero or close to zero micro-management of any ARPA program—either at BBN or anywhere else that I ever heard of. ARPA's objectives were to find people that they thought were sufficiently smart and sufficiently motivated and give them a ball and let them run with it.[54]

The committee decided to define protocols in layers. Layers, as we have indicated, are analogous to the levels of abstraction used in language studies. An example is the move up the ladder of abstraction from bricks, to walls, to rooms, and finally to houses. Architects provide a general plan for the house as well as detailed plans for walls and rooms. The house is one layer, the walls another. Similarly, computer network designers provide host-to-host protocols as well as protocols for other layers, such as host-to-IMP.[55] The Network Working Group settled upon basic ARPANET protocols in 1972, including one for facilitating host-to-host communications, which had taken two years to develop and which became known as the Network Control Program (NCP); and another to control communications from host to IMP and among IMPs.[56] In the 1970s, another protocol for host-to-host communication, the TCP/IP (Transmission Control Protocol/Internet Protocol), replaced the NCP.

Because most of the principal investigators and many of their graduate students at the intended ARPANET nodes—to be located at academic computer research centers—initially took a dim view of Roberts's plan to bring their host computers onto the ARPANET, haggling characterized some NWG meetings. Not only did the footdraggers prefer having additional resources under their unilateral control to sharing them across the network, but they also realized that adapting their host computers to the protocol software meant that "they would have to do major surgery on their operating system to get this network to talk to them."[57] Responding to this resistance, the designers of the protocols kept them simple so as to minimize the difficulty of writing the program for adapting the host computers to the requirements of the common protocols. Roberts persisted in building the network by brandishing carrots and sticks until, by August 1972, the fifteen host-computer sites connected to the network were using the NCP protocol.

Heart, as IMP team manager at BBN, found that

> it was a surprise how tough it was to agree on the host protocols. . . . The network's utility was delayed for at least two years because of a misapprehension of how hard it was going to be to get the host protocol suites in place. . . . It's like picking up the phone and calling France—if you don't speak French you've got a little problem. So even if you get the connection to the two telephones, if you don't speak French you don't communicate very well . . . I think it was misperceived how long and hard that would be to do.

And he adds:

> While they might have some differences, the overriding single bit was they were all having a great time. And they all thought it was very exciting.

]]]]] The Network Measurement Center: Testing

We have noted the critical feedback links between design and testing during the development of the Atlas missiles. The conceptual design of a technological system and its components consists often of a set of hypotheses about what might work, what might fill the system's requirements, or goals. Testing is a means of validating or invalidating the hypotheses. If tests fail, the designers modify the hypothesis. Alternatively, the testing may show that the failure resulted from prototypes used in testing not having been constructed in accord with the design specifications. The initial deployment of the four-node ARPANET amounted to a research prototype needful of testing.

The Network Measurement Center funded by ARPA and headed by Kleinrock did experimental design and stress testing. His earlier research on queuing theory and its application to evaluating computer network performance prepared him well for the task. Having written his dissertation on store-and-forward networking, he had a theoretical understanding of the functioning of the IMPs and of the communications network of which they were a part. In drawing up the preliminary specifications for the network and the IMPs in 1968, Roberts had turned to Kleinrock for advice.

Kleinrock and the graduate students created theoretical models of projected network performance (including failures such as transmission deadlock). They decided how to test the actual performance of the ARPANET and to compare this performance with an idealized network. Cerf, as a UCLA graduate student, assumed a leading role in the testing, working closely with Kahn of BBN, who shared Kleinrock's interest in theory and testing. Earlier, Kahn had tried to persuade his colleagues at BBN, including project leader Heart, that the BBN team had done insufficient theoretical analysis and testing before shipping the initial IMPs.[58] Roberts, often in contact with Kahn, tended to agree. He not only assigned the Network Measurement Center the testing project, but also gave a contract for network topology studies to the Network Analysis Corporation, a small for-profit firm headed by Howard Frank that specialized in network design analysis.[59] Frank, an electrical engineer from the University of California–Berkeley, and a friend of Kleinrock's, had made a name for himself several years earlier when he and Kleinrock analyzed and reorganized the layout of a major pipeline system, thereby saving the operators sizable operating expenses.

Encouraged by Kahn, a Kleinrock graduate student wrote software that loaded, or drove, heavy message traffic into the network in order to discover how many packets would be lost and the length of transmission delays that would result from such overloading. Kleinrock and Kahn observed the behavior of the congestion-control mechanism on the IMPs that decided when to switch packet routes. For example, packets could travel directly from UCLA up to the Stanford Research Institute, or alternatively they could move to SRI by way of the IMP at Santa Barbara. On occasions an IMP switched back and forth rapidly between two routes looking for the less congested path. If the paths were almost equally loaded and the IMP added traffic to one, then this route became the congested one, so the IMP switched to the other, and so on, back and forth.

Kahn had worried about the reliability of Crowther's design for the software that controlled the IMPs dynamic routing of messages through the IMP subnet. Kahn predicted that congestion on a heavily loaded subnet would occasionally cause a system "lockup," or bottleneck, because the storage capacity of the IMPs would be filled to capacity with message packets, thus additional incoming packets would be rejected. Katie Hafner and Matthew Lyon in their history of the ARPANET, *Where Wizards Stay Up Late* (1996),[60] describe "grand little fights" among the BBN team members about the design of the congestion-control mechanisms. Ornstein said that

"some of the things . . . [Kahn suggested] were off the wall, just wrong." Gradually, the Heart team paid less attention to Kahn's strictures: "Most of the group were trying to get Kahn out of our hair," Ornstein recalls.

In January 1970, with the four-node network in place, Kahn decided to visit Kleinrock's UCLA center in order to test his theory that the network could have a congestive failure. He took Walden with him to manipulate the IMP code so as to vary the size and frequency of the packets passing over the network from IMP to IMP. "By besieging the IMPs with packets, within a few minutes he and Walden were able to force the network into catatonia."[61] After returning to BBN, Kahn showed results from this and other tests to Heart and Crowther. Finally persuaded that the network could lock up, Crowther worked with Kahn to rectify the problem. Kahn could argue, in retrospect, that earlier theory should have guided practice; the IMP Guys could argue that theory was playing its proper role of following upon and rationalizing practice.

]]]]] The 1972 Demonstration

With ARPA regularly adding computer research centers to the network, Roberts wanted to increase net traffic. In the fall of 1971, the network ran at only about 2 percent capacity and only the inner sancta of the computer research community knew of the network's existence.[62] The slow development of network protocols partially explains the low utilization of the network. Believing that more information about the potential of the network needed to be disseminated, Roberts encouraged publication of articles about the ARPANET in professional journals. He also decided to sponsor a public demonstration of the new technology, a practice often resorted to by inventor-entrepreneurs. At the turn of the century, wireless and airplane inventors had repeatedly used public demonstrations to raise capital and to sell their patented devices.

Roberts asked Kahn in 1971 to organize a demonstration for the first International Conference on Computer Communication to be held in Washington, D.C., at the Hilton Hotel in October 1972.

By then, the Network Working Group had defined additional protocols and BBN had adapted the IMPs so that a number of remote telephone-line-connected dial-in user terminals could be linked to a host computer in a manner reminiscent of time sharing. In addition, a Network Control Center, directed at that time by Alex McKenzie, a young Stanford-trained computer engineer at BBN, and staffed by several persons at BBN, monitored the performance of ARPANET and did remote troubleshooting.

Kahn recalls that the demonstration made the ARPANET "real to others . . . people could now see that packet switching would really work. It was almost like the train industry disbelieving that airplanes could really fly until they actually saw one in flight." Over a thousand persons interested in networking watched as computers of different manufacture, operating in the display room in a Washington hotel, communicated with other computers located at various sites on the ARPANET. Kahn and his associates urged visitors to use terminals themselves to log in to various host computers, exchange data and files. Most of the present and future leaders in the networking field were on hand. "It was a major event. It was a happening," Kahn concludes.[63]

The 1972 demonstration in conjunction with the availability of protocols and the increased reliability of the ARPANET changed the image of the ARPANET. Computer engineers and scientists no longer considered it a research site for testing computer communications but saw it as a communications utility comparable to that of the telephone system. "It was remarkable how quickly all of the sites really began to want to view the network as a utility rather than as a research project," McKenzie confirms. He and the Network Control Center wanted the ARPANET to perform as reliably as an electric power utility, but he acknowledges that in the early years the IMPs were up only 98 or 99 percent of the time, which "would be an abysmal record for a power utility."[64]

The unanticipatedly heavy use of electronic mail (e-mail), especially for personal messages as contrasted with professional ones, also moved ARPANET down the utility path. As early as 1973, e-mail

constituted three-quarters of ARPANET traffic. Not intended by its developers to be a message system, the ARPANET by the end of the 1970s nonetheless derived its greatest stimulus for growth from the e-mail traffic.[65] The history of e-mail provides substantial support for those who stress the unintended consequences of invention and development.

)))))) The Internet

Our emphasis has been upon the creation of the ARPANET, not upon its post-1972 deployment and transformation as an operating utility. We can, however, summarize some of the milestones in its postinnovational history. In 1975, ARPA, in accord with its policy of turning over research and development projects to the military once the projects had become operational, transferred the management of the network to the Defense Communications Agency, which manages communications for the military. As a result, military needs increasingly shaped the further course of ARPANET.[66]

ARPA, however, continued to fund computer network research. In 1973, Kahn, who had become an administrator at IPTO and its head from 1979 to 1985, and Cerf, who began teaching at Stanford University in 1972 and in 1976 became a program manager at IPTO, together conceived of the basic architecture of an "internet" that would interconnect ARPANET with several packet-switching networks that ARPA had established after 1970. One network used radio and another used satellites, instead of telephone lines, to provide the communication subnet.

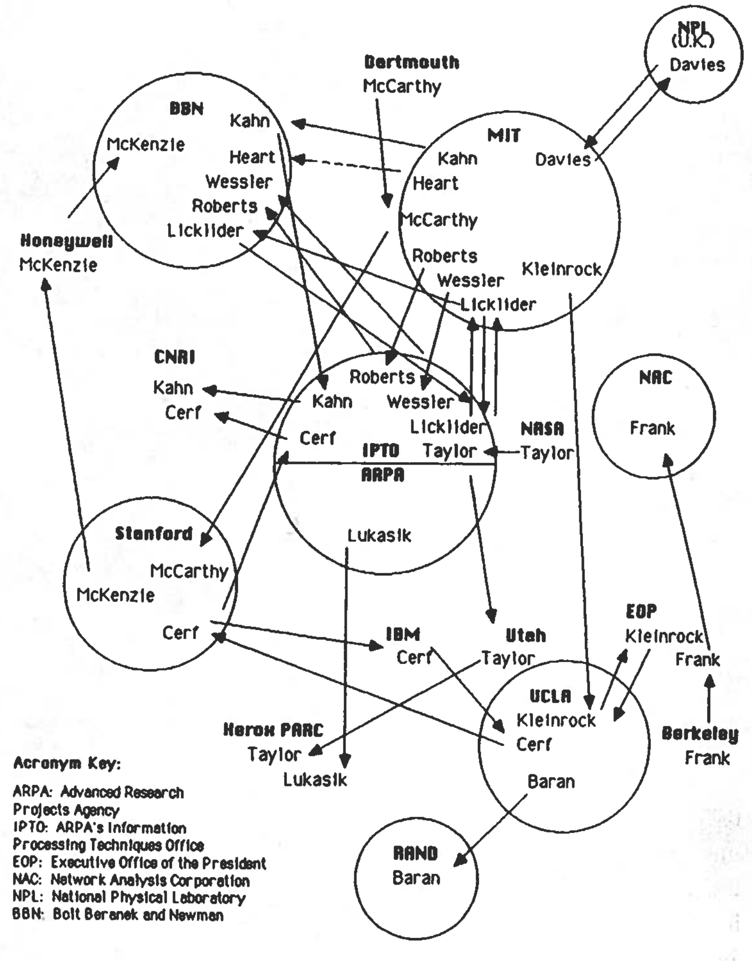
Kahn and Cerf faced the problem of interconnecting networks with differing characteristics, a problem similar to that of interconnecting different kinds of host computers on the ARPANET. Once again focusing upon the problem of defining protocols, Cerf and Kahn published a paper in 1974 in which they defined the TCP/IP protocol for use in sending messages across network interfaces. Gateway computers, placed at network interconnection points, function not unlike the motor-generator units that made possible interconnecting alternating-current and direct-current electric power networks early in the twentieth century.

Competition soon developed among those organizations favoring TCP/IP and those choosing an OSI (open systems interconnection) protocol defined by the International Organization for Standardization. In 1983, the Department of Defense helped resolve the conflict by requiring all host computers on the ARPANET to use TCP/IP. When other networks, including those located abroad, made the transition, an international internet, now known as the "Internet," came into being. In 1983, the Defense Department divided the ARPANET, which by then interconnected hundreds of host computer sites, into a smaller ARPANET and a MILNET, the former dedicated to the computer research community and the latter to military users.[67]

The National Science Foundation funded five supercomputer centers in 1986. These became the backbone of the NSFNET (National Science Foundation Net), which was used extensively by computer researchers in universities. With former ARPANET functions largely taken over by public, commercial, and private networks, including the NSFNET, the Defense Department ended ARPANET's existence as a distinct system. The Internet continued to flourish along with the establishment of an increasing number of networks that interconnected through gateway computers and the TCP/IP protocol. During the 1990s, the creation of the World Wide Web and the introduction of browsers, or search engines, have stimulated the extensive use of the Internet by individuals and profit-seeking organizations.

)))))) Management of ARPANET

In our histories of SAGE and Atlas, we stressed management of the research and development process as we focused on the upper layers of a large managerial structure that presided over thousands of contractors. Only occasionally did we catch a glimpse of the way in which a small team of ten to twenty scientists and engineers organized the management of a small-scale problem-solving project. By



DYNAMIC NETWORK OF THE ARPANET PIONEERS. THE ARROWS INDICATE MOVEMENT FROM ONE ORGANIZATION TO ANOTHER. *(Courtesy of Jane Abbate)* contrast, the history of ARPANET has directed our attention to the managerial creativity of small teams of computer scientists and engineers, such as the one at BBN, the Network Measurement Center, and the small ARPA/IPTO management team.

The language and concepts used by ARPANET engineers, scientists, and managers to describe the processes in which they were involved recall earlier episodes in the history of technology such as that of Thomas Edison and his team's invention and development of an electric lighting system, Elmer Sperry and his small team's invention and development of the airplane stabilizer, and Henry Ford and his team's invention and development of the moving assembly line.[68] The style of the ARPANET engineers, scientists, and managers also brings to mind projects of other computer research and development teams as described in Tracy Kidder's *The Soul of a New Machine* (1981) and G. Pascal Zachary's *Showstopper!* (1994).

Heart of BBN, for example, stresses that persons such as Roberts, who performed the management role for IPTO, possessed technical competence—they were "not just managers." Because of their technical backgrounds, the "not just managers" preferred, Heart believes, to discuss and arbitrate rather than to dictate when differences of opinion obtruded. They assumed that scientists and engineers, unlike persons more politically motivated, would ultimately recognize "right" reasoning instead of simply deferring to those with hierarchical authority. He concedes, however, that Roberts often participated in consensus forming with a "louder voice" than the others, in part because he controlled the funds.[69]

While Heart and others speak approvingly of consensus, they did not take an egalitarian approach to problem solving. They highly valued the ideas of people whom they considered bright and who usually came from elite university backgrounds, especially MIT. McKenzie of BBN describes ARPA's style as giving "bright people" the authority to choose other "smart" people and give them freedom to do research, emphasizing results rather than costs.[70] We should also note that ARPA/IPTO referred to computer research centers as centers of excellence, placing them in major research universities. Much as ARPA/IPTO encouraged its self-motivated and informed researchers to reach decisions by consensus, it similarly viewed the research centers as horizontally related, with authority and responsibility distributed among them. ARPA/IPTO considered hierarchical structure taboo. Not even the ARPANET was centrally controlled. We should recall the importance that Roberts, in deciding on protocols, attached to the regular deliberative meetings of the principal investigators from the computer research centers as well as his resort to the Network Working Group.

The ARPA management sustained a "collegial," even an academic, environment at the research centers. It brought scientists and engineers, on leave from research universities, to Washington for several years to manage IPTO. These university scientists and engineers had either had experience in managing research and development or had observed the techniques firsthand in working with entrepreneurial professors and researchers at institutions such as MIT. Maintaining close ties to universities, IPTO directors Licklider, Sutherland, Taylor, and Roberts kept abreast of front-edge academic research. They imbued IPTO with traditional values of academic research, such as freedom of inquiry and dissemination of information.

In the case of large projects such as SAGE and Atlas, managers motivated the engineers and scientists by stressing the national defense imperatives driving the mission-oriented project. While Roberts had to persuade several of the principal investigators that it was to their advantage to connect to the ARPANET, most of the computer scientists and engineers designing and developing the network seem to have been self-motivated by the "fun" of problem solving and by the satisfaction of working on the frontier and advancing the exciting new field of computing and communications. IPTO managers rarely spoke to the scientists and engineers of a military mission and never of possible commercial objectives. Heart sums up a widespread spirit of self-motivation:

> You know the people who were involved in this all were having a very good time. The ARPANET was a big thing in most of their lives. So most of the groups of the host sites, or the Network Analysis Corporation, or at Kleinrock's Network Measurement Center, or here, or others, were all having a very good time. They were all really having the time of their lives. While they might have some differences, the overriding single bit was they were all having a great time.

)))))) Large Contracts, Bright People

ARPA attracted technically competent and highly motivated managers by assuring them that not only could they award large contracts but that they would have a reasonably free hand in doing so. So encouraged, IPTO managers translated Licklider's emphasis on quality into a policy of nurturing a dozen or so "centers of excellence" by funding large projects, or sets of projects, at a few locations so as to concentrate expertise. Besides focusing on centers of excellence, the IPTO managers placed contracts in the hands of those whom they labeled "bright people." As we have observed, Taylor, following Licklider's lead, found those who fulfilled his criteria by questioning the large network of computer scientists and engineers with whom he regularly talked.[71] He and the other IPTO heads depended on the principal investigators at research centers and others in the computer community to feed in project proposals. But at the same time, the IPTO managers stimulated the flow of ideas by raising questions and presenting problems at the annual meetings of the principal investigators and through conversations with the computer community throughout the country.

Taylor awarded sustaining multiyear contracts to research centers. MIT, for instance, received about $3 million a year, Carnegie-Mellon several million annually, and Stanford and UCLA hundreds of thousands. He tried to award contracts to projects that fell somewhere between efforts to define "the nature of God and to count the grains of sand on the beach." Projects that had a high chance of success and that would make an order of magnitude of difference caught his attention.

Like other project managers, Roberts had to coordinate and schedule, but he managed only a handful of contractors while Atlas's Schriever and his staff dealt with hundreds. He did not have either a large control room to display contract progress reports or weekly critical-problems meetings as did Schriever. Roberts was able to monitor, schedule, and coordinate by making his own mental syntheses and analyses. Besides coordinating the ARPANET activities of BBN, the Network Measurement Center at UCLA, and the Network Analysis Corporation, Roberts also had responsibility for contracts awarded the Stanford Research Institute. This institute maintained a Network Information Center, gathering and providing information to the network community about the characteristics and performance of the ARPANET. Roberts coordinated the activities of the contractors by informal site visits during which he learned about progress being made, obstacles encountered, and additional resources needed.

)))))) Past and Present

Like so many of the managers, engineers, and scientists who played major roles in the development of government-funded projects between 1950 and 1970, ARPANET principals look back years later to a heroic period in their professional lives when they participated in the early projects. McKenzie of BBN is representative in observing:

> And I know that there has been fraud and abuse in government and in contracting and so forth, but it seems to me that the kind of rules and regulations that there are now, that are attempting to prevent that, really make it very difficult for the government to get the same kind of power out of its research dollars these days as it was able to then. I know it's hard to find a balance between accountability and free rein, and these days the government

approach seems to be more on the side of accountability and less on the side of free rein. But I think that a lot is being lost. . . . I think that ARPA in the 1970s did a really good job for the country in that way. It was a joy to be associated with the ARPANET project. It was fun. It was challenging. And I think it was good for the country. It's not so easy to find that mix now, and I think regulation is a big part of it.

VII Epilogue:

Presiding over Change

During the half-century following World War II, America continued to produce a cornucopia of material goods through modern management and engineering. Alongside this capitalistic, free-enterprise achievement, the country's capacity to create the large-scale technological systems that structure our living spaces has grown as well. Post–World War II government-funded projects have introduced a creative management and engineering style substantially different from one called modern that flourished during the period between the two world wars.

The modern, or pre–World War II, managerial and engineering approach associates management with large manufacturing firms rather than with joint ventures and projects such as SAGE, Atlas, Central Artery/Tunnel, and ARPANET. Unlike post–World War II managers and engineers involved with projects that introduced new technological systems, such as computer networks and urban highways, the prewar modern managers and engineers became employees of well-established firms whose products changed only incrementally.

The maintenance of a system for mass-producing standardized products engaged prewar modern engineers and managers. On the other hand, the postwar engineers and managers whom we encountered are not rigidly committed to standardization. They tolerate, even embrace, heterogeneity. Managers and engineers in the modern period expected that the problem-solving techniques they had mastered as young professionals would change only marginally. By contrast, post–World War II project engineers and managers need frequent refresher courses to keep abreast of the rapidly changing state of the art. Linear growth became the hallmark of modern

FROM KNOWLEDGE TO OBJECT: THE CONTESTED MEANINGS OF TECHNOLOGY

"Technology is a relentless force that creates and destroys with little pity." So opined *Business Week* magazine in a 1998 editorial contrasting the Exxon-Mobil merger with the growth of Amazon.com "Technology," insisted the magazine's editors, was "changing the very rules of the economic game." "Technology" was behind the consolidation of the oil industry, and "technology" had made possible the phenomenal market capitalization of Amazon.com despite its lack of profits. "Technology" would eventually transform Amazon too, because the "only certainties about technological change are that it is constant, painful and, in the end, positive for economic growth."[1]

This editorial reads like a caricature of technological determinism, the *bête noire* of historians of technology like myself. My colleagues in this field have spent over three decades arguing for an alternative understanding of technology, one that views technological change as the product of social, political, cultural, and economic factors. Yet *Business Week* takes little heed of historians; the magazine was so pleased with this editorial that it reprinted it as an advertisement in the *New York Times*.[2]

How is it that *technology* has assumed such potency in the English language, especially among the punditocracy and the nattering classes? As a word, technology is indeed a powerful force, whose invocation can extract funding from tight-fisted legislators and justify massive federal support for recondite scientific research. Faith in technological progress can steer huge investments into favored industries, such as the Internet or nuclear power. Mainstream opinion has no room for technological conservatives, who are dismissed as Luddites. Democrats and Republicans, conservatives and liberals all embrace technology with the fervor of revolutionaries.[3] Finally, the concept of technology works hand in hand with that of science, helping define the boundaries of science while providing powerful legitimation of the entire scientific enterprise. Without *technology*, one would be hard pressed to explain the intellectual, cultural, and material force of present-day science.[4]

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[1]"Exxon Mobil Meets Amazon.com," *Business Week*, 14 Dec. 1998, 178. By January 8, Amazon.com had more than doubled in price. In mid-March, 2001, Amazon stock had lost over two-thirds of its value, while stock in the merged Exxon-Mobil company had gained 10 percent while paying healthy dividends.

[2]*New York Times*, 5 Dec. 1998, A25. For an example of continued debate over the issue of technological determinism among historians of technology, see *Does Technology Drive History*, ed. Merritt Roe Smith and Leo Marx (Cambridge: MIT Press, 1994). For critiques and alternative approaches to the issue of technological determinism, see David Edgerton, "De l'innovation aux usages: Dix thèses éclectiques sur l'histoire des techniques," *Annales: Économies, Sociétés, Civilisations* 53 (1998): 815-837.

[3]For example, both Albert Gore, Jr. and George W. Bush blindly embrace technological progress in their political rhetoric. [Examples to be provided.] For critiques of this technological enthusiasm, see Howard P. Segal, "High Tech and the Burden of History," in *Future Imperfect: The Mixed Blessings of Technology in America* (Amherst: University of Massachusetts Press, 1994), 163-201; also Langdon Winner, "Technology Today: Utopia or Dystopia," *Social Research* 64 (1997): 989-1017.

[4]Public opinion research demonstrates that most Americans do not differentiate between science and its technological applications, and that public support for science rests almost entirely upon its

This essay seeks to uncover the origins of the meanings that give *technology* its discursive power in English. The stunning breadth and evocative power of the term only took hold in the mid-twentieth century. Yet the origins of these meanings go back to the late nineteenth and early twentieth centuries. It was then that English usage of *technology* gradually began to merge the two concepts that remained separate in Continental languages, namely *technology* and *technique*, the former referring to the study of the practical arts and the latter to these arts themselves. This blurring of boundaries helped transform *technology* from an instrument of critique into a tool for defending the rationality of modern industrial capitalism. Although the critical dimensions of *technology* survive, especially among historians of technology, the apologetic usage of *technology* has become dominant in contemporary English.

One would think that such a powerful term in present-day discourse would be the subject of extensive scholarly research. Surprisingly, the history of the English-language use of *technology* has received very little attention until recently. Along with other intellectuals, historians too suffer from what Landgon Winner terms "technological somnambulism," a condition marked by the inability to think critically about technology.[5] Yet wrapped up in the history of this word are clues to its present-day power.

Yet recent work in the history of technology has done much to uncover the ideological roots of *technology*. By far the most important analysis is Ruth Oldenziel's pioneering cultural history of the American engineering profession.[6] Inspired by Raymond Williams, Oldenziel analyzes the emergence of technology as "keyword," that is, as an organizing concept in contemporary discourse. Her work demonstrates that the present-day meanings of technology are in fact quite recent. Before Oldenziel's work, the received wisdom among historians of technology identified *technology* as entering the American lexicon in the 1829, with the publication of Bigelow's *Elements of Technology*.[7] Oldenziel has shown, however, that the nineteenth-century use of the

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supposed practical benefits. See Georgine M. Pion and Mark W. Lipsey, "Public Attitudes Toward Science and Technology: What Have the Surveys Told Us," *Public Opinion Quarterly* 45 (1981): 303-16

[5]Langdon Winner, *The Whale and the Reactor* (Chicago: University of Chicago Press, 1986), 10.

[6]Ruth Oldenziel, "Unsettled Discourses," chap. 1 in *Making Technology Masculine: Men, Women and Modern Machines in America, 1870-1945* (Amsterdam: Amsterdam University Press, 1999), 19-49. Oldenziel first presented this argument in her 1992 dissertation, "Gender and the Meanings of Technology: Engineering in the U.S., 1880-1945" (Ph.D. diss., Yale University, 1992), chap. 2. Surprisingly, this dissertation represents the first serious historical study of American meanings of *technology*. Leo Marx published a more speculative discussion of this topic two years later, and Ronald Kline drew on Oldenziel's work in his 1995 article on the concept of "applied science." Leo Marx, "The Idea of 'Technology' and Postmodern Pessimism," in *Does Technology Drive History*, ed. Merritt Roe Smith and Leo Marx (Cambridge: MIT Press, 1994); Ronald Kline, "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880-1945," *Isis* 86 (1995): 194-221.

[7]I believe that this claim was first made by Hugo Meier in the 1950s, who suggested that "the new term entered into popular usage" after the publications of Bigelow's book in 1829. Hugo A. Meier, "Technology and Democracy, 1800-1860," *Mississippi Valley Historical Review* 43 (1957): 618-19, 623. This claim is repeated without attribution by Jennifer Clark, "The American Image of Technology from the Revolution to 1840," *American Quarterly* 39 (1987): 431. Bigelow's use of *technology* was actually viewed as odd by his contemporaries. According to a friendly reviewer, "The word Technology gives

term had none of the breadth that we associate with *technology* today, referring rather narrowly to a description of the practical arts, which was indeed Bigelow's meaning. Nineteenth-century debates that historians now interpret as being about *technology* were in fact framed in terms of other concepts, such as *useful arts, manufacturing, industry, invention, applied science,* and *the machine.* Oldenziel demonstrates that these nineteenth-century terms were quite different from the present-day concept of technology, especially in connotation.[8] Not until the 1930s did *technology* become widespread in elite discourse in the United States, and not until after World War II did *technology* become a central keyword in American culture. This terminological shift, argues Oldenziel, was accompanied by a gender and class struggle over meanings. In the course of this struggle *technology* was stripped of its associations with workers and women, "only to become an emblem of Western man's superiority and civilization."[9]

Oldenziel's analysis has provided an essential map of the territory for subsequent scholars. Yet many areas remain unexplored. In particular, Oldenziel did not examine the influence of European discourse on American usage. It is only in comparison with French and German usage that the oddity of the American term becomes clear.

Dictionaries of American English help illustrate the divergence of American from Continental usage. Dictionaries must be used with caution when trying to understand the meanings of words, because they usually give equal weight to esoteric and common meanings, often fail to distinguish variations in meaning with context, and typically have a considerable lag in recognizing new meanings or identifying obsolete definitions. Nevertheless, when used with caution dictionaries reveal much of significance. According to Oldenziel, most nineteenth-century American lexicographers did not include *technology* as an entry.[10] One of the earliest British entries for the term appeared in George Crabb's *Universal Technical Dictionary* of 1823. Crabb defined technology as "a description of the arts, especially those which are mechanical."[11] This definition makes etymological sense, since *technology* has it roots in the Greek *techne,* skill or art, and the suffix *-logy,* which is almost always reserved for a field of study. It is this sense of *technology* as a field of study that explains its choice for the Massachusetts

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but an imperfect idea of the contents of this volume," which actually "treated of the scientific and practical principles of the many of the useful, curious, and elegant arts." "Bigelow's Elements of Technology," *North American Review* 30 (April 1830): 337-38. [viewed at http://cdl.library.cornell.edu/cgi-bin/moa/moa-cgi?notisid=ABQ7578-0030-19].

[8]Questionable inferences about nineteenth-century American attitudes to *technology* are common; see for example John F. Kasson's otherwise excellent study, *Civilizing the Machine: Technology and Republican Values in America, 1776-1900* (New York: Grossman Publishers, 1976); more egregiously Clark, "The American Image of Technology," 431-49.

[9]Oldenziel, *Making Technology Masculine,* 20.

[10]Oldenziel, *Making Technology Masculine,* 23.

[11]George Crabb, *Universal Technological Dictionary, or Familiar Explanation of the Terms Used in All Arts and Sciences, Containing Definitions Drawn From the Original Writers,* 2 vols. (London, Baldwin, Cradock and Joy, 1823), s.v. "technology." The *Oxford English Dictionary* provides comparable examples of early English usage, though it errs in suggesting that nineteenth-century English usage transferred the meaning of technology from the study of the arts to the arts themselves. No doubt such usage occurred, but it was rare before the twentieth century.

Institute of Technology, founded in 1865.[12] *Technology* was used in a similar sense for treatises on the industrial arts into the early twentieth century.[13]

The term became more common in the later nineteenth century, however. The twelve-volume *Century Dictionary* published in New York in 1911 provided a sensible definition of *technology* as "that branch of knowledge which deals with the various industrial arts; the science or systematic knowledge of the industrial arts and craft, as in textile manufacture, metallurgy, etc."[14] By the late twentieth century, though, the term had undergone a substantial transformation, completely losing its meaning as a field of study. In a sense, the meaning of technology had shifted from the study of *techne* to *techne* itself. This meaning was clear to social scientists by the 1960s, as revealed by the authoritative *International Encyclopedia of the Social Sciences.* According to the entry, "technology in its broad meaning connotes the practical arts," in both their pre-industrial and industrial manifestations. Fundamentally, "technologies are bodies of skills, knowledge and procedure for making, using, and doing useful things."[15]

A variant of this definition restricts *technology* further to the practical arts construed as applied science. The respected third (1992) edition of the *American Heritage Dictionary*, for example, gives *technology* two main meanings. The second meaning, which is identified as anthropological, roughly accords with the definition given in the *International Encyclopedia of the Social Sciences*. But the first definition describes *technology* as the "application of science, especially to industrial or commercial objectives." A variant of this first definition extends *technology* to the "scientific method and material" used in such applications, thus including practices and artifacts within the scope of technology as applied science.[16]

French and German commentators have long noted the lack of a distinction between technology and technique in present-day English. Continental languages in general have not followed English in expanding technology to cover all manner of practical arts. Etymologically speaking, the appropriate modern term for the practical arts should be a cognate of the Greek *techne*, for example technics or technique, rather than cognates of *technologia*. French and German both followed this path, preserving the cognates of *technologia* for a field of study, and the cognates of *techne* for the material artifacts and practices that correspond to the present-day English-language

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[12]For a discussion of early plans for what became MIT, see *Objects and Plan of an Institute of Technology, Including a Society of Arts, a Museum of Arts, and a School of Industrial Science, Proposed to be Established in Boston* (Boston: John Wilson & Son, 1860). This document makes clear that *technology* meant the systematic study of the industrial arts; furthermore, "applied science" and "the arts" provided the key organizing concepts for this document, not *technology*.

[13]For example *Chemical Technology, or, Chemistry in its Applications to Arts and Manufactures*, ed. Charles Edward Groves and William Thorp (Philadelphia: P. Blakiston, Son & Co., 1889-1903). See also the volumes in the International Encyclopedia of Technology series published by the International Textbook Company of Scranton, Pa. in the early twentieth century for use correspondence courses, which included such titles as *Lathe Work* (1903).

[14]*The Century Dictionary* (New York: Century Co., 1911), s.v. "technology."

[15]*International Encyclopedia of the Social Sciences*, ed. David L. Sills (New York: Macmillan, 1968), s.v. "Technology" (quote on 557).

[16]*The American Heritage Dictionary of the English Language*, 3rd ed. (city: Houghton Mifflin, 1992), s.v. "technology."

technology.[17] Yet since World War II, French and German meditations on *Technik* or *technique* have generally been translated as *technology*.[18] For example Frederik Jünger's *Die Perfektion der Technik* was translated in 1949 as *The Failure of Technology*; Frederick Klemm's *Technik: eine Geschichte ihrer Probleme* became in 1959 *The History of Western Technology*; similarly Heidegger's *Die Frage nach der Technik* became *The Question Concerning Technology* in 1977; finally Jacques Ellul's influential *La Technique ou l'enjeu du siècle* was translated in 1964 as *The Technological Society*.

This confounding of the study for its object is, therefore, a semantic crime peculiar to the English-language usage of *technology*. But this crime has real-world victims. Ruth Oldenziel has shown how the concept of technology that emerged in 1930s America, with its emphasis on applied science and engineering, tended to make *technology* the province of middle-class men to the exclusion of women and workers. I believe that the shift in the meaning of technology was a key part of this process. By blurring the boundary between the formal study of techniques and the techniques themselves, the American concept of technology helped disguise the cultural expropriation of the industrial arts by large corporations and the engineering profession, a process that had been underway since the late nineteenth century.[19]

It would be wrong, however, to see this new meaning for technology as even a half-conscious process of cultural legitimation by the engineering profession and its corporate allies. Engineers in the 1920s were perfectly satisfied to seek legitimacy in science, not technology. In fact, the American concept of technology emerged not from the natural sciences or engineering, but rather from the social sciences. For roughly three decades before it erupted into general elite discourse, *technology* percolated as a

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[17]For German usage see *Historisches Wörterbuch der Philosophie*, vol. 10 (Basel: Schwabe & Co., 1998), s.v. "Technologie" and s.v. "Technik." For a discussion of *technology* that maintains the Continental distinction between *technology* and *technique*, see Jean-Jacques Salomon, "What is Technology? The Issue of Its Origins and Definitions," *History and Technology* 1 (1984): 113-56, esp. 113-16. On French usage see also Jeane-Claude Beaune, "Appendice: Définition de la Technologie," in *La Technologie Introuvable* (Paris: J. Vrin, 1980), 252-63. Russian usage of "technika" and "tekhnologiia" seem similar French and German. (Thanks to Karen Rosneck, a translator of Russian literature, for advising me on Russian usage.)

[18]One can confirm this observation by searching online library catalogs for English translations of German works with "Technik" in the title; in almost every case after World War II, "Technik" is translated as "technology," except where clearly used in the English sense of "technique." I have not examined the French case closely, but it appears a bit different. When "la technique" or "les techniques" is used in a sense clearly comparable to the English "technology," it is often rendered as "technique" or "techniques," as in Bertrand Gille's *Histoire des techniques*, translated in 1986 as *History of Techniques*. In contrast, Maurice Daumas' *Histoire générale des techniques* was translated beginning in 1969 as *A History of Technology and Invention*. A cursory perusal of titles suggests that in French "technologie" began to be used by the 1970s in the broad sense comparable to usage in English, even though "histoire des science et des techniques" remains the standard phrase used by historians of science and technology.

[19]American labor historians have discussed this process in terms of the struggle for control of the shopfloor; it is also a central theme in the history of Taylorism. See Daniel Nelson, *Frederick W. Taylor and the Rise of Scientific Management* (Madison : University of Wisconsin Press, 1980); Dan Clawson, *Bureaucracy and the Labor Process: The Transformation of U.S. Industry 1860-1920* (New York: Monthly Review Press, 1980).

term of art among several emerging fields of the American social sciences. In none of the fields, however, did *technology* truly serve as a central concept.[20]

The *American Heritage Dictionary* was in part correct in connecting *technology* with anthropology. American anthropology emerged as an organized field in the 1870s, centered around the Smithsonian Institution's Bureau of Ethnology in Washington, D.C. One area stressed by these early anthropologists was the study of material culture among "primitive" peoples. In 1879, a group of prominent social scientists founded the Anthropological Society of Washington. The original constitution of the society specified five sections, Archaeology, Somatology, Ethnology, and Philology. In 1882 *technology* replaced *archaeology* in the constitution after extensive discussions by the society's founders John Wesley Powell, Garrick Mallory, and Otis T. Mason. Although I could not find any reasoning for this change, most likely it was done to include studies of living as well as past cultures. Nor could I find a definition of technology; but in his well-known debate with Franz Boas, Powell referred to the various subfields of anthropology, including the "science of technology, which included all of the arts of mankind."[21]

Despite the formal use of *technology* as an organizing concept in the Society's constitution, the society's members rarely used the term when discussing material culture. In his 1888 annual address as president of the society, for example, John Wesley Powell made the "arts" a key factor separating savagery from barbarism. Yet he never discussed these changes in terms of *technology*. The term is also absent as an organizing concept in Otis T. Mason's influential *Origins of Invention*, even though Mason headed the Technology section of the Society in the 1880s. Nor did Lester Ward refer to *technology* when addressing the society on "Moral and Material Progress Contrasted" in 1885. In fact, *technology* was not discussed in detail in any major English-language anthropology journal before the late 1930s, despite continuing interest in subjects like material culture and invention, subjects that were later subsumed under *technology*.[22]

Anthropology fails, therefore, to explain the elevation of *technology* to the status of keyword. But the term did gain currency among a few social scientists, especially those interested in the economics and sociology of industry. In particular, it was in the American discourse of political economy that *technology* made the shift from a field of study to its object. This usage of *technology* arose in part through the influence of Karl Marx, most likely entering scholarly discourse through the 1887 English translation of volume one of *Das Kapital*.[23]

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[20]See below.

[21]Anthropological Society of Washington, *Transactions*, vol. 1 (Feb. 10, 1879 - Jan. 17, 1882), Smithsonian Miscellaneous Collections, v. 25, 6-7, 12-14; Lester Ward, "Contributions to Social Philosophy, IV, Sociology and Anthropology," *American Journal of Sociology* 1 (1896): 426; J. W. Powell, letter to the editor, *Science* 9 (1887): 614.

[22]This conclusion is based primarily on a rather cursory keyword search of anthropology journals in JSTOR; the most explicit discussion of *technology* in anthropology before 1940 that I could find was H. S. Harrison, "Presidential Address: Ethnology Under Glass," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 67 (Jan.-Jun., 1937): 1-14. Only in the 1940s did anthropologists like V. Gordon Childe make *technology* a central concept in their work.

[23]The 1887 English translation was based on the third German edition of 1883. The translators were Marx's friend Samuel Moore and his son-in-law Edward Aveling. Aveling took on the task when Moore was unable to complete it, and the final text was edited by Engels, insuring consistency.

In a classic article, Donald MacKenzie argues persuasively that Marx was not a technological determinist. MacKenzie bases his argument on a careful reading of volume one of Marx's *Capital*, especially the chapter on "Machinery and Modern Industry."[24] What MacKenzie does not discuss, however, is Marx's use of the German terms *Technologie* and *Technik*. These terms are not central to Marx's discussion, which he frames in terms of "machinery," "modern industry [*große Industrie*]," and "forces of production [*Produktivkräfte*]." But Marx does make a number of important statements about *Technologie*, which in English become comments on *technology*.[25]

Marx's use of *Technologie* in *Das Kapital* contrasts significantly with the later German debate over *Technik* in the late nineteenth and early twentieth centuries. These debates centered on the professional status of engineering in relation to other fields of learning, stimulated by engineers connected with the *technische Hochschulen* who sought to place their subject on par with the traditional fields of learning in the universities. These debates about *Technik* had wider implications as well, attracting the attention of Werner Sombart and Max Weber, who criticized Marx for his supposedly determinist views about *Technik*.[26]

But in volume one of Marx's *Kapital*, *Technik* had little role to play, appearing perhaps half a dozen times. Somewhat more common were the terms *Technologie* or *technologische*. Yet Marx's use of these terms, and their rendering as *technology* or *technological* in the 1887 English translation, shaped the meaning of technology in three important ways. First, Marx linked *technology* firmly to modern capitalist industry. Second, Marx linked *technology* to *science* (*Wissenschaft*). And third, Marx provided in one footnote a compelling argument linking the concept of technology to his method of historical materialism. But in English translation some of the specificity of *technology* was lost, so that the term appears to range from the study of capitalism production methods to those methods themselves.

Most generally, Marx used *Technologie* to refer to systematic knowledge of the production processes in modern, capitalist industry. This knowledge was used in modern industry to break the production process into its component parts, and then used to reassemble these parts "into so many conscious and systematic applications of natural science [*Naturwissenschaft*] to the attainment of given useful effects." Through this process, "technology also discovered the few main fundamental forms of motion, which, despite the diversity of the instruments used, are necessarily taken by every

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Freidrich Engels, "Preface to the English Edition," in Karl Marx, *Capital: A Critical Analysis of Capitalist Production*, vol. 1 (Moscow: Progress Publishers, 1954), 13-14.

[24]Donald MacKenzie, "Marx and the Machine," *Technology and Culture* 25 (1984): 473-502.

[25]This analysis is based on HTML versions of the *Capital*, vol. 1, in English and German. The English version is available from http://www.marxists.org/archive/marx/works/1867-c1/index.htm, and is based on the 1887 English translation. The German version is from http://www.mlwerke.de/me/me23/me23_000.htm. The corresponding print versions, to which the page number below conform, are Karl Marx, *Das Kapital*, band 1, in Karl Marx and Friedrich Engels, *Werke*, vol. 23 (Berlin: Dietz Verlag, 1968); Marx, *Capital*, vol. 1, trans. Samuel Moore and Edward Aveling (Moscow: Progress Publishers, 1954).

[26]Mikael Hård, "German Regulation: The Integration of Modern Technology into National Culture," in *The Intellectual Appropriation of Technology: Discourses on Modernity* (Cambridge: MIT Press, 1998), 36-41,56-60; Werner Sombart, "Technik und Kultur," *Archiv für Sozialwissenschaft und Sozialpolitik* 33 (1911): 305-47. See also Mitcham, *Thinking through Technology*.

productive action of the human body; just as the science of mechanics sees in the most complicated machinery nothing but the continual repetition of the simple mechanical powers."[27] *Technology* was thus an aspect of the divorce of manual from mental labor, although Marx never made this aspect of *technology* explicit. Nevertheless, *technology* made possible "the separation of the intellectual powers of production from the manual labour, and the conversion of those powers into the might of capital," a process completed by modern industry.[28]

Thus for Marx *technology* was specific to the capitalist mode of production.[29] Yet *technology* was also intimately linked to science, specifically the capitalist appropriation of science for the purposes of production. Marx viewed *Technologie* as "scientific" in the German sense of *Wissenschaft*, that is, as a systematic body of knowledge, but one applied to the practical problems of production. According to Marx, the principles of capitalist industry created the "new modern science of technology [*die ganz moderne Wissenschaft der Technologie*]." The capitalist transformation of production involved the "technological application of science [*technologische Anwendung der Wissenschaft*]."[30] The productivity of labor was also linked to the developmental stage of science ("*Entwicklungsstufe der Wissenschaft*") and its technological application ("*technologischen Anwendbarkeit*").[31] In two cases Marx used the phrase *Wissenschaft und Technik*, which was rendered into English as "science and technology." For example, Marx argued that the productivity of labor increased with the "uninterrupted advance of science and technology [*ununterbrochenen Fluß der Wissenschaft und der Technik*]."[32]

In the examples examined above, Marx's use of *Technologie* is reasonably consistent with the definition of the term as a field of knowledge. This usage is also consistent with the standard German definition of *Technologie* since the late eighteenth century, and with the term's etymological roots in ancient Greek, a language that Marx knew well.[33]

Yet Marx's most extensive discussion of *technology* in *Capital* is open to broader twentieth-century American interpretation that extends *technology* to the practical arts themselves. This discussion occurs in a well known lengthy footnote to Marx's analysis of the term *machine*, in which the English translators uniformly rendered *Technologie* as *technology*. In this footnote Marx portrayed *technology* as the material form of man's

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[27]*Capital*, 456-57; *Das Kapital*, 510.

[28]*Capital*, 399.

[29]Marx is not entirely consistent, though, referring elsewhere to the "technological comparison of different epochs of production," include pre-historical eras, which would include non-capitalist societies. *Capital*, 175.

[30]*Capital*, 584; *Das Kapital*, 652. This phrase also appears in *Das Kapital*, 528, but is rendered in English as "scientific." *Capital*, 474.

[31]*Capital*, 47; *Das Kapital*, 55. Interestingly, the 1887 English edition translated this last phrase as "practical application," but in most other cases *Technologie* and *technologische* were rendered into their English cognates.

[32]*Capital*, 567; *Das Kapital*, 631-32. Note the addition of a progressive connotation in the translation by the rendering of "Fluß" (flow) as "advance.

[33]For a history of German usage of *Technologie*, see *Historisches Wörterbuch der Philosophie*, vol. 10 (Basel: Schwabe & Co., 1998), s.v. "Technologie." This entry notes the recent influence of "Anglo-Saxon" usage that erases the distinction between *Technik* and *Technologie*.

productive relationship with nature. Marx began the note with a comment that "a critical history of technology would show how little any of the inventions of the 18th century are the work of a single individual." In this sentence, *technology* appears to refer to the inventions themselves, not to the related field of knowledge. Marx followed with an analogy that confirms this interpretation. Darwin, wrote Marx, described "the history of Nature's Technology, i.e., ... the formation of the organs of plants and animals, which organs serve as instruments of production for sustaining life." He continued with a call for a similar history of "the productive organs of man," that is, human technology. This observation led him to his most famous statement on the nature of *technology*: "Technology discloses man's mode of dealing with Nature, the process of production by which he sustains his life, and thereby also lays bare the mode of formation of his social relations, and of the mental conceptions that flow from them."[34]

With our present-day understanding of technology, it is natural to interpret technology in this passage as referring to the instruments of production themselves. Indeed this was precisely my first interpretation.[35] But on closer analysis I think it makes more sense to interpret *Technologie* in this context as referring to the <u>principles</u> of the industrial arts, not the arts or machines themselves. Marx did in fact distinguish between *Technik* and *Technologie*, even though his use of *Technik* was infrequent in *Kapital*. When Engels gave Marx's method a more technicist cast several decades later, he used framed his analysis in term of *Technik*, not *Technologie*.[36]

This footnote was well known among students of *Capital*, but its broader influence is difficult to trace, especially in the United States. Marx exerted a powerful influence on German social theorists of the late-nineteenth and early twentieth centuries. Much of the work of Max Weber and Werner Sombart can be read as a response to the perceived economic determinism of Marxist theory, yet when they addressed Marx's analysis of the industrial arts they used the term *Technik*. Marx's influence on American social science was much more muted. Except for volume 1 of *Capital*, few of Marx's more scholarly works were available in English before the second decade of the twentieth century.[37]

Marx's work did, however, help bring *technology* into the vocabulary of American social scientists. The translation also helped sow confusion on this issue by inconsistently translating *Technik* and *technische*. For example, in one context "technologischen Anwendung" was translated as "technical application," while "technisch

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[34]*Capital*, 352n; *Das Kapital*, 392n.

[35]Thanks to Tom Gieryn for pointing out to me that this pasage is consistent with the traditional definition of technology.

[36]Friedrich Engels, letter to W. Borgius, 25 Jan. 1894, quoted in *Historisches Wörterbuch der Philosophie*, s.v. "Technik," 10:945. This letter is also quoted at length in an important early scholarly discussion of Marx by a prominent American economist, who translated, Engels' *Technik* into the English *technique*. Edwin R. A. Seligman, "The Economic Interpretation of History, II," *Political Science Quarterly* 17 (Mar. 1902): 72.

[37]See the Marx citations in Seligman, "The Economic Interpretation of History, I," *Political Science Quarterly* 16 (Dec. 1901), esp. 623-40.

in Widerstreit" was rendered as "technologically incompatible."[38] *Technik*, however, was invariably translated as *technology* when rendered as a noun.[39]

This confusion in the 1887 English version of *Capital* did not insure that *technology* would become the dominant term in American discourse. Yet there was an influential conduit by which Marx's concept of technology may have been transmitted to American social science--Thornstein Veblen. Veblen knew his Marx well. One of Veblen's earliest articles, published in 1891, dealt with socialist theory. In 1906, Veblen published a nuanced assessment of Marx's economics, defending the significance of Marx's contribution while acknowledging the limitations of the theory. In his two articles on Marx, Veblen mentioned technology only in passing. But in another article published a few months earlier, Veblen deployed the concept of technology in a way that encompassed the artifacts and processes of modern industry. In a discussion of the utility of science for "technology," Veblen invoked "the broad sense in which the term includes, beside the machine industry proper, such branches of practice as engineering, agriculture, medicine, sanitation, and economic reforms." In this definition, Veblen is making an important shift from *technology* as an area of study to a field of practice.[40]

Ruth Oldenziel has correctly emphasized the centrality of Veblen in transforming *technology* into a keyword of modern industrial culture. But like other historians who have examine Veblen's attitudes to technology, Oldenziel focuses more on his later, more polemical works, especially *The Engineers and the Price System*, a collection of articles from 1919 in which Veblen proposed (with often-overlooked irony) his famous "Soviet of technicians."[41] The terms *technology* and *technological*, however, became a key part of Veblen's conceptual armory roughly 15 years earlier.

Veblen began using *technology* in its broader sense just after 1900. The term had no significant role in the work that first brought Veblen academic and popular acclaim, his *Theory of the Leisure Class* of 1899. Veblen's first substantial use of the term occurred in a 1902 review of Oscar L. Triggs' book on the arts and crafts movement. Veblen characterized the movement as impractically romantic in its rejection of the "machine process," which was the essence of "modern industry." Instead, Veblen called for a modernist aesthetic based on "the association of art with the machine process and with the *technology* of that process" (emphasis added). The "machine process" is clearly the governing concept for Veblen; *technology* is an aspect of the machine process. Veblen referred several times to "guidance of mechanical technology" and also to the "technological requirements" of the machine process. In this usage Veblen was not equating *technology* with the material aspects of modern industry. Rather, in Veblen's use the term floats ambiguously between spirit and substance, comprising the rules, method and values that govern what was then known as the industrial arts.[42]

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[38]*Capital*, 357, 360; *Kapital*, 398, 403.

[39]*Capital*, 475, 567; *Kapital*, 530, 632

[40]Thorstein Veblen, "The Socialist Economics of Karl Marx and his Followers," *Quarterly Journal of Economics* 20 (Aug. 1906): 575-95; Thorstein Veblen, "The Place of Science in Modern Civilization," *American Journal of Sociology* 11 (March 1906): 598.

[41]Oldenziel, "Gender and the Meanings of Technology: Engineering in the U.S., 1880-1945" (Ph.D. diss., Yale University, 1992), chap. 2; idem, *Making Technology Masculine*, 42-46; Thorstein Veblen, *The Engineers and the Price System* (New York: B. W. Huebsch, 1921), chap. 6.

[42]V. [Thorstein Veblen], "Arts and Crafts," *Journal of Political Economy* 11 (Dec. 1902): 108-11.

Technology remained subsidiary to the "machine process" in Veblen's next major work, his *Theory of Business Enterprise* of 1904, and retained its ambiguous position between the ideal and the material.[43] The economic use of the term appeared to be spreading around this time. In late 1904 Herbert J. Davenport, Veblen's colleague and admirer at the University of Chicago, published an analysis of the concept of capital in which he distinguished between the "technological" or "social" capital and "competitive" capital. For Davenport, "competitive" capital was defined by market valuation, whereas "technological capital" referred to "all wealth held for the purpose of production," including intermediate products. Davenport was not, however, entirely comfortable with this use of the term "technological." In a footnote, he remarked that "etymologically speaking, there are manifest objections to this use of the term 'technological' as referring especially to capital regarded in the mechanical and industrial sense; but no better term seems to be at hand."[44]

Perhaps in response to Davenport, Veblen analyzed the concept of capital in a lengthy two-part article published in 1908, "On the Nature of Capital." This article provides Veblen's most detailed and sophisticated discussion of *technology*. Central to Veblen's analysis is the idea of "technological knowledge," which referred to "knowledge serviceable and requisite to the quest of a livelihood," that is knowledge for productive purposes. Such knowledge is integral to all human communities, even the most primitive. For primitive communities technological knowledge includes language, the use of fire, the use of simple tools for cutting, and basic fiber arts. This knowledge constitutes what Veblen termed the "immaterial equipment" of a community, as opposed to the material equipment of tools and machines.[45] Technological knowledge is always collective, in that it exceeds the grasp of any single individual, and it is also cumulative, growing through experience transmitted by members of the group. Furthermore, it is this immaterial equipment that determines the utility of material equipment. Natural resources, including plants and animals, are useful only to the degree "that they have been brought within the sweep of the community's knowledge of ways and means." The history of this "intangible, technological equipment ... is the history of the development of material civilisation."[46]

Veblen's concept of technological knowledge is striking for several reasons. First, Veblen applied the term to productive pursuits in all epochs of the human species, not just to the era of modern industry. In this usage he was probably drawing on *technology* as an anthropological classification, given his thorough familiarity with the ethnographic literature. Second, in Veblen's use *technological* does not refer to the study of productive activities, but rather to the productive activities themselves. But he never referred to the material equipment itself as *technology*, even though he had many opportunities to do so. For Veblen, *technology* did not comprise tools and machines, but

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[43]See especially "The Cultural Incidence of the Machine Process," chap. 9 of *The Theory of Business Enterprise* (New York: Scribner's, 1904), 302-73.

[44]H. J. Davenport, "Capital as a Competitive Concept," *Journal of Political Economy* 13 (Dec. 1904): 31-47, quotes on 35. On Davenport as an "admirer" of Veblen, see Wesley C. Mitchell, "Thorstein Veblen," in *What Veblen Taught: Selected Writing of Thornstein Veblen* (New York: Viking, 1936), xxix.

[45]Veblen, "On the Nature of Capital," *Quarterly Journal of Economics* 22 (Aug. 1908): 517-42, quotes on 518.

[46]Ibid, quote on 521.

rather the "state of the industrial arts," a phrase that Veblen often placed in quotation marks. *Technology*, for Veblen, was an amalgam of human traits, "physical, intellectual, and spiritual," with the material environment. In a remark that would please actor-network theorists, Veblen suggested that it was "bootless" to try to separate the "human and non-human" components of technology.[47]

Finally, Veblen makes technological knowledge a key motor of history, even though for him this knowledge is very much a human product, not a force of nature. According to Veblen, the growth of technological knowledge makes particular "material items" essential to productive activities. Where these material means are in short supply, individuals can in effect monopolize the community's collective technological knowledge by controlling the material means necessary to utilize this knowledge, for example labor or land. One "technological situation" might give rise to social system based on slavery, while another would give the advantage to landed wealth. Only very recently in human history, argued Veblen, has technological knowledge made the ownership of mechanical equipment "the basis of pecuniary dominion." This recent development, claimed Veblen, is the basis of industrial capitalism.[48] This argument provided the theoretical basis for Veblen's analysis of the role of the engineer that he developed a decade later in *Engineers and the Price System*.

It is not entirely clear why Veblen did not refer to *technical* rather than *technological* knowledge. If *technology* is a field of knowledge, then "technological knowledge" seems redundant. Veblen's thinking about capital and industry was no doubt strongly influenced by Marx, yet this influence did not compel his terminology. Just a few years earlier the influential Columbia economist Edwin R. A. Seligman had published a detailed exegesis of Marx's theory of history, which Seligman termed "economic." Even though Seligman quoted Marx's famous footnote on *Technologie* from Capital, he did not take up the term himself. Instead, he referred to the Marxist argument that "the changes in technique are the causes of social progress," while insisting that "technique" encompassed broad economic factors in addition to the narrow "technical processes" of production.[49] In addition, Veblen was influenced by German social theorists like Werner Sombart, who focused their analysis on *Technik*, not *Technologie*, yet Veblen discussed Sombart's work in terms of *technology*.[50] Most likely, Veblen's stress on the immaterial dimensions of productive activity gave *technology* more appeal than *technique*.

Whatever the reasons for Veblen's terminology, his definition of *technology* was quite sophisticated, especially when compared with later use of the term. Veblen viewed *technology* as very much parallel to science, rather than subsidiary to it. In addition, Veblen saw nothing automatically beneficial in the progress of technological knowledge, particularly when used for the military or for socially pernicious

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[47]Ibid, quote on 541.

[48]Ibid, 525-26 ,534.

[49]Seligman, "The Economic Interpretation of History, II," 71. On Seligman's economic interpretation of history, see Richard Hofstadter, *The Progressive Historians* (New York: Knopf, 1968), 197-200.

