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Seeing with Sound: A Study of the Development of Medical Images

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This chapter is a contribution to a growing debate on the sociology of technological innovation. It is organized around a case study that describes some of the stages in the origination of a medical technology, the ultrasound scanner. In particular, I discuss differing ways of trying to generate two-dimensional images using high-frequency sound waves in the 1940s and 1950s. Some of these attempts were productive, and some were unsuccessful, in the sense that no fully functional equipment that could perform to the standards of diagnostic accuracy required was produced by the group concerned. However, to write of success and failure in this way only poses the problem to be explained. That is to say, the variation in design objectives, development strategy, institutional and professional background, and tenacity in pursuing the research objectives forces us, first, to ask what it means when one says that a particular technological artifact "works" and, second, to try to explain both success and failure without taking the definition of either as self-evident. As Pinch and Bijker (1984) have argued, one of the lessons of the sociology of scientific knowledge for the sociology of technology is that the selection of technological forms early in the innovation cycle and the "stabilization" of such forms are social phenomena requiring explanation, analogous to the formation of a consensus in science. Questions of inventive success and failure can be made sense of only by reference to the purposes of the people concerned.

In the case of clinical ultrasound we may be tempted to think of doctors and engineers as being the only people whose perceptions of innovative success would be important. If a piece of equipment generates information of demonstrable diagnostic value, then we could say that it "works." However, those asked by doctors to operate the equipment may find it burdensome or demeaning to do so, and their enthusiasm for and evaluation of the technology will differ. Thus in some UK hospitals midwives resist the idea that they should perform ultrasound scans because they consider that it devalues their own clinical skills. Radiographers, on the other hand, tend to be much more enthusiastic about the technology because their status derives from their competence in creating images, and ultrasound adds to the variety of tools available to them. But, although radiographers' acceptance played a part in the diffusion of this technology, there is little evidence of its playing any role in the invention of ultrasound.

Furthermore, the whole question of what the use of ultrasound in obstetrics achieves is now being questioned by some of those on whom it is used, namely, women who have been through programs of prenatal care in the hospital or in other clinical centers. Ultrasound images have considerable psychological power. Combined with the experience of being scanned, they can significantly affect feelings about a pregnancy, usually positively, although sometimes not (Hyde 1986). Doctors rely increasingly on scans during pregnancy, and the questions of what this reliance actually achieves and what comes with it are now being asked with increasing confidence by women's organizations (Association for Improvements in the Maternity Services, 1985).

A major theme in this paper concerns image generation as a process. The role of visual images in science has recently begun to receive more attention as part of a more general concern with conventions of representation and procedures and rhetorical devices for building a consensus (Rudwick 1976; Shapin 1979, 1984; Latour 1983, 1986; Lynch 1985a, b; Yoxen 1986) In this paper I emphasize the search for conventions of representation by means of which three-dimensional forms can be rendered as two-dimensional images. Lynch (1985b) has argued that scientific images are a special kind of construction, arrived at by schematizing, geometrizing, and highlighting salient features from a mass of detail. Nature is *rendered* in ways that accentuate certain features of interest. It is imaged, in a transitive sense, by operations on it. For such images to be relied on as evidence, there must be general agreement as to their value and reliability, and there must exist a set of procedures for generating them. In this case I consider the formation of a medical consensus that ultrasound images have value. Interestingly, Lynch suggests that images tend to become more schematic and more abstractly geometric as representational technique evolves. With ultrasound this is not apparent; rather the emphasis seems to have been on increasing clarity and on improving the depiction of form. Even after fifteen years of development, when the scanning machines could be said to work, there remained considerable skepticism as to the comparative value of these images. What then began was a process of enrollment of doctors, in Callon's sense, developing the view that their clinical judgments could be strengthened by a reliance on images generated through the work of others (Callon, this volume). I do not consider the mechanics of enrollment here, although it is a question fundamental to the evolution of many medical technologies.

Ultrasound in Medical Diagnosis

The physics of ultra-high-frequency sound was discussed by Lord Rayleigh in his textbook on sound (Rayleigh 1877–1878). In the aftermath of the Titanic disaster, a British patent had been taken out by E. G. Richardson on the use of ultrasound to detect icebergs at sea. With the appearance of submarines in naval warfare, attempts were made, particularly by Langevin in France, to use ultrasound to detect them. The problem was to generate sufficient power. By 1917 Langevin had succeeded, establishing the foundations of *so*und *na*vigation and *r*anging (sonar; Hackmann 1985).

In 1928 the Soviet physicist S. Y. Sokolov suggested that ultrasound could be used to reveal discontinuities in metals, for example, a flaw in a welded seam. It was 1940 before the physicist Floyd A. Firestone, at the University of Michigan, developed and patented what he called a supersonic reflectoscope. This work was done with funds from General Motors and Sperry Rand. The device was actually produced in 1940 and patented in 1942, but it was not publicized until 1944; papers describing it appeared after the war, when it became apparent that Desch and his colleagues in Great Britain had independently developed similar equipment (Firestone 1945).

In the 1920s and 1930s the predominant medical interest in ultrasound was in its possible therapeutic and destructive effects; in other words, the energy rather than the information in the sound wave was used. Two German scientists, Gohr and Wedekind, published a review in the *Klinische Wochenschrift* in 1940 that sums up the general view. They discuss the use of ultrasound in rehabilitative medicine, the production of novel therapeutic compounds, and the removal of dust from factory air. But they say nothing about imaging, although they do mention that, in studying the effects of ultrasound on tissues, fluids, and animals, they had tried measuring the amount of energy transmitted through an animal (Gohr and Wedekind 1940).

But it was the idea of a *pattern* of differentially attenuated ultrasound that occurred to Karl Theo and Friedrich Dussik, Austrian brothers, one a neurologist and the other a physicist, in the late 1930s. In other words, differences in the amount of energy transmitted through an organ could be used to create a pattern that would represent the form of that organ. This led the Dussiks to make the first claim that ultrasound could be used diagnostically.

Karl Theo Dussik was born in 1908 and trained at the University of Vienna. From 1938 to 1940 he was head of the neurology department at the Allgemeine Polyklinik in Vienna. In 1937 he obtained the first pictures of the patterned attenuation of an ultrasound beam passing through the skull at a series of positions. The intention was to discover abnormalities in the shape of the ventricles in the brain without using x-rays and dyes. The energy of the transmitted beam could be registered on a photographic plate, so that as the transmitter was moved across the skull a pattern of dark and light patches was built up on the plate. Although the process took far longer than the scanning to produce a television picture and although the image was not displayed on a cathode-ray screen, there is some similarity here with the technology of television, on which Friedrich