[50]V. [Veblen], "Der Moderne Kapitalismus," *Journal of Political Economy* 11 (March 1903): 300-5, esp. 305.

businesses.[51] Finally, Veblen was no technological determinist in the sense of viewing technological change as an autonomous process divorced from human history.[52]

Despite Veblen's importance in American intellectual history, his influence on American social science was rather attenuated. His role in elaborating a new definition for *technology* went largely unnoticed. Even in economics the term remained uncommon before the early 1930s. Instead of *technology*, the dominant terms well into the 1930s remained *invention, the machine, applied science,* and even the older nineteenth century expression *industrial arts*.[53]

Between 1890 and 1930, Americans experience a more profound technological transformation than at an other period in American history, yet they did so largely without recourse to the concept of technology. Many academics and public intellectuals grappled with questions that we would now classify as technological, but if they used the term they only did so in passing. The concept is completely absent, for example, in William Ogburn's 1922 treatise *Social Change*. Ogburn was a prominent sociologist who later became famous for his "cultural lag" theory of technological change and for his empirical research on the social effects of technology. He in fact detailed his "cultural lag" theory in 1922, but in terms of *invention* and *material culture* rather than *technology*. Similarly, *technology* found no place in S. Colum Gilfillan's early work on invention.[54] Another pioneer in the academic study of technology, the prolific economic historian Abbott Payson Usher, also had little use for *technology* before the 1930s. His 1929 classic, *History of Mechanical Inventions*, gets by quite nicely without the term.[55] Popular writers devoted much attention to the social dimensions of modern industry and their novel products, but they usually framed this discussion in terms of *invention* or *the machine*. Stuart Chase's 1928 *Men and Machines*, for example, analyzed the social impact of mechanization without the aid of *technology*.[56] Even Lewis Mumford, who pioneered history of technology as cultural critique, did not make *technology* the organizing concept of his 1934 work *Technics and Civilization*, although he did refer to "modern technology" in passing. Instead, Mumford centered his narrative on "the machine," a

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[51]Veblen, "On the Nature of Capital [Part 2]: Investments, Intangible Assets, and the Pecuniary Magnate," *Quarterly Journal of Economics* 23 (Nov. 1908), 108-11. Veblen's sophisticated view of the science-technology relationship is developed in his essay "The Place of Science in Modern Civilization."

[52]See especially his comment that understanding capital goods, and by implication technology, "is a question of how the human agent deals with the means of life, not of how the forces of the environment deal with man." Veblen, "On the Nature of Capital," 542.

[53]This claim is based on an unsystematic search of economics journals in JSTOR. See www.jstor.org.

[54]William F. Ogburn, *Social Change with Respect to Culture and Original Nature* (New York: Viking, 1922), esp. 200-213; S. C. Gilfillan, "Who Invented It?" *Scientific Monthly* 25 (Dec. 1927): 529-534. Even as late as 1935, in his influential *Sociology of Invention* of 1935, Gilfillan pointedly avoids the term, referring in his subtitle to "Technic Invention." Idem, *Sociology of Invention: An Essay in the Social Causes of Technic Invention and Some of its Social Results; Especially As Demonstrated in the History of the Ship* (Chicago: Follett, 1935).

[55]Abbott Payson Usher, *A History of Mechanical Inventions* (New York: McGraw-Hill, 1929).

[56]Stuart Chase, *Men and Machines* (New York: Macmillan, 1929).

metaphor for modern technology as a system, and "technics," no doubt by analogy with the German literature on *Technik*, with which Mumford was thoroughly familiar.[57]

Despite the neglect of *technology* by those scholars closest to its subject matter, an important change in the use of the term began to occur in the late 1920s and early 1930s. In this period, *technology* began to assume highly ideological connections with the idea of progress. At the vanguard of this conceptual shift was the progressive historian Charles Beard. Beard is best known for his economic interpretation of the U.S. constitution. His thinking was shaped by both Karl Marx and John Ruskin. Beard was no socialist, but rather a passionate liberal supporter of Progressive reform. In 1904, Beard joined the faculty at Columbia University, where he embraced the economic interpretation of history advocated by his colleague Edwin Seligman. Beard's *Economic Interpretation of the Constitution*, published in 1913, profoundly influenced generations of historians. Even after leaving Columbia in 1917, Beard's influence on American history and political thought remained considerable.[58]

Like many Progressives, Beard's enthusiasm for reform was coupled with a profound faith in the progress of civilization. The economic view of history, stripped of its Marxist dialectic, promised that material progress would lay the basis for moral progress.[59] In the late 1920s, Beard added *technology* to economics as a motive force in history. This shift to *technology* occurred at a time when Beard's faith in economics as the principle motor of history was beginning to fade.[60]

Beard first granted this new role to *technology* in his 1926 presidential address to the American Political Science Association. In his talk, Beard imagined how incredible the changes in American society would seem if predicted in 1783. Even more astounding than political changes like women's suffrage, suggested Beard, would have been predictions of the transformations wrought by the steam engine and spinning machinery. These immense changes of the past, claimed Beard, suggested important lessons for the future. Two key ideas "thrust themselves upon us" when looking forward in history, first the "pitiless reality of the time-sense," and second "the ideas of indefinite progress--the continuous conquest of material environment by applied science." It is this second idea, the march of material progress, that Beard formulated explicitly in terms of *technology*.[61]

Although Beard's parade of the wonders of material progress was standard rhetoric in American culture, his use of the *technology* was not. Not only was Beard among the first to link *technology* explicitly to the idea of progress, he did it in paradigmatic language that has echoed into the present.

> Not one whit less inflexible [than time] is technology--also a modern and Western Leviathan. Like time, it devours the old. Ever fed by the

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[57]Lewis Mumford, *Technics and Civilization* (New York: Harcourt, Brace, 1934), esp. 3-7.

[58]Hofstadter, *Progressive Historians*.

[59]Hofstadter, *Progressive Historians*, 200.

[60]On the tempering of Beard's economic determinism, see David W. Marcell, *Progress and Pragmatism: James, Dewey, Beard, and the American Idea of Progress* (Westport, Conn.: Greenwood Press, 1974), 274-76.

[61]Beard, "Time, Technology, and the Creative Spirit in Political Science," *The American Political Science Review* 21 (Feb. 1927): 3-5.

> irrepressible curiosity of the scientist and inventor, stimulated by the unfailing acquisitive passion--that passion which will outlive capitalism as we know it...--technology marches in seven-league boots from one ruthless, revolutionary conquest to another, tearing down old factories and industries, flinging up new processes with terrifying rapidity, and offering for the first time in history the possibility of realizing the idea of progress so brilliantly sketched by Abbé de Saint-Pierre.

Under the "convulsive pressures of technology," continued Beard, all systems of thought, including political science, would need to be transformed.[62]

Beard's conception of *technology* was crucial for its later history.[63] First is the notion of autonomous technological change, metaphorically likened to the inalterable movement of time, driven by "curiosity" and "acquisitive passion" grounded in human nature itself. Second, *technology*, not economics, becomes the key determining force in history, ruthlessly transforming not just material culture but also intellectual and spiritual life. Third, this autonomous, deterministic force is not to be lamented but rather embraced as an agent of beneficent progress. Finally, Beard divorces *technology* from capitalism, insisting that its influence does not depend on any specific economic system. Except perhaps for this last point, Beard's prose could serve as present-day editorial copy for the technological enthusiasts at *Business Week*.

Although Beard was an admirer of Veblen and was undoubtedly influenced by his work, Beard's concept of technology had fundamentally different ideological implications. Veblen's vision of *technology* was grounded in human communities, both pre-industrial and industrial, and he explicitly gave women a central technological role in "primitive" culture.[64] Veblen's conception of technology as "immaterial equipment" belonging to the entire community served as a tool of critique aimed at corporate capitalism, a direct challenge to the "Vested Interests," whose legitimacy was based on ownership of material equipment. Despite the common interpretation of *Engineers and the Price System* as a manifesto for technocracy, Veblen makes it clear that engineers did not constitute a new elite, but rather owed their training to "the community at large" and their knowledge to "the community's joint stock of accumulated experience."[65]

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[62]Ibid., 5.

[63]In the next few years Beard made similar sweeping statements about *technology*, perhaps most importantly in his introduction to the American edition of Bury's *Idea of Progress* in 1932. Here Beard chided Bury for his lack of attention to *technology*, given that "technology is the fundamental basis of modern civilization, supplies a dynamic force of inexorable drive, and indicates the methods by which the progressive conquest of nature can be effected." Beard, "Introduction, " in J. B. Bury, *The Idea of Progress: An Inquiry Into Its Origin and Growth* (New York: Macmillan, 1932), xx. Beard attempted to implement his 1926 call to bring *technology* into political science in his 1930 textbook on American government, which he co-authored with his son (and MIT graduate) William. The opening sentence of the book announces that "this volume is the result of an effort to unite politics, government, and technology as reflected in the federal system of the United States, with emphasis on the newer functions created under the pressures of the machine age." Charles A. Beard and William Beard, *The American Leviathan: The Republic in the Machine Age* (New York: Macmillan, 1930), vii. Reviewers echoed, to varying degrees, the prominence Beard gave to *technology*. See for example Carl Becker, "The Idea of Progress: An Inquiry into its Origin and Growth," *American Historical Review* 38 (1933): 306; David S. Muzzey, "The American Leviathan," *Political Science Quarterly* 46 (1931): 109-11.

[64]Veblen, "On the Nature of Capital," 522.

[65]Veblen, *The Engineers and the Price System* (New York: Harcourt, Brace, 1963), 82.

Beard, in contrast, portrayed *technology* as an impersonal, deracinated force, firmly linked to modern engineering and therefore, as Oldenziel has argued, implicitly male. Beard's concept of *technology*, with its firm faith in human progress, was better suited to defending the established order, in particular the role of engineers in modern industry. But one key step remained before technology could fully play this legitimating role: its subordination to the rhetoric of applied science.

Ronald Kline has admirably described the history of the rhetoric of applied science among American scientists and engineers from the nineteenth century through World War II.[66] Claims about the utility of natural philosophy have been commonplace since the early seventeenth century, and eighteenth-century British Newtonians freely attributed advances in the arts to natural philosophy. Newtonian popularizers like John Desaguliers used model Newcomen steam engines to demonstrate the supposed fruits of Newtonian science.[67] Many British manufacturers of the Industrial Revolution fervently believed in the utility of natural philosophy, however difficult it might be for present-day historians to link specific technical developments to scientific theories.[68] The Baconian idea of applying science to the useful arts gathered more steam in mid-nineteenth century America, helping motivate the creation of schools like the Massachusetts Institute of Technology. An 1860 prospectus for MIT stressed the increasingly scientific character of the industrial arts, which "no longer confining themselves to a mere empirical routine, seek to refer their processes to scientific laws, and, in many departments, justly claim the dignity of applied science." The growing "co-operation of intelligent culture with industrial pursuits" justified the creation of the institute.[69]

What the application of science to the useful arts meant to nineteenth-century Americans, however, was not always clear. *Science* in mid-nineteenth century America did not belong to the scientist as much as to the entire educated community. With the simultaneous professionalization of both science and engineering in post-Civil-War America, *applied science* took on contested, ideological charged meanings. As Kline has shown, professional leaders among both engineers and scientists embraced the idea of applied science, yet the term carried quite different meanings. Scientists deployed the term *pure science* to distinguish their work from the *applied science* of engineers and inventors. Henry Rowland popularized the term in a famous lecture in which he contrasted the "noble pursuit" of *pure science* with the "vulgarity" required for its application. Rowland never, however, questioned the utility of pure science. For example, in what became a standard trope among scientists, Rowland credited the

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[66]Ronald Kline, "Construing 'Technology' as 'Applied Science,'" *Isis* 9 (1995): 194-221.

[67]Larry Stewart, "The Selling of Newton: Science and Technology in Early Eighteenth-Century England," *Journal of British Studies* 25 (April 1986): 178-92 . [check]

[68]Among the best works on this theme is Neil McKendrick, "The Rôle of Science in the Industrial Revolution," in *Changing Perspectives in the History of Science*, ed. Mikulas Teich, and Robert Young (London: Heinemann, 1973), 274-319. I find unconvincing Arnold Thackray's attempt to reduce manufacturers' faith in the utility of natural philosophy to a quest for social status. See Arnold Thackray, "Natural Knowledge in Cultural Context: The Manchester Model," *American Historical Review* 79 (1974): 672-709 .

[69]*Objects and Plan of an Institute of Technology*, 3-4.

entirety of the electrical industries to the selfless work of Michael Faraday, who "died a poor man" despite the vast wealth that lesser minds gained from his discoveries.[70]

Future scientists rarely echoed Rowland's hostility towards inventors and others who applied the work of pure science, a tone that may have resulted from unsuccessful business dealings with Thomas Edison. As David Hounshell has shown, Rowland was quite a hypocrite, working as an industrial consultant, taking out patents, and teaching in a program on "Applied Electricity" at Johns Hopkins.[71] Nevertheless, his pure science ideal served a powerful ideological function, drawing from the rhetoric of American republicanism to distinguish virtuous science from the corrupt commerce of its application.[72] By insisting on the virtue of unfettered science along with the utility of its application, Rowland managed to have his cake and eat it too, making the case for both the professional autonomy of science and its financial support. With but minor variations, this argument has been repeated by scientists up to the present day.[73]

Yet Rowland's model of the utility of pure science is based on a completely unsupportable understanding of the relationship between scientific theory and technological practice, as historians of technology have repeatedly shown. Rowland and his followers proposed what was in effect a "conveyor belt" model of the science-technology relationship, in which discoveries in pure science automatically yielded useful applications. There was, however, nothing automatic about the practical benefits of pure science. To turn a laboratory discovery into a practical device often required as much, if not more, creativity that the initial discovery. Discoveries by scientists often inspired the development of new technologies that pushed well beyond the explanatory power of scientific theory, as in the case of the steam engine. Most advances in the practical arts are made without direct aid from pure science, and the most pure research has no discernible practical consequences.[74]

What made this rhetoric so strong, despite our present-day insistence on its inadequacy? There are, I believe, two main reasons. First is the ambiguity in the term *science*, which still retained much of its meaning as organized knowledge well into the twentieth century, even while *science* was becoming increasingly identified as the property of scientists, specifically natural scientists. Thus professional scientists could claim as science all knowledge of nature and the arts, when it suited their purposes. This broad meaning is clear, for example, in the 11-volume *History of Science* published

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[70]Henry Rowland, "A Plea for Pure Science," *Science* n.s. 2 (1883): 242-50, quotes on 242, 243. See the discussion in Kline, "Construing Technology," 198-200.

[71]Kline, 200.

[72]This idea is developed in Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science* 17 (1987): 479-518

[73]See for example D. Allan Bromley, "Science and Surpluses," *New York Times*, March 9, 2001, A19.

[74]There is a vast historical literature on this topic. For versions of the ideas expressed here, see Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm* (New York: Viking Penguin, 1989); Otto Mayr, "The Science-Technology Relationship As a Historiographic Problem," *Technology and Culture* 17 (1976): 663-73 ; Alexander Keller, "Has Science Created Technology?" *Minerva* 22 (1984): 160-82 . On the steam engine and the science of heat see D. S. L. Cardwell, *From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age* (London, Heinemann Educational, 1971).

from 1904 to 1910, written by the American physician and popular science writer Henry Smith Williams. This work has no intellectual standing in the history of science, yet it nicely captures the broad conception of science still operating in the early twentieth century, despite Rowland's plea. Williams cited "the familiar definition of Herbert Spencer [that] science is organized knowledge." Williams used this broad definition to lay claim not only to Rowland's pure science, but also to all manner of folk knowledge. He claimed that even primitive peoples possessed science: "a barbarian who could fashion an axe or a knife of bronze had certainly gone far in his knowledge of scientific principles and their practical application." The first five volumes focused on sciences more or less "pure," but the next four volumes examined topics that would all now be classified as technology, such as the steam engine, dynamo, printing, papermaking, the telegraph, and aviation. Williams had no need for *technology* to describe these topics, being perfectly happy to portray then as manifestations of *science*. Williams justified his shift from the purer sciences by claiming that "even the most visionary devotee of abstract science is forever being carried into fields of investigation trenching closely upon the practicalities of every-day life." The apparently "radical distinction between theoretical and practical aspects of science," argued Williams, was little more than the "differences between two sides of a shield." Williams remained true to his word, portraying the major technical developments of modern industry as the direct consequences of scientific knowledge. For example, he credited the steam engine to the natural philosopher Denis Papin, portraying its most plausible inventor, the ironmonger Thomas Newcomen, as contributing "only a change in mechanical details."[75]

The second major source of strength for the applied science model came, somewhat surprisingly, from the American engineering community itself, which embraced the definition of engineering as applied science. As engineers professionalized alongside scientists, engineers sought to wrap themselves in the mantle of science in order to insure their distinction from skilled laborers, while at the same time maintaining their heavily gendered identity as managers of men.[76] As Kline clearly shows, however, when engineers spoke of *applied science* they often meant something quite different from what scientists understood by that phrase. Kline identifies four distinct uses, the application of scientific theories to engineering problems, the application of scientific methods, an autonomous body of technical knowledge, and the practices of engineering in general, including research, teaching and innovation. Yet, I believe, even when engineers described their work as the application of scientific theories, they almost never conceived this application as

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[75]Henry Smith Williams, *A History of Science*, 11 vols. (New York: Harper & Brothers, 1904-1910), quotes on 1:3, 1:5, 6:1-2, 6:90. This interpretation of the Newcomen engine as a direct product of Papin's research was common from the late eighteenth to the mid-twentieth century. For the origins of this myth and its refutation, see L. T. C. Rolt, *Thomas Newcomen: The Prehistory of the Steam Engine* (Dawlish [Eng.]: David and Charles, 1963).

[76]On gender issues see Oldenziel, *Making Technology Masculine*; on science as an ideology for engineers, see Edward Layton, *Revolt of the Engineers*. Engineers' rhetoric no doubt varied in different countries with the structure of the engineering professions; in Germany, for example, the lower status of the *Technische Hochschulen* encouraged engineers to embrace *Technik* as a form of *Kultur*, comparable to other branches of higher learning. See Hård, "German Regulation." This rhetoric was undoubtedly different in Britain, where apprenticeships continued to provide the backbone of engineering training until after World War II.

implying any form of intellectual subordination to professional scientists. More sophisticated engineers argued that their profession was both a science and an art, or perhaps the art of applying science. For the most part, however, most of what engineers and scientists said about *applied science* was predictable prattle, the stuff of after-dinner speeches by retiring presidents of professional societies, revealing a bit about professional identities but little about the concrete relationship between the work of scientists and engineers.[77]

On the whole, however, the rather simplistic understanding of engineering as *applied science* served the engineering profession quite well before World War II. Nevertheless, the spread of *technology* from the social sciences into broader elite discourse in the 1930s provided engineers with an attractive alternative concept. Given the continued association of *technology* with the study of the industrial arts through institutions of higher education, it was relatively easy for engineers to equate *technology* with *applied science*. *Technology*, however, carried no sense of subordination to science. Furthermore, as *technology* broadened its meaning to encompass the arts themselves as well as knowledge of the arts, the term gave engineers another claim to territory that had previously belonged to skilled workers. Engineers in corporate service had long contested this territory under the guise of Frederick Taylor's scientific management.[78]

Two events in the early 1930s helped extend *technology* beyond the social sciences. First was the debate over "technological unemployment," and second was the controversy over the Technocracy movement. Concern about the displacement of workers by machines intensified in 1920s America as growth in industrial employment failed to match the rapid growth in output, but this concern was not labeled *technological unemployment* until shortly before the stock-market crash of 1929.[79] After the crash, though, *technological unemployment* becomes a major concern among American social scientists. Amy Sue Bix has ably described the rise of this debate, which engaged social scientists, labor unions, scientists, engineers, and government officials. Most of the popular debate was conducted in terms of *machines* and *inventions*, but among social scientists *technological unemployment* was the preferred designation. Even Charles Beard used the term, which meshed nicely with his view of *technology* as a driving force of history.[80] The debate over technological unemployment was raised to a new level by the rise of the Technocracy movement in 1933, which sought to cure the economic crisis by replacing the captains of industry with scientists and engineers, who would run the economy using the principles of thermodynamics. The Technocrats were little more than crackpots, but in the deep economic crisis of 1933 their ideas drew considerable

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[77]Kline, 201-2, 212-13. For a sophisticated clash over the role of science in engineering, see the symposium on "The Teaching of Mathematics to Students of Engineering," *Science* n.s. 28 (1908): 161-70, 257-68.

[78]This analysis is speculative.

[79]This claim is based on a JSTOR search. For examples see Leo Wolman, "Some Observations on Unemployment Insurance," *American Economic Review* 19 (Mar. 1929): 23; Harry Jerome, "Production," *American Journal of Sociology* 34 (1929): 1002. Jerome puts the phrase in quotation marks, suggesting its novelty.

[80]Amy Sue Bix, *Inventing Ourselves Out of Jobs? America's Debate over Technological Unemployment, 1929-1981* (Baltimore: Johns Hopkins University Press, 2000); Charles Austin Beard, "The Dislocated Soldier of Industry," in *Unemployment and Adult Education* (New York: American Association for Adult Education, 1931), 9-12.

attention.[81] By making a claim to technical expertise in the name of science and engineering, the Technocrats infringed on engineers' and scientists' claim to professional authority. As Oldenziel shows, the engineers in particular promoted "the mobilization of technology against Technocracy," using the applied-science definition of technology to deny any responsibility for the economic crisis. "Technocracy is destructive; technology is creative," declared the prominent industrial chemist Arthur D. Little in 1933, using rhetoric typical of the elite engineers who defended corporate capitalism against its Technocratic critics.[82]

These struggles over meaning did not make *technology* a household word, but they did bring it to the attention of a broader public. In 1937 the social-scientific use of the term merged with public policy in the influential report, *Technological Trends and National Policy*, produced by the Subcommittee on Technology of the National Resources Committee, a group of senior New Deal officials with responsibility for national resource issues.[83] The Technology Subcommittee was chaired by William Ogburn, who was clearly the moving force behind the report. Although as Bix notes, Ogburn was not as insistently optimistic about the consequences of technological change as leading engineers and scientists, the report did enshrine his thoroughly deterministic understanding of *technology* based on his cultural-lag theory of social change. Ogburn hoped to predict the social effects of technological change so that government policy would be able to cope better with the consequences. Like Beard, for Ogburn *technology* was the unmoved mover, the part of culture that rushed ahead while law, religion, and family life lagged behind. This hefty volume, appearing as an official government publication, helped link government policy with a deterministic view of *technology*.

Ogburn's *Technological Trends* did much to cement a deterministic, engineering-centered, applied-science definition of technology in elite discourse. By the late 1930s, this notion of technology had become dominant. Dissenting voices were rarely heard, although there were stirrings of a more sophisticated understanding among historians of science.[84] Certainly by the late 1930s *technology* had assumed the full set of contradictory meanings that it would carry into the postwar era. Yet despite its diffusion the term remained secondary to concepts like *engineering, the machine, invention* and *science*. Not until after World War II would *technology* assume its central place in popular discourse as the avatar of modern science, an organizing principle for understanding the modern world.[85]

There were, therefore, profound ideological implication in the shift in *technology* from a rather narrow, recondite term in nineteenth-century English to its emergence by the early 1930s as a broad term of scholarly discourse covering both the practical arts the formal knowledge associated with them. *Technology* came to embody these

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[81]Bix, 118-21.

[82]Oldenziel, 47-48; Kline, 217; Little quoted in Oldenziel, 48.

[83]*Technological Trends and National Policy* (Washington: GPO, 1937).

[84]See especially Abraham Wolf, *A History of Science, Technology, and Philosophy in the 16th & 17th Centuries* (London: George Allen & Unwin, 1935), 450-52; and Robert K. Merton's *Science, Technology & Society in Seventeenth Century England*, originally published in 1938.

[85]This enshrining of technology was marked in part by its function as the beneficiary of basic research in Vannevar Bush's report, *Science: The Endless Frontier*. See Kline, 218-20.

contested, ideological meanings while the term was still largely confined to academic discourse, particular discourse in the social sciences. Central to this change in meaning was the shift in the term from the study of the arts to the arts themselves. With considerable irony, this shift was inspired in large measure by critics of industrial capitalism, including Karl Marx, Thornstein Veblen and Charles Beard. Yet Beard in particular undermined *technology* as a critical concept by linking it firmly to the concept of progress, thus transforming *technology* into a cultural resource for apologists of industrial capitalism. It was these apologists, particularly among elite scientists and engineers, who embraced the model of *technology* as applied science.

Fischer (2)

AMERICA CALLING

A Social History of the Telephone to 1940

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CHAPTER ONE

Technology and Modern Life

In 1926 the Knights of Columbus Adult Education Committee proposed that its group meetings discuss the topic "Do modern inventions help or mar character and health?" Among the specific questions the committee posed were

"Does the telephone make men more active or more lazy?"

"Does the telephone break up home life and the old practice of visiting friends?"

"Who can afford an automobile and under what conditions?"

"How can a man be master of an auto instead of it being his master?"

The Knights also considered whether modern comforts "softened" people, high-rise living ruined character, electric lighting kept people at home, and radio's "low-grade music" undermined morality. The preamble to the questions declared that these inventions "are all indifferent, of course; the point is to show the men that unless they individually master these things, the things will weaken them. The Church is not opposed to progress, but the best Catholic thinkers realize that moral education is not keeping up with material inventions."[1]

Worry about the moral implications of modern devices was especially appropriate in 1926, for middle-aged Americans had by then witnessed radical material changes in their lives. Despite the awe that many express about today's technological developments, the material innovations in our everyday lives are incremental compared to those around the turn of the century. Major improvements in food distribution and sanitation lengthened life and probably lowered the birth rate. Streetcars brought average Americans easy and cheap local travel. Telephone and radio permitted ordinary people to talk and hear over vast distances. Electric lighting gave them the nighttime hours. Add other innovations, such as elevators, movies, and refrigerators, and it becomes apparent that today's technical whirl is by comparison merely a slow waltz.[2]

The questions the Knights pondered were widely addressed. Many, especially representatives of business, gave rousing answers: Modern inventions liberated, empowered, and ennobled the average American. The American Telephone and Telegraph Company (AT&T) issued a public relations announcement in 1916 entitled "The Kingdom of the Subscriber." It declared:

> In the development of the telephone system, the subscriber is the dominant factor. His ever-growing requirements inspire invention, lead to endless scientific research, and make necessary vast improvements and extensions. . . .
>
> The telephone cannot think or talk for you, but it carries your thought where you will. It's yours to use. . . .
>
> The telephone is essentially democratic; it carries the voice of the child and the grown-up with equal speed and directness. . . .
>
> It is not only the implement of the individual, but it fulfills the needs of all the people.[3]

Less self-interested parties made similar claims. In 1881 *Scientific American* lauded the telegraph for having promoted a "kinship of humanity." Forty years later a journalist extolled the radio for "achieving the task of making us feel together, think together, live together."[4] The author of *The Romance of the Automobile Industry* declared in 1916 that the "mission of the automobile is to increase personal efficiency; to make happier the lot of people who have led isolated lives in the country and congested lives in the city; to serve as an equalizer and a balance." Many urban planners and farm women, to take two disparate groups, shared similar images of the automobile as a liberator.[5]

But others, notably ministers and sociologists—in those days not always distinguishable—warned that these inventions sapped Americans' moral fiber. In 1896 the Presbyterian Assembly condemned bicycling on Sundays for enticing parishioners away from church—a forecast of complaints about the automobile. Booth Tarkington's fictional automobile manufacturer in *The Magnificent Ambersons* reflects:

> With all their speed forward they may be a step backward in civilization—in spiritual civilization. It may be that they will not add to the beauty of the world, nor to the life of men's souls. I am not sure. But automobiles have come, and they bring a greater change in our life than most of us suspect. They are here, and almost all outward things are going to be different because of what they bring. . . . I think men's minds are going to be changed in subtle ways because of automobiles; just how, though, I could hardly guess.

Robert and Helen Lynd, the former a cleric turned sociologist, claimed in their classic *Middletown* (1929) that the automobile and the enticements it brought within reach—roadhouses, movies, and the like—undermined the family and encouraged promiscuity. College administrators in the 1920s argued that automobiles distracted students from their studies and led many to drop out. Observers worried less often about the telephone, but some objected that it encouraged too much familiarity and incivility and that it undermined neighborhood solidarity.[6]

These comments, whether by industry representatives or viewers-with-alarm, reflected genuine and widespread concerns, at least by elites, about the social implications of modern inventions. The concerns are, in turn, rooted in a larger meditation in Western societies about modernity.

MODERN TIMES

Modernity is an omnibus concept and, like the omnibus of the nineteenth century, carries a variety of riders—an eclectic assortment of ideas about economic, social, and cultural changes over the past several generations.* Most sociologists and historians writing about

*Large bodies of literature in sociology, history, and the humanties address the concepts of modern, modernization, and modernity. They identify many sociocultural modernization focus on industrial and commercial development: the rise of the factory, market, or corporation, and the increase in affluence. Others stress changes in social organization, such as the evolution of the nation-state and the small household. Still others emphasize alterations in culture and psyche, for example, the growth of individualism, sentimentality, or self-absorption. Modernization theorists also differ about when in the past three centuries the critical transformations happened. Most, however, implicitly agree that modernity comes as a coordinated set of changes. Whichever change is depicted as the conductor of this omnibus, the rest inevitably come along for the ride, for modernization is a global process.[7]

Contemporary writers follow the path trod by the founders of social science, theorists such as Emile Durkheim, Max Weber, Karl Marx, Ferdinand Tonniës, and Georg Simmel. Living from the mid-nineteenth century through the early years of the twentieth and surrounded by severe disjunctures in material culture, they believed that a new society was being born. The theorists largely concentrated on changes in economic organization, but much of their attention also turned to social life—to personal relations, family, and community. Modernity in these spheres followed in part from changes in how people made a living, but modernization also directly transformed private life. The growth of cities, wider communication, more material goods, mass media, and the specialization of land use and institutions—these kinds of changes, the early social theorists argued, altered personal ties, community life, and culture. More specifically, modernization fostered individualism and interpersonal alienation, abraded the bonds of social groups, and bred skepticism in place of faith. Some theorists described these developments as the liberation of individuals from the shackles of oppressive communities, others as

attributes as *the* trait that distinguishes the modern from the premodern (never mind the postmodern): rationality, individualism, secularism, organization (*Gemeinschaft*, usually defined as "society," as opposed to *Gesellschaft*, "community"). The conceptual statements usually beg an empirical question by assuming that this property is more common now than it was "then" (whenever and wherever "then" was). Since I am concerned precisely about the empirical assumptions, my usage is simple. By modernity I mean the style of social life and culture typical of twentieth-century America, as contrasted to earlier eras, especially the nineteenth century. Some, especially those who locate the great transition a few centuries ago, will find that a misuse of the term. Presumably, whatever the criterion is, it nevertheless ought to have become more evident over the past four generations.

the isolation of individuals from loving communities. Two sides of the same coin. Much, perhaps most, of modern sociology and increasingly of the field of social history involves variations on this motif.[8]

Modernization theory, by now implicit in the language used to discuss contemporary society, is open to several criticisms. Critics debate whether such transformations really happened. The assumption that economic, social, and psychological changes would occur together is debatable. Charles Tilly, for example, has challenged the theoretical assumption that " 'social change' is a coherent general phenomenon, explicable *en bloc*." Darrett Rutman has poked fun at the tendency of his colleagues in the field of history to locate the "lost community" ever backward in time: "Some have said we lost it when we disembarked John Winthrop from the *Arbella*"—all of which "has made us appear to be classic absent-minded professors regularly losing our valuables."[9]

Still, the concerns addressed by modernization theorists, and in simpler forms by nonacademics like the 1926 Knights of Columbus, are profound. The material culture of twentieth-century society differs strikingly from that of earlier eras. How has that difference altered the personal lives of ordinary people? In this book I am concerned with the manner in which turn-of-the-century technologies made a difference to North Americans' ways of life, in particular to community and personal relations. I use the telephone as a specific instance of that material change, bringing in the automobile for comparison.

The results of this inquiry suggest, in broad strokes, that while a material change as fundamental as the telephone alters the conditions of daily life, it does not determine the basic character of that life. Instead, people turn new devices to various purposes, even ones that the producers could hardly have foreseen or desired. As much as people adapt their lives to the changed circumstances created by a new technology, they also adapt that technology to their lives. The telephone did not radically alter American ways of life; rather, Americans used it to more vigorously pursue their characteristic ways of life.

The next section of this chapter pursues theoretical issues in the study of technology. Some readers may wish to turn to a later section of this chapter—"Why the Telephone?"—where explicit discussion of the telephone begins (p. 21).

DOES TECHNOLOGY DRIVE SOCIAL CHANGE?

Technological change in the personal sphere is a central dynamic of all theories of modernity.[10] Today's instruments of daily life—food preservatives, artificial fabrics, cars, and so on—are at least necessary, if not sufficient, conditions for what we consider modern society. Interest in whether and how such technologies alter social life generated a field of study, "technology and society."

Once a sociology of technology focused on these matters. It flourished until the early 1950s under the leadership of the University of Chicago's William F. Ogburn, but "passed into oblivion in slightly more than two decades." Currently, scholarship on technology rests largely in the hands of historians and economists, although a band of more sociologically oriented scholars are active. Historians have superbly documented the technological developments that mark Western modernization. Yet they usually write on the social sources of technological change (for example, how national cultures shaped the development of trolley systems) rather than on the technological sources of social change. Economists tend to focus on immediate and straightforward applications of technical advances. Neither group, and few scholars generally, have looked closely at how the use of major technologies affects personal and social life.[11] There are important exceptions. Most noteworthy are several historians who have studied housework technologies. They have striven to understand how vacuum cleaners, stoves, and the like altered the lifestyles and well-being of American women.[12] In general, however, scholars have neglected the social role of technology and left "theorizing"—that is, accounting for the influence of technology on social life—to the older Ogburn approaches or to common sense.

Others, quite different, have eagerly addressed the social implications of technology. These, loosely termed "culture critics," contend that technology has created a modern *mentalité*. They have posed some challenging ideas. Where Ogburn and others saw the nuts and bolts of a technology, they see its symbolism and sensibility.

But both perspectives on technology are problematic. Our way of thinking about the *causal* link between technology and social action impedes our understanding of technology's role. Even the language we employ can be a problem, as in the common use of the word *impact* to describe the consequences of technological change.

Defining Technology

The dictionary defines technology as applied science. Some have construed it more broadly, as "practical arts," the knowledge for making artifacts, or even the entire set of ways that people organize themselves to attain their wants. Put that broadly, the concept comes to subsume almost all human culture, including magic. As the label stretches—as it becomes, for example, a synonym for rationality—"technology" becomes less a subject of study and more a rhetorical term.[13]

Let us restrict the idea to the more tangible, physical aspects of technology, to devices and their systems of use. And since this study concerns the everyday domestic sphere, technology here is similar to the idea of material culture. For some people, items of material culture, such as refrigerators, bicycles, telephones, phonograph records, and air conditioners, may seem too mundane for serious study. Yet Siegfried Giedion offers another viewpoint in the opening pages of *Mechanization Takes Command*:

> We shall deal here with humble things, things not usually granted earnest consideration, or at least not valued for their historical import. But no more in history than in painting is it the impressiveness of the subject that matters. The sun is mirrored even in a coffee spoon.
>
> In their aggregate, the humble objects of which we shall speak have shaken our mode of living to its very roots. Modest things of daily life, they accumulate into forces acting upon whoever moves within the orbit of our civilization.[14]

The prosaic objects of our culture form the instruments *with* which and the conditions *within* which we enact some of the most profound conduct of our lives: dealing with family, friends, and ourselves.

For most culture critics these objects are the focus of concern. The key question usually is: What has the automobile, or the television, or the skyscraper, or whatever *thing*, done to us? Of course, a material object itself, lying bare on the ground, is of no interest. As historian Thomas Hughes has emphasized, there is a "system" around a functioning technology—a commercial broadcasting system around the television; appliance, electrical, and food-packaging systems around the refrigerator. References to the material object, as in "the diffusion of the automobile," are shorthand for the larger system.[15] The point is not merely a matter of lexicon. Separable parts of a technological system may have separable consequences. Television, for example, can be analyzed by its specific content—such as the sexual titillation, violence, and commercials it broadcasts—or by its technical features—such as the flickering of images, dissociation of place, and passivity of watching.

Intellectual approaches to technology and society can be divided into two broad classes: those that treat a technology as an external, exogenous, or autonomous "force" that "impacts" social life and alters history, and those that treat a technology as the embodiment or symptom of a deeper cultural "logic," representing or transmitting the cultural ethos that determines history.[16] Each approach is problematic.

Impact Analysis

The older, Ogburn analysis is a "billiard-ball" model, in which a technological development rolls in from outside and "impacts" elements of society, which in turn "impact" one another. Effects cascade, each weaker than the last, until the force dissipates. So, for example, the automobile reduced the demand for horses, which reduced the demand for feed grain, which increased the land available for planting edible grains, which reduced the price of food, and so on. A classic illustration is Lyn White's argument that the invention of the stirrup led, by a series of intermediate steps, to feudalism.[17]

Economic rationality is an implicit assumption in the billiard-ball metaphor. A technology is considered imperative to the extent that it is rational to adopt it. Adopting it in turn alters related calculations, leading to further changes in action. The model allows for unintended consequences, particularly during Ogburn's famous "cultural lag" (a period of dislocation when changes in social practice have not yet accommodated the new material culture), but change largely follows the logic of comparative advantage among devices. More contemporary versions of this impact model appear in the literature on technology assessment.[18] Such thinking about technology is deterministic: Rationality requires that devices be used in the most efficient fashion.

Critics have challenged the assumption that technological change comes from outside society as part of an autonomous scientific development and that application of a device follows straightforwardly from its instrumental logic. Instead, these critics contend that particular social groups develop technologies for particular purposes—such as entrepreneurs for profits and the military for warfare. The developers or other groups, operating under distinctive social and cultural constraints, then influence whether and how consumers use the new tools.[19] Some scholars have argued, for example, that the automobile, tire, and oil industries, through various financial stratagems, killed the electric streetcar in the United States to promote automobile and bus transportation.[20] In this view technological change is better understood as a force called up and manipulated by actors in society. Historian George Daniels puts the challenge broadly:

> No single invention—and no group of them taken together in isolation from nontechnological elements—ever changed the direction in which a society was going. . . . [Moreover,] the direction in which the society is going determines the nature of its technological innovations. . . .
>
> Habits seem to grow out of other habits far more directly than they do out of gadgets.[21]

Against the metaphor of ricocheting billiard balls, we have perhaps the metaphor of a great river of history drawing into it technological flotsam and jetsam, which may in turn occasionally jam up and alter the water's flow, but only slightly.

Others reject technological determinism less completely, granting that material items have consequences, but claiming that those consequences are socially conditioned. Societies experience technological developments differently according to their structure and culture. For example, John P. McKay has shown how the trolley system developed more slowly but more securely in Europe than in the United States. Others have argued that France's autocratic centralism retarded the diffusion of the telephone.[22] More generally, historians of technology often explain that a technological development may have unfolded otherwise were it not for social, political, or cultural circumstances. For instance, some historians of housework contend that American households might have developed communal cooking and laundering facilities with their neighbors, but instead most individual American families own small industrial plants of ovens and washers, expensive machines that are idle 90 percent of the time. This is not economically efficient, critics contend; rather, it is the outcome of American institutions and culture. (More on this "social constructivism" perspective later.) The blunt conclusion from the last generation of scholarship is that the whig analysis of technology cannot hold. The ideas that technologies develop from the logical unfolding of scientific rationality, that they find places in society according to principles of economic optimization, that their use must be comparatively advantageous to all, and that the only deviation from this rationality is the brief period of social disruption labeled "cultural lag"—this model has long been rejected as conceptually and empirically insufficient.

But another form of determinism has arisen: the "impact-imprint" model. According to this school of thought, new technologies alter history, not by their economic logic, but by the cultural and psychological transfer of their essential qualities to their users. A technology "imprints" itself on personal and collective psyches.

Stephen Kern's *The Culture of Time and Space, 1880–1918*, which illustrates this approach, is a well-received and thoughtful analysis of space-transcending technologies developed before World War I: the telegraph, telephone, bicycle, and automobile. Together, Kern contends, these new technologies "eradicated" space and shrank time, thus creating "the vast extended present of simultaneity." Without barriers of space and time, we moderns can reach and be reached from all places instantly, an experience leading to heightened alertness and tension.

The crux of Kern's argument is that the essences of the technologies—the speed of the bicycle and automobile, the instancy of the telegraph and telephone—transfer to their users. For example, Kern cites a 1910 book on the telephone (subsidized, it turns out, by AT&T) claiming that with its use "has come a new habit of mind. The slow and sluggish mood has been sloughed off . . . [and] life has become more tense, alert, vivid." Similarly, he quotes a French author on how driving an automobile builds skills of attention and fast reaction. The technologies passed on their instancy and speed to the users and, through them and through artists, to the wider culture.[23]

But how can a technology pass on its properties? Ultimately, the argument rests on metaphor become reality. At points, Kern lays out a plausible causal explanation. For example, he contends that unexpected telephone calls at home promote anxiety and feelings of helplessness.[24] He does not, however, pursue this kind of speculation consistently. Had he done so, he might have found that it did not always lead in the same direction. The telephone might also promote calm because its calls reassure us that our appointments are set and our loved ones are safe. Kern might also have more consistently compared the psychological consequences of these technologies with those of their precursors. While he compares the suddenness and demand of the telephone call to the leisureliness of the letter, he does not compare it to the surprise and awkwardness of an unexpected visitor at the door. The power of Kern's general argument rests ultimately on the impact-imprint metaphor: The jarring ring of the telephone manifests itself in a jarred and nervous psyche.

Kern's analysis also raises issues of evidence. Most of his material comes from literary and artistic works, suggestive and significant to be sure, but not to be taken at face value.* Even more, he and his sources typically reason from the properties of the technologies to the uses of them and then to the consequences. For example, the essence of the automobile is speed; it is used in a speedy way; thus its users' lives are speedier. Instead of reasoning from the properties of the tools, however, one might look at what people do with the tools. In the case of the automobile, one could reason that the replacement of the horse and train by the automobile would have sped up users' experiences. This may sometimes be so, but not always or perhaps even mostly. Touring by car rather than train probably led, according to a historian of touring, to a more leisurely pace. People could pull over and enjoy the countryside, "smell the roses." Similarly, farmers who replaced their horses with motor vehicles could travel faster to market, but many apparently used the saved time to sleep in longer on market day.[25] Kern's *Space and Time* exemplifies a mode of thinking about technology that, while more sophisticated than the earlier simple technological determinism, is still deterministic.

Joshua Meyerowitz's *No Sense of Place* presents a similar logic. In this award-winning volume Meyerowitz combines McLuhanesque insights with some sociology to create an argument both similar to and different from Kern's. Electronic media "lead to a nearly total dissociation of physical place and social 'place.' When we communicate through telephone, radio, television, or computer, where we are physically no longer determines where and who we are socially."[26] All places become like all others; cultural distinctions among places are erased, privacy is reduced, and areas of life previously sheltered from public view—the "backstage"—are revealed. Like Kern, Meyerowitz reasons from the properties of the technologies to their consequences: Electronic media are "place-less," so people lose their sense of place.

The problems of this approach are similar to Kern's. Meyerowitz, for example, argues that, unlike letter writers, telephone callers can

*By which I mean: Artists do not simply mirror their society. Instead of merely describing reality, they often "play" with reality by, for example, depicting escapes from it, ironic twists on it, fears about it, or romanticizations of it.

pierce other people's facades by hearing sounds in the background of the other party. Thus the telephone breaks down privacy. But why not instead compare the telephone call to the personal visit or to the front-stoop conversation? If telephone calls have replaced more face-to-face talks than letters, then the telephone has increased privacy. On empirical issues Meyerowitz relies on "common sense" or news stories for evidence and produces very few historical accounts. To take a minor illustration, Meyerowitz argues that "electronic messages . . . steal into places like thieves in the night. . . . Indeed, were we not so accustomed to television and radio and telephone messages invading our homes, they might be the recurring subjects of nightmares and horror films." Perhaps. But while accounts of early telephony (pronounced teh-LEH-feh-nee) suggest a wide range of reactions, including wonder and distaste, they do not indicate that early users had nightmares about invading messages.

The two forms of technological determinism reviewed here differ. The older one was "hard," simple, and mechanistic; the newer is "soft," complex, and psychocultural. But both are deterministic. A technology enters a society from outside and "impacts" social life. Both describe a form of cultural lag, during which sets of adaptive problems arise because we, by nature or by historical experience, are unable to use a new technology to meet our needs and instead are used by it. Ironically, because the newer form of determinism is more cultural and thus more holistic (and thus also in some ways like the "symptomatic" approach discussed in the following section), it typically describes a convergence of similar effects—for example, in Meyerowitz's electronic media and placelessness. Different specific technologies change us in the same ways. This logic can be even more deterministic than that of Ogburn, since his analysis contains the possibility that specific cause-and-effect trajectories may diverge. In either case, such impact analyses ought to be abandoned. The first is too rationalized, mechanical, and lacking in social context. The latter is too reliant on imagery rather than evidence. It suffers from what historian David Hackett Fischer labels "the fallacy of identity."[27] Indeed, we should abandon the word *impact*. The metaphor misleads.

Symptomatic Approaches

"Symptomatic" analysts, to use literary critic Raymond Williams's term, describe technologies not as intrusions into a culture but as expressions of it. Langdon Winner uses the term "technological politics" for a theory that "insists that the *entire structure* of the technological order be the subject of critical inquiry. It is only minimally interested in the questions of 'use' and 'misuse,' finding in such notions an attempt to obfuscate technology's systematic (rather than incidental) effects on the world at large." Typically, the underlying *Geist*, or spirit, is an increasing rationalization of life, carrying with it mechanization, inauthenticity, and similar sweeping changes. Specific material goods are in essence manifestations of this fundamental *Geist*.[28]

Much of Lewis Mumford's later writings are in this vein, for example:

> During the last two centuries, a power-centered technics has taken command of one activity after another. By now a large part of the population of this planet feels uneasy, indeed deprived and neglected unless it is securely tied to the megamachine: to an assembly line, a conveyor belt, a motor car, a radio or a television station, a computer, or a space capsule. . . . Every autonomous activity, one located mainly in the human organism or in the social group, has either been bulldozed out of existence or reshaped . . . to conform to the requirements of the machine.[29]

More popular writings, such as those of Ellul on *technique* and Schumacher in *Small Is Beautiful*, also describe a deep force that spawns a homogeneous set of technologies.

A specific technology matters little. It may be the actual instrument of a deeper process or just a sign of it, a synecdoche for all technology. Leo Marx has shown how nineteenth-century American Romantics used the railroad as an emblem for social change. More recently, writers have held that other technologies, such as the engine, assembly line, and automobile, epitomize deeper conditions such as cultural modernity.[30]

The symptomatic approach raises its own problems. The causal logic is usually opaque: How does a *Geist* shape psyche and culture? Do people learn, say, rationalization, by using specific devices? Or, is using a device the expression of rationalization learned in other ways, say, through mass media? The approach carries a major assumption about technology that seems both logically and empirically unwarranted: that modern technologies form a coherent, consistent whole—a contention that follows almost necessarily from the idea of an underlying process. Jennifer Stack has pointed out that "by assuming, and therefore searching for, only correspondences [of technologies with the *Geist*] writers deny the possibility that a technology might embody elements that truly contradict the essence of the totality or simply express something other than the essence."[31] This holism appears in several forms.

One form is the implicit claim that these technologies operate in parallel with homogeneous effects. Mumford makes that claim in his list of devices that people cling to, and others make it in arguments that modern technologies generally lead to routinization or that they necessarily alienate users from nature. But do all these modern tools operate in parallel? Perhaps not. Take, as another example, philosopher Albert Borgmann's inquiry on *Technology and the Character of Contemporary Life*. He defines modern technology as "the typical way in which one in the modern era takes up with reality," a truly global definition. Borgmann then distinguishes modern (1700 to now) *devices* from largely premodern *focal things*. Things are objects whose operations we understand and that can "center and illuminate our lives"—like fireplaces, violins, and national parks. They are good. Devices are objects whose internal workings are mysteries and that merely deliver some end to us—like central heating, stereos, and motor homes. They are bad. (The evaluations are explicit in Borgmann's book.)[32] One immediate problem, among others, is that Borgmann equates so many diverse objects—toasters to telemetry—and asserts that they all deeply affect relations and psyches in the same way.*

There is little theoretical and less empirical reason to lump these diverse objects into a single category *a priori* and to assume parallelism. Such an action forecloses rather than broadens scholarly inquiry. (It assumes a "myth of cultural integration."[33]) The various uses of different technologies may clash with one another. Perhaps, for example, movies helped bring people into public spaces more, but television reversed that. Or take the idea of routinization. Some have suggested that the railroads enforced a rigidity about time through their fixed schedules. If so, the automobile must have contradicted this trend by allowing people to come and go as they pleased. Or take housework. Ruth Cowan has persuasively argued that some household appliances brought functions into the home and others extruded functions

*Other problems include the difficulty any other observer would have in distinguishing a focal thing from a device, the evident subjectivity of the distinction. As in many other cultural critiques, we have a catalog of class prejudices. Violins, Borgmann claims, are focal, because he presumably can play and enjoy them; the operations of stereos are alienating mysteries. Of course, for others, the reverse is true. Similarly, computers are mere devices to Borgmann, although to many they are engrossing and fulfilling, constituting a focus of community.

from it. Or, finally, take the set of technologies Malcolm Willey and Stuart Rice call "agencies of communication," some of which they claim increased cultural standardization (radio, movies) and some of which they claim reduced it (telephone, automobile).[34] If even within such narrow sets of technologies there could be such varieties of possible consequences, how can we assume homogeneous consequences across the hodgepodge of modern tools?*

Another corollary is the assumption that the several effects of any device operate in parallel and are the same for all people. A technology could, instead, have contradictory consequences or different ones for different groups. For example, farmers' use of the automobile may have simultaneously solidified rural communities by increasing local interaction and weakened them by allowing farm families to tour distant locales. And use of the automobile may have increased the social mobility of blacks in the South more than that of whites. The workplace computer may both degrade the skills of middle managers and upgrade those of secretaries.[35]

Another dubious corollary is that technology has cumulative effects: The more of the cause, the more of the consequence; for example, the more powerful computers are, the more "placelessness" there is, to use Meyerowitz's term. Sometimes this may be so, but often it probably is not. When televisions were scarce, for instance, family members and even neighbors came together to watch, but as televisions became common, it seems that people increasingly watched them alone. Similarly, early washing machines may have encouraged collective housework, drawing homemakers to laundromats, but the later, cheaper machines probably encouraged privatization of housework by allowing homemakers to do the wash at home.[36]

Since those writing in the symptomatic mode assume that history has a grand direction, they often tend to extrapolate developments almost *ad infinitum*. Video games provide a cautionary tale. In the early 1980s many commentators projected the PacMan-ization of American youth. Yet the video craze collapsed almost as fast as it grew (and then it rebounded with Nintendo games, but perhaps only for a while).

*Sigmund Freud made a similar point in *Civilizations and Its Discontents*: "Is there, then, no positive gain in pleasure, no unequivocal increase in my feeling of happiness, if I can, as often as I please, hear the voice of a child of mine who is living hundreds of miles away . . . ? [But] if there had been no railway to conquer distances, my child never would have left his native town and I should need no telephone to hear his voice. . . . " (translated by James Strachey, Norton edition, 1962, p. 35).

Claims about the computerization of the American home appear to be similarly mistaken.[37]

The symptomatic approach widens our view of technology from simply mechanical and instrumental attributes to the cultural and symbolic contexts within which devices are developed and employed. It reinforces the need to incorporate social context into our explanations. In some ways, however, this approach is more problematic than simple technological determinism. Because its proponents locate the source of change in a global *Geist* and therefore disdain serious attention to any particular technology, this approach cannot explain how people come to use a technology and thereby change their lives. Its holism may conceal and confuse matters more than the piecemeal nature of technological determinism.

Social Constructivism

Several historians and sociologists, particularly European scholars, have in recent years formalized an approach that stresses the indeterminacy of technological change. Mechanical properties do not predestine the development and employment of an innovation. Instead, struggles and negotiations among interested parties shape that history. Inventors, investors, competitors, organized customers, agencies of government, the media, and others conflict over how an innovation will develop. The outcome is a particular definition and a structure for the new technology, perhaps even a "reinvention" of the device. The story could always have been otherwise if the struggles had proceeded differently. That is why the same devices may have different histories and uses in different nations. I have already mentioned the example of streetcar systems. Similarly, radio frequencies became privately owned franchises broadcasting commercially sponsored entertainment in the United States because of social conditions and political arguments specific to this country. (Critics of a more deterministic bent might rejoin, however, that such national differences in radio operations pale in comparison to their similarities.)[38]

This perspective brings us closer to incorporating end users into the analysis. Carolyn Marvin, for example, describes debates among electrical experts of the late nineteenth century about the social implications of lights and telephones and what ought to be done to manage those implications. Users are represented in "negotiations" that reshape innovations and channel their use by interest groups and ultimately by the purchase decisions of individual customers and the actual use to which those individuals put the technology. By this process, the technology is transformed into something different. In the case of the telephone, we will see how AT&T leaders, pressed in part by consumers, eventually tried to redefine their product from a totally practical service into a "comfort," a luxury, of the modern lifestyle.[39]

Most social constructivism has concentrated on the producers, marketers, or experts of a technological system. I intend to go further, to emphasize the mass users of technology, to go to what Ruth Schwartz Cowan has labeled the "consumption junction"—the point at which the final consumers choose, employ, and experience a technology. What we ultimately need, as Cowan argues and illustrates with the history of stoves, is a focus on the consumer if we are really to understand the social implications of technology.[40]

A User Heuristic: From the Consumer's Viewpoint

Once we have understood the genesis of a technology, its development and promotion, we can begin looking at consequences. Here we should ask: Who adopted the device? With what intention? How did they use it? What role did it play in their lives? How did using it alter their lives? This angle, an extension of social constructivism, emphasizes human agency and intentionality among end users. People are neither "impacted" by an external force, nor are they the unconscious pawns of a cultural *Geist*. Instead of being manipulated, they manipulate. We assume that users have purposes they mean the technology to serve, and—this is a point of method—that users can understand and tell us about those ends and means.*

This rational, individualistic model is, by itself, inadequate. Social and cultural conditions largely determine people's ends, be those ends the desire to be entertained, or to see family, or to appear *au courant*. Moreover, social and cultural conditions limit people's choices. People choose within obvious constraints, such as the income they have and the costs they face. They also choose within the constraints of their information, their skills, formal and informal rules, and the like. So, for example, teenagers who do not understand pregnancy cannot

*This discussion is akin to von Hippel's on "users as innovators." In the arena of producer goods he documents how often users develop innovations for a technology, modifications that are later commercialized by the original manufacturers (von Hippel, *Sources of Innovation*).

reasonably choose a birth control device, older people unfamiliar with electronics will shy away from computers, and men exposed to cultural images that depict cooking as feminine may be unable to master oven controls. People also choose within the constraints imposed by the distribution system of the technology. If telephone services are not provided in their community, people cannot use them. Alternatively, distributors can force people to use a new technology by eliminating other options, as, for example, when banks make it hard to use human tellers and thus constrain customers to use automatic tellers.* The sensibility of users can thus operate only within narrow social and cultural limits.

From this perspective the consequences of a technology are, initially and most simply, the ends that users seek. People, however, have multiple, often contradictory, purposes, so that use of a technology may have nonobvious consequences. In particular, some technologies can alter the trade-offs among people's goals and yield paradoxical results or even no evident effects at all. For example, the nature of the urban housing market means that many Americans must trade proximity to their jobs for spacious homes farther away. Some urban scholars suggest that most Americans have used automobiles not to shorten their work trips but to move farther away from their jobs and thereby purchase larger but cheaper housing. Thus the automobile may have led, not to shorter commutes, but to more spacious housing. Similarly, some historians suggest that the mechanization of housework saved American homemakers considerable time, but most women used the time savings not to gain respite but to attain even greater cleanliness, and thus they ended up devoting the same amount of time to housework as they had before. As a final example, most Americans may have decided that the time they saved using modern transportation to keep in touch with their kin should be spent, not for more frequent contact with those relatives, but for the same frequency of contact at greater distances. More generally, people can put technologies to various ends—which may include keeping some activities just as they were. In these ways, some major technologies may have few direct and overt consequences.

So far, I have addressed intended consequences, but new technologies may also have second- and third-order consequences that are unintended. Individuals directly experience the unintended consequences of their own choices. For example, spending money on a new device means limiting other expenditures. Touring by automobile exposes travelers to new cultural influences.

*This point was suggested by Ilan Solomon.

More interesting and less controllable, individuals indirectly experience the unintended collective consequences of *others'* use. Over the years, shopping by automobile probably encouraged the dispersal of stores and so perhaps increased everyone's need to have an automobile. As more people use telephones to get services, service providers reorganize to deal with calls and perhaps thereby pressure nonsubscribers to get telephones. These examples illustrate one kind of collective byproduct of adopting a new technology: An optional device becomes necessary. Other collective consequences include what economists call "externalities," such as the increased demand for oil because of the automobile or the decline in slide-rule skills because of the calculator. These reverberations can be paradoxical. For example, congestion on streetcars may have encouraged Americans to switch to automobiles for commuting, which eventually led to yet another form of traffic congestion.[41]

These externalities illustrate that a technology can be both a *tool* for an individual user and, aggregated, become a *structure* that constrains the individual. Individuals may not choose to watch television, but they must still contend with television in popular culture, children's fantasy lives, politics, public schedules (at least one presidential inauguration has been worked around the Superbowl), and so on. At either level of analysis, individual or structural, the center of the process is the purposeful user employing, rejecting, or modifying technologies to his or her ends, but doing so within circumstances that may in some instances be so constraining as to leave little choice at all.

This "heuristic," or instructive tool for thinking about technology, may be closer to the instrumental model I described earlier than to the symptomatic model, but it emphasizes the users rather than the imperative properties of the technology,* stresses social ends and social contexts, and denies the determinism of the billiard-ball metaphor.

One implication of this perspective is that empirical, historical research is of critical importance. If we can neither deduce a technology's social role from its manifest properties nor easily extrapolate it from a cultural *Geist*, if it matters more what individual users choose

*It is therefore possible for people to "misuse" a technology, at least from the point of view of its providers, as we shall see in the case of the telephone.

to do with a device and how these choices aggregate, then we must look closely at the histories of specific technologies. (Oliver Wendell Holmes once wrote that on some points "a page of history is worth a volume of logic.") Of course, we always seek to simplify, to group together specific instances, or find a few underlying dimensions (for Kern, the key category is space-transcending implements; for Meyerowitz, electronics). And so we should. But until we have reason—or better, evidence—to the contrary, we should assume that each technology may be used differently and play a different social role, and that different people may use the same technology to different effect.

This, too, is a serious problem in the field: the shortage of reliable evidence, compared to the plenty of impression, anecdote, and abstracted inference. Borgmann again illustrates. He purposely eschews any empirical literature and concentrates instead on philosophical discourse (in part because he distrusts social science as a technological, alienating "device"). Instead of research, he says, we should rely on our "common intuitions." This is a mistake, since few intuitions are so common as to be indisputable and even common intuitions are often false (for example, the world is flat, "blood tells," and so on). Borgmann's essay, like most supposedly theoretical discourses, rests on many empirical assumptions, some plausible, some dubious, most unexamined.[42] But even less polemical writers rely often on impression in place of hard evidence. For example, many a scholar has repeated the claim that the railroad companies developed standard time zones to rationalize their work. Recent research shows, however, that scientists were the ones who pushed the standardization; the railroads were not terribly interested.[43]

We need to study how specific devices were introduced and adopted, what people used them for, how that use changed as the technology evolved, how those uses altered other actions, how patterns of use changed the context for other actors, and so on. (Again, social constructivists have explored some of these concerns in concrete case studies.) To address questions about twentieth-century modernity, such studies ought to examine the key technologies of the transition, such as the automobile, assembly line, radio, and refrigerator. Historians have documented the development of many of those technologies, but have rarely described their social roles (the research on housework being an exception). Once we understand how the technologies emerged, we need to ask a few key questions: First, why and how did

individuals use the technology? Second, how did using it alter other, less immediate aspects of their lives? Third, how did the collective use of a technology and the collective responses to it alter social structure and culture?

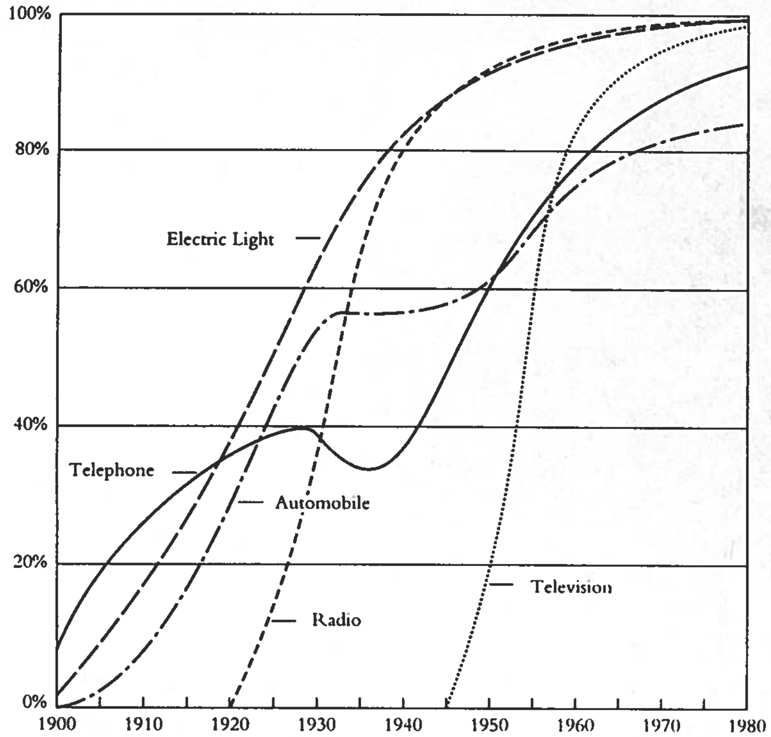
In this book I try to follow this "use heuristic" as far as the evidence will allow. Although the next two chapters take the common social constructivist path, examining how producers of the telephone developed and marketed their service, later chapters turn more toward Cowan's "consumption junction." They focus on users as individuals and as communities. Since the users were, as years passed, increasingly a mass population, the way of studying them becomes more sociological and statistical rather than historical and biographical. (More on method in a later section.) But the intent is always to discern how the average user reacted to and employed the technology.

WHY THE TELEPHONE?

Concerns about modernity, technology, and community motivated this study. I wanted to understand an aspect of the coming of modern society by examining the technological changes that were integral to it. According to the argument I have just laid out, this requires diligent empirical study of how people adopted and used a specific technology. Many technologies could and ought to be studied. Producers used some new technologies to alter goods production and delivery—new machines, materials, communication systems, control processes, and the like—with profound consequences for work and the economy.[44] I chose to focus, however, on a technology that people used daily in private life, a technology that may have affected social relations, community, and culture.

That still leaves a wide range of technologies. Figure 1 shows how a few key consumer products spread in the twentieth century. There are many other possibilities as well. Recent women's history scholars, for example, have studied technologies used for food preparation and cleaning. I further narrowed my choice to point-to-point, space-transcending technologies, such as the railroad, automobile, telephone, and streetcar.

The ability to travel and speak across space changed fundamentally between 1850 and 1950: from horsepower for the few to railroads, streetcars, bicycles, and automobiles for the masses; from military semaphore to business telegraphs and then telephones for the masses.



Note: Smoothed lines.

FIGURE 1. U.S. HOUSEHOLDS WITH SELECTED CONSUMER GOODS, 1900–1980. This figure shows how several domestic technologies spread among Americans in the twentieth century. Slowed in part by the Depression, the telephone and automobile did not diffuse as rapidly as the three electronic devices. (Source: U.S. Bureau of the Census, *Historical Statistics* and *Statistical Abstract 1990.*)

These new technologies undergirded other material changes, such as increasing production and the rise of national markets. To the classical sociologists—most explicitly, to Emile Durkheim[45]—the multiplication and extension of interpersonal contacts were crucial to the development of modern society. More interaction generated economic and social specialization, brought cultures together and accentuated their discords, and shifted the bases of social solidarity from bloodline and place to occupation and taste. If we understand this change

in social interaction, classical theory suggests, we understand much of modern society. Finally, to understand changes in private life and personal relations, it is appropriate to examine the means by which people conducted those relations.

Of the several space-transcending technologies, I selected the telephone, for two major reasons. First, the telephone captures most cleanly the magnification of social contact, without the complications of freight hauling or commuting involved in, say, the automobile or railroad. In 1875 Americans who wanted to send a message had to travel or use an intermediary who traveled; the messages were brief and one-way; the range and volume of communication were severely limited. (Use of the telegraph was highly restricted to business and rare emergencies.) In 1925 most Americans could speak to one another across town or across country quickly, back and forth, and fully. The possibilities of personal communication expanded vastly. How did people adopt and adapt to such a drastically new condition of social life?

The second reason is that among the space-transcending technologies of this era the telephone has been studied least. (Since I began this research in the early 1980s, some serious work has appeared. See the bibliographic essay in Appendix A.) In truth, none of these technologies has been studied *sociologically* in any depth. Compared to the shelves of research on, for example, television and its consequences, even the automobile is a mystery. Moreover, except for a few business historians, scholars have all but ignored the telephone. Why? Perhaps the moment of the telephone's notoriety preceded the era of social research. Or perhaps few social problems seem tied to the telephone. Or as one literary analyst has suggested, perhaps the telephone belongs to the class of "anonymous objects . . . so imbedded in daily routine as to have become undifferentiated from the rest of our immediate landscape."[46]

I want, then, to understand the introduction of the telephone, the uses to which people put it, and its evolving social role in daily life. To understand these developments, one must do more than reason forward from the properties of the telephone; one must study the historical process itself. One must do more than catalog the commentaries of contemporary observers; one must look at the conduct of daily life itself. One must do more than study telephone use today; one must examine change over time. The telephone began as a novelty, became business's substitute for the telegraph, and then evolved into a mass product, an everyday device for handling chores and having conversations. The role of the telephone unfolded over time. To what effect?

Such assessments can best be done by establishing some benchmarks for comparison. I chose to compare the social history of the telephone to that of the automobile. Although as an object and a system the automobile differs greatly from the telephone—gas and electric services are more like the telephone in form—from the user's point of view they are comparable. The automobile provides some of the same space-transcending functions of the telephone, albeit more slowly. Where possible, therefore, I contrast the diffusion and social uses of the telephone to those of the automobile.

THE TELEPHONE'S SOCIAL ROLE: SOME SPECULATIONS

Despite the paucity of research, there have been some speculations about the social implications of the telephone. Ithiel de Sola Pool, one of the few researchers in this field, compiled a long list of forecasts made before 1940 about the telephone's role. Commentators predicted a range of consequences, from the disappearance of regional dialects to the elimination of written records for historians.[47]

Two topics illustrate the range of the weightier claims. One: Some have argued that use of the telephone altered the physical layout of American cities. Because telephone conversations erase the "friction of space"—the time and cost of crossing distances—they also reduce the importance of central location. Businesses and people can therefore more easily move to the urban periphery.[48] Two: Some serious commentators, as well as many industry representatives, have described the telephone as a force for democracy, because it permits citizens to communicate, to collaborate, and even to conspire uncontrolled by a central authority.[49] As intriguing as these and many other speculations are, we have very little, if any, solid evidence on their plausibility, much less their factuality.

This study looks more closely at a few other sets of speculations. One is the broad concern over whether the telephone has expanded or diminished personal relations. The industry itself said that telephone calls enriched social ties, offering "gaiety, solace, and security," even making of America "a nation of neighbors." Less interested parties, as well, described the telephone as a device that worked on behalf of social attachments.[50] The most common claims were that the telephone allowed rural people to overcome isolation, perhaps even saving many farm wives from insanity (see Chapter 4). Others, however, charge that the telephone provides but an echo of true human communication. "It brought people into close contact but obliged them to 'live at wider distances' and created a palpable emptiness across which voices seemed uniquely disembodied and remote," writes Stephen Kern. It is, in such views, an impersonal instrument whose use spreads impersonality.[51]

A second and widespread conviction is that telephone use weakens local ties in favor of extralocal contacts and national interests. Some make this claim approvingly, stating that the telephone is "an antidote to provincialism." Increased communication promises to advance contact among cultures, to help bring "the brotherhood of man." But for others the telephone is yet another of modernity's blows against local *Gemeinschaft*, the close community. We get larger "electronic neighborhoods . . . but shallower kinds of community." Ron Westrum has argued that devices such as the telephone "allow the destruction of community because they encourage far-flung operations and far-flung relationships." At an even deeper level the telephone contributes to placelessness, and without rootedness both community and identity are at risk.[52]

Few have argued against the delocalization claim, but Malcolm Willey and Stuart Rice did so in the most comprehensive study of the new communications' effects, a monograph published in 1933 for President Hoover's Commission on Social Trends. They argued that people use the telephone, like other point-to-point media, to augment local ties much more than extralocal ones and that calling strengthens localities against homogenizing cultural forces, such as movies and radio. "The telephone replaced the back fence and so was local in its influence," as another author put it.[53]

A third general concern has been for the subjective implications of telephone use. Many have ruminated on subtle psychological effects, for example, the possible creation of an alert, tense, "speedy" frame of mind. People are on edge, conscious that a call may occur at any instant, always impatient because the telephone has trained them to expect immediate results. Yet others describe the telephone as providing a calming sense of security.[54] Similarly, commentators have worried about privacy and "privatism." Carolyn Marvin wrote: "The telephone was the first electric medium to enter the home and unsettle customary ways of dividing the private person and family from the more public setting of the community." One common complaint in the nineteenth century was that the telephone permitted intrusion into the domestic circle by solicitors, purveyors of inferior music, eavesdropping operators, and even wire-transmitted germs. Among some communication theorists the telephone's intrinsic social psychological character wears away privacy: Messages come unbidden; background sounds reveal intimacies of the home to the caller; speakers cannot prepare for or reflect upon the discussion as they can in letters; callers' voices are disembodied from context; and so on.[55]

Others, however, blame telephone use, as well as television watching, suburban backyards, and the like, for creating "a general withdrawal into self-pursuit and privatism." One concern in the earliest days was that the telephone allowed people to conceal from community scrutiny inappropriate activities, such as illicit romances or liquor purchases. With the telephone and other devices people need public spaces less often and thus disengage from public life, burrowing into familial cocoons.[56]

These speculations revolve around what might be called the first-order consequences of telephone use: what its use means for the users. There are also second-order consequences: what widespread use of the telephone means for others and for the community. For example, at some point people with telephones began to assume that others would be instantly reachable. As Willey and Rice put it in 1933, "to be without a telephone or a telephone listing is to suffer a curious isolation in the telephonic age."[57]

There is little confirmation of the validity of these speculations, either in reports by contemporary observers or, much less, in systematic comparative evidence. The claims depend on an analysis of the inherent "logic" of the telephone, on impressions (not always unbiased), on anecdotes and second-hand tales. The dominance of opinion over evidence in this area is illustrated by a trivial example that came to intrigue me. Repeatedly, writers claimed that the telephone made construction of skyscrapers possible. The first instance of this claim seems to have been in 1902, and the latest I found was in 1989. Its greatest publicist was AT&T's chief engineer in the early 1900s, John J. Carty. A telephone was useful in managing construction high above the ground, he argued, but was even more important in solving the messenger problem:

> Take . . . any of the giant office buildings. How many messages do you suppose go in and out of those buildings every day[?] Suppose there was [*sic*] no telephone and every message had to be carried by a personal messenger. How much room do you think the necessary elevators would leave for offices? Such structures would be an economic impossibility.

This contention lacks both evidence and plausibility. The historical timing is off, and other means of sending messages—pneumatic tubes, for example—were available. Yet this claim has been repeated for over 80 years without serious examination.[58] If we know so little about such a simple, material issue, consider how little we really know about the role of the telephone in personal relations, families, and community life.

Claims about the automobile's role are numerous, and some of them—especially those dealing with the changing physical layout of North American cities—have been well researched. But many claims about the automobile's role in the social lives of its users are as contradictory and as undocumented as those about the telephone.

Many blame or credit the automobile for the decline of local attachments in favor of placeless ties, whether for better—"the unshackling of the age-old bonds of locality," according to Robert Heilbroner—or for worse. A few, conversely, claim that the automobile instead abetted a retreat from urban cosmopolitanism into suburban provincialism.[59] Many commentators, particularly in the 1920s, lamented that the automobile undermined the family by permitting its members to pursue their pleasures at movies, roadhouses, campsites, and lovers' lanes. More recently, others say that the automobile encouraged extreme familism, an encapsulated privatism.[60] For some observers the automobile has been a tool for women's liberation (and another antidote to farmwife insanity), but for others it helped shackle women to their domestic chores.[61] Unfortunately, for many of these speculations, and especially for the seamier ones, there is but one major source of historical evidence, itself sometimes debatable, a chapter in the Lynds' *Middletown*.[62]

This quick review of speculations about the telephone and automobile suggests at least two points: that these technologies may have affected basic features of American life and that we have few facts about these phenomena.

We will look closely at the telephone in the development of modern American life, making brief comparisons to the automobile. We will dwell most on personal relations, local community, and subjective reactions. We do *not* ask what the "impacts" or "effects" of the telephone were. That is the wrong language, a mechanical language that implies that human actions are impelled by external forces when they are really the outcomes of actors making purposeful choices under constraints. Instead, we ask who adopted the telephone, when, where, how, and why; for what ends; and to what uses. By these uses—and by the second-order constraints generated by common use of telephones—we can understand what role the telephone played in modernization.

We may discover negative answers to these questions. We may find that the role of the telephone or automobile in these spheres was negligible, that relations, local ties, families would have been little different without the devices. Historian Daniel Boorstin asserts that "the telephone was only a convenience, permitting Americans to do more casually and with less effort what they had already been doing before."[63] That would be a fascinating conclusion because it would imply that people can assimilate drastic alterations in material conditions—here, the capacity to talk instantly with almost anyone—and continue the same social patterns they had before. It would show a powerful tendency toward homeostasis. Indeed, most of the evidence we will review suggests that Americans assimilated the telephone easily, even becoming nonchalant about it by the 1920s. It also suggests that Americans used this device to pursue their ends, not "more casually," but more aggressively and fully.

The next section discusses the methods used to pursue these questions. In the section after that, the reader will find an outline of the book. Chapter 2 begins the study with a summary history of the telephone and automobile in North America.

A NOTE ON METHOD

This study spans history and sociology, two disciplines that have grown closer in the past generation. Many historians have realized that they do far more than simply narrate, that their stories convey causal explanations, even if only implicitly. Many sociologists have abandoned the naive model of a physical science, realizing instead that their discipline, like the other life sciences, describes and explains historical events. Thus the work of historians and sociologists has converged in the study of certain issues—for example, mobilization in great revolutions, the adaptation of immigrant groups—in ways that sometimes make it difficult to divine the authors' pedigrees.

Yet a gap remains. Sociologists and historians differ in intent, historians usually seeking to provide a fully realized account for an event and

sociologists usually seeking to extract general principles. Rhetorical styles vary. Sociologists usually persuade by weaving subtle and complex correlations into a simple, plausible, theoretical fabric. Historians more often rely on narrative structure, story lines featuring flesh-and-blood actors rather than bodiless attributes. "Historians want readers to remark that things became *really different* and for a coherent set of reasons—and to remember this in something like a story form" writes the social historian John Model. "Historians simplify reality for literary reasons, and then aim to overcome that simplification with concreteness (hence, quotations; hence, examples) and evocation."[64] Preferred causal explanations often differ, with historians more commonly stressing human agency, sociologists more likely attributing action to structural circumstances. Standards of evidence diverge. A first-hand account that historians might consider concrete and contextualized sociologists might dismiss as "anecdotal," that is, idiosyncratic and biased. A statistical pattern of covariation that sociologists might hail as revealing historians might dismiss as an abstracted conflation of diverse cases, without context, and lacking in any persuasive cause-and-effect narrative.

I am interested in a historical "moment" for its intrinsic significance and for its ability to reveal, in a general way, how people deal with changing material conditions. My sociological heritage, however, will be obvious. The reader will find more attention to the accurate generalization than to the telling anecdote, more effort to organize an argument than to establish a chronology, more persuasion by weight of data than by the logic of narrative. Nevertheless, I use a combination of typical historians' and sociologists' methods and hope that the outcome will inform both schools.

My general strategy was to combine several levels and modes of investigation to understand how Americans adopted and used the telephone—and the automobile—in the years up to World War II. These were the years during which the two technologies became staples of middle-class American life. In this period we could observe people coming to know, adopt, use, and adapt to the innovations.

The research includes a study of how the telephone industry marketed its product. How did the vendors, whose livelihoods were at stake, comprehend the public demand for the technology? They were not, as we shall see, always accurate in their perceptions. Nevertheless, their knowledge of the market, the advertising they designed, and the consumer responses they surveyed all provide indirect evidence of popular reaction. The next step, still closer to the user orientation, is an analysis of the patterns of diffusion: Who adopted the telephone, where, and when? By examining adoption patterns, we may, admittedly with some error, infer motivations and uses. Yet another strategy is to trace the integration of the technology into daily life: Where does the telephone appear in regular activities? How do people use it? What can we infer then about its social role?

These general approaches are translated into several concrete studies. The major ones are

1. A history of how the telephone industry marketed its product to North American households, with special attention to rural and working-class customers. This study draws largely on industry and government archives: publications, reports, internal correspondence, and the like.

2. Statistical analyses of state-level data on telephone and automobile adoption, assessing the factors that apparently encouraged or discouraged diffusion.

3. The largest and most complex segment, a triad of community studies, reported first in Chapter 5, on three towns in the San Francisco Bay Area—Antioch, Palo Alto, and San Rafael. The research included a few different components: (a) a social history of each town from 1890 to 1940, focusing on community social life; (b) an account of how the two technologies entered each town; (c) statistical analyses of telephone and automobile diffusion; and (d) statistical analyses of social change.

4. A statistical analysis of who adopted the telephone when. We drew samples of households from each of our three towns for five years, selected from the period of 1900 to 1936, and by linking telephone directory entries to census or city directory lists were able to find out what sorts of households were most or least likely to adopt the telephone in which year. We also used a national survey conducted during World War I and a census of Iowa farmers in 1924.

5. Oral histories with 35 elderly people living in the three towns (described more fully in Chapter 8). It would be valuable to have first-hand accounts written by typical Americans about their encounters with telephones in the early twentieth century. But, besides the problem long noted by social historians that few ordinary people leave memoirs and diaries, getting or using a telephone was not, as we shall

see, a remarkable event. Even our elderly interviewees had to be encouraged to think about it.

Because there are few first-hand accounts of everyday life generations ago, and because the claims of interested parties must be viewed with caution, we rely in many places on sociological data. These data, such as censuses and surveys, speak only indirectly about individual action, hide personalities, and require interpretation, yet they are more representative and systematic than the—yes—anecdotal evidence one must otherwise rely upon.

These are the major components of the research, augmented by other bits here and there. The research includes both conventional archival research and conventional econometric analyses. The specific methodologies are described in the appropriate chapters, in appendixes, or in related articles.*

A GUIDE TO THE BOOK

America Calling generally moves from the telephone industry to the user to the social role of the telephone, from the national to the local to the personal level.

Chapter 2 presents a brief, nontechnical history of the telephone in North America. Chapter 3 explores the various ways that the telephone industry, especially AT&T, marketed its service to households, exploring the manner in which the industry understood or misunderstood subscribers' use of the telephone. Chapter 4 tracks the diffusion of the telephone across the United States, assessing the factors that encouraged or retarded its spread. It also contrasts the telephone's

*A major concern for some historians may be the collaborative nature of the local histories. (I personally gathered almost all the material from industry archives.) The tradition in history is that the lone researcher fingers each scrap of parchment to judge its authenticity and to place it in context. The time and effort required plainly narrow the scope of any single historian's research. Although an important standard, this value must be traded off against other research values, such as the desirability of *comparing* cases, without which it is difficult to draw any general conclusions. (On the value of multiple, comparative studies, see, for example, Dykstra and Silag, "Doing Local History.") In this study I compromised by focusing on three communities and by assigning each of my research assistants to do the primary research on a single town. Such collaborative research seems atypical in history, but when it is supervised by a single scholar and parallel guidelines are followed, as in our research, this approach can be fruitful. Where the community stories coincide, we draw confidence in making generalizations; where they diverge, we are stimulated to seek out the sources of difference.

diffusion in rural as opposed to urban areas and in the working as opposed to the middle class. Chapter 5 further examines diffusion, but at the level of the local community and the household. It recounts the response to the telephone in Antioch, Palo Alto, and San Rafael and then uses census data to determine which households in those towns adopted the telephone in which years. In most of these studies we use the automobile as a comparative benchmark.

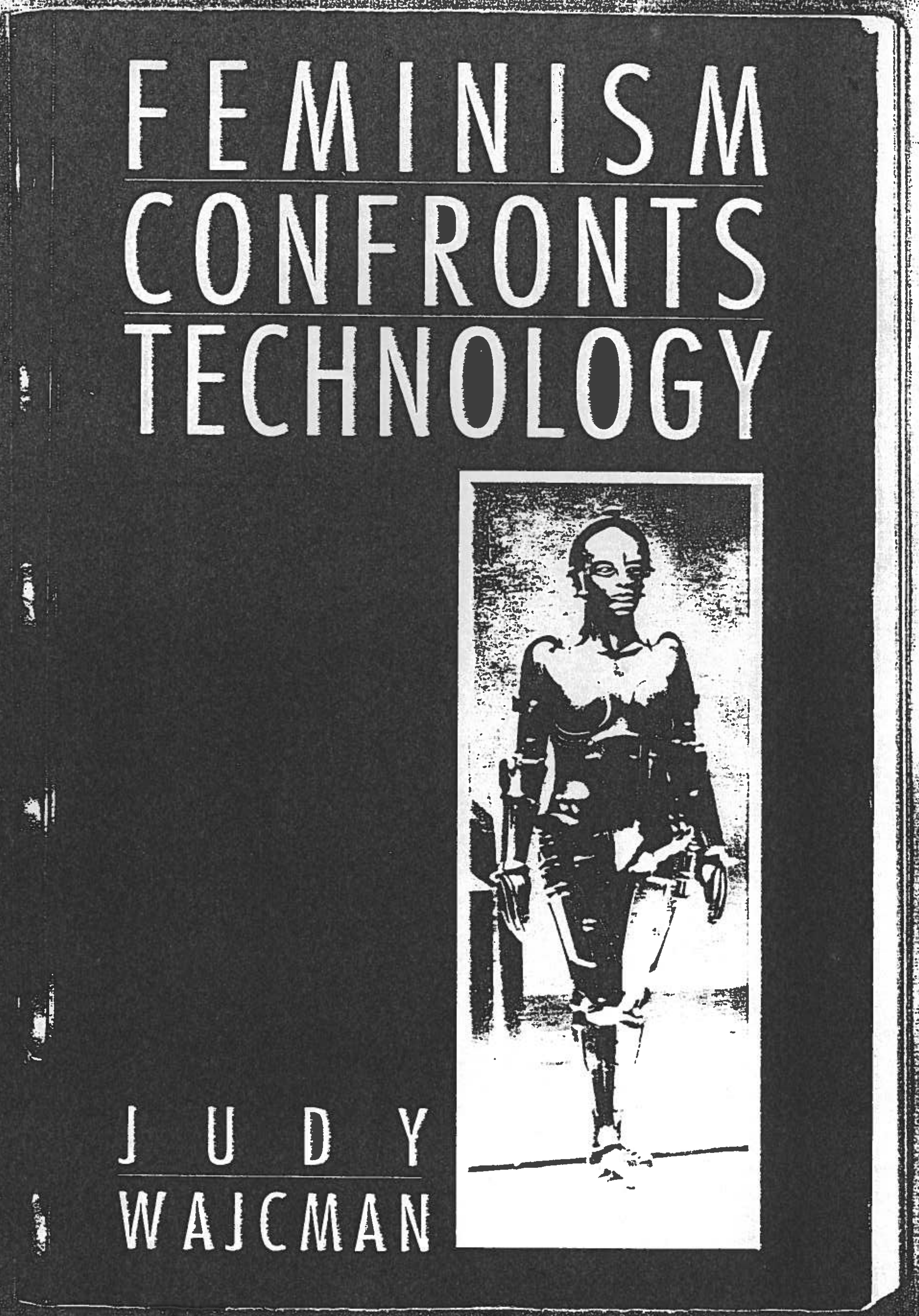
Chapter 6 employs a variety of evidence, from etiquette manuals to counts of advertisements, to chart how the telephone became an accepted part of everyday life. Chapter 7 looks at social change in our three towns, focusing on localism: Did residents become less involved in and less attached to their towns as the half-century passed? Chapter 8 looks more closely at individuals, asking how they reacted to the telephone and how they used it in their personal lives. In that context the chapter also analyzes the differences between men and women in regard to the telephone. Chapter 9 outlines telephone history from 1940 and summarizes the findings and implications of this study.

CHAPTER TWO

The Telephone in America

Alexander Graham Bell's fabled first words over the telephone, "Mr. Watson, come here, I want you," may not have been as dramatic as those dit-dotted by Samuel Morse during the first major exhibition of the telegraph, "What hath God wrought," but telephony's early years contained great drama nevertheless. Tinkerers and scientists raced to improve the primitive device; entrepreneurs struggled to rescue a failing company that would grow into a great industrial empire; its leaders battled attackers to secure their monopoly; gritty linemen risked their lives in blizzards to keep the wires humming; and telephone operators bravely stayed at their switchboards during fires and floods to make calls that barely averted tragedy. Such drama is the stuff of most telephone histories. Even skeptics must acknowledge the accomplishments of North America's telephone pioneers. They built an outstanding industry and public service.

Our purpose here, however, is to understand how the telephone system developed in America from 1876 to 1940. Consistent with the theoretical charge of the previous chapter, we take the perspective of residential consumers rather than engineers concerned with the machinery, corporate executives concerned with financial issues, or the



WAJCMAN

First published 1991 by Polity Press
in association with Blackwell Publishers

Reprinted 1993

Editorial Office:
Polity Press, 65 Bridge Street,
Cambridge CB2 1UR, UK

Marketing and production:
Blackwell Publishers
108 Cowley Road, Oxford OX4 1JF, UK

ISBN 0–7456–0777–2
ISBN 0–7456–0778–0 (pbk)

British Library Cataloguing in Publication Data

A CIP catalogue record for this book is available from the British Library.

Typeset in 10½ on 12 pt Times
by Colset Private Limited, Singapore
Printed in Great Britain by T. J. Press Ltd, Padstow, Cornwall.

Contents

Perhaps it is the new technologies of human biological reproduction that have been most vigorously contested, both intellectually and politically, by feminists in recent years. Chapter 3 explores the arguments, placing them in the wider context of the growing supremacy of technology in Western medicine.

There is now a substantial body of feminist writing on domestic technologies and their bearing on housework. Chapter 4 examines this research in conjunction with more mainstream (malestream) sociological theories regarding the impact of technologies on the 'post-industrial' home.

Chapter 5 deals with the built environment. The first section considers the design of houses and their urban location. I argue that sexual divisions are literally built into houses and indeed into the whole structure of the urban system. The last section scrutinises transport technology and demonstrates how women in particular have been disadvantaged by the design of cities around the automobile.

Picking up on issues from the previous four chapters, chapter 6 presents an analysis of technology as a masculine culture. I argue that the close affinity between technology and the dominant ideology of masculinity itself shapes the production and use of particular technologies. The correspondingly tenuous nature of women's relationship to this technical culture is the subject of the second part of the chapter.

In the conclusion, I hope to convince the reader that a recognition of the profoundly gendered character of technology need not lead to political pessimism or total rejection of existing technologies. The argument that women's relationship to technology is a contradictory one, combined with the realization that technology is itself a social construct, opens up fresh possibilities for feminist scholarship and action.

NOTES

1 For an introduction to this literature, see McNeil's (1987, pp. 227–9) bibliography on 'Development, The "Third World" and Technology'. See also Ahmed (1985).

2 Throughout this book I use the term 'sex' and 'gender' interchangeably. This is symptomatic of the blurred boundaries that mark the distinction between what is construed as 'natural' and what is construed as 'social'.

1
Feminist Critiques of Science and Technology

Writing in 1844 about relations between men and women, Marx said that '[i]t is possible to judge from this relationship the entire level of development of mankind' (1975, p. 347). More commonly it is the level of scientific and technological development that is taken as the index of a society's advancement. Our icons of progress are drawn from science, technology and medicine; we revere that which is defined as 'rational' as distinct from that which is judged 'emotional'. As we approach the twenty-first century however we are no longer sure whether science and technology are the solution to world problems, such as environmental degradation, unemployment and war, or the cause of them. It is not surprising therefore that the relationship between science and society is currently being subjected to profound and urgent questioning.

The development of a feminist perspective on the history and philosophy of science is a relatively recent endeavour. Although this field is still quite small and by no means coherent, it has attracted more theoretical debate than the related subject of gender and technology. It will become apparent in what follows, however, that feminists pursued similar lines of argument when they turned their attention from science to technology. I will therefore start by examining some approaches to the issue of gender and science, before moving on to look at technology.

The Sexual Politics of Science

The interest in gender and science arose out of the contemporary women's movement and a general concern for women's position in the professions. Practising feminist scientists have questioned the historical and sociological relationships between gender and science at least since the early 1970s. The publication of biographical studies of great women scientists served as a useful corrective to mainstream histories of science in demonstrating that women have in fact made important contributions to scientific endeavour. The biographies of Rosalind Franklin and Barbara McClintock, by Anne Sayre (1975) and Evelyn Fox Keller (1983) respectively, are probably the best known examples. Recovering the history of women's achievements has now become an integral part of feminist scholarship in a wide range of disciplines. However, as the extent and intransigent quality of women's exclusion from science became more apparent, the approach gradually shifted from looking at exceptional women to examining the general patterns of women's participation.

There is now considerable evidence of the ways in which women have achieved only limited access to scientific institutions, and of the current status of women within the scientific profession. Many studies have identified the structural barriers to women's participation, looking at sex discrimination in employment and the kind of socialization and education that girls receive which have channelled them away from studying mathematics and science. Explaining the under-representation of women in science education, laboratories and scientific publications, this research correctly criticises the construction and character of feminine identity and behaviour encouraged by our culture.

However these authors mainly pose the solution in terms of getting more women to enter science – seeing the issue as one of access to education and employment. Rather than questioning science itself, such studies assume that science is a noble profession and a worthy pursuit and that if girls were given the right opportunities and encouragement they would gladly become scientists in proportion to their numbers in the population. It follows that remedying the current deficiency is seen as a problem which a combination of different socialization processes and equal opportunity policies would overcome.

This approach, as Sandra Harding (1986) and others have pointed out, locates the problem in women (their socialization, their aspirations and values) and does not ask the broader questions of whether and in what way science and its institutions could be reshaped to accommodate women. The equal opportunity recommendations, moreover, ask women to exchange major aspects of their gender identity for a masculine version without prescribing a similar 'degendering' process for men. For example, the current career structure for a professional scientist dictates long unbroken periods of intensive study and research which simply do not allow for childcare and domestic responsibilities. In order to succeed women would have to model themselves on men who have traditionally avoided such commitments. The

equal opportunities strategy has had limited success precisely because it fails to challenge the division of labour by gender in the wider society. The cultural stereotype of science as inextricably linked with masculinity is also crucial in explaining the small number of women in science. If science is seen as an activity appropriate for men, then it is hardly surprising that girls usually do not want to develop the skills and behaviours considered necessary for success in science.

When feminists first turned their attention to science itself, the problem was conceived as one of the uses and abuses to which science has been put by men. Feminists have highlighted the way in which biology has been used to make a powerful case for biologically determined sex roles. Biology has been central to the promotion of a view of women's nature as different and inferior, making her naturally incapable of carrying out scientific work. For example, sex differences in visual-spatial skills are said to explain why there are so many more male scientists. In confronting biological determinists, many feminists inquired as to how and why the study of sex differences had become a priority of scientific investigation. They set out to demonstrate that biological inquiry, and indeed Western science as a whole, were consistently shaped by masculine biases. This bias is evident, they argued, not only in the definition of what counts as a scientific problem but also in the interpretations of research. It followed that science could not be genuinely objective until the masculine bias was eliminated. As we shall see below, this approach leaves unchallenged the existing methodological norms of scientific inquiry and identifies only bad science and not science-as-usual as the problem.

The radical political movements of the late 1960s and early 1970s also began with the question of the use and abuse of science. In their campaigns against an abused, militarized, and polluting science they argued that science was directed towards profit and warfare. Initially science itself was seen as neutral or value-free and useful as long as it was in the hands of those working for a just society. Gradually, however, the radical science movement developed a Marxist analysis of the class character of science and its links with capitalist methods of production. A revived political economy of science began to argue that the growth and nature of modern science was related to the needs of capitalist society. Increasingly tied to the state and industry, science had become directed towards domination. The ideology of science as neutral was seen as having a specific historical development. One of the most characteristic formulations of this position, associated with the radical science movement, was that 'science is social relations'.

The point was that the distinction between science and ideology could not be sustained because the dominant social relations of society at large are constitutive of science.

During this same period a radical shift took place in the history, philosophy and sociology of science, which added weight to the view that science could no longer be understood simply as the discovery of reality. Thomas Kuhn's *The Structure of Scientific Revolutions* (1970) marked the beginning of what was to become a major new field of study known as the sociology of scientific knowledge.[1] Its central premise is that scientific knowledge, like all other forms of knowledge, is affected at the most profound level by the society in which it is conducted.

Much research has examined the circumstances in which scientists actually produce scientific knowledge and has demonstrated how social interests shape this knowledge. Studies provide many instances of scientific theories drawing models and images from the wider society. It has also been demonstrated that social and political considerations enter into scientists' evaluations of the truth or falsity of different theories. Even what is considered as 'fact', established by experiment and observation, is social. Different groups of scientists in different circumstances have produced radically different 'facts'. Numerous historical and contemporary studies of science, and the social processes through which inquiry proceeds, highlight the social aspects of scientific knowledge.

Despite the advances that were made through the critique of science in the 1970s, gender-conscious accounts were rare. The social studies of natural science systematically avoided examining the relationship between gender and science in either its historical or sociological dimensions. Similarly, the radical science movement focused almost exclusively on the capitalist nature of science ignoring the relationship of science to patriarchy. In short, gender did not figure as an analytical tool in either of these accounts of science.

It is only during the last decade with writers such as Carolyn Merchant (1980), Elizabeth Fee (1981), Evelyn Fox Keller (1985), Brian Easlea (1981), Nancy Hartsock (1983), Hilary Rose (1983) and Ludmilla Jordanova (1980) that Western science has been labelled as inherently patriarchal.[2] As Sandra Harding (1986) expresses it, feminist criticisms of science had evolved from asking the 'woman question' in science to asking the more radical 'science question' in feminism. Rather than asking how women can be more equitably treated within and by science, they ask 'how a science apparently so deeply involved in distinctively masculine projects can possibly be used for emancipatory ends' (p. 29). It is therefore time to consider the main feminist critiques of science itself.

Scientific Knowledge as Patriarchal Knowledge

The concern with a gender analysis of scientific knowledge can be traced back to the women's health movement that developed in Britain and America during the 1970s. Regaining knowledge and control over women's bodies – their sexuality and fertility – was seen as crucial to women's liberation. Campaigns for improved birth control and abortion rights were central to the early period of second-wave feminism. There was a growing disenchantment with male medical theories and practices. The growth and consolidation of male expertise at the expense of both women's health and women's healing skills was the theme of an American study, *Witches, Midwives and Nurses: A History of Women Healers* (Ehrenreich and English, 1976). This documented how the growth and professionalization of male-dominated medicine had led to the marginalization of female health workers. At the same time, critiques of psychiatry and the treatment of women's depression as pathological were being expounded. Asking why the incidence of mental illness should be higher among women than men, feminists exposed the sexist bias in medical definitions of mental health and illness. Implicit in these analyses was a conviction that women could develop new kinds of knowledge and skills, drawing on their own experience and needs. The insights of the radical science movement contributed to the view of medical science as a repository of patriarchal values.

If medical scientific knowledge is patriarchal, then what about the rest of science? As Maureen McNeil (1987) points out, it was a short step to the emergence of a new feminist politics about scientific knowledge in general. Some feminists re-examined the Scientific Revolution of the sixteenth and seventeenth centuries, arguing that the science which emerged was fundamentally based on the masculine projects of reason and objectivity. They characterized the conceptual dichotomizing central to scientific thought and to Western philosophy in general, as distinctly masculine. Culture vs. nature, mind vs. body, reason vs. emotion, objectivity vs. subjectivity, the public realm vs. the private realm – in each dichotomy the former must dominate the latter and the latter in each case seems to be systematically associated with the feminine. The general issue of whether conceptual dichotomizing is itself distinctly masculine or part of the Western philosophical tradition is beyond the scope of this book.[3] My concern is with the way dualistic gender metaphors such as those used above reveal the underlying social meanings in purportedly value-neutral scientific thought.

There has been a growing awareness of the use of female metaphors for nature and natural metaphors for women. An examination of the texts of science highlights the correspondence between the way men treated women in particular historical periods and the way they used nature. Some feminist historians have focused on the rape and torture metaphors in the writings of Sir Francis Bacon and the other fathers of modern science. Merchant (1980) argues that during the fifteenth to seventeenth centuries in Europe both nature and scientific inquiry were conceptualized in ways modelled on men's most violent and misogynous relationships to women and this modelling has contributed to the distinctive gender symbolism of the subsequent scientific world view.

Eighteenth and nineteenth century biomedical science in France and Britain deployed similar gender symbolism to conceptualize nature: '. . . science and medicine as activities were associated with sexual metaphors which were clearly expressed in designating nature as a woman to be unveiled, unclothed and penetrated by masculine science' (Jordanova, 1980, p. 45). Anatomically, males were depicted as representing active agents and females as passive objects of male agency. From her study Jordanova concludes that biomedical science intensified the cultural association of nature with passive, objectified femininity and of culture with active, objectifying masculinity. This strikingly gendered imagery of nature and of scientific inquiry is not just an historical relic, as these same dichotomies and metaphors can be found in contemporary writing on science. As Harding asks, is it any wonder that women are not an enthusiastic audience for these interpretations?

Rather than pointing to the negative consequences of women's identification with the natural realm, some feminists celebrate the identification of woman and nature. This finds political expression in the eco-feminism of the eighties which suggests that women must and will liberate the earth because they are more in tune with nature. For them, women's involvement in the ecology and peace movements was evidence of this special bond. As Susan Griffin expressed it: 'those of us who are born female are often less severely alienated from nature than are most men' (1983, p. 1). Women's biological capacity for motherhood was seen as connected to an innate selflessness born of their responsibility for ensuring the continuity of life. Nurturing and

caring instincts are essential to the fulfilment of this responsibility. Conversely, men's inability to give birth has made them disrespectful of human and natural life, resulting in wars and ecological disasters. From this perspective, a new feminist science would embrace feminine intuition and subjectivity and end the ruthless exploitation of natural resources. Rejecting patriarchal science, this vision celebrates female values as virtues and endorses the close relationship between women's bodies, women's culture and the natural order.

While eco-feminism sees women's values as having a biological basis, another approach to the question of women and science has been informed by psychoanalysis. The object-relations school of thought has been particularly influential in the feminist conceptualizations of science. This theory describes the mechanisms through which adult women and men come to model themselves and their relation to the world in different ways. To acquire his masculine identity the boy must both reject and deny his former dependencies, attachment and identification with the mother. The resulting conflicts in men over masculinity create a psychology of male dominance.

Using this theory Keller argues that girls and boys have different cognitive skills. As the male distinguishes himself from the mother, he also learns to differentiate sharply between subject and object, between himself and others. According to Keller, as scientists are men this male mind set, obsessed with detachment and mastery, has been written into the norms and methods of modern science. A radically different scientific method is described by Keller (1983) in her influential biography of Barbara McClintock. A Nobel prize-winning geneticist, McClintock is described as a scientist who merged subject and object in her 'feeling for the organism' and whose work was imbued with a holistic understanding of, and reverence for, nature. According to Keller, this woman's work provides us with 'a glimpse of what a gender-free science might look like' by combining masculine and feminine characteristics. Rather than celebrating a woman-centred science as do the eco-feminists, this project insists on the possibility of a gender-neutral science produced by androgynous individuals.[4]

While emphatically rejecting the possibility of a neutral objective science, other feminist writers have shared a concern with the exclusion of woman-centred values from science. However, they attribute such values not to the individual psyche but to a socially and historically constructed gender division of labour. They trace the way in which, as the spheres of public and private life became increasingly separated during the course of the eighteenth century, women became confined to the private sphere of hearth and home. Skills such as reasoning and objectivity became associated with public life, and feeling and subjectivity with private life. These dichotomies have become historically associated with the development of distinctive feminine and masculine worldviews.

In a well-known article, Rose (1983) locates herself within the radical science tradition and endorses the Marxist characterization of bourgeois science as a form of alienated and abstract knowledge. It is the division of mental and manual labour, integral to capitalist production, which gives rise to this form of knowledge. Rose takes issue with this tradition however for its failure to question the impact of the gender division of labour on science. The focus of the radical science critique on the relations of production to the exclusion of reproduction negates women's experience, which in turn impoverishes science. Science has been denied the input of women's experience of the caring, emotionally demanding labour which has been assigned exclusively to women. According to Rose, a feminist science would need to encompass this emotional domain and thereby fuse subjective and objective ways of knowing the world. It would thus be a more complete, truer knowledge because it is based on women's 'shared experience of oppression'. Rose concludes that the reunification of 'hand, brain *and* heart' would foster a new form of science, enabling humanity to live in harmony with nature.

A Science Based on Women's Values?

These debates about science mirror the more general preoccupations that have engaged feminists over the last two decades. Much early second-wave feminism was of a liberal cast, demanding access for women within existing power structures, such as science. In principle, equality could be achieved by breaking down gender stereotypes: for instance by giving girls better training and more varied role models, and by introducing equal opportunity programmes and anti-discrimination legislation. Such feminist writing focused on gender stereotypes and customary expectations, and denied the existence of any fundamental sex differences between women and men. This first approach, liberal feminism, was based on an empiricist view of science as (gender) neutral. Sexism and androcentrism were therefore conceived of as social biases correctable by stricter adherence to the existing methodological norms of scientific inquiry. I would argue that the limitations of this approach have been made apparent by the

sociology of scientific knowledge and the profound critique of empiricism that has occurred in the last few decades.

By the late 1970s however a new form of radical feminism, or cultural feminism as it is known in North America, had emerged which exalted femininity for its own sake. These writers emphasize gender difference and celebrate what they see as specifically feminine, such as women's greater humanism, pacifism, nurturance and spiritual development. Some of these authors abandoned the idea that what was 'specifically feminine' was socially produced and notions of ineradicable difference have flourished.

This return to an emphasis on natural or psychological gender difference is a common thread in many of the feminist views of science. They promote women's values as an essential aspect of human experience and seek a new vision of science that would incorporate these values. At this juncture therefore, I think it appropriate to point to some fundamental problems with the general assertion of a science based on women's values.

Essentialism, or the assertion of fixed, unified and opposed female and male natures has been subjected to a variety of thorough critiques.[5] The first thing that must be said is that the values being ascribed to women originate in the historical subordination of women. The belief in the unchanging nature of women, and their association with procreation, nurturance, warmth and creativity, lies at the very heart of traditional and oppressive conceptions of womanhood. 'Women value nurturance, warmth and security, or at least we believe we ought to, precisely because of, not in spite of, the meanings, culture and social relations of a world where men are more powerful than women' (Segal, 1987, p. 34). It is important to see how women came to value nurturance and how nurturance, associated with motherhood, came to be culturally defined as feminine within male-dominated culture. Rather than asserting some inner essence of womanhood as an ahistorical category, we need to recognize the ways in which both 'masculinity' and 'femininity' are socially constructed and are in fact constantly under reconstruction.

Secondly, the idea of 'nature' is itself culturally constructed. Conceptions of the 'natural' have changed radically throughout human history. As anthropologists like Marilyn Strathern and others have pointed out, 'no single meaning can in fact be given to nature or culture in Western thought; there is no consistent dichotomy, only a matrix of contrasts' (Strathern, 1980, p. 177). These feminist anthropologists have questioned the claim that in all societies masculinity is associated with culture and femininity with nature. Moreover, they argue that there is no behaviour or meaning which is universally and cross-culturally associated with either masculinity or femininity. What is considered masculine in some societies is considered feminine or gender-neutral in others and vice versa. Indeed, they suggest that even where the nature/culture dichotomy exists, we must not assume that the Western terms 'nature' and 'culture' are adequate or reasonable translations of the categories other cultures perceive. The historical research by Merchant and Jordanova referred to above also points to the historical specificity of these gender metaphors. As Harding says: 'the effect of these studies is to challenge the universality of the particular dichotomized set of social behaviors and meanings associated with masculinity and femininity in Western culture' (Harding, 1986, p. 129).

If we look at other cultures such as those of African and Aboriginal peoples, we find concepts of nature quite different from dominant European ones. Their world views posit a more harmonious relationship between mankind and the living universe of nature which strikingly parallels what is claimed to be a distinctively feminine world view. And what the African and Aboriginal world views designate as European is similar to what feminists designate as masculine. Even within the traditions of Western philosophy there are schools of thought which claim these values for themselves. Karl Mannheim (1953) describes romantic-conservatism as an anti-atomistic style of thinking which advocates holism, organic unity, and the qualitative rather than the quantitative as the preferred style of thought. Once more it is difficult to claim that a holistic approach in harmony with nature is specific to gender.

These arguments cast serious doubt on the projects for a feminist science presented above. Once it is recognized that 'masculinity' and 'femininity', as well as the idea of 'nature', are changing cultural categories then it no longer makes sense to base a science on feminine intuition rooted in nature. Authors like Keller, Rose and Hartsock also call for a science which incorporates women's values, although they expressly dissociate themselves from this radical feminist essentialism. Harding groups these authors under the label of the 'feminist standpoint epistemology'. This proposal argues that 'men's dominating position in social life results in partial and perverse understandings, whereas women's subjugated position provides the possibility of more complete and less perverse understandings' (Harding, 1986, p. 26). These feminist critiques of science ground a distinctive feminist science in the universal features of women's experience. Nevertheless, they all hover near the edge of biologism. Like the radical feminists,

they endorse versions of a science based on subjectivity, intuition, holism and harmony. While Rose and Hartsock in particular base their materialist analyses on the gender division of labour, they fail to take fully into account that 'nature' is not a fixed category and that the division of labour is not unchanging. Therefore women's subjectivity, caring, holism and harmony, to which they appeal, cannot be universal aspects of women's experience. Their identification between women's caring labour and the new values to be incorporated into science cannot be construed as fixed or in any way as arising 'naturally'.

One attempt to overcome the limitations of the 'standpoint approach' is the critique of a feminist science from the point of view of feminist postmodernism or deconstructionism. Harding has correctly warned that the feminine qualities celebrated by feminists do not accurately reflect the social experience of all women as their experience is divided by class, race and culture. If a new feminist science is to be created from the standpoint of women's experience, should there be a feminist science based on the experience of 'Black women, Asian women, Native American women, working-class women, lesbian women?' Taking her cue from feminist postmodernism, Harding argues that the problem with feminist standpoint epistemologies is that they assume that there is a single privileged position from which science can be evaluated. There is no 'woman' to whose social experience the feminist empiricist and standpoint approaches can appeal; there are instead the 'fractured identities of women'. This approach is useful in that it takes account of the differences between and within individuals, and highlights the tension between a unitary and a fragmented conceptualization of the voice of feminism.

However the fact that there are class, race and cultural differences between women and between men does not mean that gender difference is 'either theoretically unimportant or politically irrelevant' (Harding, 1986, p. 18). In virtually every culture, gender difference is fundamental to social organization and personal identity. Qualities associated with manliness are almost everywhere more highly regarded than those thought of as womanly. Women have in common the fact that they have been marginalized from every powerful institution of our society, especially from scientific institutions. This acknowledgement of the universality of women's subordination is not incompatible with a recognition of the specific and variable forms of this subordination. Different groups of women have different needs and interests.

I share McNeil's (1987) view that rationality and intuition must themselves be seen as historically specific social products and that we should engage in social practices to redefine them. Her essay expresses well the spurious dilemma facing those feminists who feel forced to choose between scientific rationality or feminine intuition.[6] Furthermore, it is important to stress that the basis of men's power is not simply a product of the ideas we hold and the language we use, but of all the social practices that give men authority over women. Ideas are mediations of social relations and to transform them we need to transform the fundamental character of scientific institutions in contemporary society and the forms of political power that science bestows on specific social groups.

It may be that the search for the most appropriate feminist epistemology, however philosophically sophisticated (as Harding indeed is), is misdirected. The more philosophically oriented feminist work on science suffers from the problem of dealing with ideas divorced from social practices. Indeed, as amply shown by these authors, statements of 'The Scientific Method' do typically contain male visions of what it is to know and what the world is really like. Scientific practice is in no sense determined by statements of method. The latter are better seen as political pronouncements, as legitimations, rather than as descriptions of what scientists actually do. They serve to say something about the place of science in the wider society, or to bolster a more scientific speciality or discipline against its competitors (Richards and Schuster, 1989).[7]

It is in this light that we should see attempts to spell out a specifically feminist scientific method. They are politically useful in that they turn the feminist spotlight on the content of scientific knowledge instead of simply highlighting questions of recruitment to science. We need to be cautious in presuming that the adoption of a 'feminist' scientific method would lead to differences in scientific practice without a thoroughgoing change in the relations of power within science. The danger is that what might parade as feminist science would simply amount to the same scientific practice by another name.

From Science to Technology

While there has been a growing interest in the relationship of science to society over the last decade, there has been an even greater preoccupation with the relationship between technology and social change. Debate has raged over whether the 'white heat of technology' is radically transforming society and delivering us into a post-industrial

age. A major concern of feminists has been the impact of new technology on women's lives, particularly on women's work. The introduction of word processors into the office provided the focus for much early research. The recognition that housework was also work, albeit unpaid, led to studies on how the increasing use of domestic technology in the home affected the time spent on housework. The exploitation of Third World women as a source of cheap labour for the manufacture of computer components has also been scrutinized. Most recently there has been a vigorous debate over developments in reproductive technology and the implications for women's control over their fertility.

Throughout these debates there has been a tension between the view that technology would liberate women – from unwanted pregnancy, from housework and from routine paid work – and the obverse view that most new technologies are destructive and oppressive to women. For example, in the early seventies, Shulamith Firestone (1970) elaborated the view that developments in birth technology held the key to women's liberation through removing from them the burden of biological motherhood. Nowadays there is much more concern with the negative implications of the new technologies, ironically most clearly reflected in the highly charged debate over the new reproductive technologies.

A key issue here is whether the problem lies in men's domination of technology, or whether the technology is in some sense inherently patriarchal. If women were in control, would they apply technology to more benign ends? In the following discussion on gender and technology, I will explore these and related questions.

An initial difficulty in considering the feminist commentary on technology arises from its failure to distinguish between science and technology. Feminist writing on science has often construed science purely as a form of knowledge, and this assumption has been carried over into much of the feminist writing on technology. However just as science includes practices and institutions, as well as knowledge, so too does technology. Indeed, it is even more clearly the case with technology because technology is primarily about the creation of artefacts. This points to the need for a different theoretical approach to the analysis of the gender relations of technology, from that being developed around science.

Perhaps this conflation of technology with science is not surprising given that the sociology of scientific knowledge over the last ten years has contested the idea of a non-controversial distinction between science and technology. John Staudenmaier (1985, pp. 83–120) comments that although the relationship between science and technology has been a major theme in science and technology studies, the discussion has been plagued by a welter of conflicting definitions of the two basic terms. The only consensus to have emerged is that the way in which the boundaries between science and technology are demarcated, and how they are related to each other, change from one historical period to another.

In recent years, however, there has been a major re-orientation of thinking about the form of the relationship between science and technology. The model of the science–technology relationship which enjoyed widespread acceptance over a long period was the traditional hierarchical model which treats technology as applied science. This view that science discovers and technology applies this knowledge in a routine uncreative way is now in steep decline. 'One thing which practically any modern study of technological innovation suffices to show is that far from applying, and hence depending upon, the culture of natural science, technologists possess their own distinct cultural resources, which provide the principal basis for their innovative activity' (Barnes and Edge, 1982, p. 149). Technologists build on, modify and extend existing technology but they do this by a creative and imaginative process. And part of the received culture technologists inherit in the course of solving their practical problems is non-verbal; nor can it be conveyed adequately by the written word. Instead it is the individual practitioner who transfers practical knowledge and competence to another. In short, the current model of the science–technology relationship characterizes science and technology as distinguishable sub-cultures in an interactive symmetrical relationship.

Leaving aside the relationship between technology and science, it is most important to recognize that the word 'technology' has at least three different layers of meaning. Firstly, 'technology' is a form of knowledge, as Staudenmaier emphasizes.[8] Technological 'things' are meaningless without the 'know-how' to use them, repair them, design them and make them. That know-how often cannot be captured in words. It is visual, even tactile, rather than simply verbal or mathematical. But it can also be systematized and taught, as in the various disciplines of engineering.

Few authors however would be content with this definition of technology as a form of knowledge. 'Technology' also refers to what people do as well as what they know. An object such as a car or a vacuum cleaner is a technology, rather than an arbitrary lump of matter, because it forms part of a set of human activities. A computer without programs and programmers is simply a useless collection of bits of metal, plastic and silicon. 'Steelmaking', say, is a technology: but this implies that the technology includes what steelworkers do, as well as the furnaces they use. So 'technology' refers to human activities and practices. And finally, at the most basic level, there is the 'hardware' definition of technology, in which it refers to sets of physical objects, for example, cars, lathes, vacuum cleaners and computers.

In practice the technologies dealt with in this book cover all three aspects, and often it is not useful to separate them further. My purpose is not to attempt to refine a definition. These different layers of meaning of 'technology' are worth bearing in mind in what follows.

The rest of this chapter will review the theoretical literature on gender and technology, which in many cases mirrors the debates about science outlined above. However, feminist perspectives on technology are more recent and much less theoretically developed than those which have been articulated in relation to science. One clear indication of this is the preponderance of edited collections which have been published in this area.[9] As with many such collections, the articles do not share a consistent approach or cover the field in a comprehensive fashion. Therefore I will be drawing out strands of argument from this literature rather than presenting the material as coherent positions in a debate.

Hidden from History

To start with, feminists have pointed out the dearth of material on women and technology, especially given the burgeoning scholarship in the field of technology studies. Even the most perceptive and humanistic works on the relationship between technology, culture and society rarely mention gender. Women's contributions have by and large been left out of technological history. Contributions to *Technology and Culture*, the leading journal of the history of technology, provide one accurate barometer of this. Joan Rothschild's (1983, pp. xii–xiv) survey of the journal for articles on the subject of women found only four in twenty-four years of publishing. In a more recent book about the journal, Staudenmaier (ibid., p. 180) also notes the extraordinary bias in the journal towards male figures and the striking absence of a women's perspective. The history of technology represents the prototype inventor as male. So, as in the history of science, an initial task of feminists has been to uncover and recover the women hidden from history who have contributed to technological developments.

There is now evidence that during the industrial era, women invented or contributed to the invention of such crucial machines as the cotton gin, the sewing machine, the small electric motor, the McCormick reaper, and the Jacquard loom (Stanley, forthcoming). This sort of historical scholarship often relies heavily on patent records to recover women's forgotten inventions. It has been noted that many women's inventions have been credited to their husbands because they actually appear in patent records in their husbands' name. This is explained in terms of women's limited property rights, as well as the general ridicule afforded women inventors at that time (Pursell, 1981; Amram, 1984; Griffiths, 1985). Interestingly, it may be that even the recovery of women inventors from patent records seriously underestimates their contribution to technological development. In a recent article on the role of patents, Christine MacLeod (1987) observes that prior to 1700 patents were not primarily about the recording of the actual inventor, but were instead sought in the name of financial backers.[10] Given this, it is even less surprising that so few women's names are to be found in patent records.

For all but a few exceptional women, creativity alone was not sufficient. In order to participate in the inventive activity of the Industrial Revolution, capital as well as ideas were necessary. It was only in 1882 that the Married Women's Property Act gave English women legal possession and control of any personal property independently of their husbands. Dot Griffiths (1985) argues that the effect of this was to virtually exclude women from participation in the world of the inventor-entrepreneur. At the same time women were being denied access to education and specifically to the theoretical grounding in mathematics and mechanics upon which so many of the inventions and innovations of the period were based. As business activities expanded and were moved out of the home, middle-class women were increasingly left to a life of enforced leisure. Soon the appropriate education for girls became 'accomplishments' such as embroidery and music – accomplishments hardly conducive to participation in the world of the inventor-entrepreneur. In the current period, there has been considerable interest in the possible contributions which Ada Lady Lovelace, Grace Hopper and other women may have made to the development of computing. Recent histories of computer programming provide substantial evidence for the view that women played a major part.[11]

To fully comprehend women's contributions to technological development, however, a more radical approach may be necessary. For a start, the traditional conception of technology too readily defines

technology in terms of male activities. As I have pointed out above, the concept of technology is itself subject to historical change, and different epochs and cultures had different names for what we now think of as technology. A greater emphasis on women's activities immediately suggests that females, and in particular black women, were among the first technologists. After all, women were the main gatherers, processors and storers of plant food from earliest human times onward. It was therefore logical that they should be the ones to have invented the tools and methods involved in this work such as the digging stick, the carrying sling, the reaping knife and sickle, pestles and pounders. In this vein, Autumn Stanley (forthcoming) illustrates women's early achievements in horticulture and agriculture, such as the hoe, the scratch plow, grafting, hand pollination, and early irrigation.

If it were not for the male bias in most technology research, the significance of these inventions would be acknowledged. As Ruth Schwartz Cowan notes:

> The indices to the standard histories of technology . . . do not contain a single reference, for example, to such a significant cultural artifact as the baby bottle. Here is a simple implement . . . which has transformed a fundamental human experience for vast numbers of infants and mothers, and been one of the more controversial exports of Western technology to underdeveloped countries – yet it finds no place in our histories of technology.(1979, p. 52)

There is important work to be done not only in identifying women inventors, but also in discovering the origins and paths of development of 'women's sphere' technologies that seem often to have been considered beneath notice.

A Technology Based on Women's Values?

During the eighties, feminists have begun to focus on the gendered character of technology itself. Rather than asking how women could be more equitably treated within and by a neutral technology, many feminists now argue that Western technology itself embodies patriarchal values. This parallels the way in which the feminist critique of science evolved from asking the 'woman question' in science to asking the more radical 'science question' in feminism. Technology, like science, is seen as deeply implicated in the masculine project of the domination and control of women and nature.[12] Just as many feminists have argued for a science based on women's values, so too has there been a call for a technology based on women's values. In Joan Rothschild's (1983) preface to a collection on feminist perspectives on technology, she says that: 'Feminist analysis has sought to show how the subjective, intuitive, and irrational can and do play a key role in our science and technology'. Interestingly, she cites an important male figure in the field, Lewis Mumford, to support her case. Mumford's linking of subjective impulses, life-generating forces and a female principle is consistent with such a feminist analysis, as is his endorsement of a more holistic view of culture and technological developments.

Other male authors have also advocated a technology based on women's values. Mike Cooley is a well-known critic of the current design of technological systems and he has done much to popularize the idea of human-centred technologies. In *Architect or Bee?* (1980, p. 43) he argues that technological change has 'male values' built into it: 'the values of the White Male Warrior, admired for his strength and speed in eliminating the weak, conquering competitors and ruling over vast armies of men who obey his every instruction . . . Technological change is starved of the so-called female values such as intuition, subjectivity, tenacity and compassion'. Cooley sees it as imperative that more women become involved in science and technology to challenge and counteract the built-in male values: that we cease placing the objective above the subjective, the rational above the tacit, and the digital above analogical representation. In *The Culture of Technology*, Arnold Pacey (1983) devotes an entire chapter to 'Women and Wider Values'. He outlines three contrasting sets of values involved in the practice of technology – firstly, those stressing virtuosity, secondly, economic values and thirdly, user or need-oriented values. Women exemplify this third 'responsible' orientation, according to Pacey, as they work with nature in contrast to the male interest in construction and the conquest of nature.

Ironically the approach of these male authors is in some respects rather similar to the eco-feminism that became popular amongst feminists in the eighties. This marriage of ecology and feminism rests on the 'female principle', the notion that women are closer to nature than men and that the technologies men have created are based on the domination of nature in the same way that they seek to dominate women. Eco-feminists concentrated on military technology and the ecological effects of other modern technologies. According to them, these technologies are products of a patriarchal culture that 'speaks violence at every level' (Rothschild, 1983, p. 126). An early slogan of the feminist anti-militarist movement, 'Take the Toys from the Boys', drew attention to the phallic symbolism in the shape of missiles. However, an inevitable corollary of this stance seemed to be the representation of women as inherently nurturing and pacifist. The problems with this position have been outlined above in relation to science based on women's essential values. We need to ask how women became associated with these values. The answer involves examining the way in which the traditional division of labour between women and men has generally restricted women to a narrow range of experience concerned primarily with the private world of the home and family.

Nevertheless, the strength of these arguments is that they go beyond the usual conception of the problem as being women's exclusion from the processes of innovation and from the acquisition of technical skills. Feminists have pointed to all sorts of barriers – in social attitudes, girls' education and the employment policies of firms – to account for the imbalance in the number of women in engineering. But rarely has the problem been identified as the way engineering has been conceived and taught. In particular, the failure of liberal and equal opportunity policies has led authors such as Cynthia Cockburn (1985) to ask whether women actively resist entering technology. Why have the women's training initiatives designed to break men's monopoly of the building trades, engineering and information technology not been more successful? Although schemes to channel women into technical trades have been small-scale, it is hard to escape the conclusion that women's response has been tentative and perhaps ambivalent.

I share Cockburn's view that this reluctance 'to enter' is to do with the sex-stereotyped definition of technology as an activity appropriate for men. As with science, the very language of technology, its symbolism, is masculine. It is not simply a question of acquiring skills, because these skills are embedded in a culture of masculinity that is largely coterminous with the culture of technology. Both at school and in the workplace this culture is incompatible with femininity. Therefore, to enter this world, to learn its language, women have first to forsake their femininity.

Technology and the Division of Labour

I will now turn to a more historical and sociological approach to the analysis of gender and technology. This approach has built on some theoretical foundations provided by contributors to the labour process debate of the 1970s. Just as the radical science movement had sought to expose the class character of science, these writers attempted to extend the class analysis to technology. In doing so, they were countering the theory of 'technological determinism' that remains so widespread.

According to this account, changes in technology are the most important cause of social change. Technologies themselves are neutral and impinge on society from the outside; the scientists and technicians who produce new technologies are seen to be independent of their social location and above sectional interests. Labour process analysts were especially critical of a technicist version of Marxism in which the development of technology and productivity is seen as the motor force of history. This interpretation represented technology itself as beyond class struggle.

With the publication of Harry Braverman's *Labor and Monopoly Capital* (1974), there was a revival of interest in Marx's contribution to the study of technology, particularly in relation to work. Braverman restored Marx's critique of technology and the division of labour to the centre of his analysis of the process of capitalist development. The basic argument of the labour process literature which developed was that capitalist–worker relations are a major factor affecting the technology of production within capitalism. Historical case studies of the evolution and introduction of particular technologies documented the way in which they were deliberately designed to deskill and eliminate human labour.[13] Rather than technical inventions developing inexorably, machinery was used by the owners and managers of capital as an important weapon in the battle for control over production. So, like science, technology was understood to be the result of capitalist social relations.

This analysis provided a timely challenge to the notion of technological determinism and, in its focus on the capitalist division of labour, it paved the way for the development of a more sophisticated analysis of gender relations and technology. However, the labour process approach was gender-blind because it interpreted the social relations of technology in exclusively class terms. Yet, as has been well established by the socialist feminist current in this debate, the relations of production are constructed as much out of gender divisions as class divisions. Recent writings (Cockburn, 1983, 1985; Faulkner and Arnold, 1985; McNeil, 1987) in this historical vein see women's exclusion from technology as a consequence of the gender division of labour and the male domination of skilled trades that developed under capitalism. In fact, some argue that prior to the industrial revolution women had more opportunities to acquire technical skills, and that capitalist technology has become more masculine than previous technologies.

I have already described how, in the early phases of industrialization, women were denied access to ownership of capital and access to education. Shifting the focus, these authors show that the rigid pattern of gender divisions which developed within the working-class in the context of the new industries laid the foundation for the male dominance of technology. It was during this period that manufacturing moved into factories, and home became separated from paid work. The advent of powered machinery fundamentally challenged traditional craft skills because tools were literally taken out of the hands of workers and combined into machines. But as it had been men who on the whole had technical skills in the period before the industrial revolution, they were in a unique position to maintain a monopoly over the new skills created by the introduction of machines.

Male craft workers could not prevent employers from drawing women into the new spheres of production. So instead they organized to retain certain rights over technology by actively resisting the entry of women to their trades. Women who became industrial labourers found themselves working in what were considered to be unskilled jobs for the lowest pay. 'It is the most damning indictment of skilled working-class men and their unions that they excluded women from membership and prevented them gaining competences that could have secured them a decent living' (Cockburn, 1985, p. 39). This gender division of labour within the factory meant that the machinery was designed by men with men in mind, either by the capitalist inventor or by skilled craftsmen. Industrial technology from its origins thus reflects male power as well as capitalist domination.

The masculine culture of technology is fundamental to the way in which the gender division of labour is still being reproduced today. By securing control of key technologies, men are denying women the practical experience upon which inventiveness depends. I noted earlier the degree to which technical knowledge involves tacit, intuitive knowledge and 'learning by doing'. New technology typically emerges not from sudden flashes of inspiration but from existing technology, by a process of gradual modification to, and new combinations of, that existing technology. Innovation is to some extent an imaginative process, but that imagination lies largely in seeing ways in which existing devices can be improved, and in extending the scope of techniques successful in one area into new areas. Therefore giving women access to formal technical knowledge alone does not provide the resources necessary for invention. Experience of existing technology is a precondition for the invention of new technology.

The nature of women's inventions, like that of men's, is a function of time, place and resources. Segregated at work and primarily confined to the private sphere of the household, women's experience has been severely restricted and therefore so too has their inventiveness. An interesting illustration of this point lies in the fact that women who were employed in the munitions factories during the First World War are on record as having redesigned the weaponry they were making.[14] Thus, given the opportunity, women have demonstrated their inventive capacity in what now seems the most unlikely of contexts.

Missing: The Gender Dimension in the Sociology of Technology

The historical approach is an advance over essentialist positions which seek to base a new technology on women's innate values. Women's profound alienation from technology is accounted for in terms of the historical and cultural construction of technology as masculine. I believe that women's exclusion from, and rejection of, technology is made more explicable by an analysis of technology as a culture that expresses and consolidates relations amongst men. If technical competence is an integral part of masculine gender identity, why should women be expected to aspire to it?

Such an account of technology and gender relations, however, is still at a general level.[15] There are few cases where feminists have really got inside the 'black box' of technology to do detailed empirical research, as some of the most recent sociological literature has attempted. Over the last few years, a new sociology of technology has emerged which is studying the invention, development, stabilization and diffusion of specific artefacts.[16] It is evident from this research that technology is not simply the product of rational technical imperatives. Rather, political choices are embedded in the very design and selection of technology.

Technologies result from a series of specific decisions made by particular groups of people in particular places at particular times for their own purposes. As such, technologies bear the imprint of the people and social context in which they developed. David Noble (1984, p. xiii) expresses this point succinctly as follows: 'Because of its very concreteness, people tend to confront technology as an

irreducible brute fact, a given, a first cause, rather than as hardened history, frozen fragments of human and social endeavor'. Technological change is a process subject to struggles for control by different groups. As such, the outcomes depend primarily on the distribution of power and resources within society.

There is now an extensive literature on the history of technology and the economics of technological innovation. Labour historians and sociologists have investigated the relationship between social change and the shaping of production processes in great detail and have also been concerned with the influence of technological form upon social relations. The sociological approach has moved away from studying the individual inventor and from the notion that technological innovation is a result of some inner technical logic. Rather, it attempts to show the effects of social relations on technology that range from fostering or inhibiting particular technologies, through influencing the choice between competing paths of technical development, to affecting the precise design characteristics of particular artefacts. Technological innovation now requires major investment and has become a collective, institutionalized process. The evolution of a technology is thus the function of a complex set of technical, social, economic, and political factors. An artefact may be looked on as the 'congealed outcome of a set of negotiations, compromises, conflicts, controversies and deals that were put together between opponents in rooms filled with smoke, lathes or computer terminals' (Law, 1987, p. 406).

Because social groups have different interests and resources, the development process brings out conflicts between different views of the technical requirements of the device. Accordingly, the stability and form of artefacts depends on the capacity and resources that the salient social groups can mobilize in the course of the development process. Thus in the technology of production, economic and social class interests often lie behind the development and adoption of devices. In the case of military technology, the operation of bureaucratic and organizational interests of state decision-making will be identifiable. Growing attention is now being given to the extent to which the state sponsorship of military technology shapes civilian technology.

So far, however, little attention has been paid to the way in which technological objects may be shaped by the operation of gender interests. This blindness to gender issues is also indicative of a general problem with the methodology adopted by the new sociology of technology. Using a conventional notion of technology, these writers study the social groups which actively seek to influence the form and direction of technological design. What they overlook is the fact that the absence of influence from certain groups may also be significant. For them, women's absence from observable conflict does not indicate that gender interests are being mobilized. For a social theory of gender, however, the almost complete exclusion of women from the technological community points to the need to take account of the underlying structure of gender relations. Preferences for different technologies are shaped by a set of social arrangements that reflect men's power in the wider society. The process of technological development is socially structured and culturally patterned by various social interests that lie outside the immediate context of technological innovation.

More than ever before technological change impinges on every aspect of our public and private lives, from the artificially cultivated food that we eat to the increasingly sophisticated forms of communication we use. Yet, in common with the labour process debate, the sociology of technology has concentrated almost exclusively on the relations of paid production, focusing in particular on the early stages of product development. In doing so they have ignored the spheres of reproduction, consumption and the unpaid production that takes place in the home. By contrast, feminist analysis points us beyond the factory gates to see that technology is just as centrally involved in these spheres.

Inevitably perhaps, feminist work in this area has so far raised as many questions as it has answered. Is technology valued because it is associated with masculinity or is masculinity valued because of the association with technology? How do we avoid the tautology that 'technology is masculine because men do it'? Why is women's work undervalued? Is there such a thing as women's knowledge? Is it different from 'feminine intuition'? Can technology be reconstructed around women's interests? These are the questions that abstract analysis has so far failed to answer. The character of salient interests and social groups will differ depending on the particular empirical sites of technology being considered. Thus we need to look in more concrete and historical detail at how, in specific areas of work and personal life, gender relations influence the technological enterprise. This book focuses on gender, although it is often difficult to disentangle the effects of gender from those of class and race. The chapters that follow are organized around substantive areas of technology – the technology of production, reproductive technology, domestic technology and the built environment.

Throughout the book I will be stressing that a gendered approach to technology cannot be reduced to a view which treats technology as a set of neutral artefacts manipulated by men in their own interests. While it is the case that men dominate the scientific and technical institutions, it is perfectly plausible that there will come a time when women are more fully represented in these institutions without transforming the direction of technological development. To cite just one instance, women are increasingly being recruited into the American space-defence programme but we do not hear their voices protesting about its preoccupations. Nevertheless, gender relations are an integral constituent of the social organization of these institutions and their projects. It is impossible to divorce the gender relations which are expressed in, and shape technologies from, the wider social structures that create and maintain them. In developing a theory of the gendered character of technology, we are inevitably in danger of either adopting an essentialist position that sees technology as inherently patriarchal, or losing sight of the structure of gender relations through an overemphasis on the historical variability of the categories of 'women' and 'technology'. In what follows I will try to chart another course.

NOTES

1 For an introduction to this literature, see Barnes and Edge (1982) and Knorr-Cetina and Mulkay (1983).
2 In order to map the field of gender and science, I have drawn heavily on two excellent and comprehensive surveys by Harding (1986) and Schiebinger (1987).
3 This issue is discussed in Harding (1986). For a fuller account of the debate about whether Reason itself is male, see Lloyd (1984).
4 For an excellent discussion of Keller's work, see Dugdale (1988).
5 For two useful socialist feminist critiques of universalist and essentialist elements in some versions of radical feminist theory, see Eisenstein (1984) and Segal (1987).
6 For an account of the way the binary couple 'empiricism-inductivism'/ 'intuitive-speculative theory building' has been played upon since the seventeenth century, see Schuster and Yeo (1986).
7 For a clever comparison of the biographies of McClintock and Franklin and their respective scientific methodologies, see Richards and Schuster (1989).
8 Staudenmaier (1985, pp. 103–20) outlines four characteristics of technological knowledge–scientific concepts, problematic data, engineering theory, and technological skill.

CHAPTER 3

Some Thoughts on the Early History of CERN

Dominique Pestre and John Krige

The history of the first decade and a half of the life of CERN, the European Organization for Nuclear Research, has now been written.[1] For some six years we have immersed ourselves in the social, political, and institutional, as well as the scientific and technical, aspects of the organization's birth and development. Now, in this chapter, we step back a little and focus attention on two major themes that have emerged from our work. We have chosen them for their methodological interest, and because they help to bring out how the situation at CERN, an intergovernmental laboratory built from scratch,[2] differed from that in "comparable" American high-energy physics laboratories.

1 The results have been published in Hermann et al., *History of CERN*, vols. 1 and 2. Our chapters in these books are based on papers we have found in the CERN archives and some national archives and are extensively documented. In the interests of efficiency we shall thus not refer to primary source material in what follows, preferring simply to indicate the chapters in these two volumes where further details may be found.

2 There is an extensive and growing literature on the development of big science facilities in the United States. Besides other chapters in this book, see Leslie, "Playing the Education Game," for Stanford; Heilbron, Seidel, and Wheaton, *Lawrence and His Laboratory*, and Seidel, "Accelerating Science," for Berkeley; Hoddeson, "KEK and Fermilab," and Westfall, "The First 'Truly National Laboratory'" for Fermilab. For studies of similar developments in Japan, see Hoddeson, "KEK and Fermilab," and Traweek, Chapter 4, this volume.

Pestre Krige

Pestre Krige

Physicists, Politicians, and State Bureaucracies in Europe and at CERN

The How and the Why of the Birth of CERN

To develop the points we want to make on this issue, we need first to give a thumbnail sketch of the main events and personalities involved in the launching of CERN.[3]

Toward the end of 1949 several persons associated with nuclear matters in Europe began to think seriously about the possibilities for multinational cooperation in this area. The most important of the first initiatives was that taken by Raoul Dautry, Administrator-General of the French Commissariat à l'Energie Atomique (CEA). At a European Cultural Conference in Switzerland in December 1949, he had a resolution passed recommending that studies be undertaken for the creation of a European institute for nuclear science "directed toward applications in everyday life." Six months later Isidor I. Rabi, inspired in part by the launch of the Brookhaven National Laboratory, put a resolution to the annual conference of UNESCO in Florence, which he attended as a member of the American delegation. Rabi invited the states who so wished to create one or more regional European laboratories, including one in nuclear science. The resolution was adopted by UNESCO's General Assembly on June 7, 1950.

Two small groups took up these proposals in the following months. One comprised a handful of specialists in classical nuclear physics (people such as Lew Kowarski in France and Peter Preisswerk in Switzerland) and in cosmic rays (most notably Edoardo Amaldi in Italy and Pierre Auger in France). The other group was composed of three important administrators of science—Raoul Dautry, Gustavo Colonnetti (president of the Italian Consiglio Nazionale delle Ricerche), and Jean Willems (director of the Belgian Fonds National de la Recherche Scientifique). In December 1950 a first gathering of scientists and administrators organized by Auger—also the director of UNESCO's Department of Exact and Natural Sciences—and Dautry proposed that the biggest accelerator in the world (i.e., about 6 billion electron volts, so just bigger than the Bevatron) be constructed. A reactor was ruled out for political reasons, notably the problems posed by military and industrial applications.

In May, October, and November of the following year (1951), Auger, along with a number of scientific consultants, further refined the project advocated in Geneva. In December 1951 their recommendations were submit-

3 This section is based on Hermann et al., *History of CERN*, vol. 1, chapters 2–8 and 14. See also Pestre and Krige, "La naissance du CERN."

ted to a European intergovernmental conference officially called by UNESCO but in fact orchestrated by Auger himself. After lengthy discussions that reflected serious differences of opinion among the scientists attending, the conference proposed that a temporary organization be established. It was endowed with $200,000 and given eighteen months to present potential member-states with worked-out technical, organizational, and financial plans. The formal agreement embodying these proposals was signed on February 15, 1952, by all nations represented, except the United Kingdom. Early in May, with the $200,000 guaranteed, and five signatures ratified, the agreement entered into force.

The provisional CERN Council held its first meeting on May 5, 1952. The technical groups to design the accelerators and plan the laboratory were set up. In October Geneva was adopted as the site for the laboratory, and construction of a 25- to 30-billion-electron-volt proton synchrotron embodying the new alternating gradient principle recently announced at Brookhaven was decided on. This meant that a research and development effort—with its associated risks—was needed, and that the machine would take some five or six years to build. In January 1953 the British government was represented officially in the Council for the first time, and the discussion of the text of the convention establishing the permanent organization began in earnest. On July 1, 1953, this convention was signed by eight of the eleven member-states of the provisional CERN and by the United Kingdom. It entered officially into force fifteen months later, and on October 7, 1954, the "permanent" CERN Council met for the first time.

Now that we have some idea of the circumstances surrounding the birth of CERN, we want to discuss critically one of the more conventional ways in which its creation has been explained. Against the tendency to limit the account to a static analysis in terms of sociopolitical forces (the European movement, the military, etc.), we would stress that, if one really wants to understand what happened, it is crucial to follow *also* the events as they unfolded, to recompose the exact historical process leading to the CERN we know.[4] Of course it is important, in historical work, not to restrict oneself to the narrative dimension, to the ways in which individual actors relate concretely to one another—but neither must one ignore this dimension and focus only on more global aspects and large-scale explanations. A balance between the two must be found, a balance that is not the same always and everywhere and depends very much on the subject under study. Our conclusion is that, in the *particular* case of CERN's creation, the former dimension was the more

4 The importance of studying the minutiae of the process of decision making was brought home to us by Allison, *Essence of Decision*, and other works by the same school, and reinforced by Rudwick, *The Great Devonian Controversy*. For a more extensive bibliography, see Pestre, "Les décisions de très gros équipements."

decisive, that an analysis in terms of *process* provides the key to understanding. The basic reason for this is that no historical "necessity" imbued the birth of CERN, that this laboratory "might not have been" or might have emerged with a very different shape from the one it has.

As a working hypothesis the assumption of a degree of inevitability may be defensible if one were to write the history of the atomic energy establishments created in the scientifically advanced European countries in 1945 and 1946, bodies like France's CEA or Britain's Harwell. As Gilpin, Salomon, and others have emphasized, with the explosion of the bomb, science, and nuclear science in particular, moved from the periphery to the center of the political process. The governments of major powers had little choice but to develop their own atomic energy programs if they wished to retain their influence. In the case of CERN, however, there was less compulsion, and the situation was far more fluid, indeterminate, and subject to the day-to-day course of events. Here it is more valuable to accept that there were coincidental elements in the creation of the organization, that "chance" also played a role in the precise definition of what became CERN. Thus the obligation to lose nothing of the concrete process through which events gradually evolved.[5]

The opposite of what we believe should be done is illustrated by the way in which the "founders" of CERN, writing in the 1960's and 1970's, described the birth of the organization.[6] Seeking to explain the existence of CERN—or, more precisely, why it *could not but exist*—they identified two main historical forces. The first was that of the politicians then favoring collaborative European bodies like the European Economic Community; the second was that of the nuclear physicists who held that no single European state had either the financial or human resources needed to build the big laboratories that were the key to the future of physics. At the intersection of these two historical forces we find CERN, a *European* laboratory devoted to fundamental *particle physics*, a field sharing in the glamour of nuclear science but free of the nuclear "problem"—applications, particularly military.

The fascination of this kind of explanation is clear: above all, it seems to grasp immediately the essence of the matter. And though we are the first to admit that it is of some considerable value, we make two radical objections to it, all the same. First, it appeals to a statics of forces indifferent to the actual course of events and sees CERN simply as the "inevitable" resultant. Second, it is retrospective because it tends to consider the result (CERN in 1954) as

5 The references are to Gilpin, *American Scientists and Nuclear Weapons Policy*, and Salomon, *Science et politique*. The relative absence of "necessity" in the birth of the international organization that is CERN has been brought out in our conclusion to Hermann et al., *History of CERN*, vol. 1, chapter 14, while Krige, "The Installation of High-Energy Accelerators in Britain" illustrated the contrasting "inevitability" in the launch of a new national accelerator building project immediately after the war.

6 Typically, Kowarski, "New Forms of Organization."

having been the conscious goal of all from the start, as if the outcome were the simple, logical, and necessary response to an immutable and unambiguously posed question: how to equip *Europe* with a *prestigious* collaborative institute in *fundamental* nuclear physics. As it turns out, this is factually wrong and leads to unacceptable simplifications in the description of what actually happened. Let us merely say that the scientific community was neither united nor clearly "aware" of where its "best" interests lay. For Niels Bohr, James Chadwick, and Hendrik Kramers, for example, it was not obvious that the construction of the most powerful accelerator in the world was either necessary or desirable. The European spirit was neither as widespread nor as decisive as the story would lead us to believe—it counted for little if anything in Britain, for example. And many states hesitated about getting involved in a business whose long-term development was difficult to foresee and which they did not control.[7]

The problem with this kind of explanation, then, is that it "forgets" that it is dealing with a specific historical process and that, in the very particular case of CERN, the main actors enjoyed a large degree of autonomy with respect to the scientific establishment *and* to the state bureaucracies of the day. This was possible because at the end of the 1940's most European countries had neither a clearly formulated policy for science nor organs of state in charge of such questions. Individuals were thus left "free" to act as champions of "products" that they then managed to "sell" to key people in their government. Although each state's attitudes differed, particularly with the passage of time, they shared one characteristic: the states as such played a relatively passive role or, more precisely, were kept at arm's length from the process of CERN's creation, in a reactive position, and were not given any real chance to take the initiative. Power remained effectively in the hands of a group of people who were at once influential at home and free to act from personal conviction without having to wait for an official mandate. In a sense—and here we touch on a decisive conjuncture that forbids us to argue simply in terms of big forces explaining the (necessary) how of CERN—this facility was fortunate in being the *first* postwar European collaborative scientific venture. A decade later, when scientists connected with CERN tried to pull it off again, by setting up comparable bodies for space research, they encountered a stronger resistance by most of the states—and they found it far more difficult to control "their" project.

Was There Not an "Intimate Embrace" of Science and the Military Behind the Birth of CERN?

One of the major contemporary themes in American historiography of science is the importance of the role played by the military in the postwar

7 The opposition to "Auger's" project by leading members of the European scientific establishment has been studied in depth by Pestre in Hermann et al., *History of CERN*, vol. 1, chapters 5 and 6. The situation in Britain is described at length by Krige in Hermann et al., *History of CERN*, vol. 1, chapters 12 and 13.

development of fundamental research in the United States.[8] That granted, it is only natural to wonder whether a similar situation did not prevail on the other side of the Atlantic, whether, as Pickering has put it, "the wartime embrace of science and the military was not dissolved in peace" in Europe as in America. Put differently, and against the line of argument we have just developed, the American situation may lead one to suspect that there was, in fact, at least one major sociopolitical force that imbued the birth of CERN with "necessity," namely, the military.

Now there is no doubt that the military in Europe were *kept informed* of the launching of CERN, and were "aware" of the strategic importance of nuclear science. At the same time all the evidence suggests that rather than showing a strong interest in the laboratory, the "European" military—the military establishments in France, Britain, Italy, and so on—were relatively *indifferent* to it. If they were willing to let it be set up, it was because this laboratory was in no way one of their priorities. Perhaps it could serve as a training ground for a pool of unique expertise that could be useful elsewhere—as the scientists pushing the project reminded their governments—but this was not enough to convince the military to play an active part, a direct role, in the process leading to the creation of CERN.

At the most general level this attitude is not surprising, and seems to be consistent with our overall thesis. All the same, granted the importance of this question, we need to go a little further than this. Let us begin by asking why governments were apparently ready to finance the project laid before them by some of their advisors. The answers: foreign policy (to build Europe), to make up a gap in science and technology (the Continent vis-à-vis Great Britain and the United States), to help put a country back on the international map (this was the case for Germany)—*and* because CERN, an *international* organization, did not disturb the major European political–military equilibrium. Because CERN was to be *restricted* to doing fundamental research, no expert (even among the military) believed that CERN would meaningfully affect *national* interest (military interests included).

One objection that can be made to this line of reasoning is that the military and industrial interests of science, and of basic nuclear research in particular, were sometimes put forward to stimulate a positive attitude toward CERN. We know that some physicists (Werner Heisenberg, Francis Perrin), as well as certain high state officials (Gustavo Colonnetti in Italy, Sir Ben Lockspeiser in Britain), did this on some occasions.[9] Such arguments, however, never ap-

8 Some recent studies are those of Kevles, Chapter 12, this volume, Leslie, "Playing the Education Game," Pickering, "Pragmatism in Particle Physics," Sapolsky, "Military Support for Academic Research," Schweber, "Some Reflections on the History of Particle Physics," and "The Empiricist Temper Regnant," and the whole edition of *Historical Studies in the Physical and Biological Sciences* edited by Seidel. The quotation in the following sentence is from Pickering's review of Hermann et al., vol. 1.

9 This is discussed in a little more depth in Hermann et al., *History of CERN*, vol. 1, chapter 14, section 9. In this volume there is the reproduction of a letter from Gustavo

pealed to benefits that might flow directly from CERN, and were put forward at a time when it was more or less uncritically assumed that fundamental research "automatically" produced useful technology. In other words, these arguments seem always to have been advanced within the vague if classical political framework of "who knows what might come out of basic science." And because they were used infrequently and unsystematically, we believe that they were of secondary importance, just a tactic, and not a particularly central one at that, used to sell the project.

It might then be argued that we are naive, that we have been the unwitting victims of a kind of conspiracy of silence: if there is little reference to military importance in the correspondence or in the minutes of top-level meetings inside governments, it does not prove that the military were not extremely interested. It simply shows that they were prudent, or that the matter was so evident to all that it was left unsaid. We make three observations in reply to this. First, when the military did speak in interministerial meetings—as they sometimes did—they said quite explicitly that they were not opposed to the CERN project *because* they expected no spin-offs from its research. When they did hope for useful results, even if only in the long term, they asked for the projects to remain *national* (such as Harwell's high-intensity linear accelerator). Second, at no time and among none of the member-states did the military show any desire to "control" the laboratory even a little, leaving the departments of foreign affairs (in France, for example) or the departments responsible for basic civil research (such as the Department of Scientific and Industrial Research [DSIR] in Great Britain) to take charge of the matter. Finally, the military never considered paying a penny, even under the cover of another national institution. We add one more argument. Because CERN was to be *multinational,* involving countries as diverse as neutral Switzerland, the United Kingdom, and Yugoslavia (in the context of the Korean War!), its protagonists, as well as the governments, tried their best to "de-ideologize" the project, to disconnect it from everything that could be of military interest. In fact, it was precisely by "depoliticizing" CERN—so carefully avoiding any interactions with the military—that the Council could win the support of the member-states while retaining the freedom it wanted. After all, CERN would only do "pure" science for the benefit of everybody.

To conclude this point on the creation of CERN, we want to insist again on the originality of our case. Unlike America's Lawrence Radiation Laboratory or France's CEA, CERN was not a body that grew *organically* out of the

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Colonnetti to Alcide de Gasperi, the President of the Italian Council of Ministers, arguing that Italy should join CERN and stressing the importance of the need to "mobilize science and scientists for national defense." Colonnetti also argued that the money channeled to (all sectors of) science through the main civil research council, the Consiglio Nazionale delle Ricerche, should be considered "as an integral part of defense expenditure."

national soil, in an "intimate embrace" with national and military interests. On the contrary, it was an *unnatural, multinational* creation, endowed with a very special shape, the product of a unique gestation process, during which nothing necessitated that it come into being and in which the military played virtually no role at all. In brief, we are inclined to maintain the uniqueness of CERN with respect to equivalent national laboratories in the United States *and* in Europe.

The Council and the Member-States in the 1950's and 1960's

CERN is of interest to those who like to explore the enduring relationships between states and big science laboratories for two reasons. First, we are dealing not with a simple relation between one political and one scientific network but, because of CERN's multinationality, with the interactions between many such networks. Second, among the many examples of communal laboratories in Europe, CERN is almost always regarded as the most obvious success, the one that has found the right recipe, the right balance in its dealings with national governments.

What characterized the "CERN system" during this period was the existence of a central group, composed equally of scientists and "political" personalities, the CERN Council.[10] This body was at once extremely powerful and blessed with a large degree of autonomy from state authorities. Formally located between the national state bureaucracies, which paid for CERN and gave its members their "directives," and the CERN Director-General, whom it appointed but who was the real master of the laboratory, the Council knew how to make itself the central pivot of CERN's policy. Though legally comprising delegates appointed by the national governments, it appears in fact to have been a body not administratively constituted from above. At its core lay a group of virtually immovable men who rotated the powerful posts among themselves. Consisting essentially of personalities who played a leading role in CERN's birth in 1951 and 1952, this group enjoyed a kind of historical legitimacy that the states never challenged—except once in 1961 when the United Kingdom tried and failed. Welded together through a struggle that had lasted for years, determined to see their child prodigy succeed completely, they became known as the "founding fathers."

Aware of the balance of forces between countries and within each country, careful not to offend anyone, this group always tried to achieve unanimity in the Council, thereby aiming to give governments as little opportunity as possible to intervene directly, or to complain. However, they always carefully avoided having this search for consensus become a formal institutional procedure; the rules for making decisions in the Council never required unanim-

10 On this notion of CERN system, see Pestre in Hermann et al., *History of CERN,* vol. 2, chapter 7, especially section 1.3.

ity, even for the adoption of the budget. In this way the historical core managed to maintain a real feeling of unity and adventure in the body of the Council—and ensured that no one state alone could block the functioning of the organization (one or two "recalcitrant" governments could always be outvoted). In other words, the Council was not only the organ representing the states and responsible for *controlling* CERN, but also the body expected to *advise* the same states on matters concerning CERN and high-energy physics. And it saw its meetings as providing an opportunity for collective reflection and elaboration, particularly on how best to plead for the development of the organization before the national authorities.

Underpinned by this wish for cohesion, and by the desire to see CERN grow as best it might, this group thus kept on with the original adventure into which it had been launched at the very beginning of the 1950's, keeping the states at their distance, but for the "greater good" of each government. Because it operated in a field considered prestigious, high-energy physics, and exemplified stable collaboration and technical efficacy, it raised little opposition in the member-states. The support of the more determined governments dragged along the more hesitant ones, the situation varying according to the hazards of international politics, changing economic circumstances, and the evolution of domestic policies for science.

This very brief summary calls for one important refinement. United as it was around its shared roots and the determination to see "its" laboratory succeed, the Council always worked very closely with the European high-energy physics establishment. Around 1962 and 1963 this mixed group of diplomats and influential scientists conceived the project of integrating CERN more obviously into a European "pyramid" of institutions and laboratories whose development could be achieved only collectively, in planned harmony of one with the other. There was a tactical dimension to this wish to associate all European laboratories with CERN's work; it amounted to having everyone accept CERN's place at the apex of the European accelerator pyramid and it avoided making enemies of those who had paid for CERN. Given a place apart, outside any direct competition for money, CERN had a unique and specific task all the same: to be as good as the best American institutions. In 1963 this way of seeing things was ratified during the first meetings held by the European Committee for Future Accelerators (ECFA).[11]

Having considerable influence over their national authorities, and increasingly enjoying the support of the European high-energy physics establishment, the members of the CERN Council were able to ensure that the pursuit of national interests, insofar as it had any importance at all, generally tended to reinforce the development of CERN rather than to stifle it. This is not to say that one can look to CERN as a model for multinational collaboration. On the contrary, the specificity of the case, the circumstances surrounding its birth,

11 For more details on the role of ECFA, see Pestre in Hermann et al., *History of CERN*, vol. 2, chapter 12, notably sections 4 and 5.

and the unique nature of its research facility in Europe indicate that the species could not—and would not—be reproduced easily. The member-states that have lived with the "CERN system" for over three decades now would not allow it.

The Structure of the European High-Energy Physics Community and Its Effects on Research

Learning to Do "Big Physics"

In the early 1960's the Europeans had to learn to work together in this organization created out of nothing, this CERN without a past, without history, without tradition—but endowed with basic equipment more or less as good as the best across the Atlantic. The result was what they themselves saw as a difficulty to adapt to a scale of experimentation two or three orders of magnitude greater than what they were used to. What they remembered were the big discoveries they let slip through their fingers to the benefit of America's Brookhaven National Laboratory. It was there that the existence of two neutrinos was confirmed experimentally in 1962, where the Ω^- particle predicted by the new SU(3) classification was identified the following year, where substantial evidence for the violation of charge conjugation and parity invariance (CP) was acquired in 1964. The clear initial superiority of Brookhaven in the production of important scientific results, followed by a more comparable performance after 1964, was confirmed by two British researchers a few years ago using the Science Citation Index (SCI) and counting the articles cited more than 30 or 100 times in the four years after their publication.[12] More interesting for our purposes, however, is that Irvine and Martin took the opportunity of their study to ask about 200 American and European physicists the reason for this initial "gap" between CERN and Brookhaven. In the physicists' view four kinds of factors were involved (see Fig. 3.1).

1. CERN's management erred in planning the equipment needed to exploit the accelerator. Already spoken of at CERN in 1961, this *unpreparedness* came down to a lack of magnets, quadrupole lenses, and separators to build secondary beams, and a delay in the building of big detectors, primarily, but not only, bubble chambers.

2. Not only were qualified experimentalists far fewer in Europe than in the United States, they *lacked experience* and had difficulty elaborating a research program focused on the most important physics questions. In 1962 CERN's research director, Gilberto Bernardini, gave this reason as the most important for CERN's trailing behind its American rivals.[13]

3. There were the effects of CERN being *multinational*, effects particularly

12 The findings were presented by Martin and Irvine, "CERN's Position in World High-Energy Physics," and Irvine and Martin, "Scientific Performance of the CERN Accelerators."

13 See CERN Council minutes, June 13, 1962, pp. 14–19.

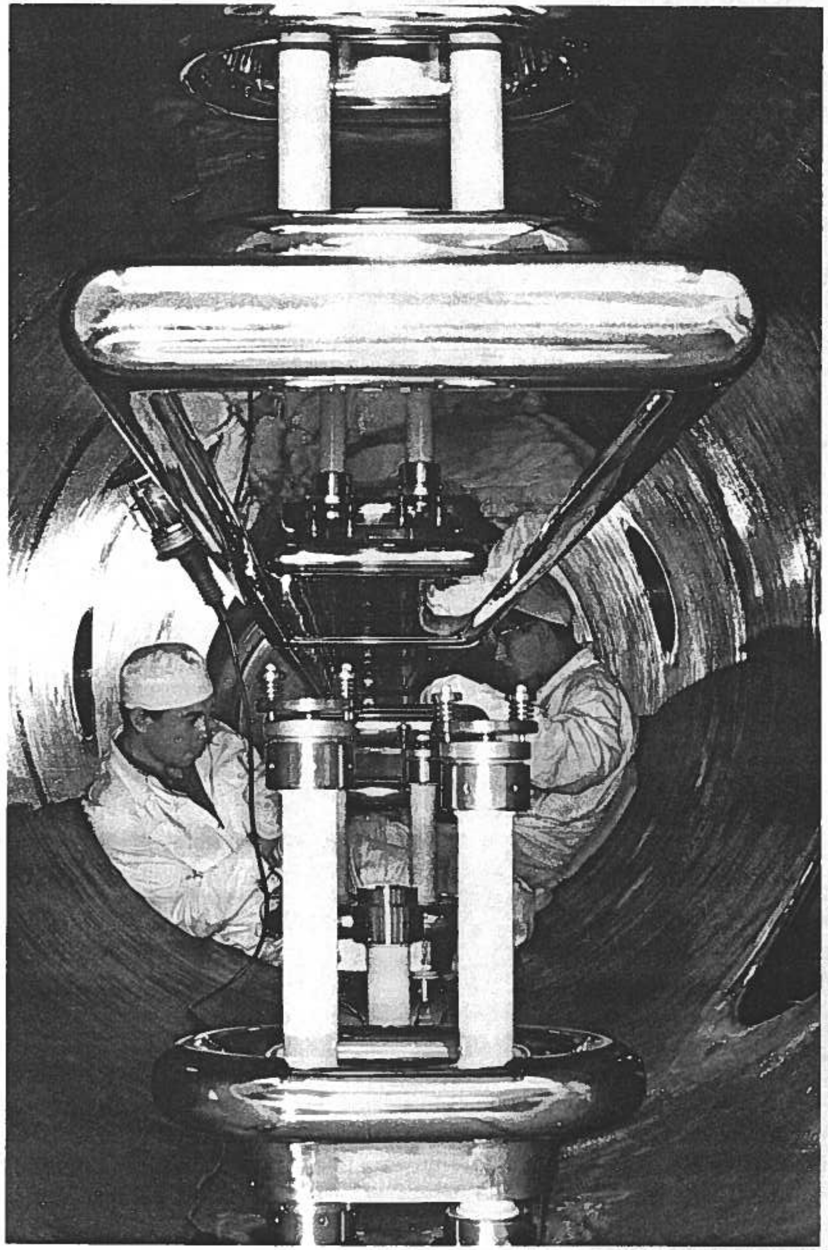

Fig. 3.1. The CERN 2-meter hydrogen bubble chamber, with its surrounding installation, December 1964. Photo courtesy PHOTO CERN.

noticeable in the "cheap proton" policy—have the maximum possible number of experimental groups working around the machine—and the structure of the committees responsible for settling experimental priorities. Cumbersome and slow, CERN's decision making was compared to that at Brookhaven, which was felt to be much more supple and quick because it was concentrated in a few hands, those of Maurice Goldhaber, the director, in particular. While more "cautious" and "democratic," the CERN system was also seen as less efficient than the more "autocratic" procedure at Brookhaven.

4. Finally, *cultural differences* between Americans and Europeans were mentioned, the former being described as more bold and speculative in their approach, the latter as more conservative, more likely to proceed gradually. This was supposedly revealed in the tendency at Geneva to "overdesign" equipment, to design equipment more reliable in the medium to long term at the price of making it available to experimentalists that much later. (The converse of this difference is that the Europeans always produced more systematic and refined results—as Irvine and Martin confirmed.) In the accounts given by American physicists, these last two themes were seen to reflect differences in "style." In contrast to the Europeans, the Americans described themselves as knowing how to get around organizational restraints, as more quick to adapt and to turn a mistake to their advantage, as more capable of grasping the essentials of what has to be done and to ignore "junky" research—in short to be more alert and clever than their European colleagues in what is, above all, a high-pressure race to be the first to make big discoveries.

If we confine ourselves to impressionistic evidence—and accept of course that all that really matters is the race for a Nobel Prize—these accounts can seem convincing. In essence they seek to explain the difference in the production of a few results that the community deems to be decisive by identifying a number of "gaps" between the United States and Europe. The difficulty, however, is that such very global arguments tend to explain "too much," that when confronted closely with concrete cases they appear sometimes true, sometimes false, and sometimes quite irrelevant. Accordingly, to show what we mean, we consider in turn, and in some detail, the exact problems the experimentalists at CERN had to confront in the early 1960's.

Let us begin by looking at the question of the standard equipment necessary for the installation of secondary beams. Since there is no ideal stance from which one can judge the preparations that CERN "ought" to have made, the best alternative is to compare CERN and Brookhaven. First, CERN does not seem to have been later than Brookhaven in placing its first order for magnets and quadrupole lenses. CERN's order was not quantitatively smaller, but it was far less varied (magnets of the same length, relatively fewer quadrupoles, etc.). Second, CERN apparently did not place its second order as quickly as did Brookhaven, nor in the second did CERN rectify the limitations of the first. In seeking to explain these developments we have been led to conclude that no one of sufficient authority at CERN placed a high priority on the design of beams, no one dedicated himself to keeping up with developments in this field, no one thought design of "standard" beams sufficiently important to be preferred over more noble tasks such as building bubble chambers or more sophisticated equipment. At Brookhaven, on the contrary, the secondary beam problem was studied in detail from 1959 onward. If there is an equipment gap to be found here, it is associated with an underestimation of the importance of the problem, with the fact that no one at CERN saw the implantation of "everyday" equipment of this type as being a particularly important job. It was rather neglected in the distribution of key tasks in the organization.[14]

As for less standard beam material, such as electrostatic particle separators, it is interesting that both CERN and BNL initially planned to build similar devices (10-meter tanks), and at about the same time. CERN's separators, however, were ready much later than Brookhaven's, seriously impeding the bubble chamber program until early in 1962. The problem here was twofold. On one hand, starting from nothing, major research and development had to be undertaken in Europe; Brookhaven, for its part, relied on its experience and decided to build smaller devices (about 5 meters long) modeled on those in use at the Cosmotron. This does not tell the whole story, however, because the Europeans *chose* (be it consciously or not) to construct sophisticated, multipurpose separators that took far longer to build than the conventional ones ordered at Brookhaven.[15] This brings to mind the general argument that the Europeans tended to overdesign their apparatus. The trouble with such a formulation is that it obscures differences more fundamental, more at the root of the specific problem we are trying to illuminate here, namely, that in Europe the gap between physicists and engineers was still great, and this gap was inscribed in the structure of the laboratory and its power relations. This gap made it possible for the engineers and builders at CERN to act with considerable autonomy once a task had been given to them, and it allowed them to indulge a tendency to seek technological perfection. It also allowed them to be relatively insensitive to the demands of the physicists for whom big discoveries often meant acting quickly, for whom having an "imperfect" piece of equipment ready at the right moment was often more important than having a "perfect" one ready when the dust of the battle had settled. This clearly happened in the case of electrostatic separators for secondary beams at CERN: the engineering division building them did so at its own pace, without really worrying about physics[16] (see Fig. 3.2).

14 See Krige in Hermann et al., *History of CERN*, vol. 2, chapter 9.

15 *Ibid.*

16 On the institutional reality of these differences, see Pestre in Hermann et al., *History of CERN*, vol. 2, chapter 7, section 2; on the autonomy of "engineers," see the debate about the intersecting storage rings and the 300-GeV PS discussed by Pestre in Hermann et al., *History of CERN*, vol. 2, chapter 12, sections 3 and 6.

Fig. 3.2. European "perfectionism" vs. American "pragmatism." This is one of three giant, very sophisticated electrostatic separators built by CERN in the early 1960's to purify secondary particle beams. Brookhaven initially had similar grandiose plans, but opted for a simpler, more conventional device. CERN's decision meant that the laboratory was late to exploit the potential of its new proton synchrotron when it first worked, although in the years to follow it built some of the best separated beams in the world. Photo courtesy PHOTO CERN.

Before coming back to the differences between physicists and engineers in Europe, we continue with our analysis of the precise problems facing European scientists from 1960 to 1962. Now we want to focus on big detectors, and to try to understand why there was a notable time lag between CERN and BNL—not to speak of Berkeley—in the operation of big bubble and spark chambers. Our impression is that we need to introduce another element here: the fact that European *physicists,* understood in the narrow sense of the term, were not leaders in the field of experimental high-energy physics in the 1950's and 1960's, and that, as a result, they were not the brains behind new instruments *directly connected to the art of experimentation.* At that time at CERN the development of *new* kinds of detectors did not spring from ideas generated by local practice but was generally based on the importation of concepts born and tried out elsewhere. This dependence had two kinds of effects. Unlike a group that innovates, betting on its idea and doing a lot of preliminary research and development before being able to show that its equipment works and is qualitatively superior to other devices, groups not at the heart of the initial research tend to wait and see if the idea is worthwhile.[17] In the case of bubble and spark chambers, the first reaction was to keep an eye on developments, and the decision to "take the plunge" was made only when the advantages of the devices were there for all to see. This is not an indication of a hesitant "nature" or of a systematic propensity to be conservative, but merely an attitude to be expected from *any* group—be it European or American—that is not at the heart of the action. This process leads one, moreover, when the decision is taken to enter the field, to position oneself with regard to the leader(s) and their latest choices and to skip the intermediate stage: this often seems to be the only way to avoid always having obsolete equipment. On the other hand, it accentuates "backwardness" in the short to medium term because the most advanced equipment takes longer to build. These considerations were clearly at work at CERN when it was decided to build a 2-meter hydrogen bubble chamber, the key argument being that Europe's *central* laboratory could not afford to have equipment inferior to what the Americans were building. As a result CERN was without its own big hydrogen bubble chamber in the first years after the proton synchrotron (PS) worked.[18]

Finally let us consider big and sophisticated equipment, which is a response to needs that can be formulated well in advance and are less directly linked to the development of experimentation. We are thinking here of things like radio frequency (RF) separators, whose principles were known but required several years of research and development, or of the neutrino horn invented by Simon

17 See Krige in Hermann et al., *History of CERN,* vol. 2, chapter 9, and Krige and Pestre, "CERN's First Large Bubble Chambers."

18 Krige and Pestre, "CERN's First Large Bubble Chambers," and Pestre in Hermann et al., *History of CERN,* vol. 2, chapter 8, section 6.

van der Meer. Here the Europeans were often leaders and revealed a considerable capacity to imagine and to innovate. We find the same thing in the design and construction of accelerators such as the 28-billion-electron-volt PS, a novel machine based on the newly discovered strong-focusing principle, which the Europeans unhesitatingly chose to build before the Americans had launched their program. The concept of the intersecting storage rings, advocated at CERN from 1960 to 1965, was a response to an innovative drive of a similar kind.[19] In such cases the image of Europeans as more conservative and less innovative than Americans does not fit. Employed indiscriminately, and without specifying to whom it is supposed to apply, the distinction stops one from seeing the more important differences we have already identified. In fact, one must distinguish between instances in which physicists strictly speaking were decisive, in which instrumental developments were associated directly with the art of detection and rooted in physics questions; and cases in which, by contrast, the aim was rather to solve research and development problems, to develop and to improve radically devices whose features were already glimpsed, even if vaguely. In the latter the Europeans do not seem to have been backward at all. On the contrary, they might be considered too bold, too innovative—indeed unwilling—to devote themselves to projects, like standard beam transport equipment, that did not seem to pose a sufficient technical challenge.

American and European Physicists

We have now arrived at the heart of the matter. What happened in the United States between the 1930's and the 1960's—a phenomenon from which the Europeans were largely excluded—was the emergence of a profound symbiosis previously unknown in basic science, a fusion of "pure" science, technology, and engineering. It was the emergence of a new practice, a new way of doing physics, the emergence of a new kind of researcher who can be described at once as *physicist,* in touch with the evolution of the discipline and its key theoretical and experimental issues, as *conceiver of apparatus and engineer,* knowledgeable and innovative in the most advanced techniques (like electronics at that time) and able to put them to good use, and *entrepreneur,* capable of raising large sums of money, of getting people with different expertise together, of mobilizing technical resources. The most successful examples of such men were to be found around Ernest O. Lawrence, who was one of the first to orient his group in this direction. It was men like Luis W. Alvarez, E. Lofgren, Edwin M. McMillan, Wolfgang Panofsky, and Robert R.

19 For the PS, see Krige in Hermann et al., *History of CERN,* vol. 1, chapter 8, section 5; for the intersecting storage rings, see Pestre in Hermann et al., *History of CERN,* vol. 2, chapter 12, section 3.

Wilson, for example, who became the masters of the new physics and who imposed their rhythm on world science.[20]

What characterized them was a pragmatic and utilitarian approach notable for its clear stress on "getting numbers out," an approach preferring results and practical efficacy to means and aesthetic harmony. The approach was rooted in "an educational philosophy that emphasize[d] the empirical, the experimental practice" (Schweber), and was kept alive by the institutional arrangements in American universities that encouraged theoreticians, experimentalists, and apparatus builders to work together.[21] Then there was the experience of the war, which meant lavish financial and technical means, multidisciplinarity, and the linking of people with different educational backgrounds. The war gave Americans an imperative to succeed at whatever cost by using all the technical and industrial resources available. This reinforced the "full-blooded empiricists," the "radical pragmatists," the people for whom all was permissible methodologically, who preferred a heuristic emphasizing improvisation and risk. In physics this stimulated phenomenological approaches and discouraged "a sustained focus and effort on fundamental theory" (Schweber); in practice it brought the engineering side of laboratory work to the fore, notably in the demand for industrially available material to be used in new and interesting ways.[22] Subsequently, once these methods had proved their indisputable efficacy, there was the added bonus of the Cold War and the growing importance of applied research. The American system for supporting science—notably by the Department of Defense and its famous summer schools—as well as the plethora of unexpected and exciting experimental results generated with the new means at hand, consecrated this technical approach to treating problems, far and away the most efficient means for imposing structure and order on a field dragged forward by experimental and technological practices. As a result, fifteen years after the war, the gulf between the United States and Europe was impressively wide.[23]

By contrast, European physicists in the years 1945–60 appear above all as the heirs to a tradition that continued to attach great importance to "pure" science and kept "applied" science separate. It kept fundamental theory, something refined, apart from experimental phenomenology, still regarded as of lesser importance in the elaboration of knowledge. Without the stimulus of a war effort European physicists did not become apparatus builders *before all else,* and—even in Britain[24]—remained people for whom the building of big and sophisticated equipment did not derive directly from their expertise. Being experimentalists in the classical sense of the word, they did not become managers immediately able to handle the new scale of activity demanded by big science. An experiment—and even if nuances are needed depending on whether we are talking of electronic detectors or track chambers—remained something one did in the short term, on a human scale, something that was not in itself a permanent race to use equipment constantly having to be changed. In short, experimenting remained primarily the *practice of an art,* secondarily the *mastery of techniques.*[25]

It is for this reason that the engineers who worked around European physicists enjoyed so much autonomy. Indispensable by virtue of the size of certain undertakings, they were put in charge of "all big equipment," and this often led them to become the "real bosses" of the laboratory. In effect, the physicists had to go through them, and only they were ready to manage centers employing one or two thousand people and in which investments were made on a five- or ten-year basis. Kept, by contrast, on the periphery of physics proper—because, after all, that was not their main preoccupation—they remained detached from the urgency of research and the needs growing from it. Excellent at designing equipment whose goals were clearly defined, they could not imagine, starting from an experimental practice they did not have, the new detectors invented by the Americans. Capable of being the first to install a 28-billion-electron-volt PS embodying a new focusing principle, they were not in a position to "invent," to "think of," to "have the idea for" a bubble or spark chamber. Standing back from day-to-day experimental practice—and constrained by certain cultural and educational traditions peculiar to Europe—they tended to prefer technology *per se,* to be "pure" technicians, to refuse boring and unimaginative tasks, to demand the license to explore new avenues, to work on challenging projects. For want of an interface, since no Alvarez or Panofsky existed in Europe, a hiatus was always possible. And more: since this intellectual and professional difference was inscribed in the organization's structure, the phenomenon was amplified, perpetuated, and rigidified.[26]

Now that we have identified the crux of the problem, the core of the

20 For this section, see Holton, "Les hommes de science," Schweber, "Some Reflections on the History of Particle Physics" and "The Empiricist Temper Regnant," the special edition of *Historical Studies in the Physical and Biological Sciences* edited by Seidel, and autobiographical works such as Alvarez, "Recent Developments in Particle Physics," and York, *Making Weapons Talking Peace.* Remarks of the same kind could be made about entrepreneur-engineers like Vannevar Bush or Frederick Terman at Stanford.

21 This is directly inspired by Schweber's remarks on American theoretical physics in "Some Reflections on the History of Particle Physics."

22 This is also inspired by Holton, "Les hommes de science."

23 Among many works, see Godement, "Aux sources du modèle scientifique américain," Kevles, Chapter 12, this volume, and the issue of *Historical Studies in the Physical and Biological Sciences* edited by Seidel.

24 For example, see Hoch, "Crystallization of a Strategic Alliance."

25 Victor Weisskopf was well aware of the situation when he gave his first speech at CERN as Director-General (after having spent a year there already). It was reported by Pestre in Hermann et al., *History of CERN,* vol. 2, chapter 7, section 2.4.3.

26 The story of the decision to build the intersecting storage rings conveys much of this feeling—see, for example, Pestre, "Les décisions de très gros équipements."

difference between CERN and the equivalent American laboratories, we are in a position to consider a last argument used to account for the "delays" or the "setbacks" at CERN in the years 1960–65, namely the multinational character of the body, which was said to weigh on and to impede the functioning of its experimental program (the question of the experimental committees) and to allow physicists to perform many experiments of little interest at the expense of giving priority to decisive experiments of lasting significance (the so-called "cheap proton" policy).[27]

We have already alluded to the fact that, because of its *multinationality*, rules and regulations have always been more formalized at CERN than at "ordinary" laboratories. There is, however, an ambiguity in the expression "CERN as multinational laboratory." It conceals two notions that are not distinguished, the fact that CERN brings together several sovereign *states*, and the fact that it is a *central* laboratory for a polycentric physics community (that it is, one might say, a laboratory shared by various groups, similar to Brookhaven). In the early years the first aspect was dominant; CERN was a new business, the wishes of its various member-states did not yet converge, and the relationships between CERN and the national physics communities were somewhat formal. Once *concrete* experimental work got under way around the PS, however, and once it was officially accepted (in 1961 and 1962) that budgets should grow as a matter of policy, there was a radical shift, and the second aspect became prominent. This is clear from the increased autonomy that the executive enjoyed with respect to the Council and the Scientific Policy Committee (SPC)—the latter no longer intervened in the organization of the experimental work after 1961, and we find *very* few "nationalistic" complaints by them regarding the choice of experiments made by the laboratory's directors—and by the fact that the problem became the more general one of the relationship between CERN scientific *staff* and the *visitors-users*. What CERN had to deal with, then, was not what one might imagine—rivalries between national groups that had to be managed by a complex system of experimental committees—but the rapid emergence of a problem common a few years later to *all* big science laboratories, notably the American national laboratories when faced by a user rebellion (Brookhaven in 1964, for example) or trying to spell out how to manage the 200-billion-electron-volt accelerator then being designed near Chicago.[28]

27 Martin and Irvine, "CERN's Position in World High-Energy Physics," and Irvine and Martin, "Scientific Performance of the CERN Accelerators," reporting the opinions of many physicists.

28 See Westfall, "The First 'Truly National Laboratory': The Birth of Fermilab." The point had already been raised during the planning of the Stanford linear accelerator when a number of still unresolved issues—"how allocation of beam time should be determined," "the rights and responsibilities of researchers not holding physics department faculty appointments"—rose to the surface in 1954. See Galison, Hevly, and Lowen, Chapter 2, this volume.

One might object that, even if CERN's multi*nationality* was not responsible for its experiments committee system, this system was very cumbersome all the same and introduced rigidities into the laboratory's functioning. Highly decentralized, comprising three committees specialized in different detection techniques, track chamber, emulsion, electronic, it suffocated rapid adaptations to changing circumstances and impeded the implementation of a central policy concerned above all with crucial experiments.[29] This conclusion, too, would be too hasty, however, and actually would *invert* cause and consequence. Our impression is that the committee system *reflected* the conditions prevailing in the European physics community, that it took the form it did because at the time there were no physicists in Europe having the aura of an Alvarez or a Maurice Goldhaber, because it allowed a community not always that sure of itself to reduce the risks inherent in any autocratic system of management. In this sense it was not so much the source of an overly "prudent" experimental program as a structural counterpart of the situation of European physicists at the time, be they from CERN or from national laboratories, namely, of not yet quite knowing how best to use equipment of the scale of that in Geneva. If the CERN of the years 1960–65 appeared to be a "big photocopier" reproducing and improving novel work done elsewhere,[30] it was because the European *physicists* were learning how to experiment, were learning new ways of doing things with which their American colleagues had been familiar for almost two decades. Their "conservatism" and their "prudence" on the one hand, "the heaviness of their decision-making system" on the other, were thus merely two manifestations of the gap between the two practices, two manifestations of the apprenticeship the Europeans were serving—in the absence of a master, and by a method of trial and error.

29 See Pestre in Hermann et al., *History of CERN*, vol. 2, chapter 8, section 4.

30 The phrase was Pierre Germain's in an interview with Dominique Pestre held on November 14, 1988 (along with Gordon Munday and Peter Standley). Germain was the director of CERN's PS division in the 1960's.

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Knowing Machines
Essays on Technical Change

Donald MacKenzie

The MIT Press
Cambridge, Massachusetts
London, England

Contents

4

From the Luminiferous Ether to the Boeing 757

Inertial navigation systems are central to modern navigation. They permit wholly self-contained navigation of remarkable accuracy. They are now standard in long-range civil aircraft and most modern military aircraft, as well as in ballistic missiles, cruise missiles, space boosters, and submarines. They are increasingly to be found in shorter-range tactical missiles, in tanks and self-propelled artillery, and in some surveying applications.

At the heart of inertial navigation are the inertial sensors themselves: gyroscopes, which sense rotation, and accelerometers, which measure acceleration. During the last twenty years, the former have undergone what those involved see as a technological revolution. Since the beginnings of inertial navigation in the 1930s, the gyroscopes used had remained analogues—however sophisticated—of the child's spinning toy, reliant in their detection of rotation on the mechanics of a rapidly revolving rotor. But they have now been challenged by inertial sensors in which the detection of rotation is achieved by optical rather than mechanical means: laser gyroscopes. All but one of the major corporate suppliers of inertial technology are heavily committed to laser gyroscope technology. A basic shift has thus taken place in this key modern technology.

This chapter begins with the conceptual origins of the laser gyroscope, which are remote from the "high-tech" world of the modern device. They lie in experiments probing the controversial question of the existence of the ether, the massless substance that pre-Einsteinian physics took to be the medium of the transmission of light. In particular, the physicist Georges Sagnac (1869–1928) believed that his work on the optical detection of rotation refuted Einstein. The second section of the chapter describes the move of what became known as the "Sagnac effect" from science to technology, a move that took place between 1959 and 1963. The invention of the laser was fundamental to this move, but more was involved than just a new light source. As quantum electronics flowered, the optical detection of rotation was reconceptualized.

On January 7, 1963, a prototype laser gyroscope first detected rotation, and that date can be taken as indicating the end of the process of "inventing" the laser gyroscope and the beginning of the "development" phase of the device's history. That development phase is the subject of the third section. It stretched from 1963 to the first unequivocally successful tests of a practical laser gyro in 1975, and it proved as crucial and as troublesome in the case of the laser gyro as elsewhere in the history of technology.[1] The fourth section describes the growing acceptance of the laser gyro after 1975. It highlights the single most crucial event in that process of acceptance: the decision to adopt the new device as the core of the standard navigation and attitude reference system for Boeing's new civil air transports, the 757 and the 767.

The chapter ends with a discussion of what can be learned from this episode about the nature of technological change. The history of the laser gyroscope underlines the significance of the fusion of scientific and technological concerns in the new field of quantum electronics. It supports those who have noted the pervasiveness of military involvement in quantum electronics, while showing that the resultant technology may not bear the stamp of any specifically military need. The history of the laser gyroscope is one in which economic considerations, market processes, and corporate structures are central, yet it is a history that does not correspond to orthodox economic theory, with its assumption of profit maximizing by unitary firms. Perhaps most interesting of all, the process of the acceptance of the laser gyroscope reveals the role of self-fulfilling prophecy in technological revolutions.[2]

Searching for the Ether

The ether was a paradoxical substance. It was believed to pervade the universe and to be the medium for such phenomena as electromagnetism, gravitation, and nervous impulses. Yet it was also thought to be devoid of the qualities that made the grosser forms of matter easily perceptible. It could not be seen, felt, or touched. It played a crucial role in orthodox physics, chemistry, and even biology; it was of theological significance too. The physicist Sir Oliver Lodge was not alone in seeing the ether as "the primary instrument of Mind, the vehicle of Soul, the habitation of Spirit." "Truly," he wrote, "it may be called the living garment of God."[3]

The most famous attempt to demonstrate the existence of the ether was the series of experiments conducted in the 1880s by the physicist Albert A. Michelson and the chemist Edward W. Morley.[4] If the ether was at rest in absolute space, as most assumed, then as the Earth moved it would be moving relative to the ether. From the point of view of an observer on the Earth, an "ether wind" would thus exist. It would not be directly perceptible to the senses, but it would affect the speed of transmission of light, since light was a wave in the ether. Michelson and Morley's apparatus split a beam of light into two, one part traveling parallel to the Earth's motion and one at right angles to it, and sought to detect the predicted effect of the ether wind in the interference pattern when the two beams were recombined in an interferometer.[5] Michelson and Morley were unable to find that effect.[6] The fame of their experiments lies in this null result. Later, the null result was taken as proof of the nonexistence of the ether and as leading to Einstein's Special Theory of Relativity, a key postulate of which is that the velocity of light is the same for all observers and therefore no difference is to be expected between "looking" along the direction of the Earth's motion through space and "looking" at right angles to it.

Matters were not, however, quite as clear as this simple hindsight history suggests.[7] When Morley's colleague Dayton C. Miller repeated the experiments, he believed he did find at least some significant effect.[8] Furthermore, a null result by no means compelled rejection of the ether. It could, for example, be taken as showing simply that the moving Earth dragged the ether along with it, so that no "ether wind" would be found at the Earth's surface.[9]

So the search for the ether did not end with the Michelson-Morley experiments, and here Georges Sagnac enters the story. Sagnac was a professor of physics, first at Lille and then at the University of Paris. His early work had been on the recently discovered x rays. In his ether experiment, he sought to create an ether wind in the laboratory by mounting an interferometer on a rotating platform. A beam from an electric light was split, and the two resulting beams, R and T, were sent in opposite directions around a path formed by four mirrors, M_1, M_2, M_3, and M_4 (figure 1). Sagnac used a camera to observe the interference patterns when the two half beams were recombined.[10] As Sagnac's apparatus rotated, first in one direction and then in the other, the camera did indeed record a shift in the interference fringes. He reported his results in a brief, exuberant paper to the Académie des Sciences in 1913. The fringe shift occurred, he claimed, because his apparatus was rotating in

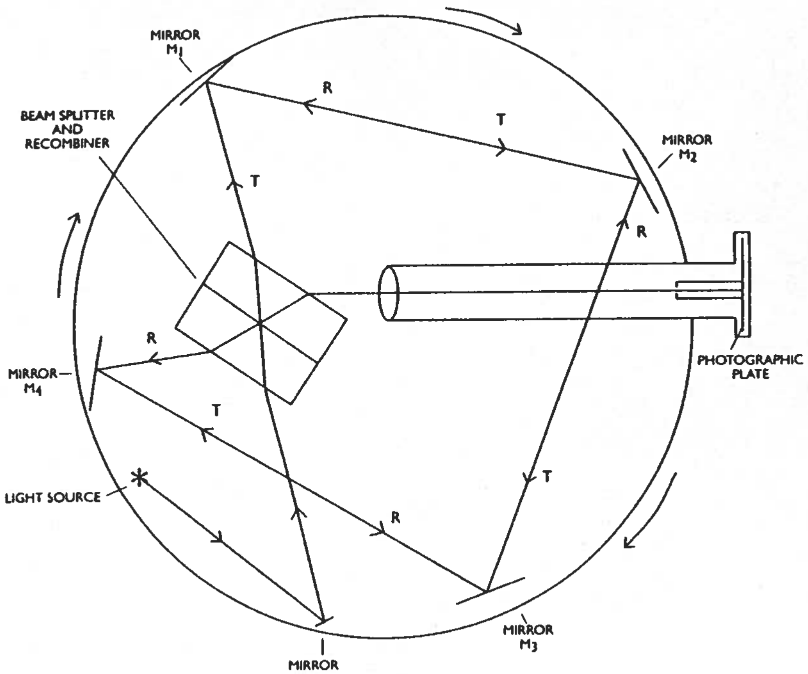

Figure 1
Sagnac's interferometer on its turntable. Simplified from diagram in G. Sagnac, "Effet tourbillonnaire optique: la circulation de l'éther lumineux dans un interférographe tournant," *Journal de Physique*, fifth series, 4 (March 1914), p. 187.

the ether. Relative to the turntable, one beam was retarded, and the other accelerated, according to the direction of the turntable's rotation in the ether. Sagnac calculated what the effect of this on the interference pattern ought to be and found that the measured shift was as predicted. His experiment, he concluded, was "a proof of the ether"; the interferometric effect "directly manifested the existence of the ether."[11]

Though Einstein's name was not mentioned, the challenge could not have been clearer; and it was made within a French scientific community predominantly hostile to relativity. (Even as late as the 1950s, "with rare exceptions, teaching, textbooks, and university programs" did not allow detailed attention to relativity to disturb an image of "Science . . . as a fully realized achievement, encased in certainty, organized around Newtonian categories."[12]) The relativist Paul Langevin vigorously disputed Sagnac's interpretation of his results.[13] Nevertheless, the Sagnac effect seems to have counted in France as evidence for the ether. Thus,

when Sagnac was awarded the Pierson-Perrin Prize of the Académie des Sciences in 1919, his experiment was described as having verified the theory of the ether. It was repeated in a different form (with the "observer" fixed in the laboratory, rather than rotating) in 1937, and again the results were found to confirm "classical theory" and to violate the predictions of relativity.[14]

In the Anglo-Saxon world matters were different. Sagnac had his defenders there too, notably the anti-relativist Herbert E. Ives. But mainstream opinion was firmly in favor of Einstein, and to the extent that Sagnac's work was considered at all it was dismissed. There were doubts about the reliability of Sagnac's results.[15] But, more important, the conclusion became accepted that the theory of relativity could explain them just as well as ether theory. With a rotating system, the relevant aspect was argued to be general, not special, relativity. According to the former, "two observers, traveling around a closed path that is rotating in inertial space, will find that their clocks are not in synchronization when they return to the starting point (traveling once around the path but in opposite directions). The observer traveling in the direction of rotation will experience a small increase, and the observer traveling in the opposite direction a corresponding small decrease in clock time." If the two "observers" are photons, each traveling at the speed of light, "the time difference appears as an apparent length change in the two paths," causing the shift in the interference fringes reported by Sagnac.[16]

Therefore, it did not help the case against Einstein when, in 1925, Michelson and his colleague Henry Gale also reported a change in interference pattern as a result of rotation. They employed the Earth itself as the turntable. Using a rectangular system of pipes in which they created a vacuum, they constructed an optical circuit a mile in circumference (figure 2). A smaller rectangular circuit provided a "fiducial mark from which to measure the displacement" of the interference fringes formed by the clockwise and counterclockwise beams in the larger circuit.[17]

Michelson and Gale's results were in agreement with "the calculated value of the displacement on the assumption of a stationary ether," just as Sagnac's had been. However, concluded Michelson and Gale, they were "in accordance with relativity too." There was little doubt where Michelson's heart lay—in 1927 he wrote of "the beloved old ether (which is now abandoned, though I personally still cling a little to it)"—but the ambiguous experiment did not help bring the ether back to life.[18]

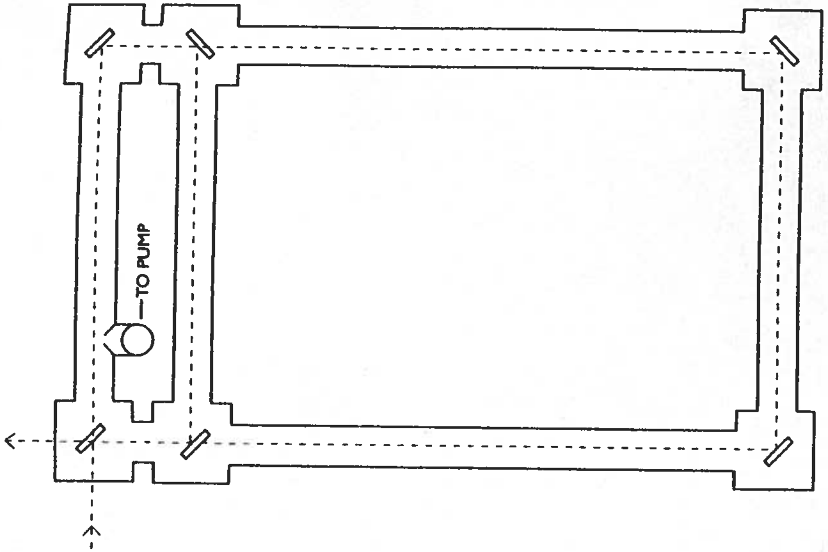

Figure 2
Ground plan and arrangement of mirrors in Michelson-Gale experiment. Based on diagram in A. A. Michelson and Henry G. Gale, "The effect of the earth's rotation on the velocity of light: Part II," *Astrophysical Journal* 61 (April 1925), p. 141.

As late as 1965 there were still those who claimed that Sagnac had indeed "discovered the existence of a luminiferous ether" and denied that relativity theory explained his results. By then, though, this was a distinctly unusual opinion to hold. True, the author of this claim could point out that, using the novel technology of the laser, "the Sagnac experiment has been repeated, with the same but more refined outcome."[19] The meaning of that replication had, however, shifted decisively. There was indeed widespread interest in it, but the question of the existence of the luminiferous ether was certainly not the source.

From Science to Technology

Sagnac had speculated that it might be possible to use his effect to measure rotation in a practical context:

> I hope that it will be possible to repeat these measurements of the optical whirlwind effect [*l'effet tourbillonnaire optique*] with an optical circuit at least some tens of meters square, fastened to the rigid sides of a ship. If the circuit is horizontal, the displacement of the central [interference] fringe will make known at each instant the speed of rotation of the ship about a vertical axis; slow rotations could thus be revealed without any external benchmark. . . . A circuit installed parallel to one of the vertical planes of the ship would permit similar observation or photographic recording of the speed of oscillatory rotation in roll and pitch.[20]

This 1914 speculation is, however, as far as the practical application of the Sagnac effect went for many years. Yet when interest in the optical detection of rotation revived around 1960, theoretical issues (though not absent) quickly became less salient than technological ones.

In the intervening half-century, the measurement of rotation had become a central technical activity. When Sagnac was conducting his experiments on the eve of the First World War, the practical application of the mechanical gyroscope was a relatively new field: the first successful trials of a marine gyrocompass, for example, had taken place in 1908.[21] Between then and the late 1950s, the marine and aircraft uses of the gyroscope had grown in importance and sophistication and had been joined by the new and uniquely demanding field of inertial guidance and navigation. Inertial systems were seen as having one decisive advantage over other forms of navigation: being wholly self-contained, they could not be disrupted by either hostile action or bad weather. Though inertial navigation had yet to find significant civilian applications, by the late 1950s it was a crucial military technology.[22]

That did not mean, however, that the place of the mechanical gyroscope was secure. The dominant variety in inertial navigation in the United States—the fluid-floated gyro—could be made highly accurate, but it was difficult to produce and therefore expensive. The mechanical gyros of the 1950s also suffered from reliability problems. There was thus a conscious search for alternative means of detecting rotation.

That search led at least one military organization in the United States back to the ether experiments. The Navigation and Guidance Laboratory of the Air Force Systems Command at Wright-Patterson Air Force Base had been "interested for several years in an angular rate sensing device without moving parts for the obvious reason of reliability," its chief wrote in 1962. Since an optical circuit a mile in circumference was patently too large for a practical navigation system, the laboratory had sought to "miniaturize the Michelson-Gale experiment."[23] Its attempts, however, were "notably unsuccessful at both optical and gamma ray wavelengths."[24] Success was to require the transformation, and not merely the miniaturization, of the Sagnac and Michelson-Gale experiments.

That transformation was wrought by quantum electronics. This new field fused science, notably quantum theory, with the technological concerns of radar and radio engineering. Like inertial navigation, it emerged in large part under military tutelage. The U.S. military supported the field financially, organized key conferences, and actively sought defense applications for its products.[25]

A key element in quantum electronics was experience in the use of resonant cavities, in which large quantities of electromagnetic radiation are generated at a frequency such that the wave "fits" the cavity exactly (in other words, the length of the cavity is an integral number of wavelengths). An example crucial to radar was the resonant cavity magnetron, a powerful new microwave generator developed at the University of Birmingham (England) in 1940.[26] Another element in quantum electronics was the physics of quantum transitions, in which electrons move from higher to lower energy orbits or vice versa. These two elements were brought together in the development in the 1950s of the maser (an acronym for microwave amplification by stimulated emission of radiation). In this device, electrons in an appropriate material are "pumped" by an input of energy to higher energy orbits. If then properly stimulated in a suitable resonant cavity, they will return to lower-energy orbits in unison, producing a powerful output of coherent microwave radiation. By 1954 the first maser was working, and by 1956–57 there was already interest in moving to light frequencies, and thus to an optical maser or laser (for light amplification by stimulated emission of radiation). T. H. Maiman of the Hughes Aircraft Company demonstrated the first such device, a solid-state ruby laser, in July 1960. In February 1961 a gas laser, using as its material a mixture of helium and neon, was announced.[27]

Between 1959 and 1961, three people independently saw that it was possible to transform the Sagnac and Michelson-Gale experiments, which they probably knew about primarily through the account in an optics textbook of the day, R. W. Ditchburn's *Light*.[28] Not only did they see that the electric light of the earlier experiments could be replaced by a laser; a conceptual shift was involved. The first hint of this shift came in the autumn of 1959, before the operation of the first laser. There was no reference to either masers or lasers, but the source was a man with considerable experience of the general field of quantum electronics. Ohio State University physicist Clifford V. Heer was working as a consultant for Space Technology Laboratories, an offshoot of Ramo-Wooldridge (later TRW) set up to manage the intercontinental ballistic

missile program of the U.S. Air Force. In September 1959, Heer proposed to the firm's Guidance Research Laboratory a "system for measuring the angular velocity of a platform [that] depends on the interference of electromagnetic radiation in a rotating frame." He noted that in experiments such as Sagnac's a path enclosing a large area was necessary to achieve sensitivity, and this would clearly be a limitation on their technological use. He suggested investigating four areas in the light of this problem, including "the use of resonant structures in a rotating frame."[29] A month later, in a patent disclosure, he added a further new element to the idea of using resonance: that frequency differences, as well as the interference effects used by Sagnac and Michelson, could be used to measure rotation. As a resonant structure rotated, there would be a shift in resonant frequencies.[30]

Those two elements—using a resonant structure and detecting rotation by frequency differences rather than changes in interference patterns—were central in the conceptual shift that led to the laser gyroscope. In 1959, however, Heer was not necessarily thinking of light as the appropriate form of electromagnetic radiation to use. He was at least equally interested in employing radiation of "lower frequencies such as radio and microwave frequencies" confined in a "coaxial cable or waveguide," with "N turns of cable or guide . . . used to increase the phase difference over that for one traversal."[31] In the version of his ideas presented for the first time in public, at the January 1961 meeting of the American Physical Society, he even suggested that the interference of matter waves in a rotating system could be studied.[32]

Heer's first proposal to study the use of masers (including optical masers) in the measurement of rotation came in March 1961, but only as nonhighlighted aspects on the third and fourth pages of a proposal for research on "measurement of angular rotation by either electromagnetic or matter waves."[33] Though copies were sent to NASA, the Air Force Office of Scientific Research, and the Office of Naval Research, funds were not forthcoming. Heer's interest in the use of the laser rapidly grew, however, in part as a result of his attending the Second International Conference on Quantum Electronics at Berkeley, at which Ali Javan of the Bell Laboratories described the first gas laser, in late March 1961. In October 1961, Heer forwarded his original proposal to the Chief Scientist of the Aeronautical Systems Division of the Air Force Systems Command, along with a cover letter stating: "The experiments in the microwave region remain of considerable interest, but in view of the recent development of the optical masers I feel a study of the feasibility of the use of optical masers and the eventual use of optical masers must be given consideration." In January 1962, Heer sent potential sponsors a further paper containing a description of a square resonant structure with "laser amplification along the path." Such a structure a meter square, he noted, would make possible the measurement of "angular rotation rates as small as 10^{-6} radians/sec."[34]

By October 1961 a second researcher, Adolph H. Rosenthal of the Kollsman Instrument Corporation, had also become convinced that, in the words of a paper he read to the Optical Society of America, "interferometry methods making use of optical maser oscillations . . . permit [us] to increase considerably the accuracy of the historical relativistic experiments of Michelson, Sagnac, and others, and have also potential applications to studies of other radiation propagation effects."[35] Before Rosenthal died in July 1962, he had developed his ideas sufficiently that a posthumous patent application using them in a "optical interferometric navigation instrument" could be submitted.[36]

One member of Rosenthal's audience at the Optical Society had already been thinking along the same lines. He was Warren Macek, a young physics-and-mathematics major working for the Sperry Rand Corporation. Much of the original strength of that company had been built around Elmer Sperry's use of the mechanical gyroscope for navigation, stabilization, and aircraft instruments.[37] However, Macek worked not on gyroscopes but in a new optics group Sperry Rand had set up in 1957. After the announcement of the ruby and gas lasers, the optics group built its own versions of each, with help from specialists on microwave resonant cavity devices.

Macek had read Ditchburn's *Light* for a course in physical optics he had taken as part of his Ph.D. work at the Brooklyn Polytechnic Institute, and through that he knew of the Sagnac and Michelson-Gale experiments. In October 1961, when he heard Rosenthal's paper, Macek was already working on a proposal to Sperry management which included, among other novel rotation sensor techniques, the idea of building an interferometer, analogous to that used in the ether experiments, using a laser as its light source.[38]

In early 1962, Macek and colleagues at Sperry set to work to construct a device in which lasers would be used to measure rotation, adapting resources they already had on hand.[39] They used gas laser tubes the optics group had built. Sufficiently good mirrors were hard to find, so one mirror used by Macek was coated in gold by a relative of his who worked for a gold-plating firm. An old radar pedestal was modified to form the

turntable on which the apparatus was placed. One of the group's technicians who was a radio "ham" tuned the device to achieve resonance.

On January 7, 1963, their device worked successfully.[40] Four helium-neon lasers were arranged in a square a meter on each side (figure 3). These lasers were modified so that, unlike conventional lasers, they radiated light from both ends. Mirrors at the corners of the square reflected the light from one laser tube into the next. In this way, laser oscillations were sustained in both directions around the ring, clockwise and counterclockwise (until this was achieved in the Sperry work, it was not clear that oscillations could be sustained in both directions). One of the four mirrors was only partially coated. Some light from both beams passed through it, and, with use of a further reflector, light from both beams fell on a photomultiplier tube used as a detector.

Although the paper reporting the Sperry work cited Sagnac and Michelson and Gale, it made clear that what was being detected was not the conventional optical interference fringes they had used, and here the input from quantum electronics was clearest. Like all lasers, the device was a resonant cavity, with resonant frequencies "determined by the condition that the cavity optical path length must equal an integral number of wavelengths."[41] When the system was not rotating, the clockwise and

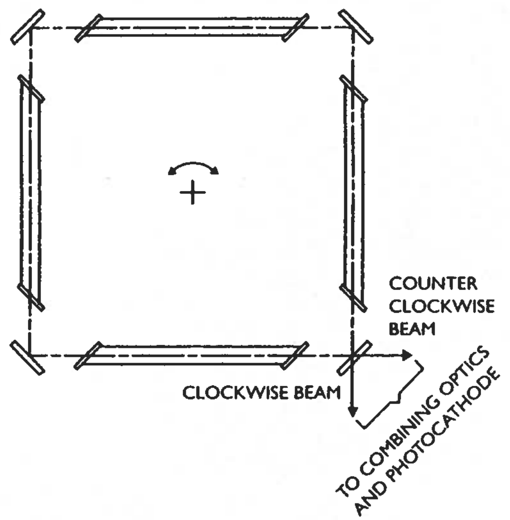

Figure 3
Schematic diagram of the Sperry ring laser. Based upon diagram in W. M. Macek and D. T. M. Davis, Jr., "Rotation rate sensing with traveling-wave ring lasers," *Applied Physics Letters* 2 (February 1, 1963), p. 67.

counterclockwise path lengths were identical, so the frequencies of clockwise and counterclockwise waves were the same. When the system was rotating, however, the path lengths became unequal.[42] The frequencies of the two waves were no longer exactly the same, so, when they were superimposed, the combined wave oscillated in amplitude with a "beat" frequency proportional to the difference in their frequencies, and thus to the rotation rate of the platform. It was those beats that formed the device's output. Such a use of the beats resulting from the superimposition of waves of slightly different frequencies—"heterodyne" action—was a radio engineering method already widely used in laser work. As the platform was rotated at between 20 and 80 degrees per minute, the beat frequencies changed in a satisfactorily linear fashion.

The technological meaning of what they had done was clear to the members of the Sperry team: "The principle demonstrated in this experiment may be utilized for rotation rate measurement with high sensitivity over an extremely wide range of angular velocities. Such sensors would be self-contained, requiring no external references."[43] Along with the conceptual work of Heer (who, together with a doctoral student, P. K. Cheo, had his own device working by August 1963, with funding finally obtained from the National Science Foundation),[44] and that of Rosenthal, the construction of this prototype can be said to constitute the invention of the laser gyroscope.

Developing the Laser Gyro

What had been achieved by January 1963 needs to be put in perspective. At the time, an "inertial grade" mechanical gyroscope was one with a drift rate of a hundredth of a degree per hour, corresponding roughly to an average error of a nautical mile per hour's flying time in an aircraft inertial navigator. The 20°/minute threshold of the Sperry device meant a sensitivity several orders of magnitude poorer. Both Heer and Macek were predicting much better future performance, but that remained a prediction. Furthermore, the meter-square prototype was much larger than the small mechanical gyros (2 inches in diameter, or thereabouts) then available, and the theory of the laser device indicated that its sensitivity would decrease in proportion to any reduction in the area enclosed in the path. Finally, the laser device had many competitors as a potential replacement for the conventional mechanical gyroscope. The gamut of physical phenomena was being searched for new ways to detect rotation. One review listed 29 candidate technologies, several of which—dynamically tuned, electrostatically supported, fluid sphere, nuclear magnetic resonance, and superconductive, as well as laser—were being pursued actively.[45]

So the invention of the laser gyro need not necessarily have led anywhere. Macek and the Sperry group realized this clearly, and what they did once they had their prototype working is of some interest. Instead of keeping their work confidential within the company, they immediately and effectively sought the maximum publicity for it—even though this might be expected to generate competition, and indeed did so. Within a week of its first successful operation, Macek and a colleague had dispatched a paper describing their device to *Applied Physics Letters*; the paper was published within $2\frac{1}{2}$ weeks. They rigged up an impressive audio-visual display, with glowing lasers and beat frequencies relayed through a loudspeaker. Among those they invited to see their device was an influential technical journalist, Philip J. Klass. A mere month after their laser gyro first worked, he rewarded them with an article describing their work (in which the term "laser gyro," which Klass may have coined, was used for the first time) in the widely read *Aviation Week and Space Technology*, and with a color picture on the cover.[46]

Publicity was necessary because the most immediate problem facing Macek and his colleagues was their own company's management. Their original proposal had been rejected on the grounds of infeasibility, and in the company that had pioneered the mechanical gyroscope in the United States the commitment to the existing technology was strong. Even the name "laser gyro" was taboo at Sperry: "the company shuns the use of the word 'gyro' because the device lacks the familiar spinning mass."[47] Competition arguably turned out to be harmful to the long-term interests of the company as a whole: Sperry's laser gyroscopes had less market success than those of the company's competitors. However, competition was in the immediate interest of the team developing the device—that others took it to be feasible was a powerful argument to use with a skeptical management—and certainly was to the benefit of the overall development of the laser gyro.[48]

Several different research and development teams in the United States—and groups in the Soviet Union, the United Kingdom, and France—began laser gyro work soon after the device's invention and the success of the Sperry prototype became known.[49] The American researchers included groups at the Kearfott Division of General Precision, the Autonetics Division of North American Aviation, the Hamilton Standard Division of United Aircraft, and the MIT

Instrumentation Laboratory.[50] Most consequential, however, was a team at Honeywell, members of which freely admit to having learned of the laser gyro from Klass's article in *Aviation Week*.[51]

Like quantum electronics more generally, this R&D effort was strongly supported by the armed services—particularly in the United States, where there was keen appreciation of the military importance of inertial guidance and navigation and of the deficiencies of existing systems. Much of the work within corporations received military funding, and the Bureau of Naval Weapons and the Air Force Systems Command sponsored an annual series of classified symposia on "unconventional inertial sensors" at which work on the laser gyro—and on its competitors—was presented and discussed.[52]

Military support was not, on its own, sufficient to move the laser gyro from prototype to product. At Autonetics, for example, "every year we [the laser gyro developers] told them [higher management] that ring lasers were going to take over everything, and every year they kept us on the back burner. . . . They wanted to stay up with the technology but weren't willing to commit. It costs lots and lots of money to go into production. Because their [Autonetics's] marketplace was strategic vehicles and high accuracy devices, and the devices they were manufacturing were successful, there was no real reason to develop a new product." The founder of MIT's Instrumentation Laboratory, Charles Stark Draper, considered the laser gyro a diversion from the pursuit of ultimate accuracy through the evolutionary refinement of floated mechanical gyros.[53]

The long-term significance of the Honeywell team was thus that they, more than any other group, were able to sustain the development of the laser gyro through the extended period it took to turn the invention into a navigational instrument able to compete on the market. The team, the most central members of which were Joseph E. Killpatrick, Theodore J. Podgorski, and Frederick Aronowitz,[54] possessed not only theoretical and technological expertise but also a capacity to persuade Honeywell's management of the need to do more than keep the laser gyro work on a risk-free, military-funded "back burner." Defense Department support was crucial, especially when the project ran into difficulties within Honeywell. Over the years, however, government funding was matched by a roughly equal volume of internal funding. Honeywell was also prepared to develop a laser gyro production facility in the absence of any firm military orders.[55]

Honeywell's unique position with respect to the inertial navigation business helped make it possible for the laser gyro team to extract this level of commitment from corporate management. Important mechanical gyroscope development work had been done at Honeywell in the 1950s and the early 1960s. Whole navigation systems had been built, too, but they were largely for small-volume and highly classified programs.[56] As a wider military market and then a civil-aviation market for inertial navigation opened up in the 1960s and the early 1970s, Honeywell was largely excluded. It was successful in producing inertial components to others' designs, especially those of the MIT Instrumentation Laboratory, but not in designing and selling its own inertial systems. This meant that at Honeywell (in contrast with Autonetics, for example) there was no existing, successful product line that was threatened by novel inertial sensor technologies, and indeed the latter were seen as providing an opportunity to move Honeywell from the margins to the center of the inertial market. The first technology with which Honeywell attempted this was the electrostatic gyro—a mechanical gyroscope, without conventional bearings, in which the spinning mass is a sphere suspended in an electrostatic field. This device temporarily brought Honeywell an important share of the high-accuracy strategic bomber navigation market, but it was defeated in its primary intended niche, ballistic missile submarine navigation, by a similar gyro produced by the niche's established occupant, Autonetics.[57] Furthermore, the electrostatic gyro never became accepted in the largest market of all: the market for medium-accuracy (around 1 nautical mile per hour error) military and civil aircraft navigators.

Success in this last market was what Honeywell sought with the laser gyro. The potential advantages of the device had been listed in Klass's *Aviation Week* article: it "has no moving parts and, in theory, should be long-lived, sensitive and stable," and, because it measures discrete beats, "its output is available in digital form, for use by digital guidance computers." But to turn this promise into practice clearly required replacement of what those involved would certainly have admitted were "bulky and unwieldy" experimental configurations.[58] This could have been done by modification of these configurations—that in essence was the strategy adopted in further ring laser gyro development at Sperry—but the Honeywell team chose instead to simplify the design radically.[59] They moved from a square to a triangular path drilled in a single solid quartz block (figure 4). In their "monolithic" design, there is no distinction between the path and the laser. Lasing in the entire triangular

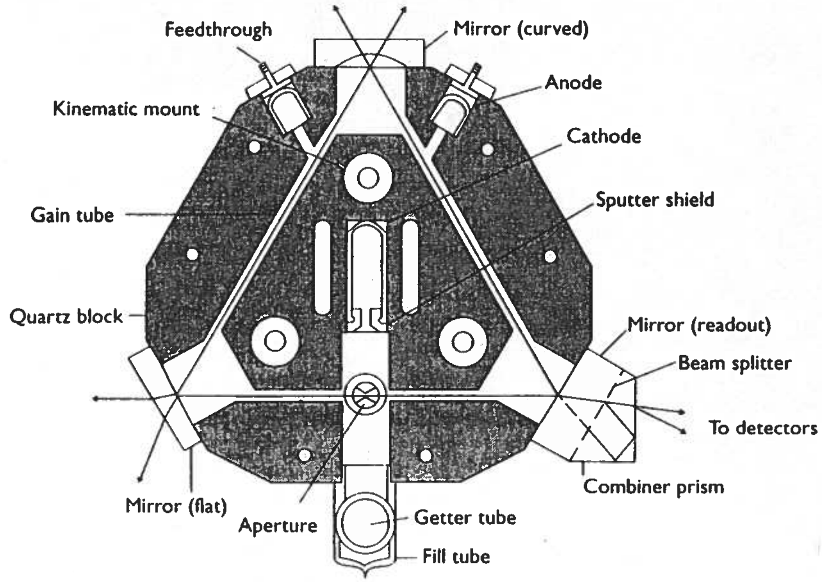

Figure 4
Monolithic solid block ring laser gyro as developed at Honeywell. Based on diagram provided by Theodore J. Podgorski, Military Avionics Division, Honeywell, Inc.

path is sustained by energy supplied by a high voltage difference between a cathode and two anodes.

A second change from the early prototype laser gyros was perhaps even more consequential, because it differentiated the approach taken at Honeywell from those of the other development efforts. All the developers quickly identified a major problem in developing a laser gyro that would be competitive with mechanical gyros: at low rotation rates the laser gyro's output vanished (figure 5). Below a certain threshold (which could be as high as 200°/hour), rotation could not be measured. If uncorrected, this would be a fatal flaw in a device whose mechanical competitors were by the 1960s sensitive to rotations of 0.01°/hour or less.

The cause of the phenomenon now seems obvious, but it was not immediately so to the early investigators. The scattering of light from imperfect mirrors and various other causes meant that the two beams were not in practice wholly independent. They acted like coupled oscillators in radio engineering, "pulling" each other's frequencies toward convergence, and therefore toward zero output and the phenomenon those involved call "lock-in."[60]

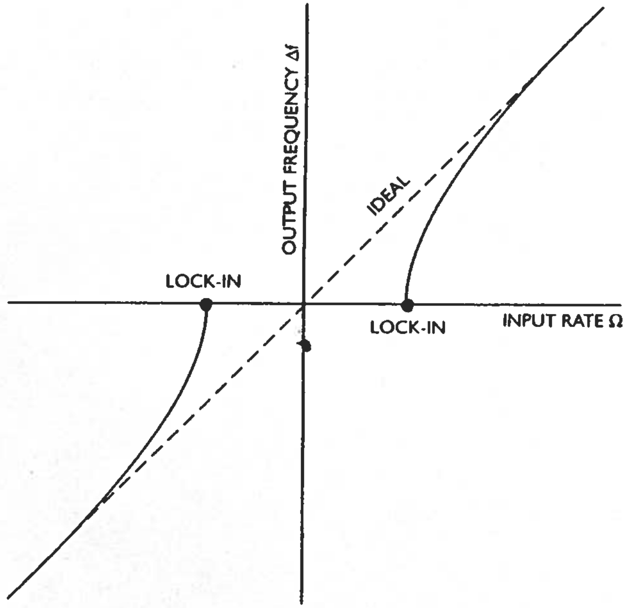

Figure 5
The input-output function for an "ideal" laser gyro and for the actual device. Based on diagram in "Presentation of the Elmer A. Sperry Award for 1984 to Frederick Aronowitz, Joseph E. Killpatrick, Warren M. Macek, Theodore J. Podgorski."

One approach to solving the problem of lock-in was to seek an electro-optical means of preventing the beams from coupling at low rotation rates. The team at Sperry introduced a "Faraday cell" into the cavity (figure 6). This increased the effective travel path of one of the beams more than the other; the device was thus "biased" so that the region where lock-in would occur was no longer within the gyro's normal operating range. Later the Sperry workers substituted an alternative electro-optical biasing technique, the "magnetic mirror."

For the laser gyro to measure rotation rates accurately, however, the bias had to be dauntingly stable, according to calculations at Honeywell. Joseph Killpatrick, the most prominent champion of the laser gyro at Honeywell, had an alternative solution to the problem of lock-in. This was, in effect, to shake the laser gyro rapidly so that it would never settle into lock-in. The idea flew in the face of the "no moving parts" image of the laser gyro that had been created by the publicity for it, such as Klass's article; thus it met considerable resistance: "Shaking it was just

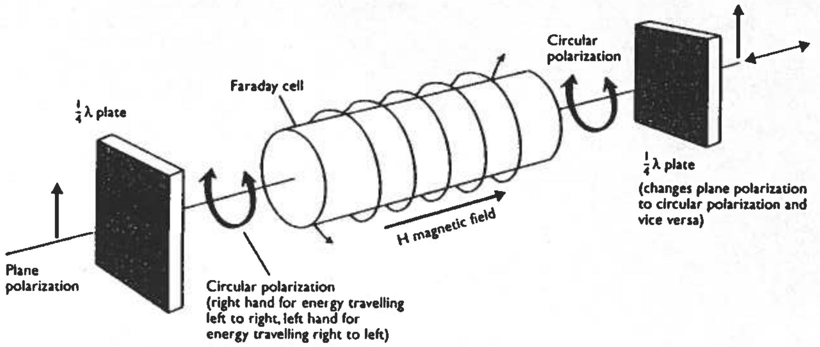

Figure 6
Use of a Faraday cell to bias a laser gyro. Based on diagram in Joseph Killpatrick, "The laser gyro," *IEEE Spectrum* 4 (October 1967), p. 53.

repugnant to people, and so the natural thing was to build in a Faraday cell, but [that] was wrong." Mechanical dither, as it is known, triumphed nevertheless, first at Honeywell and then more widely, even though its incompatibility with the laser gyro's "image" meant that research funds continued to be readily available for investigations of other ways of circumventing lock-in.[61]

Crucial in persuading Killpatrick's colleagues at Honeywell of the virtues of dither were experiments conducted there in 1964. A prototype laser gyro was placed on a large spring-mounted granite block in the Honeywell laboratories, and the block was set oscillating. The results were a remarkable improvement: the device detected the Earth's relatively slow rotation with considerable accuracy. Paradoxically, though, Killpatrick found that too regular a dither motion would lead to considerable errors as a result of the cumulative effect of the short periods of lock-in when the device was at rest at the extremities of its dither motion. "Noise"—a random element in the dither motion—prevented this cumulation.[62]

During this period, the laser gyro was increasingly connected to a hoped-for major redesign of inertial systems. Traditionally, accelerometers and gyroscopes had been mounted on a platform supported by a complex gimbal structure that gave it freedom to change its orientation with respect to the vehicle carrying it. Any rotation of the platform with respect to the fixed stars (or, in some systems, with respect to the local direction of gravity) would be detected by the gyroscopes, and a feedback system would cancel out the rotation, thus maintaining the plat-

form in the desired orientation irrespective of the twists and turns of the vehicle carrying it. The configuration was, therefore, called a "stable platform."

During the 1960s, there was growing interest in the mechanically much simpler "strapdown" configuration, in which the gyroscopes and accelerometers would simply be attached to the body of the vehicle carrying them. There were two barriers to implementing this. One was that a powerful onboard computer would be needed. Because the instruments were no longer in a fixed orientation, more complex mathematical processing of their output was needed to permit velocity and position to be calculated. With digital computers growing more powerful, smaller, and more robust, this first barrier was rapidly eroding by the late 1960s. The laser gyroscope promised to remove the second barrier. In a stable platform the gyroscopes had to be highly accurate, but only over a limited range of rotations. Strapdown gyroscopes had to maintain that accuracy over a much wider range. This was acknowledged as hard to achieve with most forms of mechanical gyroscope, and one of the most crucial claims for the laser gyroscope was that "excellent linearity" had been achieved in the measurement of rotation rates as high as 1000°/second.[63]

Simultaneous with the attempts to improve the laser gyro practically and to make it the centerpiece of a reconfigured inertial system, a more sophisticated theoretical understanding of it was developing. Though many contributed, including Heer, the theoretical effort at Honeywell was led by Frederick Aronowitz, a physics graduate student hired by Killpatrick from New York University. Drawing on both classical electromagnetic theory and quantum mechanics, Aronowitz had by 1965 developed an elaborate mathematical theory of the operation of the laser gyro, a theory he continued to develop over the following years.[64]

By 1966, then, the laser gyroscope had been considerably refined from the earliest prototypes, a role for it and a solution to the main development problem had been found, and it was well understood theoretically. It was no longer restricted to the laboratory. Honeywell had a military contract with the Naval Ordnance Test Station at China Lake, California, to develop not a full inertial navigator but a prototype attitude reference system (orientation indicator) for launching missiles from ships. The laser gyro attitude reference system constructed by Honeywell was small and rugged enough to be operated while being transported by air to China Lake in September 1966, allowing Honeywell to claim the first flight test of a laser gyro system. The

Honeywell group's confidence was high: they were already able to measure rotation rates of 0.1°/hour, and they believed that "within a year" they would achieve the goal of measuring 0.01°/hour.[65]

That "year," however, stretched into almost a decade. At issue was not merely achieving the final order-of-magnitude increase in accuracy but increasing reliability (the working lifetimes of the early devices were typically less than 200 hours) and reducing size (though considerably smaller than the laboratory prototype, laser gyros were still typically larger than their mechanical competitors). Achieving these goals required ingenuity, considerable resources, and far more time than had been forecast: "the late sixties–early seventies were trying times." Even within Honeywell, the patience of higher management began to run out—"internal funding went almost to zero because one vice-president had something bad to eat or something"—and military funding, especially a contract from the Naval Weapons Center, was crucial in keeping development going.[66]

Almost every element in the laser gyro was refined and changed in the continuing Honeywell development effort: the material of the block (which was changed from quartz, through which the helium leaked, to the new glass ceramic Cer-Vit), the mirrors, the seals, the cathode, the quantum transition employed (which was shifted from 1.15 microns, in the infrared spectrum, to 0.63 microns, in the visible spectrum), the dither motor, and the output optics.

Slowly, these efforts bore fruit. By 1972, Cer-Vit, improved seals, and a new "hard coating" mirror fabrication process led to laser gyros that finally began to live up to the original promise of high reliability. This enabled Honeywell, rather than its competitors Sperry and Autonetics, to win the key contract from the Naval Weapons Center that helped permit resolution of the device's other problems. Worth $2.5 million, that contract was again not for a full inertial navigator but for prototypes of a more modest system for the guidance of tactical missiles. As these became more sophisticated, there was increasing interest in providing them with inertial guidance systems. The simplicity of strapdown, the fast reaction of the laser gyroscope (with no fluid to be heated or rotor to "spin up"), and the apparent insensitivity of the laser gyro to acceleration-induced errors all made laser systems seem an attractive option for such applications. At a time when pessimists had begun to doubt whether the laser gyro would ever achieve the "magic" figure of a 0.01°/hour error, its application to tactical missiles had the advantage of permitting drift rates much worse than that.[67]

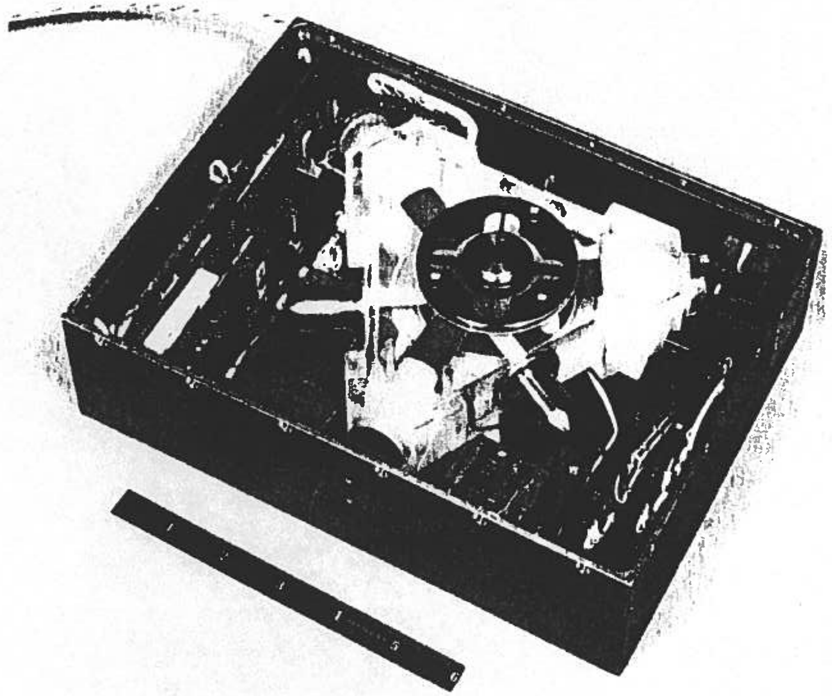

Figure 7
Early version of Honeywell GG 1300 laser gyro. The ruler (marked in inches) gives an indication of the device's size. Courtesy of Theodore J. Podgorski, Military Avionics Division, Honeywell Inc.

Yet 0.01°/hour, and with it the mainstream aircraft navigation market, remained the goal of the Honeywell team, particularly Killpatrick, and they continued to seek higher accuracy. In 1974, laser gyros finally began to demonstrate the 0.01°/hour error level in Honeywell's own laboratory tests. In February and March 1975, laboratory tests of prototype inertial systems delivered to the U.S. Navy under the tactical missile contract yielded an accuracy figure of 0.64 nautical miles per hour—far beyond the demands of that contract, and indeed better than the one-nautical-mile criterion for an aircraft navigator.[68]

In May 1975, Honeywell submitted an inertial navigation system based around its new GG1300 laser gyro (figure 7) for flight testing at the most authoritative military test center, the Central Inertial Guidance Test Facility at Holloman Air Force Base in New Mexico. Its accuracy was assessed there in flight as well as in laboratory tests. The official report on the tests concluded that they "demonstrated the successful application of ring laser gyros to strapdown inertial navigation system technology," and that the Honeywell system "appears to be better than a 1 nautical mile per hour navigator."[69]

The Laser Gyro Revolution

It was a turning point. Quiescent laser gyro programs at other inertial suppliers were infused with resources even before the successful tests—whose likely significance was underlined in January 1975 by Philip J. Klass in *Aviation Week*. Several firms outside the traditional inertial business also began laser gyroscope development, seeing an opportunity to break into the market.[70] After the excitement of the early 1960s and the long struggle of the late 1960s and the early 1970s, the laser gyro had finally proved itself a competitor to its established mechanical rivals.

Yet even this success was not, on its own, sufficient to ensure the laser gyro's future. Its test accuracy and reliability, though now acceptable, by no means surpassed those of contemporary mechanical gyroscopes, and its cost advantages were "projected."[71] Only prototypes had been built.

Military interest in the United States was nevertheless keen. A Ring Laser Gyro Navigator Advanced Development Program was set up within the Naval Air Systems Command to further refine and evaluate the Honeywell system. Funding increased sharply as the technological focus began to shift from performance to production. A tri-service (Army, Navy, Air Force) laser gyro manufacturing and producibility program provided Honeywell with $8 million. Honeywell's competitors benefited too, as the armed services, fearing future dependence on a single supplier, also funded work at Sperry, Litton, and elsewhere.[72]

Despite this support, however, a military market for the laser gyroscope opened up only in the mid 1980s, several years behind the civil market. The delay was due in part to remaining performance difficulties. By the late 1970s, the U.S. Air Force was demanding from fighter aircraft inertial navigators an error rate of 0.8 nautical miles per hour. Given the often violent maneuvers of military aircraft, which impose a greater strain on a strapdown system than the gentle flight path of an airliner, this remained a demanding goal when combined with strict limits on the size and weight of operational (rather than test) inertial systems. The accuracy specifications for bomber navigation were tighter still. Furthermore, a military aircraft navigator must provide information not just on position but also on velocity for accurate bombing or missile launches. In 1980, after the device's breakthrough into the civil market, Major General Marc Reynolds told the Joint Services Data Exchange Group for Inertial Systems that, in the Air Force's opinion, the laser gyro "does not yet have the velocity accuracy required for fighter aircraft." Another problem (at least as seen from Honeywell) was that the U.S. military was less centralized in its decision making than the civil aviation world: "If you deal with Boeing, at some point you're going to find a . . . man who is empowered to make a decision. If you go to the Air Force, you can never find a guy who is going to make a decision. You can find advocates . . . but you can't find a decision maker."[73]

Boeing was, in fact, central to the most crucial decision in the laser gyro revolution. In the late 1970s, Boeing was designing two new airliners: the 757 and the 767. Mechanical gyro inertial navigation systems had proved their worth on the long-range 747 "jumbo jet." Though the 757 and the 767 were to be smaller, medium-range planes, Boeing engineers believed that there was a role for strapdown inertial systems on them, especially if the orientation information they provided was used to eliminate the previously separate attitude and heading reference system.

These engineers became enthusiasts for the laser gyro. The 757 and the 767 were to be the most highly computerized civil aircraft yet built by Boeing, and the laser gyro's digital output would fit in well with this vision. The laser system's fast reaction reduced the risk that a takeoff would be delayed because the inertial navigator was not ready for use. Its promise of high reliability was attractive in an airline environment that was conscious not only of the initial cost of buying a system but also of the cost of maintaining and repairing it over its lifetime. Finally, the sheer glamour of the laser gyro was appropriate to the "high-tech" image that Boeing was cultivating for the new planes.

An informal alliance developed between proponents of the laser gyro within Honeywell and Boeing. Both groups knew that winning a commitment from Boeing to the laser gyro required an equally visible prior commitment from Honeywell. Specifically, Honeywell had to build a laser gyro production facility, in advance of any contract to sell the device, and this would require a large and apparently risky corporate investment. (The military funding, though helpful, fell far short of what was needed to build such a facility.) The night before a crucial meeting with Honeywell's top managers, Boeing and Honeywell engineers met at the house of a Honeywell engineer to prepare. Next day, as planned, the Boeing engineers emphasized the need for Honeywell investment: "Honeywell had got to put some money into that laser stuff or we're never going to put it on the airplane."[74]

This informal alliance succeeded in its twin tasks. Honeywell's top management was persuaded that the risk of investment in a laser gyro production facility was worthwhile, and Boeing's top management was persuaded of the virtues of a laser system for the 757 and the 767. More than the two managements needed convincing, however. New-generation avionics specifications are decided not by the manufacturer alone but by a wider semiformal body, which includes representatives of all the main aircraft manufacturers, the avionics companies, and the airlines. The Airlines Electronic Engineering Committee, as it is known, is a section of ARINC (Aeronautical Radio, Incorporated), created in December 1929 by the U.S. airlines to provide radio communications with aircraft. Despite the apparently *ad hoc* nature of the arrangement and the considerable potential for conflict of interest, the system works remarkably smoothly to define "Characteristics"—agreed understandings of the function, performance, physical dimensions, and interfaces of avionics equipment.[75] To seek to market a new system in advance of a Characteristic, or in violation of it, would be self-defeating.

The laser gyroscope was able to meet any plausible accuracy requirement. Extremely high accuracy has never been demanded in civil air inertial navigation; average error as great as 2 nautical miles per hour is acceptable. Rather, the crucial aspect of the Characteristic was physical size. (The weight of laser systems was also an issue, but it was around size that debate crystallized.) State-of-the-art mechanical systems, using sophisticated "tuned rotor" designs, were substantially smaller than the Honeywell laser gyroscope system, despite the continuing efforts to make the latter smaller. If the manufacturers and the airlines opted to save physical space by adopting a small box size, the laser gyro would be ruled out and the new mechanical systems would triumph by default.

"We met individually with every guy on the committee," recalls Ron Raymond of Honeywell. The crucial 1978 meeting was held in Minneapolis, where Honeywell is based. Some 300 delegates were present. Honeywell bought advertising space at airline gates throughout the country, "getting our message to the guys coming out on the planes."[76]

Honeywell carried the day on size, obtaining in the key specification, ARINC Characteristic 704, a box size 25 percent larger than what was needed to accommodate the mechanical systems. Because nothing prevented manufacturers and airlines from opting for mechanical systems,

a pricing battle had also to be won. Bolstered by what turned out, for the reasons outlined above, to be a grossly optimistic (or at least premature) forecast of a market for 12,000 laser gyro systems in military aircraft, Honeywell priced its civil laser gyro system very keenly.

Honeywell's laser gyro system was selected for the 757 and the 767. With the predicted military market slow to appear and the production costs higher than anticipated, quick profits were not to be found. The financial details are confidential, but the industry's consensus in the mid 1980s was that Honeywell had yet to recoup its investment in the laser gyro. (U.S. law permits such an investment to be set against corporate taxes, which reduces the effect of any loss on a large, diversified corporation such as Honeywell.)

Although profits were slow in coming, market share was not. Despite fierce competition from Litton Industries, including legal battles over alleged patent and antitrust violations, Honeywell has secured a dominant share of the world's market for inertial navigation systems in civil aircraft (around 50 percent by the mid 1980s, and perhaps 90 percent by 1990).[77]

During the latter part of the 1980s, the laser gyro also established Honeywell firmly in the military market for inertial navigation. In 1985 the U.S. Air Force began to make large purchases of laser gyro systems, selecting Honeywell and Litton as competitive suppliers of laser inertial navigation units for the C-130, the RF-4, the F-4, the EF-111, and the F-15.[78] International military sales climbed rapidly as laser systems became standard on new military aircraft and as the retrofitting of older planes increased. In the United States, Honeywell, Litton (the previously dominant supplier of mechanical gyro systems for military aircraft), and Kearfott (now a division of the Astronautics Corporation of America) competed vigorously for the military market.

The form taken by competition in the market for inertial systems, both civil and military, changed during the 1980s. At the beginning of the decade, laser systems were striving to establish a foothold in a market dominated by mechanical systems. By the end of the decade, competition was almost always between laser systems offered by different companies. Although Sperry developed and sold several laser devices, it never successfully entered the air navigation market, and in 1986 the Sperry Aerospace Group was bought by Honeywell. Litton began a low-level laser gyro effort in 1973. In mid 1974, under the leadership of Tom Hutchings, the program was expanded. By the end of 1980 Litton had achieved satisfactory flight test results with its laser gyro system. Though its work lagged behind that of Honeywell, the desire of airlines to avoid dependence on a single supplier helped a Litton laser system win the next major civil air transport contract, for the Airbus Industrie A310.[79] Kearfott also developed laser systems, as did all but one of the other U.S. suppliers of inertial systems, the European firms, and Japan Aviation Electronics Industry, Limited.

With the exception of Sperry, which continued to use electro-optical biasing, the laser systems developed by these other firms generally followed the main features of Honeywell's design. There were differences, such as Litton's use of a square path with four mirrors rather than a triangular path with three, but the monolithic solid-block design and the use of dither supplemented by noise predominated. Honeywell's patents on these features did not prevent their use by other firms. Honeywell sued Litton for alleged patent infringement, but the action was settled out of court, and other firms seem to have been able to employ these features with impunity.[80]

The success of the laser gyro during the 1980s cannot be attributed to its exceeding its mechanical competitors in accuracy, although by the end of the decade the accuracy advantage of mechanical systems was eroding as substantial U.S. military research and development funds were devoted to improving the laser gyro and development money for mechanical gyros diminished. In 1984 Honeywell received $60.9 million, and Litton $74.8 million, to develop laser gyro guidance systems for a proposed new U.S. missile, the Small ICBM. Success in this would have been an enormous step toward acceptance of the laser gyro, since self-contained prelaunch alignment of a ballistic missile guidance system to the accuracy required of the Small ICBM is extraordinarily demanding of gyroscope performance. Error rates between 0.0001° and 0.00001° per hour are needed, rather than the 0.01°/hour of aircraft navigation. The former figures are close to what is held to be a physical limit on the performance of laser gyroscopes roughly comparable in size to mechanical gyros—a limit arising ultimately from quantum effects. In the end, though, the Air Force, advised by the Draper Laboratory (formerly the MIT Instrumentation Laboratory), concluded that the laser system could not provide the requisite accuracies and opted to modify the existing mechanical gyro guidance system of the MX.[81]

Nor did the laser gyro turn out (at least in the short term) to possess the clear advantage over mechanical gyros in cost of production that had been hoped for.[82] Rather, reliability has been the major claimed (and widely accepted) advantage of the laser gyro. A typical Honeywell

advertisement contrasted the 8000 hours mean time between failures achieved by its laser system on the Boeing 757 and 767 with the much lower mean times between failures achieved by its competitors' previous-generation mechanical systems in military aircraft.[83]

There are still skeptics, however, even on the question of reliability. They argue that it is unfair to contrast civil systems with traditionally less reliable military ones; that the large box size won by Honeywell meant that the laser system worked at a lower temperature than mechanical ones, and temperature was the crucial determinant of failure; that Honeywell engaged in extensive preventive maintenance, especially mirror replacement, to keep the mean time between failures high; that modern mechanical gyros are as reliable as laser gyros; and that the main determinant of a system's reliability is the electronic components (which were more modern and thus more reliable in the Honeywell system than in its older competitors), not the gyros.[84] These counterarguments counted for little, however, as the laser gyro revolution became irreversible. The skeptics worked for firms that had seen no alternative to heavy investment in laser gyroscopes, and even they did not disagree with that decision. As one proponent of the laser gyro put it: "Anyone who wants to play in the future has got to have a laser gyro. Spinning iron won't do any more. Even if spinning iron was truly better, you can't do it—it doesn't have the technology charisma."[85]

Often the decision seems to have been an either/or one: commitment to the laser gyro meant a reduction in support for continued development of mechanical devices. At Kearfott, for example, research was focused in the early 1970s on a sophisticated new mechanical design, the Virex gyro. Its development was going well, but when Kearfott's vice-president of engineering heard of Honeywell's success with the laser gyro he insisted that the Virex work be stopped and that the resources be devoted to the laser gyro instead.[86]

The one major firm to stand aside from the laser gyro revolution has been the Delco Division of General Motors. As AC Spark Plug, Delco pioneered inertial navigation for civil aviation. Its Carousel system, based on traditional spinning-wheel gyros and used in the 747, was the first successful system of its kind. During the mid 1960s, Delco researchers had become interested in the idea of a "hemispherical resonator gyro" (figure 8). (The device is analogous to a ringing wine glass; it senses rotation through changes in vibration patterns.) When other firms set up or revived their laser programs in the mid 1970s, Delco instead devoted resources to the resonator gyro. Delco believes the

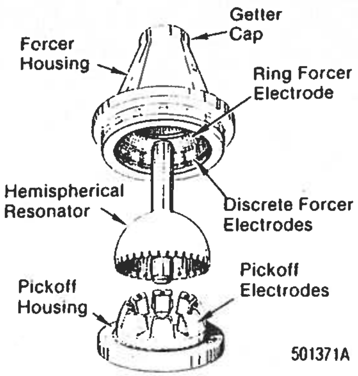

Figure 8
Hemispherical Resonator Gyro. Courtesy David Lynch, Delco Systems Operations, General Motors Corporation.

hemispherical resonator gyro to have even greater reliability than the laser gyro, together with an important military advantage: lack of susceptibility to the electromagnetic pulse from a nuclear explosion.[87]

Like Warren Macek with the first laser gyro 30 years before, Delco's researchers understand why it can be better for technologists to have competitors also seeking to develop the same device: that makes it easier to "keep management on board."[88] Unlike Macek, however, they have not succeeded in generating competitors. The fate of their solitary dissent from the laser gyroscope revolution remains to be seen.

Conclusion

Several issues concerning the relationships among science, technology, and society emerge from the history of the laser gyroscope. There was no direct path from "science" (the ether experiments of Sagnac and Michelson) to "technology" (the laser gyroscope). The crucial intermediary was the development of quantum electronics, a field that involved fundamental physics but did not fit the traditional stereotype of "pure science." The "greater and rapidly growing part of quantum electronics owed its very existence to wartime radar work,"[89] and its postwar direction was still shaped by technological concerns and at least to some extent by military interests. The development of the laser gyroscope (and quantum electronics more generally) may best be seen as what Bruno Latour calls "technoscience"—the construction of an interconnected network of elements of "science," "technology," and "social processes" or "social interests."[90]

No single element of this network was able to write the script of the story of the laser gyroscope. "Science" did not determine "technology": the meaning of the "Sagnac effect," for example, was radically transformed in its passage from being a claimed proof of the existence of the ether to being the oft-cited foundation of the laser gyroscope. Neither, however, was there any internal logic of technological change that led of necessity from the mechanical to the optical sensing of rotation. Inertial navigation's "founding father," Charles Stark Draper, and the researchers at Delco saw the path of technical evolution quite differently, and it would be rash to assert that either was definitely wrong.

Nor did social processes and interests have free rein: they had to interact with an only partially tractable material world. The members of the Honeywell team were adroit engineers of social support (from their management and the military) as well as of cavities and mirrors, yet what is most impressive about what they did is their persistence in the face of obstacles they could shift only slowly. The successful development of the laser gyroscope (and perhaps even its invention) is hard to imagine without the U.S. military, yet the resultant technology was not shaped (initially, at least) by specifically military needs. Indeed, where those needs are most specific—in the guidance of strategic ballistic missiles, with its extreme demands for accuracy—the laser gyroscope has not met with success, and it was accepted in military aviation only after its triumph in the civil sphere.

Similarly, despite the central importance of economic phenomena—markets, profits, and the like—to the history of the laser gyroscope, the history cannot be told in the terms of orthodox neoclassical economics, with its all-seeing, unitary, rationally maximizing firms. Honeywell, the central firm in the story, was not all-seeing: the laser gyroscope proponents within Honeywell had to work to keep their vision of the future in front of the eyes of senior management. Neither was Honeywell (or Sperry, or other firms) unitary: the story of the laser gyroscope cannot be understood without understanding the tensions between engineers and their senior managers, or the informal alliances that can develop between staff members of different firms (notably Honeywell and Boeing). Nor was Honeywell in any demonstrable sense a rational maximizer. Profit calculations were certainly prominent in the decisions of senior managers, but the data on which the crucial early calculations were based (particularly the estimates of production costs and the size of the market for the laser gyroscope) appear in retrospect to have been little better than guesses (brave and consequential guesses though they were).

If an economic theory of the laser gyroscope revolution is sought, then the neoclassical economists, with their assumption of finely tuned optimization, are less relevant than Joseph Schumpeter, who emphasized product-based rather than price-based competition, "gales of creative destruction," and what John Maynard Keynes called the "animal spirits" of entrepreneurs. Although they were corporate rather than individual entrepreneurs, the Honeywell staffers possessed those "spirits" in good measure. They aimed high, they took risks, and they knew that to achieve their goal they had to shape the market as well as meet its demands (as is demonstrated by their intensive lobbying to secure a Characteristic that the laser gyro could meet).[91]

The history of the acceptance of the laser gyroscope reveals at least one interesting facet of the dynamics of "technological revolutions."[92] It is difficult to attribute the device's success to any unambiguously inherent technological superiority over its rivals. It has not yet succeeded in ousting mechanical systems in applications that demand the greatest accuracy; the hopes that it would be much cheaper to make were unfulfilled for a long time; and its claims to intrinsically superior reliability, though highly influential, are not universally accepted. Until recently, laser systems have been bulkier and heavier than mechanical systems of comparable accuracy. The laser gyro's digital output and its compatibility with the simpler strapdown configuration of inertial systems gave it a certain "systemic" advantage, but even that is not unique. The analog output of other devices can be digitized. Compatibility with strapdown was one of the main initial attractions of the electrostatically suspended gyro; dynamically tuned mechanical gyros have been developed for strapdown configurations, and the hemispherical resonator gyro has been used in a strapdown system. Other varieties of gyro also offer quick startup.

There is a sense, however, in which the intrinsic characteristics of different gyroscope technologies are irrelevant. What matters in practice are the *actual* characteristics of such technologies and the systems built around them, and these reflect to a considerable degree the extent of the development efforts devoted to them.

There is thus an element of self-fulfilling prophecy in the success of the laser gyroscope. In the pivotal years of the revolution (from 1975 to the early 1980s), firms in the business of inertial navigation had to make a hard decision on the allocation of development funds. Was a technological revolution about to occur? Would they be able to compete in the mid or late 1980s without a laser gyroscope? All but Delco decided that the revolution was likely and that the risk of not having a laser gyroscope was too great. Accordingly, they invested heavily in the development of laser gyroscopes and systems incorporating them while cutting back or even stopping development work on mechanical gyroscopes and systems. And some firms without mechanical gyroscope experience began laser programs in anticipation of the revolution.

The result was a rapid shift in the balance of technological effort—even by 1978, "optical rotation sensor . . . technology [was] being pursued more broadly for inertial reference systems applications than any other sensor technology"[93]—that helped make the laser gyroscope revolution a reality. By the end of the 1980s, laser gyro systems were beginning to seem unequivocally superior to their traditional mechanical rivals, at least in aircraft navigation. Proponents of traditional mechanical systems claim that with equivalent development funds they could still match or outstrip laser systems; however, the argument has become untestable, as no one is now prepared to invest the necessary sums (tens of millions of dollars) in further development work on traditional systems.

There is nothing pathological in this aspect of the laser gyro revolution. The outcome of a political revolution, after all, depends in part on people's beliefs about whether the revolutionaries or the established order will be victorious, and on the support the different parties enjoy as a consequence. Indeed, it has been argued, convincingly, that all social institutions have the character of self-fulfilling prophecies.[94] Technology is no exception, and the role of prediction and self-fulfilling prophecy in technological change, especially technological revolution, is surely worthy of particular attention.

Acknowledgments

The interviews drawn on here were made possible by a grant from the Nuffield Foundation for research on "the development of strategic missile guidance technology" (SOC442). Their further analysis was part of work supported by the Economic and Social Research Council under the Programme on Information and Communication Technologies (A35253006) and the Science Policy Support Group Programme of Science Policy Research in the Field of Defence Science and Technology (Y307253006). I am grateful to Wolfgang Rüdig for assistance in the research.

55. Coombs et al., *Economics and Technological Change*, pp. 6–7.

56. This is the central theme of an old but still valuable paper by Donald Schon, "The Fear of Innovation," as reprinted in *Science in Context: Readings in the Sociology of Science*, ed. B. Barnes and D. Edge (MIT Press, 1982). The original discussion is to be found in Frank H. Knight, *Risk, Uncertainty and Profit* (Houghton Mifflin, 1921).

57. There is no absolute way the distinction can be made *ex ante*. See Schon, "The fear of innovation," pp. 293–294.

58. I have argued elsewhere that there is a productive analogy to be drawn between the testing of technology and scientific experiment as analyzed by the sociology of scientific knowledge. See Donald MacKenzie, "From Kwajalein to Armageddon? Testing and the social construction of missile accuracy," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. D. Gooding et al. (Cambridge University Press, 1989).

Chapter 4

1. See the following papers in *Technology and Culture* 17 (July 1976): Thomas P. Hughes, "The development phase of technological change: Introduction"; Lynwood Bryant, "The development of the diesel engine"; Thomas M. Smith, "Project Whirlwind: An unorthodox development project"; Richard G. Hewlett, "Beginnings of development in nuclear technology"; Charles Susskind, "Commentary." See also John M. Staudenmaier, *Technology's Storytellers: Reweaving the Human Fabric* (MIT Press, 1985), pp. 45–50. There are, of course, definite limits to the usefulness of dividing the process of technological change into separate phases of "invention," "development," "innovation," and "diffusion." Much important "invention," for example, takes place during "diffusion." See, for example, James Fleck, "Innofusion or diffusation? The nature of technological development in robotics," paper presented to workshop on Automatisation Programmable: Conditions d'Usage du Travail, Paris, 1987.

2. Much of the funding of laser gyroscope development in the United States (and elsewhere), like that of the laser itself, was conducted under military auspices and was thus subject to varying degrees of security classification. The recent Laser History Project addressed this problem by having two separate researchers, one using open materials, the other conducting classified interviews and working with classified archives: see Joan Lisa Bromberg, *The Laser in America, 1950–1970* (MIT Press, 1991), p. xii. As a foreign national, without security clearance, I have had to work solely with unclassified materials and have not, for example, enjoyed access to the holdings of the Defense Technical Information Center. However, some defense sector documents on the laser gyroscope have never been classified, and some originally classified material has now been cleared for public release. See the bibliographies produced by the National Technical Information Service—for example, Laser Gyroscopes (September 70–January 90): Citations from the NTIS Bibliographic Database (1990)—although these are far from comprehensive. I am grateful to interviewees, particularly Professor Clifford V. Heer, for providing me with otherwise inaccessible documents from the early years of the laser gyroscope. Heer's own "History of the laser gyro" (*SPIE* [Society of Photo-Optical Instrumentation Engineers] 487 (1984) [*Physics of Optical Ring Gyros*]: 2–12) was of considerable help to me in preparing this chapter. The documentary record, though important, is not on its own sufficient to convey an understanding of the history of the laser gyro. This is not a result of security classification alone; it turns out to be equally the case for parts of the history where there is no direct military involvement, such as the adoption of the laser gyro in the civil market. Indeed, commercial confidentiality was, if anything, a greater constraint on the gathering of documentary sources for this paper than military classification. Therefore, essential to what follows are interviews with surviving pioneers of the laser gyroscope (and its competitor technologies). These interviews were cross-checked for mutual consistency and for consistency with documentary sources; several interviewees were also kind enough to comment by letter on the draft of this article.

3. O. Lodge, *Ether and Reality: A Series of Discourses on the Many Functions of the Ether of Space* (Hodder and Stoughton, 1925), p. 179. I owe the reference to Brian Wynne, "Physics and psychics: Science, symbolic action and social control in late Victorian England," in *Natural Order: Historical Studies of Scientific Culture*, ed. B. Barnes and S. Shapin (Sage, 1979). See also David B. Wilson, "The thought of late Victorian physicists: Oliver Lodge's ethereal body," *Victorian Studies* 15 (September 1971): 29–48, and *Conceptions of Ether: Studies in the History of Ether Theories, 1740–1900*, ed. G. Cantor and M. Hodge (Cambridge University Press, 1981).

4. See L. S. Swenson, Jr., *The Ethereal Aether: A History of the Michelson-Morley-Miller Aether-Drift Experiments, 1880–1930* (University of Texas Press, 1972).

5. If two beams from the same source of light cross, in the region of their crossing they sometimes reinforce each other and sometimes cancel each other out. The phenomenon, known as "interference," can be seen in the distinctive pattern of light and dark areas, called "fringes," thus produced. In an interferometer, such interference is deliberately created under closely controlled conditions. An interferometer can be used for optical experiments and also, for example, for highly accurate measurements of length. Interference is discussed in any text of physical optics: see, e.g., chapter 13 of F. A. Jenkins and H. E. White, *Fundamentals of Optics*, third edition (McGraw-Hill, 1957), which contains a good description of the Michelson interferometer.

6. A. A. Michelson and E. W. Morley, "Influence of motion of the medium on the velocity of light," *American Journal of Science*, third series, 31 (May 1886): 377–386; "On the relative motion of the Earth and the luminiferous æther," *Philosophical Magazine*, fifth series, 24 (December 1887): 449–463. The latter paper appeared also in *American Journal of Science*, third series, 34 (November 1887): 333–345.

7. Gerald Holton, "Einstein, Michelson, and the 'crucial' experiment," *Isis* 60 (summer 1969): 133–297.

8. See Swenson, *Ethereal Aether.*

9. This was suggested by Michelson and Morley themselves ("On the Relative Motion," pp. 458–459). Another possible explanation, reported by Oliver Lodge, was "suggested by Professor [George F.] Fitzgerald, viz., that the cohesion force between molecules, and, therefore, the size of bodies, may be a function of their direction of motion through the ether; and accordingly that the length and breadth of Michelson's stone supporting block were differently affected in what happened to be, either accidentally or for some unknown reason, a compensatory manner." See Lodge, "Aberration problems—a discussion concerning the motion of the ether near the Earth, and concerning the connexion between ether and gross matter; with some new experiments," *Philosophical Transactions*, series A, 184 (1893), pp. 749–750. Elaborated by the Dutch theoretical physicist H. A. Lorentz, this suggestion became known as the Lorentz-Fitzgerald contraction hypothesis.

10. G. Sagnac, "L'éther lumineux démontré par l'effet du vent relatif d'éther dans un interféromètre en rotation uniforme," *Comptes Rendus* 157 (1913): 708–710; Sagnac, "Effet tourbillonnaire optique: La circulation de l'éther lumineux dans un interférographe tournant," *Journal de Physique*, fifth series, 4 (March 1914): 177–195. Both Lodge and Michelson had earlier suggested the use of rotation to detect the ether, but neither had actually performed their proposed experiments. See Oliver Lodge, "Experiments on the absence of mechanical connexion between ether and matter," *Philosophical Transactions*, series A, 189 (1897): 149–166; A. A. Michelson, "Relative motion of Earth and æther," *Philosophical Magazine*, sixth series, 8 (December 1904): 716–719. The first actual experiment along these lines—using a ring of glass prisms, rather than the Earth—was described by Franz Harress in a 1911 Jena University doctoral thesis, published as *Die Geschwindigkeit des Lichtes in bewegten Körpern* (Erfurt, 1912). Harress's work, however, remained relatively unknown; for descriptions see B. Pogány, "Über die Wiederholung des Harress-Sagnacschen Versuches," *Annalen der Physik*, fourth series, 80 (1926): 217–231; Pogany, "Über die Wiederholung des Harresschen Versuches," *Annalen der Physik*, fourth series, 85 (1928): 244–256; André Metz, "Les problèmes relatifs à la rotation dans la théorie de la relativité," *Journal de Physique et le Radium* 13 (April 1952): 224–238. Note that some Anglo-Saxon writers have not had access to Harress's original work. E. J. Post ("Sagnac Effect," *Reviews of Modern Physics* 39 (April 1967): 475–495) writes: "Harress' objective was quite different from Sagnac's. Harress wanted to measure the dispersion properties of glasses . . . and he felt that a ring interferometer would be a suitable instrument." In fact, Harress's concern with questions of the ether and relativity is clear (see *Geschwindigkeit des Lichtes*, pp. 1–7).

11. Sagnac, "L'éther lumineux démontré," pp. 709–710.

12. Michel Paty, "The scientific reception of relativity in France," in *The Comparative Reception of Relativity*, ed. T. Glick (Boston Studies in the Philosophy of Science, volume 103 (Reidel, 1987)), pp. 113, 116.

13. P. Langevin, "Sur la théorie de relativité et l'expérience de M. Sagnac," *Comptes Rendus* 173 (1921): 831–835.

14. "La grandeur et le sens absolu du déplacement des franges sont conformes à la théorie de l'éther immobile de Fresnel et en constituent une vérification": Daniel Berthelot et al., "Prix Pierson-Perrin," *Comptes Rendus* 169 (1919), p. 1230; Alexandre Dufour et Fernand Prunier, "Sur l'observation du phénomène de Sagnac par un observateur non entraîné," *Comptes Rendus* 204 (1937): 1925–1927.

15. Swenson, *Ethereal Aether*, pp. 182, 237. An example is a skeptical footnote on p. 298 of L. Silberstein's "The propagation of light in rotating systems," *Journal of the Optical Society of America* 5 (July 1921): 291–307; see Heer, "History," p. 2.

16. Here I draw on Joseph Killpatrick's account of the relativistic explanation for a nonspecialist audience: "The laser gyro," *IEEE Spectrum* 4 (October 1967), p. 46.

17. A. A. Michelson and Henry G. Gale, "The effect of the Earth's rotation on the velocity of light, part II," *Astrophysical Journal* 61 (April 1925), p. 142.

18. Ibid., p. 143; Michelson as quoted in Swenson, *Ethereal Aether*, p. 218.

19. John E. Chappell, Jr., "Georges Sagnac and the discovery of the ether," *Archives Internationales d'Histoire des Sciences* 18 (1965), pp. 178, 190.

20. Sagnac, "Effet tourbillonnaire optique," p. 191.

21. On gyroscope work in this period see Thomas P. Hughes, *Elmer Sperry: Inventor and Engineer* (Johns Hopkins University Press, 1971).

22. D. MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (MIT Press, 1990).

23. David D. Dean to C. V. Heer, February 26, 1962.

24. Ibid.

25. See *Quantum Electronics: A Symposium*, ed. C. Townes (Columbia University Press, 1960); Townes, "Ideas and stumbling blocks in quantum electronics," *IEEE Journal of Quantum Electronics*, 20 (1984): 547–550; J. L. Bromberg, *The Laser in America*; Bromberg, "Engineering knowledge in the laser field," *Technology and Culture* 27 (October 1986): 798–818; Robert W. Seidel, "From glow to flow: A history of military laser research and development," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 111–147; Seidel, "How the military responded to the laser," *Physics Today* (October 1988): 36–43; Paul Forman, "Behind quantum electronics: National security as basis for physical research in the United States," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149–229.

26. Henry A. H. Boot and John T. Randall, "Historical notes on the cavity magnetron," *IEEE Transactions on Electron Devices* 23 (July 1976): 724–729.

27. Bromberg, *The Laser in America*.

28. R. W. Ditchburn, *Light* (Blackie, 1952), pp. 337–340. The role of Ditchburn's text was confirmed to me by the two survivors of the trio: Clifford V. Heer

(telephone interview, March 27, 1985) and Warren Macek (interview, Great Neck, N.Y., April 5, 1985).

29. V. Heer to D. J. Farmer, Measurement of Angular Rotation by the Interference of Electromagnetic Radiation in Rotating Frames, Space Technology Laboratories interoffice correspondence, September 30, 1959, pp. 1, 3; interview with Heer conducted by Wolfgang Rüdig, Columbus, Ohio, January 19–21, 1987.

30. C. V. Heer, Measurement of Angular Velocity by the Interference of Electromagnetic Radiation in the Rotating Frame, Space Technology Laboratories Disclosure of Invention form, October 8, 1959. No actual patent was taken out, according to Heer, because staff members at Space Technology Laboratories "just didn't believe it would work" (interview with Heer by Rüdig). A fuller version of Heer's ideas was circulated as Measurement of Angular Velocity by the Interference of Electromagnetic Radiation in the Rotating Frame, Space Technology Laboratories Technical Memorandum STL/TM-60-0000-09007.

31. Heer, Disclosure of Invention, p. 2. With the invention of optical fiber, it later became possible to use this latter technique for light in the "fiber optic gyroscope." This device, though important in some fields (notably tactical missile guidance), has generally been held to be insufficiently accurate for aircraft inertial navigation, which has been the main market for the laser gyro.

32. C. V. Heer, "Interference of electromagnetic and matter waves in a nonpermanent gravitational field." The full paper was never published, but the abstract appears in *Bulletin of the American Physical Society* 6 (1961): 58. Heer submitted a longer version entitled "Interference of matter-waves in a non-permanent gravitational field" to *Physical Review*, but the paper was rejected on the basis of a referee's report that "the effect would be exceedingly difficult to observe," and that it "would not be very interesting" because of its analogy to the Sagnac-Michelson work and the theoretical ambiguity of the latter (report appended to letter from S. A. Goudsmit to C. V. Heer, September 18, 1960).

33. Research proposal, Ohio State University Research Foundation, March 16, 1961.

34. C. V. Heer to John E. Keto, October 26, 1961; Heer, "Interference of electromagnetic waves and of matter waves in a nonpermanent gravitational field," sent to NASA, Air Force Office of Scientific Research and Office of Naval Research, January 29, 1962. According to Heer ("History," pp. 3–4), this extension of his earlier paper was prompted by "the first indication of interest [in his proposal] by Dr. Chiu and Dr. Jastrow of the Institute for Space Studies on 5 January 1962."

35. A. H. Rosenthal, "Regenerative circulatory multiple-beam interferometry for the study of light propagation effects," *Journal of the Optical Society of America* 51 (December 1961): 1462. This is the abstract of the paper read by Rosenthal to the Optical Society's annual meeting. An extended version appeared in *Journal of the Optical Society of America* 52 (October 1962): 1143–1148.

36. Adolph H. Rosenthal, Optical Interferometric Navigational Instrument, U.S. Patent 3,332,314, July 25, 1967, filed April 8, 1963. A further patent from this period is Jack B. Speller, Relativistic Inertial Reference Device, U.S. Patent 3,395,270, July 30, 1968, filed June 28, 1962, which cites the experiments by Sagnac and Michelson and describes a range of devices that "utilize . . . the principles of the general relativity theory of Albert Einstein in order to detect angular motion by means of energy circulating in a loop path subjected to angular motion having a component in the plane of the loop." Speller went on to construct at least one such device, and he described its operation to a classified conference in 1963. It was not a laser gyroscope, though, but a "coaxial cable resonant cavity a few feet in length and with tunnel diode amplifiers," according to Heer ("History," p. 6).

37. Hughes, *Elmer Sperry*. Sperry Rand was formed in June 1955 by a merger of the Sperry Gyroscope and Remington Rand corporations.

38. Macek interview.

39. Aside from Macek, the group at Sperry working on the laser gyro consisted of Daniel T. M. Davis, Jr., Robert W. Olthuis, J. R. Schneider, and George R. White.

40. For descriptions see W. M. Macek and D. T. M. Davis, Jr., "Rotation rate sensing with traveling-wave ring lasers," *Applied Physics Letters* 2 (February 1, 1963): 67–68, and Philip J. Klass, "Ring laser device performs rate gyro angular sensor functions," *Aviation Week and Space Technology*, February 11, 1963: 98–103.

41. Macek and Davis, "Rotation rate sensing," p. 68.

42. For an explanation of this in terms of the general theory of relativity, see the introduction to this chapter.

43. Macek and Davis, "Rotation rate sensing," p. 68.

44. Heer, "History," pp. 5–6; P. K. Cheo and C. V. Heer, "Beat frequency between two traveling waves in a Fabry-Perot square cavity," *Applied Optics* 3 (June 1964): 788–789.

45. Robert C. Langford, "Unconventional inertial sensors," *Astronautica Acta* 12 (1966): 294–314.

46. Macek and Davis, "Rotation rate sensing"; Macek interview; Klass, "Ring laser"; *Aviation Week and Space Technology*, cover of issue of February 11, 1963. Klass coined the term "avionics" and was the leading journalist in that field.

47. Klass, "Ring laser," p. 98.

48. Macek interview. The main success enjoyed by Sperry laser gyro systems was in controlling the firing of naval guns. See R. W. McAdory, "Two decades of laser gyro development," in Proceedings of the Fourteenth Joint Services Data Exchange Group for Inertial Systems (Clearwater Beach, Florida, 1980).

49. There is a 1961 Soviet patent for the use for guidance purposes of laser light reflected by mirrors around a closed path, but the proposed device relies on displacement of the light beam on the surface of the mirrors rather than on the effect used in the laser gyro: B. N. Kozlov, Device for Stabilizing in Space the Course of a Moving Body, Soviet Patent 751787/26-10, November 15, 1961, described in *Soviet Inventions Illustrated* (February 1963), p. 22. (I owe the reference to Heer, "History," p. 19.) Early Soviet ring lasers are described in the following publications: S. N. Bagaev, V. S. Kuznetsov, Yu. V. Troitskii, and B. I. Troshin, "Spectral characteristics of a gas laser with traveling wave," *JETP Letters* 1 (1965): 114–116; I. L. Bershtein and Y. I. Zaitsev, "Operating features of the ring laser," *Soviet Physics JETP* 22 (March 1966): 663–667; E. M. Belenov, E. P. Markin, V. N. Morozov, and A. N. Oraevskii, "Interaction of travelling waves in a ring laser," *JETP Letters* 3 (1966): 32–34. Laser gyro work was being done in the U.K. in the 1960s at the Royal Aircraft Establishment, at the Services Electronic Research Laboratory, at the Admiralty Compass Observatory, and at EMI Ltd. (interviews with C. R. Milne and Sidney Smith, Farnborough, March 17, 1986; Michael Willcocks, Slough, March 19, 1986; Michael Wooley, Bracknell, March 21, 1986). Publicly available documentary records are scarce; two exceptions are A. F. H. Thomson and P. G. R. King, "Ring-laser accuracy," *Electronics Letters* 11 (November 1966): 417 and A. Hetherington, G. J. Burrell, and T. S. Moss, Properties of He-Ne Ring Lasers at 3.39 Microns, Royal Aircraft Establishment Technical Report 69099, Farnborough, Hampshire, 1969. French research began around 1964 in the laboratory of the Compagnie Générale d'Électricité and was then taken up by SFENA (Société Française d'Équipements pour la Navigation Aérienne), with military funding (interview with Bernard de Salaberry, Versailles, July 21, 1987).

50. For the work at Kearfott see Clement A. Skalski and John C. Stiles, Motion Sensing Apparatus, U.S. Patent 3,433,568, March 18, 1969, filed January 21, 1964. For that at Autonetics see F. Vescial, O. L. Watson, and W. L. Zingery, Ring Laser Techniques Investigation: Final Technical Report, report AFAL-TR-71-339, Air Force Avionics Laboratory, Wright-Patterson Air Force Base, Ohio, November 1971, and T. J. Hutchings, J. Winocur, R. H. Durrett, E. D. Jacobs, and W. L. Zingery, "Amplitude and frequency characteristics of a ring laser," *Physical Review* 152 (1966): A467–A473. For the work at Hamilton Standard see George Busey Yntema, David C. Grant, Jr., and Richard T. Warner, Differential Laser Gyro System, U.S. Patent 3,862,803, January 28, 1975, filed September 27, 1968. For that at the Instrumentation Laboratory see Joseph D. Coccoli and John R. Lawson, Gas Ring Laser Using Oscillating Radiation Scattering Sources within the Laser Cavity, U.S. Patent 3,533,014, October 6, 1970, filed June 4, 1968; Cynthia Whitney, "Contributions to the theory of ring lasers," *Physical Review* 191 (May 10, 1969): 535–541; Whitney, "Ring-laser mode coupling," *Physical Review* 191 (1969): 542–548.

51. Interview with Joseph Killpatrick, Minneapolis, March 7, 1985. Killpatrick recalls receiving an Air Force "request for proposal" for an investigation of the "Michelson-Gale effect." Being unable to figure out what this was, he called the relevant Air Force office; he was told that if they did not know what it was they

52. The Republic Aviation Corporation was a further sponsor, and the Army Missile Command and NASA were also involved in the setting-up of the series. See Proceedings of the 1964 Symposium on Unconventional Inertial Sensors (Farmingdale, N.Y.), p. vi. For the general background of the military's interest in inertial systems see MacKenzie, *Inventing Accuracy*.

53. Interview with Tom Hutchings, Woodland Hills, Calif., February 20, 1985; interview with Charles Stark Draper, Cambridge, Mass., October 2 and 12, 1984.

54. In 1984 their laser gyro work won these three, together with Warren Macek, the Elmer Sperry award for advancing the art of transportation.

55. Early military contracts for Honeywell's laser gyro work included $500,000 from the Army Missile Command, $110,000 from the Naval Ordnance Test Station, and $51,000 from the Army's Frankford Arsenal. NASA also contributed $106,000. See Philip J. Klass, "Laser unit challenges conventional gyros," *Aviation Week and Space Technology*, September 12, 1966: 105–113. In general, NASA's support for laser gyroscope development was more modest than that of the armed services. See Jules I. Kanter, "Overview of NASA programs on unconventional inertial sensors," in Proceedings of the 1966 Symposium on Unconventional Inertial Sensors.

56. Interview with John Bailey, Minneapolis, March 7, 1985; letter from Bailey, April 12, 1990.

57. Donald MacKenzie and Graham Spinardi, "The shaping of nuclear weapon system technology: US fleet ballistic missile guidance and navigation," *Social Studies of Science* 18 (August 1988): 419–463; 18 (November): 581–624.

58. Klass, "Ring laser," p. 98.

59. Theodore J. Podgorski, Control Apparatus, U.S. Patent 3,390,606, July 2, 1968, filed March 1, 1965. The Sperry and Honeywell work can be traced in the companies' reports to their military sponsors. See, e.g., Electro-Optics Group, Sperry Gyroscope Company Division, Sperry Rand Corporation, Electromagnetic Angular Rotation Sensing: Final Report, report ALTDR 64-210, Air Force Systems Command, Research and Technology Division, Wright-Patterson Air Force Base, August 1964; Honeywell Inc., Aeronautical Division, Three-Axis Angular Rate Sensor, quarterly report 20502-QR1, Army Missile Command, Redstone Arsenal, Alabama, July 15, 1966.

60. W. M. Macek et al., "Ring laser rotation rate sensor," in Proceedings of the Symposium on Optical Masers (New York, 1963); q.v. Robert Adler, "A study of locking phenomena in oscillators," *Proceedings of the Institute of Radio Engineers and Waves and Electrons* 34 (June 1946): 351–357. Frederick Aronowitz (interview, Anaheim, Calif., February 27, 1985) cited Ali Javan, developer of the gas laser, as first having suggested this explanation in a private conversation.

61. Aronowitz interview; see Joseph E. Killpatrick, Laser Angular Rate Sensor, U.S. Patent 3,373,650, March 19, 1968, filed April 2, 1965. Dither was not

Killpatrick's first attempt to find a solution to lock-in—see Killpatrick, Apparatus for Measuring Angular Velocity having Phase and Amplitude Control Means, U.S. Patent 3,323,411, June 6 1967, filed June 29, 1964.

62. The results of the dither experiments were not presented publicly until 1971: Frederick Aronowitz, "The laser gyro," in *Laser Applications*, volume 1, ed. M. Ross (Academic Press, 1971). On noise see Joseph E. Killpatrick, Random Bias for Laser Angular Rate Sensor, U.S. Patent 3,467,472, September 16, 1969, filed December 5, 1966.

63. Killpatrick, "Laser gyro," pp. 48, 53.

64. F. Aronowitz, "Theory of traveling-wave maser," *Physical Review* 139 (1965): A635–A646.

65. Donald Christiansen, "Laser gyro comes in quartz," *Electronics* (September 19, 1966): 183–188; anonymous, Presentation of the Elmer A. Sperry Award for 1984 to Frederick Aronowitz, Joseph E. Killpatrick, Warren M. Macek, Theodore J Podgorski (no publisher or date of publication given), 14; Philip J. Klass, "Laser unit challenges conventional gyros," *Aviation Week and Space Technology*, September 12, 1966: 105–113.

66. Elmer Sperry Award, p. 15; Killpatrick interview. For an early overview of Navy support see John W. Lindberg, "Review of Navy activities on unconventional inertial sensors," in Proceedings of the 1966 Symposium on Unconventional Inertial Sensors.

67. Anonymous, "Laser gyro seen as applicable to missiles," *Aviation Week and Space Technology*, October 29, 1973: 60–62. By the end of the 1970s, military interest in laser gyro systems for tactical missiles seems to have cooled because of their relatively large size and high cost. In 1980, R. W. McAdory of the Air Force Armament Laboratory wrote ("Two Decades," p. 16) that laser gyro production cost "appears to be relatively constant regardless of accuracy," whereas with mechanical gyros low-accuracy systems could be produced quite cheaply. See also W. Kent Stowell et al., "Air Force applications for optical rotation rate sensors," *Proceedings of the Society of Photo-Optical Instrumentation Engineers* 157 (1978): 166–171.

68. Philip J. Klass, "Laser gyro reemerges as INS contender," *Aviation Week and Space Technology*, January 13, 1975: 48–51; K. L. Bachman and E. W. Carson, "Advanced development program for the ring laser gyro navigator," *Navigation* 24 (summer 1977): 142–151. Laboratory tests of Autonetics and Sperry prototype laser gyroscopes were still yielding drift rates well in excess of 0.01°/hour. See Central Inertial Guidance Test Facility, Ring Laser Gyro Test and Evaluation, report AFSWC-TR-75-34, Air Force Special Weapons Center, Kirtland Air Force Base, New Mexico, March 1975. For a discussion of how inertial components and systems are tested see MacKenzie, *Inventing Accuracy*, esp. pp. 372–378. As I outline there, such test results can be, and sometimes have been, challenged, but procedures for component and system testing were relatively well established by the 1970s, and the results of laser gyro tests seem generally to have been taken to be "facts."

69. Central Inertial Guidance Test Facility, Developmental Test of the Honeywell Laser Inertial Navigation System (LINS): Final Report, report ADTC-TR-75-74, Armament Development and Test Center, Eglin Air Force Base, Florida, November 1975, quotes on pp. i and 20; see also Paul G. Savage and Mario B. Ignagni, "Honeywell Laser Inertial Navigation System (LINS) test results," paper presented at Ninth Joint Services Data Exchange for Inertial Systems, November 1975, p. 1.

70. Klass, "Laser gyro reemerges"; Stowell et al., "Air Force applications," p. 166.

71. Savage and Ignagni, "Test results," p. 11.

72. Philip J. Klass, "Honeywell breaks into inertial market," *Aviation Week and Space Technology*, November 19, 1979: 78–85.

73. Stowell et al., "Air Force applications"; M. C. Reynolds, "Keynote address," in Proceedings of the Fourteenth Joint Services Data Exchange Group for Inertial Systems (Clearwater Beach, Fla., 1980), p. 10; interview with Ronald G. Raymond, Minneapolis, March 6, 1985.

74. Philip J. Fenner and Charles R. McClary, "The 757/767 Inertial Reference System (IRS)," in Proceedings of the Fourteenth Joint Services Data Exchange Group for Inertial Systems (Clearwater Beach, Fla., 1980); Raymond interview; letter from Raymond, April 8, 1990.

75. See David H. Featherstone, AEEC—The Committee that Works! Letter 82-000/ADM-218, Airlines Electronic Engineering Committee October 20, 1982. Insight into how the harmonization of interests is achieved can be found in a book by the committee's former chairman, William T. Carnes: *Effective Meetings for Busy People: Let's Decide It and Go Home* (McGraw-Hill, 1980).

76. Raymond interview; letter from Raymond, April 8, 1990.

77. The 90 percent claim comes from Litton Industries, which in 1990 filed a suit against Honeywell alleging antitrust violations and infringement of a Litton patent covering mirror-coating processes: anonymous, "Litton sues Honeywell over gyroscope," Minneapolis *Star Tribune*, April 5, 1990.

78. Anonymous, "Inertial navigation awards," *Aviation Week and Space Technology*, September 15, 1985: 61; anonymous, "Inertial navigation system to use laser gyro," *Defense Electronics*, November 1985: 17.

79. Philip J. Klass, "Litton tests laser-gyro inertial system," *Aviation Week and Space Technology*, December 1, 1980: 144–147.

80. A condition of the Honeywell-Litton settlement was that the terms of the settlement not be disclosed, so I have no further information on this case. There has also been litigation surrounding alleged violations of Speller's patent.

81. The Small ICBM was eventually cancelled. The requirements for precision azimuth alignment are given in Stowell et al., "Air Force applications," p. 167, and in W. D. Shiuru and G. L. Shaw, "Laser gyroscopes—The revolution in guidance and control," *Air University Review* 36 (1985): 62–66. The quantum limit is calculated in T. A. Dorschner et al., "Laser Gyro at quantum limit," *IEEE Journal of Quantum Electronics* 16 (1980): 1376–1379. For the MX episode, see also James B. Schultz, "En route to end game: Strategic missile guidance," *Defense Electronics*, September 1984: 56–63, and anonymous, "GE will compete for guidance system," *Aviation Week and Space Technology*, December 21, 1987: 31. Other military funding for high-accuracy laser gyros included $6.4 million from the U.S. Navy awarded to Honeywell and $5.8 million from the Air Force Avionics Laboratory awarded to Rockwell, both in 1983, with the goal of achieving a strapdown laser system with an error rate of 0.001 nautical mile per hour (comparable to that of the most accurate aircraft spinning-mass systems): anonymous, "Filter center," *Aviation Week and Space Technology*, September 26, 1983: 151.

82. See, e.g., Jay C. Lowndes, "British Aerospace pushing ring laser gyro effort," *Aviation Week and Space Technology*, November 19, 1984: 91–98.

83. Honeywell advertisement, *Aviation Week and Space Technology*, July 16, 1984: 58–59; also see Lowndes, "British Aerospace."

84. Interview with Polen Lloret, Paris, July 7, 1987; Lloret, "Centrales de reference inertielles: Perspectives et realités," paper read to École Nationale de l'Aviation Civile, May 20, 1987; Anthony King, private communication.

85. Raymond interview.

86. Interview with John Stiles, Wayne, N.J., September 25, 1986.

87. David Hughes, "Delco resonator gyro key to new inertial systems," *Aviation Week and Space Technology*, September 30, 1991: 48–49; interview with David Lynch, Goleta, Calif., September 11, 1986. The first discussion of the device that became the hemispherical resonator gyro was a report by A. G. Emslie and I. Simon of Arthur D. Little, Inc.: Design of Sonic Gyro: Report to AC Electronics Division, General Motors Corporation (July 1967). Emslie, an acoustics specialist, had been asked by Delco staff members to investigate the use of acoustic phenomena to detect rotation.

88. Lynch interview.

89. Forman, "Behind quantum electronics," pp. 201–202.

90. Bruno Latour, *Science in Action* (Harvard University Press, 1987), esp. pp. 174–175.

91. For the general debate between neoclassical and Schumpeterian approaches see, e.g., Rod Coombs, Paolo Saviotti, and Vivien Walsh, *Economics and Technological Change* (Macmillan, 1987).

92. For these revolutions more generally, see Edward W. Constant, II, *The Origins of the Turbojet Revolution* (Johns Hopkins University Press, 1980).

93. Stowell et al., "Air Force applications," p. 166.

94. Barry Barnes, *The Nature of Power* (Polity, 1988).

Chapter 5

1. See, e.g., Nathan Rosenberg, *Inside the Black Box: Technology and Economics* (Cambridge University Press, 19482).

2. For an explanation of the "floating-point" representation of numbers, see the section "Negotiating Arithmetic" in chapter 8.

3. Any definition of when "supercomputer" as a category emerged is somewhat arbitrary. The machine designed by John von Neumann and colleagues at the Princeton Institute for Advanced Study, Naval Ordnance Research Calculator (NORC) and Whirlwind, for example, would all have claims to be supercomputers. One difference between, NORC, say, and the first supercomputers I discuss, LARC and Stretch, is that the former was explicitly a unique, "once-off" machine, while LARC and Stretch were, at least potentially, commercial products to be made in multiple copies. Use of the term "supercomputer" seems to have become widespread only in the 1970s, though "super computer" (apparently as two words, not one) was used to describe the British project that became Atlas (Paul Drath, The Relationship between Science and Technology: University Research and the Computer Industry, 1945–1962, Ph.D. thesis, University of Manchester, 1973). I would be grateful to hear of other early uses of the term.

4. Werner Buchholz, *Planning a Computer System: Project Stretch* (McGraw-Hill, 1962), pp. 273–274.

5. Charles J. Bashe, Lyle R. Johnson, John H. Palmer, and Emerson W. Pugh, *IBM's Early Computers* (MIT Press, 1986), pp. 420–421.

6. Drath, The Relationship between Science and Technology; John Hendry, "Prolonged negotiations: The British Fast Computer Project and the early history of the British computer industries," *Business History* 26 (1984), no. 3: 286–300.

7. M. Bataille, "The Gamma 60," *Honeywell Computer Journal* 5 (1971), no. 3: 99–105; J. Jublin and J.-M. Quatrepoint, *French Ordinateurs—de l'Affaire Bull à l'Assassinat du Plan Calcul* (Moreau, 1976).

8. Peter Wolcott and Seymour E. Goodman, "High-speed computers of the Soviet Union," *IEEE Computer*, September 1988: 32–41.

9. See chapter 6 of the present volume. In summarizing in this way I am, of course, oversimplifying. But no firm in the United States other than Control Date and Cray Research maintained continuous development and production of a supercomputer series in a similar sustained way. Some of the more "one-off"

Gaudillière et Löwy

SCIENCE, TECHNOLOGY AND MEDICINE IN MODERN HISTORY

General Editor: **John V. Pickstone**, Centre for the History of Science, Technology and Medicine, University of Manchester, England

One purpose of historical writing is to illuminate the present. In the late twentieth century, science, technology and medicine are enormously important, yet their development is little studied. Histories of politics and literature abound, and historical biography is established as an effective way of setting individuals in context. But the historical literature on science, technology and medicine is relatively small, and the better studies are rarely accessible to the general reader. Too often one finds mere chronicles of progress, or scientific biographies which do little to illuminate either the science or the society in which it was produced, let alone their interactions.

The reasons for this failure are as obvious as they are regrettable. Education in many countries, not least in Britain, draws deep divisions between the sciences and the humanities. Men and women who have been trained in science have too often been trained away from history, or from any sustained reflection on how societies work. Those educated in historical or social studies have usually learned so little of science that they remain thereafter suspicious, overawed, or both.

Such a diagnosis is by no means novel, nor is it particularly original to suggest that good historical studies of science may be peculiarly important for understanding our present. Indeed this series could be seen as extending research undertaken over the last half-century, especially by American historians. But much of that work has treated science, technology and medicine separately; this series aims to draw them together, partly because the three activities have become ever more intertwined. This breadth of focus and the stress on the relationships of knowledge and practice are particularly appropriate in a series which will concentrate on modern history and on industrial societies. Furthermore, while much of the existing historical scholarship is on American topics, this series aims to be international, encouraging studies on European material. The intention is to present science, technology and medicine as aspects of modern culture, analysing their economic, social and political aspects, but not neglecting the expert content which tends to distance them from other aspects of history. The books will investigate the uses and consequences of technical knowledge, and how it was shaped within particular economic, social and political structures.

Such analyses should contribute to discussions of present dilemmas and to assessments of policy. 'Science' no longer appears to us as a triumphant agent of Enlightenment, breaking the shackles of tradition, enabling command over nature. But neither is it to be seen as merely oppressive and dangerous. Judgement requires information and careful analysis, just as intelligent policy-making requires a community of discourse between men and women trained in technical specialities and those who are not.

This series is intended to supply analysis and to stimulate debate. Opinions will vary between authors; we claim only that the books are based on searching historical study of topics which are important, not least because they cut across conventional academic boundaries. They should appeal not just to historians, nor just to scientists, engineers and doctors, but to all who share the view that science, technology and medicine are far too important to be left out of history.

The Invisible Industrialist

Manufactures and the Production of Scientific Knowledge

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in association with
CENTRE FOR THE HISTORY OF SCIENCE
TECHNOLOGY AND MEDICINE
UNIVERSITY OF MANCHESTER

First published in Great Britain 1998 by
MACMILLAN PRESS LTD
Houndmills, Basingstoke, Hampshire RG21 6XS and London
Companies and representatives throughout the world

A catalogue record for this book is available from the British Library.

ISBN 0–333–64753–X

First published in the United States of America 1998 by
ST. MARTIN'S PRESS, INC.,
Scholarly and Reference Division,
175 Fifth Avenue, New York, N.Y. 10010

ISBN 0–312–21254–2

Library of Congress Cataloging-in-Publication Data
The invisible industrialist : manufactures and the production of scientific knowledge / edited by Jean-Paul Gaudillière and Ilana Lōwy.
p. cm.
Includes bibliographical references and index.
ISBN 0–312–21254–2
1. Research, Industrial—Case studies. 2. Science—Social aspects—Case studies. 3. Technology—Social aspects—Case studies.
I. Gaudillière, Jean-Paul, 1957– II. Lōwy, Ilana, 1948–
T175.I58 1998
507'.2—dc21 97–32183
CIP

This book is printed on paper suitable for recycling and made from fully managed and sustained forest sources.

10 9 8 7 6 5 4 3 2 1
07 06 05 04 03 02 01 00 99 98

Printed in Great Britain by
The Ipswich Book Company Ltd
Ipswich, Suffolk

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developing art theory applicable to the medieval workshop and industrial design as well as to modern painting, introduces an informal scale of specifity, similar to that which I propose here, to his preferred alternative to influence – the artist's brief and resources for executing his/her charge; see *Patterns of Intention: On the Historical Explanation of Pictures* (New Haven: Yale, 1985) pp. 58–62 *et passim*.

120. For a more detailed explanantion, examples, and bibliography giving some of the early uses of this distinction, see F. Scott Gilbert, *Developmental Biology* (Sunderland, Mass.: Sinauer, 1985) ch. 16.

7 Disciplining Cancer: Mice and the Practice of Genetic Purity

Ilana Löwy and Jean-Paul Gaudillière

> So, the first thing you should do, I should say, in spending your money would be to insure a constant and adequate supply of controlled, known animal material on which investigations could be carried out ... if that work has to be done with them [the animals] then there has got to be knowledge of how to produce animals which are nearly as uniform as it is possible for any living higher animal to be. In other words, we can produce as nearly a chemically pure animal and as nearly alike fellows as it is possible to produce ... during this past year the little laboratory where I work has sent out over 65 000 such animals all over the United States and to Europe for research in cancer and in other experimental medicine. There is a tremendous demand for them. So the first thing to do in the spending of this money would be to have under governmental control, or at least see that there is an insurance of, a definite certainty of the availability of animal material of a controlled nature.[1]

> It is of lasting merit of a group of American investigators such as C. C. Little, L. C. Strong, W. S. Murray and others, to have recognized the importance of homogeneous material and to have provided themselves and others with the tools by which alone a highly complex situation can be tackled with a prospect of success. It was probably not an exaggeration when Heston recently (1949) said that the 'genetically homogeneous strains of mice constitute one of the greatest contributions of all times to medical research'. The introduction of inbred strains into Biology is probably comparable in importance with that of the analytical balance in Chemistry.[2]

> The results were obtained by work on animal models which *per se* are highly artificial. Subsequently, when one takes the step from the

model into the clinics, it is immediately apparent that the relevance of all this preliminary research is nil.[3]

INTRODUCTION

In his writings on experimental medicine, G. Canguilhem once pondered the strange life living beings experience in the laboratory:

> We must not forget that the laboratory itself constitutes *a new environment* in which life certainly establishes norms whose extrapolation does not work without risk when removed from the conditions to which these norms relate. For the animal or for man the laboratory environment is one possible environment among others ... for the living being apparatus and products are the objects among which he moves as in an unusual world. It is not possible that the ways of life in the laboratory fail to retain any specificity in their relationship to the place and moment of the experiment.[4]

Canguilhem's argument about artificiality is, we propose, appropriate to address the fate of animal models in modern biomedical research and the part played by industries and industrial organizations in the production, standardization and evaluation of these living instruments. The use of animal models of human diseases to establish medical knowledge is a recent practice.[5] It emerged as a routine activity in the late nineteenth century when the inoculation of rats, rabbits, dogs and so on became an obligatory passage point for physicians or scientists eager to show that a specific disease was caused by bacteria. Laboratory models of diseases are useful devices because they create an open space between the laboratory and the clinic and thus link practices prevailing in different social worlds. Animal systems have a high degree of plasticity: time and resources can be invested in order to narrow down their variability and ensure replication of experimental results.

In contrast to other organisms domesticated by geneticists or physiologists, mice crossed the threshold of laboratories to replace human bodies and to live in a biomedical niche.[6] This resulted in two features. First, mice were not investigated as mice but as workable entities representing human diseases. From the 1910s onward, physiological homogeneity and genetic purity were increasingly viewed as obligatory conditions for reasonable usage of these models. Second, after the Second World War laboratory mice became key instruments in the practice of 'big' biomedical

research. In the United States mass consumption by cancer researchers enhanced mass production and standardization. Genetically homogenous mice were transformed into genuine industrial products by virtue of the mobilization of cancer charities, governmental agencies and pharmaceutical companies. Rather than moral, the economy of the mouse was thus incontrovertibly political.

Such standardization of animal models facilitates experimental work. This process, however, generates artificial systems which often increase the distance between work in the clinic and features of interest in the laboratory. In spite of the resources invested in the production of homogeneity and of consistency, *mouse* models of *human* diseases remain metaphorical devices. Long-term stabilization is a problematic achievement and the uses of such systems result in irreducible tensions. Not surprisingly, long-term commitment to a given animal model of a human pathology has been rare. It chiefly occurred in medical bacteriology. In contrast, such consensus did not emerge in cancer research.

This chapter focuses on the twentieth-century debates surrounding the experimental uses of laboratory mice affected with tumours. Our analysis addresses the role of the industrialist in the production and changing uses of animals viewed as genetically pure. By discussing the alternation between transplanted and spontaneous tumours and the uses of inbred mice as models employed to investigate the complex causes and biological mechanisms of human cancers, we argue that inbred mice were introduced in order to regulate experimental practices. The investments made in order to establish genetic purity indeed resulted in an increased use of 'pure inbred strains' and in the black boxing of genetic homogeneity. By contrast the implicit assumption behind the search for genetic purity, namely the stabilization of the link between cancer laboratory and cancer clinic, between animal models and human cancer, was not achieved; for the clinician, the meaning of mice tumours remains an open issue.

TRANSPLANTED TUMOURS AS A MODEL OF CANCER

The modern view of tumours was developed in the second half of the nineteenth century. The cellular theory developed by Virchow, Remak, Cohnheim and Billorth distinguished between 'true' and 'inflammatory' tumours, and defined malignancies as a cluster of proliferating cells which grew from normal cells in the body and moved to distant sites via the blood and lymph system.[7] Tumours, according to this theory, were unique events induced by unknown causes. Some degree of reproducibility was, however, necessary in order to transform these unique pathological events into reproducible entities which could be studied in the laboratory. Pathologists multiplied attempts to maintain malignant tumours in laboratory animals through serial grafting of tumour fragments.

Variability was first attributed to technical difficulties. However, scientists began to perceive regularities in the pattern of rejection of these grafts. The first success of the tumour transplants was reported in dogs, but dogs were soon replaced by small rodents as the chosen animals for tumour grafts.[8] In the early twentieth century scientists who studied cancer in mice and rats found that it was impossible to transplant a tumour of one species in another species and that attempts to transplant tumours to individuals belonging to the same species were not consistently successful.[9] There were significant discrepancies between results obtained in different laboratories; sometimes the rate of success of grafts ('takes') was high: in other cases the grafts were invariably unsuccessful.[10] These discrepancies and the difficulties in obtaining reproducible results (e.g., tumours successfully transplanted in laboratory A failed to be transplanted in laboratory B) were a major obstacle for the construction of a unified domain of experimental cancer studies. Bashford, Haaland and their colleagues at the Imperial Cancer Fund, London, attempted to explain the variability of results of tumour transplants by the fluctuant virulence theory. According to this theory malignant cells, even those derived from common stock, may vary in their virulence exactly as bacteria do.[11] Other studies pointed to animal hosts (mice and rats) as the main source of variability in transplantation results. The host-based studies can be divided into two groups: studies of 'resistance' to transplanted tumours (that is, studies which focused on the mechanisms of tumour rejection), and studies of the relationships between 'hereditary factors' and susceptibility to tumour grafts.

Let us look at the 'resistance' studies first. The observation that tumour grafts are often rejected by host animals led to the hypothesis that a specific 'resistance' mechanism – roughly parallel to 'resistance' to bacterial infections – is responsible for this phenomenon. Two theories were put forward to account for this 'resistance'. The 'athrepsia' theory, developed by Paul Ehrlich, proposed that transplanted tumours perished because their cells lacked adequate receptors to absorb food elements from the recipient, while the vascularization theory developed by researchers at the Imperial Cancer Research Fund, London, sustained that tumours were rejected because they failed to be properly vascularized and nourished by the host.[12] The 'resistance' studies took a new turn when scientists observed that laboratory animals which spontaneously rejected a first graft of a tumour after a long period of acceptance, promptly rejected a second graft

of the same tumour. It was found that this 'acquired resistance' to tumours – unlike acquired immunity to bacteria – was not specific to the tumour. Accelerated rejection of the tumour could be obtained in animals which were previously injected with live normal cells (often blood cells) of the same species.[13] Around 1910, some researchers reached the conclusion that the phenomenon of 'resistance' was not restricted to malignant tissues: normal tissues were rejected too. The surgeon Alexis Carrel observed in 1910 that if a kidney removed from an animal was regrafted into the same host it was nearly always well tolerated. By contrast, kidneys exchanged between two animals belonging to the same species were invariably rejected. Carrel concluded that an unknown biological mechanism is responsible for the rejection of foreign tissue.[14] The same year the pathologist Peyton Rous, who, like Carrel, worked at the Rockefeller Institute, New York, grafted mice with normal embryonic tissue to check if transplantation of embryonic tissue would induce tumours. He found that tumours and embryonic tissue showed similar patterns of rejection and concluded that the phenomenon usually described as 'resistance to grafted tumours' is in fact 'a resistance directed against the graft as a strange tissue and is unconnected with the neoplastic qualities which that tissue happens to possess.'[15]

Rous's conclusions were not immediately taken into consideration by researchers who studied grafts of cancer, usually in loosely defined experimental systems. Studies of 'resistance' to tumours were vigorously pursued in the 1910s. This research was practice-oriented; the goal was to induce efficient resistance to naturally occurring malignancies, thus in fine with the hope that they would lead to the discovery of anticancer therapies. Thus in 1915 when James Murphy from the Cancer Laboratory of the Rockefeller Institute found a correlation between the stimulation of lymphocytes and the accelerated rejection of grafted tumours, he promptly tried to apply this phenomenon to the cure for human cancer.[16] He failed and so did all the researchers who attempted to cure spontaneously occurring cancer through the stimulation of the 'resistance reaction'. Studies of 'resistance to transplanted cancer' continued into the 1920s and 1930s, but the subject lost much of its prestige and interest.[17]

The search for 'hereditary factors' of susceptibility to tumour grafts originated in the observation, made in the early twentieth century, that the transplantation of tumours was more successful if the mice or rats belonged to the same 'race', that is, if one used mice from the same colony, usually designated either by the colony's geographic origin ('London mice', 'Paris mice'), or by its colour (black mice, brown mice, albino mice).[18] The 'race effect' was not immediately understood in genetic terms. For example when Haaland found that a given tumour grew on inoculation in nearly 100 per cent of Berlin mice, in 24 per cent of Hamburg mice and could not be inoculated to Christiana mice, he proposed that environmental factors such as changes in climate, heat, light, moisture and above all diet influenced the susceptibility of mice to transplanted cancer.[19]

The 'genetic turn' began when the pathologist Leo Loeb found that a spontaneous tumour of Japanese waltzing mice, a strain bred for a 'waltzing' trait (a defect of the inner ear) by professional animal breeders, may be transplanted with a high degree of success in these mice.[20] Loeb himself was not immediately interested in a genetic investigation, but Tyzzer took over Loeb's experimental system.[21] Tyzzer was also a pathologist who taught at Harvard Medical School. By the time he embarked on the study of hereditary factors in the genesis of spontaneous tumours, he had been appointed chief researcher and director of the Harvard Cancer Commission laboratory. His goal was

> to determine if the susceptibility to an inoculable tumour is transmitted in accordance with the principles of heredity such as are embodied in Mendel's laws ... The waltzing tumour was especially adapted for the study of the problem at hand on account of the uniformity of the results obtained from its inoculation into different varieties of mice.[22]

Tyzzer cross-bred Japanese waltzing mice with common albino mice and studied the susceptibility of the offspring to the Japanese tumour. He was unable, however, to propose an interpretation of his results that would fit the statistical distribution of Mendelian hereditary factors. Tyzzer's conclusion in 1909 was that successful tumour transplant in mice depended on three factors: the method of inoculation, the character of the individual tumour employed and the 'nature of the soil upon which the tumour is implanted'. 'Racial differences' were important, Tyzzer proposed, but their role was unclear: 'susceptibility to an inoculated tumour is neither inherited in accordance with Mendel's laws, nor are the results obtained from cross-breeding explained by any known principle of inheritance.'[23]

The French scientists Cuénot and Mercier arrived at similar conclusions. They developed two 'apparently homogenous' lines of mice: one ('une lignée riche') accepted the majority of tumour grafts (80 to 100 per cent of grafts (0 to 20 per cent of grafts were successful). Cuénot and Mércier then crossed mice of these two lines, and observed the uptake of grafts in first-generation (F_1) hybrids. Their results were not consistent with the hypothesis that susceptibility to a cancer graft is a Mendelian

character transmitted by one or two genes. The very existence of 'rich' and 'poor' lines, Cuénot and Mercier added, indicated that heredity played a role in susceptibility to cancer grafts in 'a very peculiar manner' ('d'une façon particulièrement curieuse').[24]

The study of the inheritance of 'susceptibility' to transplanted tumours took a new turn when Tyzzer started to work with the mouse geneticist Clarence C. Little. He attended Harvard University and ended up in the laboratory of the Harvard geneticist Castle, where he worked on the inheritance of coat colour in mice. Little was encouraged into cancer research by E. E. Tyzzer, who managed to have him put on the payroll of the Harvard Cancer Commission as a research assistant in charge of the study of inherited susceptibility to tumours. Tyzzer capitalized on Little's experience in long-term inbreeding. In 1909, expanding on Johansen's practice with plants, Little started systematic brother – sister mating in order to establish pure lines of mice. Most lines could not be continued because – as Castle had predicted – the animals became so weak and prone to diseases that they died before reaching a reproductive age. One line, however, survived the selection and after two dozen generations even became as strong as wild races. In 1913 Little was thus in charge of 'an homogenous stock of common mice', the dilute brown race, that he took into Tyzzer's laboratory in order to follow the transplantation process in hybrids resulting from mating with the Japanese waltzing mice.

The use of dilute brown mice was clearly viewed as a way to narrow down the variability of transplantation experiments:

> the stocks used are genetically favorable for obtaining uniform and reliable experimental results. It seems important to emphasize this phase of the work, for if mixed or relatively impure races are used, variable and inconclusive results are almost certain to be obtained. We feel that the material used is of sufficient constancy and definiteness to lend strength to any experimental results obtained in the study of its hereditary behavior.[25]

Tyzzer and Little applied the principles of Mendelian statistics previously used by Little in the mice coat colour problem to the analysis of genetic factors involved in 'susceptibility' to grafted tumours. They concluded that this 'susceptibility' was controlled by multiple hereditary factors, in this case probably as many as twelve to fourteen.[26]

In the 1920s a majority of specialists agreed that 'hereditary factors' influenced the success of tumour transplantation. It was less clear, however, whether scientists who observed the fate of grafted tumours studied transplantation of foreign tissues or cancer. Cuénot and Mercier legitimized their study of tumour transplantation with the hypothesis that the same 'hereditary factors' which determined the uptake of a grafted tumour were responsible for the growth of a spontaneously arising tumour. This argument was more difficult to maintain when genetic studies unambiguously pointed to similarities between 'hereditary factors' that governed the rejection of normal and malignant tissue. In 1922 Little and Johnson concluded that 'in all probability a susceptibility to a transplant of splenic tissue depends upon the same general principles of heredity found to apply in the case of tumour tissue, namely, multiple mendelizing factors.'[27] James Murphy then proposed that the studies of transplanted tumours be abandoned and replaced by research of either spontaneous cancers which mimic natural conditions or artificially induced malignancies, the study of which could shed new light on the malignant transformation of cells.[28] Other researchers were less quick to abandon the belief that the study of transplanted tumours could contribute to the unravelling of carcinogenic mechanism(s). Little explained in 1922 that

> studies with inoculated tumours give information as to the genetic nature of normal tissue and normal growth; studies with spontaneous tumours give information as to the genetic nature of the process involving the failure of growth control. Each supplements and amplifies the other.[29]

However, Little's point of view did not prevail. The main outcome of the attempts to homogenize the results of transplantation was a new emphasis on genetic homogeneity. Although most of the researchers who studied cancer in the laboratory agreed upon some hereditary control of the fate of transplanted tumours, no consensus emerged on the practice of genetic purity. Moreover since it was agreed that 'resistance' to transplanted tumours was not specific to cancer tissues, the phenomenon was of little interest to clinicians. Consequently in the 1920s and 1930s leading scientists in experimental cancer research seldom viewed transplanted tumours as an adequate model to study the genesis and development of malignant growths and employed transplanted tumours (e.g., Ehrlich's carcinoma in albino mice, Jensen's sarcoma in Wistar rats) mainly in biochemical or cytological investigations.[30]

GENETIC PURITY: THE CO-PRODUCTION OF INBRED LINES AND SPONTANEOUS CANCERS

In the summer of 1937 the US Congress started to discuss a proposal aimed at the creation of a National Cancer Institute (NCI). A few months

earlier cancer research had made the front cover of the country's major magazines. Mice affected with spontaneous tumours seemed to be in the forefront of the fight against the dreaded disease. On 1 March *Life* described the captains of the war against cancer below the picture of hundreds of inbred mice from the Jackson Memorial Laboratory.[31] Two weeks later *Time* focused on the part played by Clarence C. Little, then managing director of the American Society for the Control of Cancer and director of the Jackson Memorial Laboratory. The description of the setting went something like this:

> At Bar Harbor, in a small building whose solid brick walls exclude stray mice, he [Little] produces 150 000 mice a year, sells 50 000 to other scientific institutions for research, and anatomizes 25 000 to analyze their inherited characteristics, especially their susceptibility to cancer.[32]

In April *Newsweek* highlighted the work of Maud Slye, who had conducted a large-scale survey of cancer incidence in mice of known ancestry.[33] This series was part of the campaign for the establishment of the NCI. It showed that genetic factors governing tumour susceptibility in mice had successfully been turned into putative causes of human cancers, a fact confirmed a few months later when Little was invited to testify before Congress. The emergence of public interest in 'pure' and 'uniform' mice as models exemplifying the role of 'race' in the causation of cancer was no minor achievement. It can be traced back to the early days of C. C. Little's work in tumour transplants.

The collaboration of Little and Tyzzer at Harvard resulted in a significant displacement of the inheritance problem. Little and Tyzzer had considered animals of known ancestry as necessary to achieve control of the susceptibility to tumours. The local but successful routinization of transplantation experiments, achieved by using a single tumour type and 'inbred' hosts, actually turned the homogeneity problem upside down. Transplantation patterns gradually became a means to assess the purity of the genetic background. Little started to use transplantation to control inbreeding:

> The race from which the animals used in the experiments were derived (the Japanese waltzing mice) is one that had been inbred for at least six years without any addition of new individuals from outside the stock. The result must therefore have been to produce a race of great uniformity with respect to whatever inheritable factors it may possess ... The homogeneity is further shown by uniformity of their reaction to their

> tumours ... These results are in contrast with those obtained from the implantation of tumours in less inbred stocks. ... The uncertainty of the results in the experimental inoculation of the tumours of tame mice is a matter of common knowledge. Although the results obtained depend in part on the character of the individual tumour, this is to be regarded as a fairly constant factor, at least at any given transfer, so that presuming a satisfactory inoculation technic, constant uniformity in the growth of tumour implant furnishes strong evidence of racial homogeneity.[34]

This first use of racial homogeneity led to a controversy involving the geneticist Maud Slye, which sheds light on the medical issues at stake when Little dismissed the commonly bred mice.

Slye was a former psychology teacher invited by Whitman to work at the University of Chicago on the transmission of behavioural traits.[35] In 1908 she gave up her work on the waltzing habit of the Japanese mice and embarked upon a study of cancer. The debate with Little started when Slye published preliminary reports claiming that cancer susceptibility was a recessive disorder comparable to albinism in mice.[36] Little agreed with Slye the notion that 'the results obtained with small mammals could be applied to the problem of human cancer'.[37] He merely challenged her use of the Mendelian framework, especially her definition of 'dominant'.[38] Slye disdainfully replied that her aim was not to support a theory on inheritance but to show that susceptibility to cancer was inherited and that individuals could be carriers without being affected.[39]

The public controversy was resumed ten years later when the American Medical Association was ready to evaluate Slye's project for the creation of a research institute on the transmission of human cancer. Little was also involved in a new venture. In 1929 he resigned as president of the University of Michigan, following a bitter controversy with the board of trustees over his attempts to reform the university according to progressive values.[40] Little then returned to research but did not forget his social and managerial skills. He expanded his activities as a prominent member of the American Birth Control Society and the American Eugenics Association in two different ways. First, he obtained the support of automobile tycoons from Detroit, namely members of the Ford and Jackson families, in order to create a laboratory devoted to the study of mouse genetics and cancer. Second, he was appointed managing director of the American Society for the Control of Cancer. Thus 1929 resulted in a new network uniting mouse genetics and human cancer, eugenics and medical research.

The controversy opposing Little and Slye concerned the relations between experimental cancer research and clinics, the fate of eugenics and

the right way of doing genetics. Slye and Little were both committed eugenicists, but they mobilized different groups of scientists, physicians, and activists. To a large extent, contrasted networks and social techniques explain both Slye's failed attempt to secure the collaboration of prominent oncologists and Little's successful support of a multifactorial theory of cancer heredity. However, for the purposes of this chapter, the fact that the conflict between Slye and Little about genetic purity was rooted in incompatible experimental regimes is of greater interest than the political end to the controversy.

Slye's genetics was a mixture of breeding practices and anatomopathological studies. She studied the incidence of spontaneous tumours in mouse families. Consequently she argued against the study of 'artefactual' transplanted tumours.[41] Her 'mouse house' was organized around the practice of necropsies. Slye raised mouse families, carefully recording the birth of newborns and the death of old mice and kept close track of pathological symptoms, systematically completing necropsies and histological studies in order to attribute specific causes of death and trace the incidence of cancers. The most sophisticated products of this strategy were 'pedigrees' showing 'cancer families' and 'non-cancer families'. Slye's argument in favour of a cancer gene is best exemplified by the extended pedigrees she used to publish. In contrast to statistical groups, she displayed individual kinship, including on a single chart all the members of a family regardless of the causes of death. When mice died from cancer, the charts were labelled with the location and type of tumours. Since great attention was paid to individuals and kinship within mouse families usually comprising three to five generations, Slye's charts resembled human pedigrees published by physicians interested in cancer inheritance. Her arguments were thus based on collection. Her laboratory was known as a 'private Museum' where she kept mice, drawings of necropsies and microscopic slides. From 1915 onward the system expanded into an impressive machinery. In 1928 Slye could claim that her case for a cancer recessive gene was based on some 70 000 necropsies.[42]

As of 1910, Slye argued against the use of inbred mice as well as transplanted tumours.[43] Her distrust was a warning against laboratory artefactual cancers. Accordingly, she thought that the susceptibility to transplanted tumours differed from the susceptibility to spontaneous tumours, because the fate of a grafted tumour did not represent the first stages of cancer. In addition, she claimed that a long-term practice of inbreeding through consistent brother – sister matings resulted in weak and fragile animals leading to biased data, because many of them were killed by infectious diseases or other conditions before any observation of cancer could be completed. Moreover, the loss of fertility usually made it difficult if not impossible to preserve strains beyond a few generations. Thus she chose to stick to kinship and saw genetic homogeneity as a hindrance rather than a prerequisite.

The Chicago 'pedigree' culture opposed the Jackson 'inbred lines' culture in almost all respects.[44] The creation of the Jackson Memorial Laboratory turned Little's chiefly theoretical and discursive argument about the value of genetic purity into a 'system of production'.[45] The official goal of the laboratory was to select, preserve and use inbred, homogeneous lines of mice that could be compared with pure chemical reagents:

> The chemist would be helpless in his attempt to analyze unknown material if he did not have known chemical agents on the shelves of his laboratory to which he could turn for assistance in analyzing the unknown. The biologist, in the past fifteen years, has been given the tools by which he could approximate [his unknowns]. In experimental medicine today ... the use of inbred genetic material is just as necessary as the use of aseptic and anti-septic precautions in surgery.[46]

One should stress that Little's work within eugenic circles resulted in a moral economy of purity which linked the inbred lines with the issues of social reform and social control. The chemical analogy was used in both contexts:

> I happen to be working in Maine where the proportion of the old New England stock is very, very high ... I don't want to see that particular element in the situation mixed up, or mauled up. I want to keep it the way the chemist would prize a store of chemically pure substance that he wants to use for testing, that he wants to use for definite purposes when a certain element is needed.[47]

In order to obtain 'pure' lines the basic practice remained sustained matings between parents and offspring or brothers and sisters. Thus the ancestry charts were not pedigrees showing 'real' families, but cleaned and abstract lists of matings arranged in chronological order.[48] Scaling up was a key factor. While at Michigan, Little could hardly maintain more than two lines.[49] Now the purpose was to obtain a whole range of strains showing different properties. Moreover keeping a great number of animals was the only way to avoid the misfortunes predicted by Slye, to cope with the lack of resistance to diseases and injuries and to neutralize the impact of sudden changes that might modify parts of the stock. Changes of scale

were accompanied by a new emphasis on the standardization of management.[50] Environmental factors should be kept as identical as possible. Food, for instance, should be obtained from the same suppliers according to very stringent rules. Finally controls of genetic homogeneity based on transplantation experiments should be organized on a regular basis. Within a 'pure' strain, all the animals should show the same susceptibility to known tumours. Any deviation from standard patterns was to be viewed as a proof of unpredictable events, i.e. mutation or management error.

The Jackson Laboratory became increasingly similar to an industrial setting in the 1930s, following Little's decision to stop giving away the mice for free. Systematic matings were organized in order to obtain an increasing number of animals that could be sold on the market. Specialized 'production rooms' or 'mouse houses' were added to the small laboratories where one researcher and a few technicians and caretakers maintained and worked with a few lines. This extension of mice production was funded by the new resources from sales and by a major grant from the Rockefeller Foundation. In 1932–3 the Jackson laboratory was producing a few thousand mice a year. In 1935 almost 50 000 mice were shipped to consumers working in the United States. By the late 1930s the production figures reached a few hundred thousands.

Although the Jackson infrastructure continued Little's early work with inbred lines of mice, the meaning of standardization changed. Tumour transplantation was relocalized as a part of the production line, in other words as a routine but necessary practice of homogeneity assessment. Studies of relationships between heredity and cancer were focused, first of all, on the susceptibility to transplanted cancers. When the 'susceptibility' studies were replaced by research on the incidence of spontaneous tumours, standardization was no longer targeted at the production of animals with the 'same genes' but at the selection of strains showing the same incidence of cancer. Genetic purity achieved through consistent inbreeding thus became the most important criterion used to determine good animal models.

From the creation of the Jackson Laboratory, Little strongly emphasized the binding value of his mice:

> It may be pointed out in conclusion that the accepted method of making human mating, viz. by uncontrolled outcrossing combined with the inadequate records and small numbers of progeny which commonly are encountered in human families, militates against the practical use of controlled heredity as a means of reducing the incidence of cancer in man. This, however, does not prevent the genetic approach to the

problem in the laboratory and the use of controlled homogeneous inbred strains of mice from being extremely favorable material for pure scientific research in the nature and cause of cancer.[51]

Moreover, small mammals would link the abstract genetic culture of Morgan's school and the practical needs of physicians:

> [T]he gap between the laboratory phase of experimental genetics and the interest in human biology will not be bridged by research in the forms as widely separated from man as are the insects ... The bridge, if it is to function properly, must have well-developed approaches from both sides. Besides being of interest and value to humans and being based on sound genetic theory the problems must be capable of investigation in economic and suitable material ... Mice of two genera (Mus and Peromyscus) appear, however, to be even better material than is any of the other types.[52]

Purified animals were to exemplify the genetic control of the disease and to show 'pure', isolated cancer of genetic origins. The C3H strain, for example, was thought to be of interest because a fairly stable 90 per cent of the females died from mammary tumours. It could be proposed as a model system for breast cancer. Variability was not always an enemy; it could be used as a resource to create new strains. The 'A line' selected by L. Strong, a former collaborator of Little who joined the staff for a few years, illustrates the mixture of pragmatism and discipline which dominated the work at the Jackson lab.[53] The females of Strong's A line were usually affected with mammary tumours. Virgin females, however, were virtually unaffected regardless of age. Attempts at changing the age of tumour appearance or at increasing the percentage of virgin females showing tumours were quite unsuccessful. The A strain was therefore viewed as demonstrating the influence of hormones and breeding on genetically determined mammary tumours.

The circulation of semi-industrial, standardized mouse models of cancer within a 'mouse network' based at the Jackson Laboratory played a significant part in homogenizing laboratories and stabilizing local knowledge. This is best exemplified by the fate of a new tumour-inducing agency discovered at the Jackson laboratory in the late 1930s.

By 1933 the Jackson community described peculiar matings between low- and high-cancer strains resulting in the inheritance of the maternal type.[54] One staff member, John Bittner, investigated the part played by nursing. In 1936 he proposed that a 'milk influence' was involved.[55] His claim was based on results obtained through artificial nursing: a few females of the high-cancer strain A could be transformed into low-cancer animals if nursed by low-tumour mothers. According to Bittner, the transmission of something governing the formation of mammary tumours was prevented by such 'foster-nursing'. The behaviour of the new agency jeopardized the whole prospect of producing pure inbred 'cancerous' and 'non-cancerous', strains since what was previously viewed as a genetically determined cancer strain seemed to originate in standardized husbandry, namely in the nursing patterns that had allowed the continuous transfer of the milk-influence.

At the Jackson Laboratory several paths were followed to domesticate the milk influence and preserve cancer genetics.[56] Little attempted to circumvent the problem by turning the milk influence into an endocrine agent or a maternal physiological influence.[57] Bittner designed a package of practices for working with the milk influence according to the Jackson inbreeding culture.[58] For example, the main problem with the foster-nursing experiments was that a homogeneous incidence of tumour within an inbred line could no longer be used as evidence of genetic control. The endless debate about nature and nurture was once again knocking at the door of the mouse house. The matter could only be settled by a shared system of standards defining mice with high genetic susceptibility to cancer. Building on the transplantation debate, Bittner attempted to create a set of transferable susceptible lines which could be used to test the existence of a milk influence. In addition to local adjustments based on repeated trials, the use of similar reference animals, namely the Jackson strain A showing a milk agent and the susceptible but agent-free Jackson strain C 57, would reduce discrepancies.

A few years after Bittner's first experiments foster-nursing became routine practice in half a dozen laboratories. The generalization of the milk influence studies matched the distribution of inbred animals.[59] The spreading of the organisms, and its corollary, the circulation of research practices and of problems worthy of investigation, favoured the development of similar experimental strategies. Nevertheless, consensus did not come from straightforward uses of homogeneous models. It originated in a continuous tinkering with local systems that depended upon the credibility and centrality of the core production centre.

The development of milk factor studies at the National Cancer Institute may illustrate this process. Work started in 1939 when the oncologists of the Public Health Service moved from Boston to new laboratories in Bethesda (Maryland).[60] H. B. Andervont, then working on tumour transplantation, began reciprocal foster-nursing experiments.[61] Using his own colony of C3H mice he tried to convert C57 mice which develop mammary tumours infrequently into a permanent high-cancer strain. In 1940 Andervont and his group claimed a cancer rate of 14 per cent (14 mice) whereas Bittner had claimed 38 per cent (8 mice) using the Jackson C57 line fostered by strain A mice.[62] Replication was granted because there was a 'significant increase of cancer incidence'. Moreover in the same issue of the *Journal of the National Cancer Institute*, Bittner realigned his results: with 104 fostered mice the figure dropped to 11 per cent.[63] Then, the scale of operation was extended with a continued series of foster-nursing experiments going on with strain C3H at the NCI, and strain A at Bar Harbor. In 1943 Andervont reported a surprising 63 per cent (35 fostered mice), which was unexplained but welcome.[64] Bittner kept on providing low but stable rates (18 per cent with 98 mice in 1944), but at the NCI rates remained unstable (in 1948 Andervont reported a complete failure at increasing the tumour incidence). Finally assays were interrupted and the notion that the C57 stocks used at the NCI were genetically heterogeneous emerged as a consensual hypothesis. There was no point in checking some uncomfortable hypothesis about the variability of the agent; the Jackson A and C3H mice were already established standards for nursing mothers showing a stable milk influence.

The articulation with the clinic took a new turn with the expanding studies of mouse mammary tumours. Though genetic susceptibility continued to be perceived as the main factor triggering cancer, the work with the milk agent might have hampered the prospects of linking studies of genetically pure tumours with the preoccupations of clinicians. Bittner's choice was to back a multifactorial theory of mouse tumours based on the interplay of three agents: the milk influence, the hormonal stimulation cherished by clinicians, and inherited susceptibility.[65] An alternative, which became in fine Little's choice, was to give up the mammary tumour model in order to focus on mouse genetics and susceptibility factors. Bittner's move out of the Jackson Laboratory in 1942 facilitated attempts to preserve the value of the mammary tumour model. Thus, strong commitments to spontaneous tumours unexpectedly transformed some inbred lines into loose multifactorial and highly variable models to be used in correlation studies.

The existence of a Jackson centred network of users of inbred mice facilitated the mutual adjustments of results and practices. Opportunities for comparing results depended on the circulation of a set of tools that could be viewed 'as nearly uniform as it is possible for any living higher animal to be' because they came from the main production centre. The main role of this 'center of production' was not, however, to create an expanding market for the circulation of genetically homogenous organisms but rather to promote a a shared culture of standardization. Though Little and the Jackson Laboratory staff emphasized the similarities between their mice and industrially produced chemicals, the analogy was inexact because, following the importation of Jackson breeding pairs, many laboratories involved in the study of mouse mammary tumours started local production of 'cancer-mice'. Direct and public comparisons of methods and results remained circumstantial, however. A regulation regime based on local craftmanship prevailed until the late 1940s as exemplified by the mice produced at the Radium Institute.

GENETIC PURITY AS A 'COTTAGE INDUSTRY': LOW-CANCER AND HIGH-CANCER LINES AT THE RADIUM INSTITUTE, PARIS

Little, Tyzzer, Strong and their colleagues developed 'cancer lines' of mice as a part of a larger enterprise of large-scale production of genetic purity. One of the 'mammary cancer' lines of mice, however, the RIII line, was developed in a very different context: small-scale, non-commercial production of mice pursued by a single researcher and destined above all for a local use.

In 1926 the director of the medical division of Radium Institute (Paris), Claudius Regaud, suggested to one of the institute's researchers, Natalie Dobrovolskaïa- Zavadzkaïa, that she study families of cancer mice.[66] Dobrovolskaïa-Zavadzkaïa, one of the first women surgeons in Russia, left the country after the 1917 revolution and found a temporary job in Regaud's laboratory where she studied the effects of radiation on tissues.[67] Genetic studies at the Radium Institute were funded by a grant from a French industrialist, Léonard Rosenthal. Dobrovolskaïa-Zavadzkaïa first attempted to induce mutations in mice through X-ray radiation.[68] In 1928 Dobrovolskaïa-Zavadzkaïa turned to studying hereditary cancer. She bred female mice which carried spontaneous mammary tumours with males born from a cancer-bearing female. Later, if possible, mothers were fertilized by their sons. She produced ten 'lines', or rather extended families ('des familles prolongées'), which she followed for three to five generations.[69] Some of these extended families had a high incidence of cancer, while in others the incidence of malignant tumours was low or nil. These results, Dobrovolskaïa-Zavadzkaïa proposed, pointed to the existence of a hereditary factor or factors in the genesis of cancer. The question of dominance was, she explained, more complicated: her data were not in favour of a single dominant gene but rather of several organ-specific recessive genes. Slye's results, Dobrovolskaïa-Zavadzkaïa explained, could be criticized not because she did not use adequate genetic methods, but because she lumped together results about the inheritance of several distinct types of tumours (sarcoma, carcinoma, mammary tumours).[70]

In 1932 Dobrovolskaïa-Zavadzkaïa's laboratory developed several strains of inbred mice. These lines were developed without benefiting from systematic genetic controls and without checks for homogeneity through skin grafts, but years of inbreeding led to the development of lines of mice which other scientists considered to be genetically homogenous. For example, the RIII line, which developed high levels of spontaneous mammary tumours, was employed in the 1930s and 40s in several leading English and American laboratories.[71] At the same time Dobrovolskaïa-Zavadzkaïa developed a cancer-free line, XVIInc, through selective breeding of a low-cancer line XVII (1 per cent to 2 per cent of spontaneous tumours).[72] She also 'acclimatized' some of the Jackson Laboratory lines. Through local breeding Jackson CBA mice (Strong) became Radium Institute XXXIX line; Jackson C57 (Little) became XLI line, and 'leaden brown non-agouti' (Murray) became XLII line. Assessment of purity and comparison with the Jackson lines was not on the agenda, because the inbred lines developed in Dobrovolskaïa-Zavadzkaïa's laboratory were used only to study heredity (including heredity of malignant tumours), and were mainly used in studies of the interaction between hereditary and environmental factors in the genesis of cancer.

The assumption that cancer is a multifactorial disease which develops through a specific conjunction of external and internal conditions underlies much of the work done at the Radium Institute in the 1930s and 40s. The strength of the multifactorial hypothesis at the Institute may perhaps be explained by the multi-disciplinary structure of the Institute, very unusual for a French medical establishment. Indeed the Radium Institute brought together physicists, experimental biologists and physicians specialized in cancer therapy (radiotherapists, surgeons, pathologists). The multi-factorial hypothesis of the genesis of cancer also accounted for difficulties in obtaining reproducible results in experiments which investigated the interaction between internal and external environments during carcinogenesis.

In the 1930s one of the Institute's leading scientists, Antoine Lacassagne, used Dobrovolskaïa-Zavadzkaïa's mice to study the effect of hormones on the development of cancer in mice. Lacassagne, who at the outset of his career worked under Regaud, first studied the effect of X-rays on tissues.[73] He later became interested in the role of sexual hormones in embryogenesis and carcinogenesis. In 1932 Lacassagne was able to induce mammary tumours in male mice of the RIII line, developed by Dobrovolskaïa-Zavadzkaïa, through repeated injections of the female hormone oestrogen.[74] Over the next few years Lacassagne used

Dobrovolskaïa-Zavadzkaïa's 'high-cancer' and 'low-cancer' lines to show selective interaction of hereditary dispositions and environmental influences such as hormones in inducing cancer.[75] At the same time, Dobrovolskaïa-Zavadzkaïa tested the effects of chemical carcinogens on her 'high cancer' and 'low cancer' lines.[76] Studies of hormones and carcinogens were seen as complementary because hormones which selectively induced malignant tumours could also be classified as chemical carcinogens. As Lacassagne put it: 'after all, nothing prevents one from attributing to estrone itself a direct carcinogenic action in certain conditions, that is to say, according to the enigmatic power of transforming a normal cell into cancerous cell.'[77] Lacassagne also studied Bittner's 'milk influence', for him an environmental factor that helped to express hereditary predispositions. The 'milk influence,' he proposed, is a substance (presumedly a hormone-like one) which favours the stimulation of the mammary glands by oestrogens.[78] The hypothesis that the development of a malignant tumour is the result of a unique interaction of multiple external and internal factors reduced the need for the uniformization of animals used in different research laboratories.[79] Research made with 'high-cancer' and 'low-cancer' mice, Dobrovolskaïa-Zavadzkaïa explained, may help to identify statistical correlations between selected combinations of genetic and environmental cancer-inducing factors and encourage prevention policies.[80] Such research was not expected, however, to lead to a discovery of a unique 'cause of cancer' or single necessary condition leading to malignancy.[81] Thanks to the international contacts of Antoine Lacassagne, the Radium Institute's 'high mammary cancer' RIII line of mice entered a small, non-commercial distribution circuit, but this circuit cannot be compared with the semi-industrial distribution network organized by the Jackson Laboratory. We should point out, however, that the two circuits were not mutually exclusive. Biologists could used mice originating in both settings, especially when they were not originally interested in cancer genetics. Thus, for example, at the Imperial Cancer Research Fund, London, RIII mice coexisted for many years with C3H mice.[82]

THE RETURN OF TRANSPLANTED TUMOURS: SCREENING FOR ANTI-CANCER DRUGS, 1945–66

The increased production of mice at the Jackson Laboratory made the industrial basis of genetic homogeneity visible, but in the 1930s and 40s the effects of the circulation of inbred animals on research patterns remained limited to selected up-to-date laboratories. In the 1950s, however, the growth of a government-directed chemotherapeutic research resulted in the mass-production of inbred animals, and their large-scale diffusion among biomedical researchers. This diffusion also made the use of inbred mice commonplace. While NCI officials strongly emphasized the exemplar of industrial research and the management of large-scale systems, increasing investment in the production and standardization of mice was not available to end users, for whom genetically homogenous mice became as unproblematic a tool as a commercially produced purified chemical compound or measuring instrument.

The search for an 'anti-cancer' drug, a constant goal of cancer specialists, accelerated immediately after the Second World War. Before the war only one American laboratory, the US Public Service Office of Field Investigations of Cancer at Harvard University (from 1938 on part of the newly founded National Cancer Institute, the NCI), studied anti-cancer activity of natural and synthetic compounds.[83] The NCI laboratory routinely tested the anti-cancer effects of these compounds on eight different tumours, all transplanted in inbred mice. In addition, whenever possible NCI researchers used tumours which occasionally and in an unpredictable manner occurred in laboratory mice. 'Mice with cancer of spontaneous origin,' they explained, 'appear to be most desirable for therapeutic experiments since under such conditions the neoplasm arises from host tissues and is subjected to normal influences of the body's metabolism.'[84]

In the 1930s cancer chemotherapy studies were seen as a marginal, slightly disreputable subject.[85] On the other hand, the search for anti-cancer drugs became a major research subject from the mid-1940s on. Two reasons contributed to this change: the dramatic increase in funds available for cancer studies and the influence of war research. The war demonstrated the efficiency of large-scale, centrally organized research such as the Manhattan Project and, in medical research, the development of penicillin and the fight against malaria. In addition the US Office of Scientific Research and Development (OSRD) directly financed studies which led to the development of antitumour drugs. Research on war gases led to the application of nitrogen mustard to lymphoma therapy, while studies of vitamins led to the application of folic acid to the therapy of childhood leukaemia.[86] The first important screening programme for anti-cancer therapies was organized by Cornelius Rhoads, director of the Memorial Hospital, New York. During the Second World War, Rhoads was head of the Medical Division of the Chemical Warfare Service and became an enthusiastic adept of 'big science', important budgets, centralized planning and large-scale collaborative efforts. In 1945 Alfred Sloan, the president of General Motors, pledged one million dollars for a cancer

research institute at the Memorial Hospital. The institute, directed by Rhoads and named after Sloan and the director of the research division of General Motors, Charles Kettering, set as its explicit goal the 'organization of industrial techniques for cancer research'.[87] The mass-testing of anti-cancer drugs fulfilled this goal. The Sloan-Kettering programme, which screened several thousand chemical compounds (it worked in close collaboration with the pharmaceutical firm Burroughs Wellcome) used a single transplanted tumour – Sarcoma 180 – transferred in randomly-mated albino mice. The main advantage of the S-180 tumour was its cost. Non-inbred mice were cheap and easy to maintain: the use of the S-180 tumour thus reduced the costs of screening and allowed a significant increase of the number of tested compounds. S-180 was also the main tumour employed in the screening programme developed in the late 1940s and early 1950s at the NCI. Not all specialists agreed with the principle of screening drugs in transplanted tumours only. One of the pioneers of chemotherapy studies in the USA David Karnofsky, explained in 1948 that while transplanted tumours were indeed the most practical solution for the screening of chemotherapeutic compounds, 'in animals bearing transplanted tumours the host–tumour relationship should be considered abnormal, and a more critical evaluation of chemotherapeutic activity may be conducted in mice with spontaneous or carcinogen induced tumours.'[88]

Cost–efficiency considerations prevailed, however. In the early 1950s professionals as well as the lay public shared the feeling – based partly on the recent success of antibiotics – that the control of malignant disease through drugs was imminent, and that it would be achieved through the development of large-scale screening programmes, which would allow for the discovery of the 'penicillin of cancer'.[89] This widespread feeling was transformed into a direct political issue when, in 1953, the US Congress requested that the NCI develop an extramural (that is, a non-NCI) chemotherapy programme for leukaemia. The Congress also allocated one million dollars for leukaemia research. NCI scientists resisted Congressional pressure at first, and were reluctant to share control of chemotherapy studies with outsiders. The continuous pressure of Congress, together with the growing demands of non-NCI cancer specialists and of the chemical industry, led to the development of the Cancer Chemotherapy National Service Center (CCNSC) in 1955.[90] The CCNSC structure was a compromise: it was formally part of the NCI, but decision-making power was delegated to panels which included numerous extramural scientists and physicians: a chemistry panel, a clinical studies panel, a pharmacology panel, an endocrinology panel and a screening panel. Congress rapidly approved the creation of CCNSC and allocated to this organization

5.6 million dollars in 1956, 20 million in 1957 and 28 million in 1958. The explicit aim of the new programme was, in the words of one of its main organizers, Gordon Zubrod, 'to set up all the functions of a pharmaceutical house run by the NCI', while the CCNSC head, Kenneth Endicott, explained that the problems of cancer chemotherapy would be solved 'when industry–government cooperation will be as effective in the pharmaceutical areas as it is in some of the defense areas'.[91]

Preclinical screening of drugs, unlike e.g. clinical research, can be relatively easily adapted to standardized, industry-like patterns of production.[92] Uniformization of mice was one of the important elements in such standardization. The screening panel of the CCNSC decided that three transplantable tumours would be used in all the screening tests: a sarcoma S-180 (transplanted in non-inbred Swiss mice), a carcinoma Ca 755 (transplanted in C57BL/6 mice) and a leukaemia, L-1012 (transplanted in DBA/2mice). The screening panel also decided to use F_1 hybrids of DBA/2 females and C57 BL/6 males (BDF1), which accepted both Ca 755 and L-1012 tumours, and which were less fragile than the inbred parents. The decision to use two tumours transplanted in inbred strains of mice immediately created a need to increase the production of such mice. From 1956 on the CCNSC collaborated with the Institute for Laboratory Resources at the National Research Council to develop minimal standards for laboratory animals (the ILAR standards for the Care of Laboratory Animals). It developed a mouse production programme, funded by a special NCI grant. The goals of the programme were explicitly formulated in industrial terms: its directors discussed the volume of input and output of the product, the problem of standardization, and quality controls.[93] The mouse breeding programme built on the division of labour which prevailed at the Jackson Memorial Laboratory. Mass production was first conducted in Bar Harbor, and later was extended to commercial laboratories (Battelle, Texas Inbred, Simonsen Laboratories, and Pfitzer).[94] All the animals used in screening tests had to be supplied by producers accredited by the CCNSC, and all the demands for supply of mice were processed through CCNSC's Mammalian Genetics and Animal Production Section.[95] The control of genetic purity was achieved by the Jackson workers, who supplied certified breeding pairs of mice to other commercial breeders.

The CCNSC thus opened a large specialized market, which facilitated the development of commercial laboratories supplying 'more uniformly healthy, well fed mice, with known genetic background and variability'.[96] An optimal uniformization of mice was important, because all the screening activities of the CCNSC were delegated to commercial and semi-commercial laboratories: Wisconsin Alumni Research Foundation, Microbiological Associated, Armour Research Foundation (Chicago),

Beattle Memorial Institute (Columbus) and Hazelton Laboratories, Southern Research Institute, and Standford Research Institute. These laboratories were controlled by rigid protocols that ensured the comparability of test data and reliability of activity.[97] The ultimate goal of the CCNSC was to produce physiological uniformity, not genetic purity: 'mice produced by accredited breeders', the CCNSC officials explained, 'are of superior quality. They will give more dependable uniform bioassay results than mice produced under substandard conditions.'[98] In contrast to the early practice at the Jackson Laboratories, standardization did not aim at the control of one biological factor, in order to purify the model of cancer causation, but at the production of identical animals.

The adoption of the 'three screen' principle in 1956 was due to engineering and management considerations. This principle was seen as an acceptable compromise between the requirement of greater variability among tested malignancies, and the constraints of large-scale testing. Legitimacy for the decision to use three (rather than two, five or ten) transplantable mouse tumours was not sought in scientific considerations. At the time this decision was taken (1955), the organizers of the first US conference on cancer chemotherapy screening, Gelhorn and Hirshorn, after an extensive review of the existing data, concluded that 'the comparison of studies of anti-tumour activity in experimental and human neoplasms is hazardous because of the relative paucity of reliable clinical data.' Gelhorn and Hirshorn noted one exception to the poor correlation between results of screening in transplanted tumours and clinical results in humans, namely the use of mouse leukaemia as a model for screening for the cytostatic properties of anti-leukaemia drugs. Slowly growing solid cancers which depend on complicated interactions with the host were not, however, adequately represented by murine models.[99]

Four years later, Gelhorn, one of the leading US specialists in the drug therapy of cancer, strongly criticized CCNSC methods of screening for anti-tumour drugs. In his testimony before the meeting of the National Advisory Cancer Program (June 1959), Gelhorn affirmed that the expanded screening programme did not lead to the discovery of new, clinically important classes of agents. The method employed by the CCNSC, he added, was inefficient: 'the mass and mechanized type of screening now employed is less likely to be productive than the observations of the individual investigators.'[100] Scientists associated with the CCNSC programme observed, in the meantime, poor correlations between screening results obtained in the three-screen system and the results of their first clinical trials. Therefore, they proposed to enlarge CCNSC screening to include other tumours and other laboratory animals.[101] In 1961 they proposed a new list of tumours accredited by the CCNSC. This list included six additional mice tumours (four transplanted in BDF1, two in C3H/He, eight rat tumours transplanted either in 'suitable random bred' rats (6) or in Fisher/344 rats (2), and six hamster tumours.[102] The CCNSC experts also decided to reduce the number of screened compounds. These changes in the organization of CCNSC services did not, however, end the controversy between scientists and doctors who advocated an industrial-type of search for cancer-inhibiting compounds and the supporters of a more traditional style of investigation, based on individual expertise.[103] In February 1965 the Woolridge committee, an outside group which evaluated the NIH, reported that the research conducted by the CCNSC had been less scientifically distinguished than traditional research programmes supported by the NIH. The NCI leadership failed, the committee added, not because it did not fulfil its difficult goals, but because it operated without sufficient advice from the scientific community. The Woolridge committee recommended a thorough review of the administration and management of CCNSC collaborative programmes. Following this recommendation, the CCNSC was completely reorganized in 1966 under new leadership.[104]

Facing the criticism that too much public money was being spent on a programme that failed to uncover new anti-cancer drugs and was unable to develop new directions in pre-clinical or clinical research, CCNSC directors argued that beside concrete achievements in the organization of efficient testing for anti-cancer drugs, their programme brought important benefits to the scientific community as a whole. One of the most important results of the chemotherapy screening programme, they explained, was

> the development of enough high-quality animal resources to meet the needs of the programme and the entire scientific community. The programme was a key factor in anticipating and providing such resources for the major expansion of biomedical research in the past decade.[105]

The mass production of mice and other laboratory animals was thus presented as the crafting of the 'right tool for the job': the establishment of an efficient network of commercial suppliers of clean, healthy, reasonably standardized animals for a rapidly growing market in biomedical research.

GENETIC PURITY AS AN IMPURE PRACTICE: MASS-PRODUCED MICE AND TUMOUR VIRUSES

The US chemotherapy programs stabilized the commitments to inbred lines of mice. Highly visible industrial methods were imported to guarantee the homogeneity and reliability of chemotherapeutic screening, but at the same time the industrial work actually involved in standardization became invisible, because inbred mice were increasingly viewed as unproblematic, ready-made tools. At the National Cancer Institute, this "blackboxing' of genetic homogeneity paradoxically resulted in the troublesome 'observation' that the practice of genetic purity was an impure work with viruses. The change from 'genetic purity' to viruses began with the transfer of Ludwig Gross's results and practices into National Cancer Institute laboratories.[106] Gross, a researcher from the Veterans Hospital in New York, during the Second World War, endorsed a radical view of cancer as a vertical epidemic caused by parasitic agents.[107] Displacing Bittner's milk agent, Gross defined 'vertical transmission of oncogenic agents' as a new class of infectious diseases exemplified by mouse mammary tumours:

> Vertical epidemic ... designates the transmission of potentially pathogenic agents from one generation to another ... The agent, though potentially pathogenic, would remain latent for a long period of time, often perhaps throughout the entire life-span of a given carrier host ... Thus the host would remain in perfect health, while carrying and transmitting the seeds of the disease. In some hosts, however, particularly in those reaching middle age, the agent may become activated. Triggered by intrinsic or extrinsic stimuli, the hitherto latent and harmless agent would then change into a formidable pathogen, causing rapid multiplication of cells harboring it, and killing its carrier.[108]

Since the 'enemy within' was no longer a gene but a transmissible factor, Gross could mobilize the classical resources in public health in order to overcome problems linked to attempts at controlling human mating:

> It is possible to assume, however, that mammary carcinoma of mice does not represent a form of cancer different from breast cancer in other mammals. If this assumption is correct, the law of obligate communicability may in the future be established also for breast cancer in such animals as rats, rabbits, or dogs, or perhaps also in women. Should such a possibility materialize, the eradication of human breast cancer may become feasible by the simple method of artificial feeding of infants born to mothers having a family history of tumours.[109]

Gross tried to extend the example of mouse mammary tumours to other types of cancer. Two strains of mice showing high incidence of leukaemia had been selected by J. Furth (Cornell University) and J. MacDowell (Cold Spring Harbor) respectively. Gross received a breeding pair from the former and started a colony of the high-leukaemia strain Ak.[110] From 1945 onward, Gross, who before the war had worked in the immunology department of the Pasteur Institute, Paris, applied bacteriological methods to the study of cancer. He inoculated extracts of various organs from leukaemic mice into animals of the mammary-tumour strain C3H. For five years this method failed to produce malignancies. By the early 1950s, however, Gross announced that, thanks to a change in the inoculation technique, namely the use of newborn (less than 24 hours old) mice, he successfully transferred leukaemia with filtered, cell-free extracts of leukaemia tissues. In other words, he claimed that he had discovered a mouse leukaemia agent, presumably a filterable virus.[111]

Lloyd W. Law, who was in charge of leukaemia studies in the biological laboratory of the National Cancer Institute, was especially interested in Gross's results.[112] However, Law and his associates could not replicate these results: their attempts to prepare and inoculate extracts failed to increase the incidence of leukaemia in test mice.[113] Though the two experimental systems differed in many respects, Gross unsurprisingly used the issue of genetic purity to justify the discrepancy between his and Law's results. He announced that he could actually split his test C3H mice into two groups: one was very susceptible to the agent, the other failed to develop leukaemia.[114] The pedigrees of these two groups differed: susceptible mice were C3H animals originating in a breeding pair received from Bittner; non-susceptible mice originated in a pair received from NCI biologist H. Andervont. Presumably, he proposed, Law's mice were also from the non-suspectible sub-strain. The debate led to a messy assessment of local histories and putative mutations.

NCI scientists did not confirm Gross's results with C3H, but evertheless built on his system. In 1955, they confirmed a different observation made by Gross, namely that some extracts of leukaemic tissues induced the formation of large, solid tumours of the salivary glands. Moreover, Gross's announcement was rapidly followed by considerable work with local resources, i.e. inbred mice, transplantable tumours, and inoculation procedures. This tinkering bore significant resemblance to the culture of chemotherapeutic screening. First, inbred mice were used either as resource-providing viruses or as homogenous hosts. For example, the strains previously employed to mimic breast cancer were turned into simple, standardized recipients of leukaemic tissues. Secondly, the search for tumour viruses was akin to the screening of chemicals. Once the job had been decided on, there was no way of predicting which tool would be the right one. Almost anything was worth testing. Within a few years, NCI researchers announced the discovery of two other viruses. In 1957, Sarah Stewart employed local techniques in tissue culture to isolate and multiply a 'mouse-leukaemia-derived parotide tumour agent' previously associated with Gross's virus.[115] In 1958, John Moloney inoculated C3H newborns with extracts from the transplanted sarcoma S 37 used in the chemotherapeutic screen. Quite unexpectedly, the extracts did not induce a sarcoma but a leukaemia. Pursuing the viral track, Moloney isolated a leukaemia agent which was viewed as a latent virus transmitted with the transplanted tumours for almost fifty years.[116] In other words, agents causing cancer in mice and rodents had been domesticated by the NCI scientists.

The impact of this achievement was threefold. First, the mouse mammary tumour agent was no longer an odd and isolated entity. It was possible to argue that hidden viruses may be responsible for many if not all cancers in mice. NCI biologists turned into aficionados of Gross's theory that 'pure' inbred lines were contaminated, in other words, that they were viral reservoirs. They thought that the agent was an inactive particle, embedded in cells and rarely activated by triggering agents such as metabolic factors, hormones, chemicals or X-rays. This hypothesis explained why most cancers, including human leukaemia, were not infectious and did not occur in family clusters:

> [T]he question arises immediately whether human leukaemia might not also be caused by a cell free agent transmitted in an inactive, masked form from one generation to another, possibly through the germinal cells. It is evident that such an agent might become so well adapted to the human host that it would cause no symptoms of disease in the great majority of the carriers. This could perhaps explain why leukaemia is a relatively rare disease, even though its agent might conceivably be carried by a large number of human hosts.[117]

Second, the industrial model of research management introduced in the chemotherapy programmes was transferred to the study of leukaemia viruses. From 1959 onwards, J. R. Heller, then director of NCI, testified before Congress about the mouse leukaemia viruses, their relations to human cancer and the prospects of cancer vaccines:

> In our own laboratory, Dr. Sarah Stewart and Dr. Bernice Eddy took some of [Gross's] material, injected it into day-old mice and got parotid tumours. They took some of the material from the parotid tumours and placed it in tissue culture. They then took some of the fluid from the

> culture, filtered it, and injected it in day-old mice and obtained a multiple array of tumours ... As in the instance of polyomyelitis and influenza, once the virus agent has been determined, isolated, and identified, it is not too fantastic to conceive there may be a time when a vaccine will be elaborated. We are not saying we have such a vaccine. We are not even saying that viruses are responsible for human cancer ... We think that in viruses we are certainly in a worthwhile area. We have gone so far as to call it a probable breakthrough ... I do not know how long it will take, maybe 7 years or maybe a decade, but at least we are on an existing trail and the dogs are barking in great style, and I think that we may get some very exciting game here.[118]

The debates on chemotherapy programmes sensitized the members of the US Congress to the problems of cancer research in general and leukaemia studies in particular.[119] In the 1960s the Congress gave increasing approval for virus research: $2.5 million in 1960; 4.5 million in 1962; 10 million for a Special Leukaemia and Lymphoma Branch in 1964. An impressive contract system that linked NCI laboratories, major cancer hospitals and private companies was gradually established in order to find human cancer viruses and develop vaccines. The expanding network rapidly produced a dozen viruses involved in the causation of tumours in mice.

Third, the blackboxing of mice previously achieved in chemotherapeutic research spread to the entire virus programme, thanks to industrial mediation. Rather than increasing the uses of mice as models, tinkering with tumour viruses blurred the distinction between transplanted and spontaneous tumours, and reinforced the utility of inbred mice as instruments. When the focus of scientist interests shifted toward transmissible agents, inbred mice became even less visible. For example, Meloy Laboratories, company launched with NCI support, implemented a contract to

> propagate, concentrate, distribute Mouse Mammary Tumour Virus, perform immunological and biological assays for detection and quantitation; develop methods for propagation and detection of MMTV antigens; conduct studies on the control of neoplasia in the susceptible murine host by vaccination with inactivated virus.[120]

The unmentioned natural hosts and sources of virus were the locally inbred C3H mice.

Consequently, viral purity replaced genetic purity as a matter of concern and management. In 1958, Gross commented:

> It is quite possible, therefore that there exist different forms of mouse leukaemia, caused by distinct though probably related viruses, requiring different experimental conditions for cell-free transmission. Certain leukaemic agents may require newborn hosts for transmission; others may also infect adults hosts of a suitable line.[121]

In 1958, experimental oncologists like Gross dealt with an integrated system which included the agent and his host. The purity and nature of the 'suitable line' was therefore important in the assessment of the biological specificity of the agent. A few years later, when the search for immune reagents which reacted with mouse leukaemia viruses expanded within the framework of the virus programme, the host range of the virus was no longer viewed as an important issue. Since the host range changed while the potency increased, serological tests provided the best criteria for the definition of viruses:

> Recently we have studied one of the more potent leukaemogenic virus strains isolated by Dr. J. B. Moloney from a transplanted mouse sarcoma ... this virus strain is indistinguishable from the Passage A virus in its physical and pathogenic properties except for a slightly higher titer ... under proper quantitative conditions, both Passage A and the Moloney strain could be neutralized *in vitro* by the same specific serum obtained from rabbits immunized by repeated injections of Passage A virus filtrate.[122]

One more step and genetic purity would be a hindrance. In 1970, the members of a scientific committee set up by Congress in order to suggest goals for a national cancer plan, wrote:

> The actively oncogenic viruses used in the laboratory are preselected and further exaggerated by the choice of highly susceptible dominant strains whose virulence has been greatly increased by inbred animal hosts, at least for the first transfers. Thus the experimental system represents a highly simplified situation and is not at all the natural relationship that any one of these viruses has with its natural host ... The past success in isolating cancer viruses is due largely to the existence of inbred strains of animals in which genetic differences have been eliminated by inbreeding. There are no comparable inbred strains in humans, and even if there were, passage of virus from individual to individual would not be permissible ... Wholly new methods were called for and these have now been devised.[123]

These 'new methods' were cell culture techniques and 'pure' immunological assays developed by industrialists who did contractual work for the NCI.[124]

CONCLUSION

This chapter has illustrated the difficulties of stabilizing the uses of inbred mice and the meanings of genetic purity in cancer laboratories. Until the 1920s transplanted tumours were viewed as adequate models to study the fate of human tumour cells and the resistance of the body to malignancies. Cancer biologists who used this model had to cope however, with the variability of experimental results and with the similar fate of grafted tumours and grafted non-malignant tissues. They responded with a discourse on genetic homogeneity and, at the same time, switched to the use of spontaneous tumours. In the 1920s and 1930s, when a few centres started to produce, and eventually sell inbred strains of mice, concerns and interests in the genetic purity of mice emerged as a practical issue. The increasing circulation of genetically standardized mice favoured the establishment of homogenous practices in the laboratory, but it did not stabilize the association with the clinic. As models, inbred lines proved multi-factorial enough to make clinicians happy, but they were too complex and variable for advanced studies of cancer genetics, and too expensive for large-scale use. After the Second World War, oncologists therefore fell back on transplanted tumours in order to screen putative anti-cancer chemicals. The NCI organized the mass production of a few inbred strains employed as identical recipients of standardized tumours, replacing the search for genetic purity by the more practice-oriented search for uniformity and reproducibility. The development of tumour-virus programmes coordinated by the NCI finally transformed the mouse models into tools for the study of mouse viruses. Purity, once a problem of breeding and genetic control, turned into an evaluation of micro-organisms. Genetic homogeneity then evolved into a self-evident, industrially certified, and hence occulted practice.

In the 1920s, Henry Ford wrote:

> 'Mass production is not merely quantity production, for this maybe had none of the requisites of mass production. Nor is it merely machine production, which may also exist without any resemblance to mass production. Mass production is the focusing upon a manufacturing project of the principles of power, accuracy, economy, system, continuity and speed.[125]

Following Ford's definition, one may state that the development of inbred mice was turned, in the 1950s and 1960s, into mass production. In the 1930s, when the operations at the Jackson Laboratory were scaled up, Little and his staff strongly emphasized the similarities between their products and industrially produced chemicals. This was, however, standardization rhetoric rather than industrial practice. Following the importation of Jackson breeding pairs, many laboratories launched local production of 'cancer mice' based on local know-how and circumstantial comparisons with external references. The homogenization of laboratories using inbred lines was thus facilitated by the circulation of methods and results provided by the Jackson Laboratory, rather than by an increased consumption of standardized, mass-produced organisms. The regulation based on local craftsmanship prevailed until after the Second World War when the growth of government-directed chemotherapeutic research resulted in an impressive scaling-up of inbred mice production. Public (the NCI laboratories) as well as semi-private (the NCI contractors) and private settings (the Jackson Laboratory) were involved. NCI officials endorsed industrial methods for the management of large-scale research systems, while the increasing amount of work involved in the production and standardization of inbred mice was made invisible to end users. By virtue of their availability, inbred strains became prerequisites in cancer research, and genetic homogeneity was blackboxed.

Observers of science have highlighted the part played by tinkering, tacit knowledge, informal and formal exchanges of material, instruments or recipes in the homogenization of scientific practices. It is only recently, however, that they have also emphasized the role of commercially produced instruments and kits, the circulation of standards, and metrology. Accordingly, few studies have shown the processes by which industrial values and industrial commodities contributed to the construction of scientific knowledge. The fate of inbred mice in cancer research is one example of such 'standardization' of laboratory life by mass-produced entities. The transformation of artificially uniformed mice into unproblematic instruments resulted in increased uniformity of research practices. In the context of laboratory life, mice were affected with the same value as men. We would like to stress, however, that biologists did not become medical practitioners. In other words, while mice standardization played an important role in the stabilization of experimental cancer research, genetically homogeneous animals failed to play a similar role in the interplay between biological and clinical knowledge.[126] The implicit prospects behind the controlled production of mice and the debates on genetic purity, namely the use of model systems which would reveal well-defined mechanisms of human cancer and establish a genetic (or later metabolic) vision of cancer causes, were not achieved through the circulation of inbred animals. As one cancer expert put it in 1978:

> The vast effort that has gone into the immunologic study of neoplasia in laboratory animals has been predicated on and justified by the tacit assumption that experimental tumours serve de facto and de jure as valid examples of malignant growths in man. As is often the case with tacit assumptions, however, the distance between the supposition and the reality may be considerable, and tends to lengthen with time and habituation.[127]

Inbred mice linked the laboratory and the clinic in many ways, and these links were favoured by the consecutive displacement of purity debates, but, in the longterm, the ties between the bench and the bedside remained fragile. The debate on the artificiality of inbred mice and the meaning of mice tumours for the clinic which first came to the fore in the 1920s is still open seventy years later.

Notes

1. C. C. Little. Joint Hearings on Cancer Research. US Senate, Committee on Interstate and Foreign Commerce. 8 July 1937.
2. Hans Grünberg, *The Genetics of the Mouse* (The Hague: Nijhoff, 1952) p. 437.
3. A German expert in clinical cancer research, interviewed by Rainer Hohlfeld. R. Hochlfeld, 'Two Scientific Establishments Which Shape the Pattern of Cancer Research in Germany: Basic Science and Medicine', in Norbert Elias, Herminio Martins and Richard Whitley (eds) *Scientific Establishments and Hierarchies*, Sociology of Sciences Yearbook 1982 (Dordrecht, Boston and London: Reidel, 1982) pp. 145–68, on p. 155.
4. '[O]n ne doit pas oublier que le laboratoire constitue lui-même *un nouveau milieu* dans lequel certainement la vie institue des normes dont l'extrapolation, loin des conditions auxquelles ces normes se rapportent ne va pas sans aléas. Le milieu de laboratoire est pour l'animal ou l'homme un milieu possible parmi d'autres ... pour le vivant appareils et produits sont des objets parmi lesquels il se meut comme dans un monde insolite. Il ne se peut pas que les allures de la vie en laboratoire ne retiennent pas quelque spécificité

de leur rapport au lieu et au moment de l'expérience.' G. Canguilhem, *Le Normal et le pathologique* (Paris: Presses Universitaires de France, 1966) p. 95. English translation: *The Normal and the Pathological* (New York, Zone Books, 1991) p. 148.
5. We do not consider in this paper other uses of rodents in the laboratory, for example physiological studies, that started earlier.
6. R. E. Kohler, *Lords of the Fly*, Drosophila *Genetics and the Experimental Life* (Chicago University Press, 1994). K. Rader, 'Making Mice: C. C. Little, the Jackson Laboratory and the Standardization of Mus musculus for Research', PhD. thesis, Indiana University, 1995.
7. L. J. Rather, *The Genesis of Cancer: A Study in the History of Ideas* (Baltimore and London: Johns Hopkins University Press, 1978). Rather's conclusion is that the understanding of the nature of malignant tumours was achieved at the end of the nineteeth century, while the progress in the understanding of the histogenesis and cytogenesis of tumours in the tweenteeth century might be characterized as glacial. Ibid., p. 179.
8. Michael B. Shimkin, 'M. A. Novinsky: A Note on the History of Transplantation of Tumours', *Cancer*, vol. 8 (1955) pp. 653–5.
9. For example, Leo Loeb, 'On the Transplantation of Tumours', *Journal of Medical Research*, vol. 6 (1901) pp. 28–38; E. F. Bashford and J. A. Murray, 'Carcinoma Mammae in the Mouse', *Lancet*, vol. i (1907) pp. 798–800; P. Ehrlich and H. Apolant, 'Beobachtungen über maligne Mäusetumoure', *Berl. klin. Wochenschrift*, vol. 42 (1905) p. 871; A. Borrel and M. Haaland, 'Tumeurs de la souris', *C. R. Soc. Biol.*, vol. 58 (1905) p. 14; E. E. Tyzzer, 'The Inoculable Tumours of Mice', *Journal of Medical Research*, vol. 17 (1907) pp. 147–51.
10. William Woglom, *The Study of Experimental Cancer: A Review* (New York: Columbia University Press, 1913).
11. M. Haaland, 'Spontaneus Tumours in Mice', *Scientific Report of the Imperial Cancer Fund*, vol. 4 (1911) pp. 1–113; E. F. Bashford, 'The Behaviour of Tumour Cells During Propagation', *Scientific Report of the Imperial Cancer Fund*, vol. 4 (1911) pp. 131–55.
12. Paul Ehrlich, 'Experimentelle Carcinomstudien an Mäusen', *Arbeit aus dem Königlichen Institut für experimentale Therapie*, (Frankfurt) vol. 1 (1906) pp. 77; E. F. Bashford, J. A. Murray and W. Cramer 'The Growth of Cancer Under Natural and Experimental Conditions', *Scientific Report of the Imperial Cancer Research Fund*, vol. 2 (1905) pp. 1–96.
13. E. F. Bashford, J. A. Murray and M. Haaland, 'Resistance and Susceptibility to Inoculated Cancer', *Scientific Report of the Imperial Cancer Research Fund*, vol. 3 (1908) pp. 359–97; E. F. Bashford, 'The Immunity Reaction to Cancer', *Proceedings of the Royal Society of Medicine*, vol. 3 (1910) pp. 69–81.
14. Alexis Carrel, 'Remote Results of the Retransplantation of the Kidneys and the Spleen', *Journal of Experimental Medicine*, vol. 12 (1910) pp. 146–50.
15. Peyton Rous, 'An Experimental Comparison of the Transplanted Tumour and Transplanted Normal Tissue Capable of Growth', *Journal of Experimental Medicine*, vol. 12 (1910) pp. 344–69; Peyton Rous, 'Resistance to Tumour-Producing Agent as Distinct from Resistance to the Implanted Tumour', *Journal of Experimental Medicine*, vol. 18 (1913) 416–25.

16. James B. Murphy and John J. Morton, 'The Lymphocyte in Natural and Induced Resistance to Transplanted Cancer', *Journal of Experimental Medicine*, vol. 22 (1915) pp. 204–9; Ilana Löwy, 'Biomedical Research and the Constraints of Medical Practice: James Bumgardner Murphy and the Early Discovery of the Role of Lymphocytes in Immune Reactions', *Bulletin of the History of Medicine*, vol. 63 (1989) pp. 356–91.
17. William Woglom, 'Immunity to Transplanted tumours', *Cancer Review*, vol. 4 (1929) pp. 129–214; Johannes Clemensenn, *The Influence of X-Radiation on the Development of Immunity ot Heterologous Transplantation of Tumours* (Oxford: Oxford University Press, 1938). In the late 1930s Murphy employed inbred strains of mice to show that the so-called 'resistance' to tumours was in fact resistance to foreign cells. *The Rockefeller Institute: Reports of the Directors of the Laboratories*, vol. 27 (1939) p. 99; James B. Murphy, 'An Analysis of Trends in Cancer Research', *Journal of the American Medical Association*, vol. 120 (1942) pp. 107–11.
18. For example, C. O. Jensen, 'Übertragbare Rattensarcome', *Zeitschr. f. Krebsforschung* vol. 7 (1908) p. 45; F. P. Gay 'A Transmissible Tumour Considered from the Standpoint of Immunity', *Journal of Medical Research*, vol. 20 (1909) pp. 175.
19. Haaland, 'Beobachtungen über natürliche Geschwulstresistenz bei Mäusen', *Berlin. klin. Wochenschr.*, vol. 44 (1907) pp. 713.
20. Leo Loeb, 'Über Entstehung eines Sarcoms nach Transplantation eines Adenomcarcinoms einer japanischen Maus', *Zeitschr. f. Krebsforschung*, vol. 7 (1908) pp. 80.
21. E. E. Tyzzer, 'A Study of Heredity in Relation to the Development of Tumours in Mice', *Journal of Medical Research*, vol. 17 (1907) pp. 199–211; E. E. Tyzzer, 'A Series of Spontaneous Tumours in Mice with Observations on the Influence of Heredity on the Frequency of their Occurence', *Journal of Medical Research*, vol. 20 (1909) pp. 479–519.
22. *Ibid*, p. 521.
23. *Ibid*, pp. 570–1.
24. L. Cuénot and L. Mercier, 'Etudes sur le cancer des souris. L'hérédité de la sensibilité à la greffe cancéreuse', *C. R. Acad. Sci. (Paris)*, vol. 150 (1910) pp. 1443–6; L. Cuénot and L. Mercier, 'L'hérédité de sensibilité à la greffe cancéreuse ches les Souris. Résultats confirmatifs', *C. R. Soc. Biol. (Paris)*, vol. 69 (1910) pp. 645–6. Cuénot and Mercier first looked for correlations between visible racial characters and suspectibility to grafted tumour, but found none. L. Cuénot and L. Mércier, 'Etudes sur le cancer des souris. Y-a-t-il un rapport entre les différentes mutations connues chez la souris et la réceptivité à la greffe?', *C. R. Acad. Sci. (Paris)*, vol. 147 (1908) pp. 1003–5.
25. C. C. Little and E. E. Tyzzer, 'Further Experimental Studies of the Inheritance of Susceptibility to a Transplantable Tumour, Carcinoma (J. W. A.) of the Japanese Waltzing Mouse', *Journal of Medical Research*, vol. 33 (1916) pp. 393–427.
26. Later Little transplanted a different tumour of Japanese waltzing mice, J. W. B. sarcoma, into F_1 and F_2 hybrids of these mice, and interpreted his results as indicating that four to five genes controlled the susceptibility to J. W. B. tumour grafts. C. C. Little, 'The Heredity of Susceptibility to a Transplantable Sarcoma (J. W. B.) of the Japanese Waltzing Mouse', *Science*, vol. 51 (1920) pp. 467–8.

27. C. C. Little, and B. W. Johnson, 'The Inheritance of Susceptibility to Implants of Splenic Tissue in Mice: Japanese Waltzing Mice, Albinos and Their F_1 Generation Hybrids', *Proc. Soc. Exp. Biol. Med.*, vol. 19 (1921) pp. 163–7, on p. 167.
28. James B. Murphy, 'Certain Etiological Factors in the Causation and Transmission of Malignant Tumours', *American Naturalist*, vol. 60 (1926) pp. 227–33, on p. 227.
29. C. C. Little, 'The Relations of Genetics to the Problems of Cancer Research', *Harvey Lectures 1921–1922*, Series XVII (Philadelphia and London: Lippincott, 1923) pp. 65–88, on p. 83. Little and other mouse genticists continued to argue that transplantation of tumours may contribute to the understanding of basic processes that underlie malignant transformation. P. A. Gorer, 'The Significance of Studies with Transplanted Tumours', *British Journal of Cancer*, vol. 12 (1948) pp. 103–7; C. C. Little, 'Genetics and the Cancer Problem', in L. Dunn (ed.) *Genetics in the XXth Century*, (New York: Macmillan, 1958) pp. 431–72; P. B. Medawar, 'Peter Alfred Gorer, 1907–1961', *Biographical Memoirs of Fellows of the Royal Society*, vol. 7 (1961) pp. 95–109.
30. Joan Austoker, *A History of the Imperial Cancer Fund, 1902–1986* (Oxford University Press, 1988) pp. 42–54. The Wistar rat was developed as a standard tool for the physiologist. Bonnie Tocher Clause, 'The Wistar Rat as the Right Choice: Establishing Mammalian Standards and the Ideal of a Standardized Mammal', *Journal of the History of Biology*, vol. 26 (1993) pp. 329–50.
31. *Life*, 1 April 1937.
32. *Time*, vol. XXIX (1937) no. 12 p. 54.
33. *Newsweek*, 1937, 10 April.
34. Little and Tyzzer, 'Further Experimental Studies of the Inheritance', p. 396.
35. J. J. MacCoy, *The Cancer Lady: Maud Slye and Her Heredity Studies* (New York: 1977).
36. M. Slye, 'The Incidence and Inheritability of Spontaneous Cancer in Mice', *The Journal of Cancer Research*, vol. 15 (1915) pp. 159–200.
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38. C. C. Little, 'Cancer and Heredity', *Science*, vol. 42 (1915) pp. 218–19.
39. M. Slye, 'A Reply to Dr. Little', *Science*, vol. 42 (1915) pp. 246–8.
40. R. G. Clark, 'The Social Uses of Scientific Knowledge: Eugenics and the Career of C. C. Little', Master thesis, University of Maine, 1956.
41. Slye, 'The Incidence and Inheritability'.
42. M. Slye, 'The Relation of Heredity of Cancer'. *The Journal of Cancer Research*, vol. 12 (1928) pp. 83–133.
43. M. Slye, 'The Incidence and Inheritability'.
44. The organization of the Jackson Memorial Laboratory is the topic of K. Rader's thesis, 'Making Mice: C. C. Little, the Jackson Laboratory and the standardization of *Mus Musculus* for Research', University of Indiana. For personal accounts of the participants, see H. C. Morse III (ed.), *Origins*

of Inbred Mice (New York and San Diego: Academic Press, 1978); E. L. Green 'The Jackson Laboratory: A Center for Mammalian Genetics in the United States', *Journal of Heredity*, vol. 57 (1965) pp. 3–12.

45. For a discussion of similar regimes, R. E. Kohler, 'Systems of Production: *Drosophila, Neurospora* and Biochemical Genetics', *Historical Studies in the Physical and Biological Sciences*, vol. 21 (1991) pp. 87–127.
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47. C. C. Little, 'Unatural Selection and its Resulting Obligations', *Birth Control Review*, vol. 10 (1926) pp. 243–4
48. L. C. Strong, 'The Establishment of the A Strain of Inbred Mice', *Journal of Heredity*, vol. 28 (1937) pp. 21–24.
49. Rader, 'Making Mice: C. C. Little'.
50. G. D. Sneli (ed.) *The Biology of Laboratory Mouse*, by the Staff of the Jackson Memorial Laboratory (McGraw-Hill, 1941).
51. C. C. Little, 'The present status of our knowledge of heredity and cancer', *Journal of the American Medical Association*, vol. 106 (1936) pp. 2234–5.
52. C. C. Little, 'Opportunities for Research in Mammalian Genetics', *Scientific Monthly*, vol. 26 (1928) pp. 521–34.
53. Strong, 'The Establishment of the A Strain'.
54. Staff of the Roscoe B. Jackson Memorial Laboratory, 'The Existence of Non-Chromosomal Influence in the Incidence of Mammary Tumours in Mice', *Science*, vol. 78 (1933) pp. 465–36.
55. J. J. Bittner, 'Some Possible Effects of Nursing on the Mammary Tumour Incidence in Mice', *Science*, vol. 84 (1936) pp. 162–5.
56. For a vivid presentation of the problem, see C. Oberling, *The Riddle of Cancer* (Yale University Press, 1944).
57. G. W. Woolley, L. W. Law and C. C. Little, *Cancer Research*, vol. 1 (1941) 955–6; E. Fekete, C. C. Little, 'Observations on the Mammary Tumour Incidence of Mice Born from Transfered Ova', *Cancer Research*, vol. 2 (1942) pp. 525–30.
58. J. J. Bittner, 'Breast Cancer in Mice as Influenced by Nursing', *Journal of the National Cancer Institute*, vol. 1 (1940) pp. 155–68; J. J. Bittner, 'Possible Relationship of Estrogenic Hormones, Genetic Susceptibility, and Milk Influence in the Production of Mammary Cancer in Mice', *Cancer Research*, vol. 2 (1942) pp. 710–21.
59. This was not true with all Jax strains affected with mammary tumours, but for C3H which was the favoured model for breast cancer.
60. J. B. Shimkin, 'As Memory Serves. An Informal History of the National Cancer Institute. 1937–1957', *Journal of the National Cancer Institute*, vol. 59 (1977) pp. 559–600. See pp. 566–7 on Andervont's career.
61. H. B. Andervont, *The Annual Obituary* (1981) pp. 171–2;
62. H. B. Andervont, 'Influence of Foster Nursing upon Incidence of Spontaneous Mammary Tumour Cancer in Resistant and Susceptible Mice', *Journal of the National Cancer Institute*, vol. 1 (1940) pp. 147–53.
63. Bittner, 'Breast Cancer in Mice as Influenced by Nursing'.

64. H. B. Andervont. 'Influence of Hybridization Upon the Occurrence of Mammary Tumours in Mice', *Journal of the National Cancer Institute*, vol. 3 (1943) pp. 359–65.
65. Bittner, 'Possible Relationship of Estrogenic Harmones'.
66. The Radium Institute (Institute de Radium) was founded in 1919, a joint enterprise of Paris University and of the Pasteur Institute, to study biological uses of radioactivity. Its scientific division, supervised by the science faculty of Paris University, was headed by Marie Curie, while its medical division, linked with the Pasteur Institute, was directed by Claudius Regaud. Jean Regaud, *Claudius Regaud*, (Paris: Maloine, 1982); Patrice Pinell, *Naissance d'un fléau: Histoire de la lutte contre le cancer en France 1890–1940*, (Paris: Metailié, 1992).
67. Jeanne Chaine, 'Madame Dobrovolskaïa-Zavadzkaïa 1878–1954', *Bulletin du Cancer*, vol. 41 (1954) pp. 33–4.
68. N. Dobrovolskaïa-Zavadzkaïa, 'Brachyurie et structure génetique de la queue chez la souris', *C. R. Soc, Biol. (Paris)*, vol. 97 (1927) pp. 1583–7; N. Dobrovolskaïa-Zavadzkaïa, 'Sur une souche de souris présentant une mutablité insolite de la queüe', *C. R. Acad. Sci. (Paris)*, vol. 187 (1928) pp. 615–17.
69. N. Dobrovolskaïa-Zavadzkaïa, 'Sur l'hérédité de la prédisposition au cancer spontané chez la souris *C. R. Soc. Biol*, vol. 101 (1929) pp. 518–20; N. Dobrovolskaïa-Zavadzkaïa, 'Sur une ligneé de souris riche en adénocarcinome de la mamelle', *C. R. Soc. Biol.*, vol. 104 (1930) pp. 1191–5. Some of 'extended families' were transformed into lines, through sister-to-brother and mother-to-son breeding. Dobrovolskäia-Zavadzkaïa insisted on the good hygenic conditions in which she kept her mice (the mice were parasite-and infection-free and were maintained in clean and spacious cages) but she did not always supply all the details of matings that led to the establishment of a new line of mice.
70. N. Dobrovolskaïa-Zavadzkaïa, 'Heredity of Cancer', *American Journal of Cancer*, vol. 18 (1933) pp. 357–79.
71. Thus the 'high cancer Paris RIII' line was used in England by Haddow, by Cramer and by Horing. In the US, C57 mice (a 'low mammary cancer line') nursed by RIII mothers were employed by Stanley and his collaborators to isolate a mouse mammary carcinoma virus. S. Graff, C. H. Moore, W. M. Stanley H. T. Randall and C. D. Haagensen, 'Isolation of Mouse Mammary Carcinoma Virus', *Cancer*, vol. 2 (1949) pp. 755–62.
72. N. Dobrovolskïa-Zavadzkaïa, 'Efficacité de la sélection en vue de l'élimination des facteurs héreditaires responsables du cancer spontané dans une lignée de souris (Lignée XVIInc)', *C. R. Soc. Biol. (Paris)*, vol. 126 (1937) pp. 287–89. The line XVIInc remained cancer-free in the 1950s.
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75. For example, A. Lacassagne, 'Hormones esterogens et adenocarcinome mammaire', *C. R. Soc. Biol. (Paris)*, vol. 122 (1936) pp. 183–5;

A. Lacassagne, 'The Relations Between Hormons and Cancer', *Canadian Medical Association Journal*, vol. 37 (1937) pp. 112–17; A. Lacassagne, 'Tentatives pour modifier, par le progesterone ou par testosterone, l'apparition des aadenocarcinomes mammaires provoqués par l'oesterogène chez la souris', *C. R. Soc. Biol. (Paris)*, vol. 126 (1937) pp. 385–387.

76. For example N. Dobrovolskaïa-Zavadzkaïa and F. Garrido, 'Existe-t-il de souris refractaires au cancer de goudron?', *C. R. Soc. Biol. (Paris)*, vol. 122 (1936) pp. 509–11; N. Dobrovolskaïa-Zavadzkaïa, 'Les Doses minimes de 1:2:5:6 dibenzathracene capables de produire dans une seule injection souscoutane une cancer chez la souris', *C. R. Soc. Biol. (Paris)*, vol. 129 (1938) pp. 1055–7.

77. A. Lacassagne 'Hormonal Pathogenesis of Adenocarcinoma of the Breast', *American Journal of Cancer*, vol. 2 (1936) pp. 217–28, on p. 228. Lacassagne noted also that English chemists isolated polyvalent carcinogenic carbons from tar, and that all these carbons contain in their chemical structure the same chemical ring of phenatrene which exists in the formula of certain organic compounds such as sex hormons. Ibid., p. 227.

78. A. Lacassagne and Susanne Danysz, 'Apparition d'une carcinome mammaire chez un mâle d'une ligné de souris non sujete a cette tumeur alaité par une femalle d'une lignée très sujette a cette tumeur', *C. R. Soc. Biol. (Paris)*, vol. 132 (1939) pp. 395–6; A. Lacassagne and Susanne Danysz, 'Importance du rôle joué par l'allaitement dans la production du carcinome mammaire chez la souris', *C. R. Soc. Biol. (Paris)*, vol. 135 (1941) pp. 1130–2; A. Lacassagne, 'Nouveau progres de nos conaissances dans le domaine du carcinome mammaire expérimental', *Bull. Ligue Française de la Lutte Contre le Cancer* (December 1943) 69–72.

79. After the Second World War Lacassagne shifted to studies of carcinogenic effects of aromatic compounds which were seen by him also as potential anti-cancer drugs; e.g., A. Lacassagne, G. Rudali, N. P. Buu-Hoi and J. Lecoeq, 'Activité cancerigène des certains derivés methylés de benzacridines angulaires', *C. R. Soc. Biol. (Paris)*, vol. 139 (1945) pp. 955–6; A. Lacassagne, N. P. Buu-Hoï and F. Zajdela, 'Relations entre structure moleculare et activité carcinogène dans trois séries des hydrocarburates aromatiques hexacycliques', *C. R. Acad: Sci. (Paris)*, vol. 246 (1958) pp. 1477–80. These studies followed observations that aromatic compounds – like sexual hormones – may both induce and inhibit the growth of tumours. A. Haddow, 'Influence of Certain Polycyclic Hydrocarbons on the Growth of Jensen Rat Sarcoma', *Nature*, vol. 136 (1935) 1935; S. Stamer and J. Engleberth-Holm, Influence of Carcinogenic Hydrocarbons upon Transplanted Leukaemia', *Acta path. et microbiol. Scandinav.*, vol. 20 (1943) pp. 360–71. One may note that these experiments, unlike the ones conducted by Lacassagne, employed transplanted tumours;

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81. N. Dobrovolskaïa-Zavadzkaïa, 'Hereditary and Environmental Factors in the Origin of Different Cancers', *Journal of Genetics*, vol. 40 (1940) pp. 157–70.

82. L. Dmochowski, 'Mammary Tumour Inducing Factor and Genetic Constitution', *British Journal of Experimental Biology*, vol. 25 (1944) pp. 138–40.

83. M. J. Shear, 'Isolation of the Hemorrhage-producing fraction from *Serratia Marcescens* Culture Filtrate', *J. Nat. Cancer Inst.*, vol. 4 (1943) pp. 81–97; M. J. Shear, J. L. Hartwell, V. Peters, 'Some Aspects of Joint Institutional Research Program on Chemotherapy of Cancer: Current Laboratory and Clinical Experiments with Bacterial Polysaccharide and with Synthetic Organic Compounds', in F. R. Moulton (ed.) *Approaches to Tumour Chemotherapy* (Washington, DC: American Association for the Advancement Science, 1947) pp. 236–84.

84. Floyd C. Turner, 'Experimental Chemotherapy of Tumours in Mice', *Journal of the National Cancer Institute*, vol. 4 (1943) pp. 265–70, on p. 266.

85. Robert F. Bud, 'Strategy in American Cancer Research after World War II: A Case Study', *Social Studies of Science*, vol. 8 (1978) pp. 425–59, quote on p. 440.

86. Jerzy Einhorn, 'Nitrogen Mustard: the Origins of Chemotherapy for Cancer', *Int. J. Rad. Onc. Biol. Phys.*, vol. 11 (1985) pp. 1375–8; S. Farber, L. K. Diamond, R. D. Mercer, 'Temporary Remission in Acute Leukaemia in Children by Folic Acid Antagonist, 4-Aminoptenyl-Glutamic Acid (Aminopterin)', *New England J. Med.*, vol. 238 (1948) pp. 787–93.

87. Bud, 'Strategy in American Cancer Research After World War II', pp. 432–3.

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90. Kenneth M. Endicott, 'The Chemotherapy Program', *Journal of the National Cancer Institute*, vol. 19 (1957) pp. 275–93; C. Gordon Zubrod, 'Origins and Development of Chemotherapy at the National Cancer Institute', *Cancer Treatment Reports*, vol. 68 (1984) pp. 9–19; C. Gordon Zubrod, Saul A. Schepartz and Stephen C. Carter, 'Historical Background of the National Cancer Institute's Drug Development Trust', *National Cancer Institute Monographs*, vol. 45 (1977) pp. 7–11:

91. Zubrod, 'Origins and Development, op. cit. p. 12; Endicott, 'The Chemotherapy Program', op. cit. p. 292.

92. On unification and standardization of clinical trials of the CCNSC see I. S. Ravdin, 'The Clinical Studies Program', *Cancer Chemotherapy Reports*, vol. 16 (1962) pp. 5–8; Zubrod *et al.*, 'Historical Background', pp. 11–12.

93. In parallel CCNSC elaborated quality tests for tumours which included bacteriological tests, tests for growth characteristics consistant with the prototoype and tests for consistent responses to known chemotherapeutic agents. Reference tumours were maintained by Arthur D. Little, Inc. (Cambridge, Mass.). The National Program of Cancer Chemotherapy Research: Information Statement', *Cancer Chemotherapy Reports* (January 1959) no. 1, pp. 10–13; 43–64.

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95. Zubrod, *et al.*, 'Historical Background', p. 810.

96. The National Program of Cancer Chemotherapy Research: Information statement', pp. 99–104, quote on p. 100.

97. Zubrod *et al.*, 'History of the Cancer Chemotherapy program', pp. 360–1.
98. 'The National Program of Cancer Chemotherapy Reseach: Information Statement', pp. 99–104, quote on p. 100.
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102. Second Conference on Experimental Cancer Chemotherapy, Washington, November 2–3, 1961, *Cancer Chemotherapy Reports* (1962) no. 25 protocol 5: Propagation of Tumour Lines', protocol 6: 'Tumour Transplantation' and Protocol 7, 'Quality Control', 31–42.
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111. L. Gross, 'Susceptibility of Suckling-Infant, and Resistance of Adult Mice of the C3H and the C57 Lines to Inoculation with Ak Leukaemia', *Cancer*, vol. 3 (1950) pp. 1073–87; L. Gross, 'Pathogenic properties and vertical transmission of the Mouse Leukaemia Agent', *Proceedings of the Society for Experimental Biology and Medicine*, vol. 78 (1951) pp. 342–8.
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LES FORMES DE LA MÉDIATION TECHNIQUE

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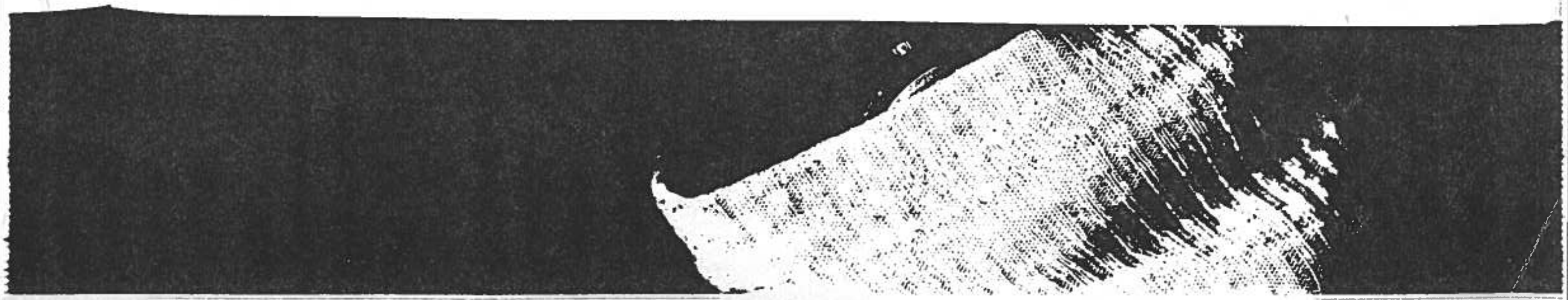


D'un certain point de vue, parler de « médiation technique » relève d'une extrême banalité : à la différence d'autres productions humaines, les dispositifs techniques n'ont de sens que comme mise en relation active entre l'homme et certains éléments de son environnement. C'est peut-être ce qui explique le faible intérêt des analystes pour l'utilisation de ce concept dans la description des liens entre phénomènes sociaux et phénomènes techniques.

La médiation technique, une banalité ?

Deux auteurs français, Ellul et Simondon, en ont cependant fait un usage appuyé qui va nous aider à préciser sous quelles conditions il devient intéressant, parce que non trivial, de recourir à cette notion.

Ellul (1) est l'un des principaux contestataires de la technologie, au nom d'une forme d'humanisme qui voit dans une croissance technique incontrôlée la fin des valeurs morales et culturelles fondamentales. Pour Ellul, les relations de l'homme au monde ne sont jamais immédiates mais, à l'inverse, toujours médiatisées par quelque chose, que ce soit la poésie, l'activité symbolique, la religion ou la technique. Le développement des sociétés modernes s'accompagne d'un déclin de ces différentes formes de médiation au profit d'une seule d'entre elles, la médiation technique. La technique médiatise les relations de l'homme au milieu naturel, les relations des hommes entre eux – l'essor des technologies de communication en est une illustration frappante ; enfin, elle médiatise ses propres relations avec les individus et la société : parce qu'elle est devenue l'unique médiatrice, il n'y a pas d'au-delà de la technique possible, il n'y a plus de système de valeurs. Elle constitue le milieu humain en milieu technicien, organise le monde et oriente les perceptions et les jugements. Elle devient système dont on ne peut sortir. Par quels mécanismes cette colonisation se produit-elle ? La forme de rationalité qui est inhérente au développement technique porte en elle-même l'exigence d'une performance, d'un progrès, d'une cohérence qui ne peuvent se réaliser que par annexion progressive de tous les domaines d'activité humaine. Chez Ellul, la technique est donc tout sauf purement instrumentale ; le terme de médiation renvoit à cette épaisseur, cette profondeur de la technique : elle ne peut être simplement rabattue sur une dimension sociale ou politique. De ce point de vue, la pensée d'Ellul contraste avec celle d'autres auteurs importants qui, sans s'être principalement attachés à l'analyse des techniques, en ont fait une pièce majeure de leur construction théorique. Qu'il s'agisse de Marx (2), de Leroi-Gourhan (3) ou de Mumford (4), la technique vient toujours en continuité par rapport à autre chose : ainsi, chez Marx, elle matérialise et inscrit dans la durée les formes d'organisation socio-économique instaurées par l'atelier puis la manufacture ; chez Mumford, elle objective

(1) ELLUL, 1977.
(2) MARX, 1977.
(3) LEROI-GOURHAN, 1964.
(4) MUMFORD, 1973.

l'organisation politique née dans l'Egypte pharaonique, alors que chez Leroi-Gourhan, elle permet que se continue l'évolution « naturelle », sous une forme excorporée. Tous ces modèles reposent de fait sur un postulat d'homogénéité entre technique et société. Lorsque Mumford appelle Méga-machine la société des Pharaons, il réalise d'emblée une opération de mise en équivalence de la technique avec le social, de même lorsque Marx décrit les machines comme des assemblages de travailleurs de fer ou lorsque Leroi-Gourhan assimile le corps humain à un ensemble de moyens techniques et le cerveau à des moyens organisationnels. Envisager la technique comme une forme de médiation spécifique comme le fait Ellul, c'est s'opposer à ce réductionnisme qui ne donne de sens à la technique qu'en en abolissant tout caractère propre. Ceci étant, Ellul ne donne pas non plus les clés qui permettraient de comprendre comment fonctionne son système technicien ; d'un certain point de vue, le concept de « médiation », qui lui tient lieu d'explication, se fait à rebours du sens ordinaire : l'emphase est placée sur le médiateur qui devient la cause d'un monde dont l'existence propre se dissout. Autrement dit, alors que l'idée même de médiation suppose la mise en relation entre différentes entités, mise en relation qui transforme ces entités, elle devient chez Ellul à la fois la relation et les termes de cette relation, puisque l'imposition de la logique technique conduit à l'indifférenciation des ordres, politique, social, économique, moral, etc., jusque, là maintenus séparés.

A l'inverse d'Ellul qui n'entre pour ainsi dire jamais dans les contenus techniques, Simondon déploie une théorie de l'évolution des objets techniques appuyée sur une analyse très fine des relations qu'entretiennent les éléments techniques entre eux et avec leur environnement. Pour Simondon, l'objet technique se conçoit comme l'assemblage de dispositifs élémentaires plurifonctionnels. Parmi l'ensemble des fonctions assurées par un élément particulier, certaines ne jouent aucun rôle positif dans la réalisation du programme d'action de l'objet technique et s'opposent même à sa bonne marche : ainsi, par exemple, tout moteur à explosion dégage de la chaleur qui doit être évacuée. L'évolution technique est pensée comme une transformation progressive des fonctions et de leur répartition entre les différents éléments techniques, transformation qui débouche sur l'intégration, au sens positif, de toutes ces fonctions dans l'objet technique. Ce processus, que Simondon désigne par le terme de concrétisation, se construit par une différenciation et une spécification des éléments techniques qui permettent de décupler la synergie de l'ensemble formé par les éléments, en supprimant les antagonismes qui résultaient antérieurement d'une plurifonctionnalité non maîtrisée. Certaines de ces transformations peuvent être décrites comme une adaptation aux conditions matérielles et humaines de production, d'autres relèvent d'une adaptation fine de l'objet technique à la tâche qui lui est dévolue; ces deux formes d'adaptation conduisent souvent à l'hypertélie, c'est-à-dire à une spécialisation exagérée de l'objet technique qui le rend vulnérable vis-à-vis des moindres variations de son environnement. Une troisième forme constitue l'objet technique en véritable médiateur de l'homme à son environnement et donne à l'invention toute sa grandeur : dans ce cas, l'objet lui-même, par sa concrétisation, crée son milieu associé.

« L'invention concrétisante réalise un milieu techno-géographique, qui est une condition de possibilité du fonctionnement de l'objet technique. L'objet technique est donc la condition de lui-même comme condition d'existence de ce milieu mixte, technique et géographique à la fois. » (5)

C'est parce que la concrétisation aboutie suppose cette transformation-création conjointe de l'objet technique et de son environnement que les objets techniques peuvent être considérés comme des médiateurs entre l'humain et le naturel, cette

(5) SIMONDON, 1958, pp. 55-56.

fonction de médiation étant ici entendue comme la possibilité d'une conversion de l'humain en naturel et du naturel en humain :

« *L'objet technique, pensé et construit par l'homme, ne se borne pas seulement à créer une médiation entre homme et nature ; il est un mixte stable d'humain et de naturel, il contient de l'humain et du naturel ; il donne à son contenu humain une structure semblable à celle des objets naturels, et permet l'insertion dans le monde des causes et des effets naturels de cette réalité humaine.* » (6)

Autrement dit, pour Simondon, la nature, l'environnement n'est pas ce qui permet d'expliquer la forme prise par les objets techniques – il s'oppose fortement à la position qui verrait dans les contraintes de la matière elle-même le principal déterminant de la technique – mais, au contraire, un des résultats de cette activité technique.

Pas plus que nous n'avons admis le déterminisme technique absolu d'Ellul, nous ne pouvons suivre Simondon dans sa genèse des objets techniques, genèse qui suppose là encore l'existence d'un moteur d'évolution intrinsèque à la technique; mais l'un comme l'autre, par leur utilisation du terme de médiation, mettent en relief certains traits spécifiques de l'analyse des techniques que nous voudrions développer dans la suite. Parler de médiation en sociologie des techniques n'a de sens que si l'on analyse à la fois le médiateur et les opérations de médiation, sans se laisser absorber par les médiateurs, ce qui reviendrait à ôter tout sens spécifique au mot même de médiateur, ni par les termes mis en relation par les opérations de médiation, ce qui rendrait incompréhensibles les mécanismes par lesquels s'établissent ces relations. Il faut redonner aux dispositifs techniques leur épaisseur, ce qui en fait des médiateurs et non de simples instruments ou encore, pour reprendre les termes de Simondon, ce qui en eux-mêmes peut être décrit comme un mixte stable d'humain et de naturel, de social et de matériel ; il faut montrer comment se constituent conjointement les techniques et leur environnement social et naturel, ou encore comment, en utilisant à nouveau Simondon, les objets techniques sont à la fois connaissances et sens des valeurs. Pour cela, nous nous appuierons sur la sociologie de la traduction qui s'est attachée à l'analyse des liens entre technique et société.

De la spécification socio-technique à la médiation

Dans un article fondateur pour la sociologie des techniques, M. Callon (7) a montré comment, dans les discussions et controverses qui accompagnent le processus d'innovation, se trouvent à chaque fois associés contenus techniques et contenus sociaux, de sorte que, lorsque deux projets s'affrontent, ce sont deux conceptions élaborées du monde et de la société, ou tout du moins de fragments de ceux-ci, qui se trouvent mis en balance. Autrement dit, rendre compte des décisions dites techniques, c'est restituer dans leur complexité les représentations que se font les acteurs de l'univers dans lequel ils se trouvent, des alliances qu'il leur faut contracter, des oppositions qu'ils doivent balayer pour faire avancer leur projet, et de l'univers dans lequel leur innovation est appelée à s'insérer. En recentrant ce point autour des dispositifs techniques eux-mêmes, ceci signifie que l'élaboration des techniques peut être décrite comme l'élaboration d'un scénario constitué d'un programme d'action, de la répartition de ce programme d'action à des entités diverses (dispositifs techniques qui font l'objet de l'innovation, mais aussi autres dispositifs auxquels l'innovation va être associée, utilisateurs bien sûr, mais encore techniques, installateurs, distributeurs, etc.) et enfin d'une représentation de l'environnement dans lequel le programme d'action peut ou doit se réaliser. Dans cette perspective, le travail du sociologue consiste à décrire les opéra-

(6) SIMONDON, 1958, p. 245.
(7) CALLON, 1981.

tions par lesquelles le scénario de départ, qui se présente essentiellement sous une forme discursive, va progressivement, par une série d'opérations de traduction qui le transforment lui-même, être approprié, porté par un nombre toujours croissant d'entités, acteurs humains et dispositifs techniques. Chaque décision technique engage une certaine distribution des compétences entre ces diverses entités ou, autrement dit, peut être lue comme l'inscription dans le dispositif technique d'une certaine forme d'environnement. Les épreuves diverses auxquelles les concepteurs se soumettent et soumettent leur innovation – tests techniques, association avec d'autres acteurs qu'ils soient techniques, financiers, ou commerciaux, expérimentation auprès d'usagers supposés – s'interprètent alors comme une confrontation entre l'environnement inscrit dans le dispositif et l'environnement décrit par son déplacement. Le mot « décrit » doit ici être entendu dans un sens fort, dans un sens actif : il ne s'agit en aucun cas de comparer un monde imaginaire, celui des concepteurs, à un monde réel qui serait là, donné par avance ; il s'agit plutôt de faire se spécifier conjointement et de manière indissociable le dispositif technique et son environnement ; c'est en ce sens que l'on peut, à notre avis, parler de médiation technique. Autrement dit, les acteurs comme les éléments naturels ou les dispositifs techniques sortent transformés de ces différentes épreuves : l'innovation en tant que processus produit à la fois des savoirs, des dispositifs techniques et des formes d'organisation. A partir d'un certain nombre de cas d'innovation que nous avons pu suivre, nous allons donner quelques exemples de ces opérations de spécification.

Des éléments naturels

Comme nous l'avons souligné plus haut, il est banal de considérer que la technique opère la médiation entre la nature et l'homme. Par cela, on entend généralement que la technique apporte un certain nombre de moyens qui permettent de transformer des ressources naturelles, données d'emblée, en éléments utiles à l'homme. Nous voudrions ici donner au mot de médiation un sens plus fort en montrant que, en particulier dans les processus d'élaboration technique, se spécifient de façon conjointe les dispositifs et les éléments naturels ; loin de pouvoir être décrit par des propriétés intrinsèques des éléments naturels, le caractère de ressource doit être appréhendé comme le rapport construit par la médiation technique entre éléments naturels et formes d'organisation socio-économiques.

Prenons par exemple le cas du programme énergies renouvelables développé par le CEA (Commissariat à l'énergie atomique) en Polynésie dans les années 80. Le CEA, désireux d'utiliser son potentiel à « autre chose que des opérations barbares », *i.e.* les fameux essais nucléaires du Pacifique, reçoit le soutien du ministre de l'Industrie de l'époque, soucieux de plaire aux écologistes, pour promouvoir les énergies nouvelles et renouvelables. Dans un premier temps, aucune exclusive ne vient limiter la définition technique du programme d'investigation : toutes les possibilités sont envisagées, depuis le nucléaire jusqu'au photovoltaïque en passant par l'hydroélectricité ou l'énergie éolienne.

L'éclectisme ou l'œcuménisme du programme initial de recherche et développement manifeste l'impossibilité qu'il y a, à ce moment-là, à déterminer qui, du soleil ou du vent, par exemple, aura le dernier mot. Seule la mise en œuvre de la recherche permet de capter ces deux éléments par l'intermédiaire de dispositifs spécifiques qui les rendent en quelque sorte homogènes en traduisant leur force respective en kilowatts-heure. Les dispositifs eux-mêmes peuvent être traduits en unités monétaires au terme d'une longue série d'opérations-expérimentations qui mettent en jeu la configuration spatiale, sociale, technique et politique de la Polynésie. Ce n'est qu'en bout de course que la comparaison des rapports entre kilowatts-heure et unités monétaires fournit une détermination possible du rapport de « force » entre le soleil et le vent ou entre un générateur photovoltaïque et une éolienne.

Dans le cas des éoliennes, deux problèmes principaux se posent. Tout d'abord, chaque implantation demande une longue étude préalable du site ; en l'absence de méthode systématique permettant de prévoir à coup sûr le bon emplacement pour un aérogénérateur, il faut disposer d'une année de mesures anémométriques. Ces délais importants de mise en place peuvent être rédhibitoires face à d'autres technologies. Le photovoltaïque se présente très différemment : la délocalisation et l'extension des mesures est immédiate dans le cas du solaire (une carte d'ensoleillement construite à partir de données récoltées en quelques points est suffisante pour dimensionner une installation), alors qu'elle est inefficace dans le cas des éoliennes. De manière plus spécifique, la configuration de l'aérogénérateur est mal adaptée à la Polynésie ; une série d'expériences montrent que la vitesse moyenne des vents est modérée (5 à 6 m/s), et qu'ils ne soufflent que 10 % du temps au-delà de 7 m/s, qui est une limite importante : c'est à cette vitesse que les aérogénérateurs atteignent leur puissance nominale. Insistons sur le fait que ces « données » sont le résultat de la recherche entreprise par le groupe énergies renouvelables (GER) : des tests de laboratoire (en l'occurence, le laboratoire ressemble plutôt à un hangar...) et diverses expérimentations in situ ont permis d'établir une première cartographie. La Polynésie de l'éolienne est irrégulière, capricieuse et imprévisible ; la petite brise légère, les arbustes et les cocotiers ainsi que les vallons s'y liguent pour mieux embrouiller la situation. Bien entendu, tout changement de l'instrument de mesure (des éoliennes dont la vitesse nominale se situerait autour de 4 m/s par exemple) modifierait la distribution des qualités respectives du vent et du soleil. Mais cela suppose des modifications considérables dans les standards de production et donc une réorganisation sur plusieurs niveaux, de l'amont (les composants de l'éolienne) à l'aval (les équipements alimentés par l'éolienne).

On pourrait donner d'autres exemples de ces processus de spécification. Ainsi dans le cas des coquilles Saint-Jacques étudié par M. Callon (8), un des éléments fondamentaux du projet concerne la capacité des larves à se fixer, capacité que les chercheurs éprouvent au moyen de filières larguées dans la baie de St-Brieuc : un fait « naturel », les compétences de certaines espèces animales, ne s'appréhende que par la médiation d'un dispositif technique, ici les filières. De même, dans le cas, sur lequel nous reviendrons, de la fabrication de briquettes à partir des tiges de cotonnier, un insecte parasite des bambous, Amphiserus Cornutu, s'avère un prédateur redoutable des tiges de coton ramassées et stockées dans un hangar. Il dévore l'intérieur des tiges ; celles-ci paraissent intactes, mais se réduisent en poussière au moindre contact. Aucune description précédente d'Amphiserus Cornutu ne mentionne ces compétences et ce comportement, car il faut la médiation de l'homme qui coupe et stocke les tiges de coton pour qu'Amphiserus puisse déployer ses talents.

Nous nous sommes surtout placés ici dans des cas de technologies « modernes », où le rapport construit avec les éléments naturels passe par la médiation d'instruments calibrés, standardisés, de sorte que les savoirs constitués lors de l'expérimentation sont en bout de course détachables du contexte particulier dans lequel ils ont été produits ; mais l'on pourrait sans difficulté étendre ce point à des techniques plus artisanales, dans lequel le triangle – homme, dispositifs techniques, éléments « naturels » – est plus difficile à défaire, parce qu'il y a incorporation dans l'homme, souvent sous forme de sensations perceptives, de rapports avec ces éléments naturels, rapports établis par la médiation des dispositifs techniques.

Des acteurs

De façon analogue, les acteurs eux-mêmes se trouvent spécifiés dans les processus techniques. Reprenons l'exemple

(8) CALLON, 1986.

que nous venons d'évoquer et qui concerne la conception d'un dispositif de récolte des tiges de coton. Au départ de cette histoire, se trouve un projet de transfert d'une machine suédoise de compactage des résidus forestiers que les Nicaraguéeens veulent utiliser pour fabriquer, à partir des tiges des arbustes de coton, des briquettes destinées à remplacer le bois de feu. Mais, pour compacter les tiges de coton, encore faut-il les récolter et, pour cela, s'assurer la coopération des haciendas cotonnières. A priori, elles ne trouvent que des avantages au projet, puisqu'il permet de tranformer une opération technique obligatoire en une activité économique: la destruction des tiges après la récolte est rendue nécessaire par l'existence de parasites qui pourraient infester la récolte suivante. Cela étant, lorsque les promoteurs du projet expérimentent la récolte manuelle des tiges, les haciendas affolées par le personnel que cette opération exige mettent leur veto à une telle forme d'organisation. Qu'à cela ne tienne, les promoteurs du projet font venir une arracheuse soudanaise qui doit être couplée à un tracteur. Malgré des résultats satisfaisants, les haciendas sont réticentes car elles n'ont que peu de tracteurs et se voient mal en affecter à une nouvelle opération, d'où finalement un effort des promoteurs qui se mettent à concevoir une nouvelle machine intégrant les contraintes des haciendas. Ainsi, toute cette phase d'expérimentation peut être décrite schématiquement sous la forme d'une série d'énoncés de plus en plus élaborés qui décrivent de plus en plus finement ce que les haciendas découvrent être leurs besoins et ce que doit être la machine :

0) les haciendas se débarrassent des tiges de coton par le brûlage

1) les haciendas sont d'accord pour qu'on les débarrasse par la récolte des tiges de coton

a) expérience : on coupe les tiges manuellement

2) les haciendas sont d'accord pour qu'on les débarrasse par la récolte des tiges de coton à condition que cela ne demande pas de main-d'œuvre.

L'épreuve a) permet de spécifier à la fois ce que veulent les haciendas et ce que doit faire le dispositif technico-organisationnel mis en place, car 2) implique que la faux doit se transformer en outil mécanisé

b) épreuve avec l'arracheuse soudanaise dans laquelle est « inscrite » la spécification de l'environnement réalisée par l'épreuve a)

3) les haciendas sont d'accord pour qu'on les débarrasse par la récolte des tiges de coton à condition que cela ne demande pas de main-d'œuvre ni de matériel agricole

c) épreuve par la construction de la machine à arracher : inscription dans cette machine des contraintes issues de b) ainsi que d'autres contraintes comme l'irrégularité du terrain, etc.

On assiste là à la spécification conjointe des éléments « sociaux » et des éléments « techniques », des haciendas et du dispositif d'arrachage ; ce n'est qu'à l'issue de ces épreuves que les haciendas savent ce qu'elles peuvent et ce qu'elles veulent – l'acteur « haciendas » s'est modifié entre le moment où le projet est un brouillon sur un papier et le moment où il est une machine sur un champ de coton – et que les promoteurs du projet de briquettes savent de quelles compétences ils doivent doter la machine d'arrachage. Ici, le passage par les dispositifs techniques, autrement dit la médiation technique, permet de transformer progressivement les connaissances et les aspirations d'un acteur et, ce faisant, de permettre son intéressement par un autre acteur, le groupe des promoteurs du projet de briquettes.

En bout de course, la stabilisation des dispositifs techniques et des formes d'organisation qui leur sont associées conduisent à une certaine naturalisation des propriétés qui ont émergé du processus d'innovation et qui sont maintenant attachées intrinsèquement aux entités mobilisées par le projet : Amphiserus a tel ou tel comportement, les haciendas veulent telle ou telle chose, la machine a telle ou telle fonction. La qualification des événements, le partage entre les causes et les effets se trouvent préformés par cette stabilisation des entités et des propriétés qui leur sont associées. En face d'un dysfonctionnement, par exemple, les acteurs vont proposer une interprétation, c'est-à-dire attribuer à telle ou telle entité la responsabilité de ce dysfonctionnement, sans finalement avoir à revenir sur l'ensemble du montage qui rend possible cette attribution. Ce n'est que dans le cas de forte controverse que la répartition des compétences entre les différentes entités pourra être remise en cause.

Des relations entre les acteurs

Nous avons vu comment la spécification technique est inséparable de la spécification des acteurs et des éléments naturels, de sorte que l'innovation peut être décrite comme un travail visant à stabiliser une répartition des compétences entre les différentes entités mobilisées. La technique se donne à voir en tant que médiation au moment de ces partages ; mais si ces partages réussissent et sont naturalisés, les dispositifs techniques peuvent n'apparaître que comme de plats intermédiaires entre des acteurs et des entités diverses. Nous allons voir maintenant, en analysant les relations entre les acteurs nouées au travers d'un dispositif technique, comment la médiation technique perdure au-delà de l'innovation. Nous prendrons ici comme point d'appui la comparaison entre deux technologies de fourniture d'électricité, le groupe électrogène et le le générateur photovoltaïque, utilisées en milieu rural dans les pays en développement : alors qu'elles sont souvent considérées comme substituables l'une à l'autre, sous réserve de quelques conditions climatiques, l'analyse des usages de ces technologies montre que les formes d'organisation qui se créent au travers des dispositifs techniques peuvent être fortement liées à certains paramètres techniques spécifiques qui rendent de fait impossible ou difficile toute substitution.

Une des utilisations les plus répandues, en milieu rural, au Sénégal, des groupes électrogènes semble être ce que nous avons appelé le « groupe festif » : une administration achète des petits groupes qu'elle distribue aux associations des jeunes des villages, les groupes pouvant être accompagnés de matériel divers comme des lampes, un électrophone, un porte-voix. L'association de jeunes s'en sert pour ses activités, théâtre, fêtes, le prête à ses membres pour leurs propres réjouissances, ceux-ci payant le carburant et l'huile nécessaire, le loue aux villageois non membres qui doivent eux aussi assurer par leurs propres moyens l'approvisionnement en carburant. L'argent de la location est séparé en deux parts, l'une qui revient au porteur et l'autre à l'association. Se greffent ainsi sur le groupe électrogène une petite foule d'acteurs qui peuvent être considérés comme autant d'appendices sur des éléments repérables du groupe.

Le châssis métallique qui supporte le groupe et permet son déplacement joue un rôle de premier ordre : c'est dans la circulation du groupe que se définissent le champ des utilisations possibles et les relations entre les divers acteurs.

Le réservoir à essence lui dispute la vedette : il opère une distinction fondamentale entre ce qu'il est convenu d'appeler coûts d'investissement et coûts de fonctionnement. Ce partage est inscrit dès le départ dans le montage social qui fait aboutir le groupe dans le village : d'un côté, l'administration qui assure l'investissement et, de l'autre, l'association qui gère le fonctionnement. Les négociations entre les deux parties se réduisent au minimum grâce au dispositif technique qui propose d'emblée un accord tout négocié ; la situation serait fort différente si nous nous trouvions, par exemple, devant un dispositif dont les coûts sont concentrés sur l'investissement comme c'est le cas pour le photovoltaïque : quel mode de relation prévoir entre l'acheteur et l'utilisateur ? Cette question se pose très pratiquement aux promoteurs du développement du photovoltaïque en Polynésie française ; quelques années après l'implantation de systèmes photovoltaïques dans le cadre de l'électrification rurale, ils n'avaient, semble-t-il, pas trouvé le moyen d'introduire un partage des coûts alors que le dispositif n'en opère aucun et, qui plus est, ne fournit aucune mesure susceptible d'être retraduite en termes socio-économiques : quelle qu'en soit l'utilisation, un panneau photovoltaïque fournit du courant, dans une quantité qui est déterminée par le climat et la position par rapport à l'Equateur ; la relation « habituelle » entre production et consommation (qui manifeste la dépendance réciproque entre deux groupes d'acteurs) se trouve coupée et remplacée par une soumission individuelle, directe et de ce fait arbitraire aux forces de la nature.

Situation là encore bien différente de celle que crée le groupe électrogène : le réservoir mesure la proportionnalité entre l'utilisation du groupe et la dépense occasionnée par cette utilisation, proportionnalité que réalise le moteur dans son ensemble. L'établissement d'un lien social particulier, celui de la location, est conditionné par l'existence de cette proportionnalité qui permet la délocalisation de la jouissance du groupe électrogène. Les groupes d'acteurs suscités par le groupe électrogène sont donc fort nombreux puisqu'il nous faut distinguer acheteurs-investisseurs, propriétaires-utilisateurs, utilisateurs-associés, utilisateurs-locataires et enfin porteurs. Ces derniers rendent encore plus « pur » le contenu de la propriété puisqu'ils la libèrent de toute servitude ; leur rétribution marque la limite de la solidarité associative : le travail d'un seul ne peut contribuer à enrichir la collectivité. Dans le même processus, le groupe électrogène construit son espace dont la géographie est sociale ; il était par exemple impensable pour les instituteurs d'un de ces villages de demander le prêt du groupe électrogène de l'association, alors qu'ils étaient à la recherche de moyens d'éclairage pour assurer des cours du soir.

Nous avons affaire à la création ou à l'extension de réseaux sociotechniques, qui s'effectue par spécification conjointe du « social » et du « technique » : l'étendue des compétences de l'association de jeunes, la forme des relations qu'elle entretient avec les autres composantes du village, la définition même de ces composantes sont précisées conjointement à la liste des éléments qui constituent le groupe électrogène. Si nous nous intéressons uniquement à la « fonction » assurée par ce dispositif à l'intérieur de l'association, nous pouvons imaginer qu'un autre système technique (photovoltaïque, raccordement au réseau...) assure le même « service » d'éclairage et de sonorisation : cela étant, les relations de l'association avec le reste du village seraient en partie différentes ou auraient atteint un degré moindre de spécification. C'est en ce sens que nous pouvons dire que les rapports des hommes avec le réel sont médiatisés par les objets techniques.

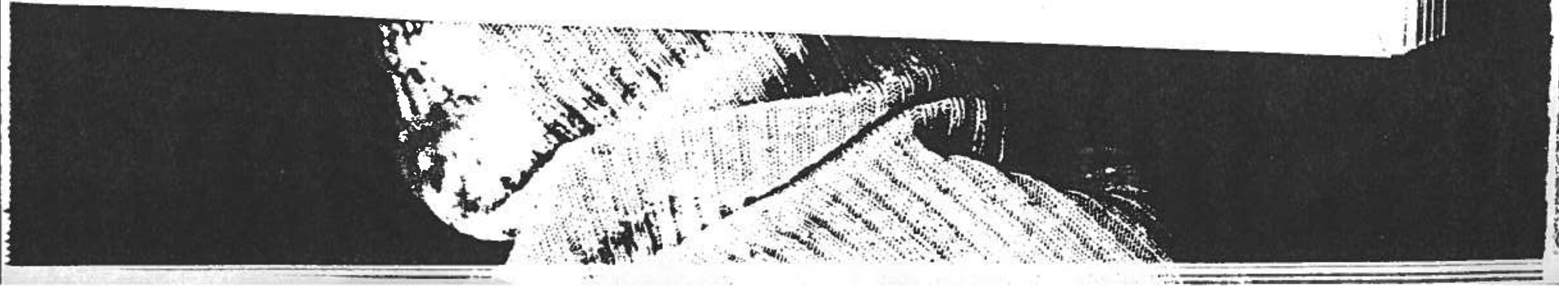
Conclusion

Dans sa tentative de concilier une théorie qui se propose de montrer comment des entités inanimées comme les objets techniques induisent des changements sociaux avec une philosophie qui dote l'individu d'une liberté et d'une capacité de choix, Ogburn (9), l'un des pionniers de la sociologie des techniques, a développé une analyse de la cause qui va nous aider à préciser ce en quoi l'introduction du concept de médiation transforme l'analyse des relations entre techniques et société. Pour que, pour Ogburn, l'on puisse parler de deux phénomènes en termes de cause et d'effet, il faut qu'ils varient de façon concomitante et que l'on puisse les relier par une chaine de médiateurs. Or, à moins de se situer sur des échelles de temps considérables, la psychologie, l'intelligence, les aptitudes individuelles sont de son point de vue des variables statistiques mais non historiques. Ce qui l'amène à considérer qu'une phrase comme « les automobiles ont causé le développement des motels » est pleinement dotée de sens, alors qu'il dénie toute vertu explicative à un énoncé du type « Denis Papin a inventé la machine à vapeur ».

Soutenir qu'il existe des formes de médiation technique, qu'en particulier l'innovation est un processus de spécification qui s'étend des dispositifs techniques aux éléments naturels et aux acteurs humains, c'est s'interdire le type de dichotomie entre, d'un côté, les individus, et, de

(9) OGBURN, 1957.

l'autre, les objets techniques, sur laquelle repose le raisonnement d'Ogburn. C'est supposer qu'il existe des formes hybrides, comme celles que nous avons évoquées sur le cas du groupe électrogène, qui repose sur un agencement inextricable entre certains éléments techniques et certaines formes d'organisation sociale. Ce point peut être étendu à des situations plus micro-sociologiques : aujourd'hui, l'évolution d'un certain nombre de technologies déplace sans arrêt les compétences et oblige à une reconfiguration partielle des individus eux-mêmes. Reconfiguration motrice : quand je passe de mon Powerbook , sur lequel la disposition du clavier me permet de poser la base des mains, à un ordinateur classique sur lequel je ne peux les poser, le taux de fautes de frappe augmente de façon notable ; en m'autorisant à poser les mains, le Powerbook prend en charge une contrainte de stabilité spatiale ; de même, le passage d'une télécommande orientée transversalement au lieu de l'être verticalement révèle, par les difficultés qu'elle occasionne chez les utilisateurs, la part prise par la géométrie de la télécommande dans la précision de la visée. Reconfiguration cognitivo-sociale, car, dans un certain nombre de cas, le dépouillement de l'action rabattue sur sa dimension technique exige de plus en plus d'intelligence de la situation de la part de l'utilisateur : que l'on songe par exemple à toutes les formes d'action par système informatique interposé, retrait d'argent, commande de billets, prélèvement d'informations, etc. Ces outils informatisés impliquent chez l'usager l'incorporation de compétences proprement sociales, définies comme la capacité à qualifier les situations et à ajuster son comportement en conséquence – mobiliser des ressources pertinentes, utiliser le vocabulaire adéquat, se conformer aux règles en vigueur, etc. Dans les dispositifs qui, en particulier, substituent au face-à-face de deux acteurs un contact médiatisé par le dispositif, un certain nombre d'éléments peuvent n'avoir d'autre fonction que de signaler à l'utilisateur son niveau d'engagement ou de le contraindre à signifier explicitement son acceptation des conséquences de l'action. Ainsi, le code secret tapé pour obtenir des billets de banque par le moyen d'une carte bancaire sert autant à protéger le possesseur de la carte contre des utilisations indues qu'à garantir vis-à-vis du créancier l'identité du débiteur : la frappe du code est alors l'équivalent de la signature qu'elle remplace d'ailleurs dans le cas des paiements effectués chez les commerçants munis d'un terminal de lecture et de contrôle des cartes. Cela implique que l'action avec des dispositifs techniques engage toujours la mise en œuvre de compétences et de savoirs proprement sociaux chez les acteurs – ce qu'est un contrat, les sanctions qu'ils encourent s'ils enfreignent certaines règles, les arguments recevables en cas de contestation, etc. Savoirs et compétences dont on peut supposer qu'ils sont à la fois incorporés sous la forme de schèmes de raisonnement intellectuel, mais aussi sous la forme de schèmes de repérage des situations qui sont déjà inscrits dans des dispositifs. Quand nous voyons une touche qui porte une inscription « validation » « VALID » ou même « VAL », nous savons de façon instantanée à la fois l'action qui est attendue de nous et les formes d'engagement que peut supposer cette action. A l'inverse, on pourrait trouver des exemples dans lesquels l'intelligence « sociale » de la situation, c'est-à-dire la connaissance de ce qu'implique une certaine action, permet de retrouver le programme d'action alors que le dispositif technique est peu explicite. Autrement dit, pas plus que nous ne pouvions abstraire le groupe électrogène des formes d'organisation mises en place au Sénégal par les associations de jeunes, nous ne pouvons décrire un grand nombre d'actions sans en passer par les dispositifs techniques qui les rendent possibles : c'est aussi ce qui est susceptible de donner son plein sens au concept de médiation technique.



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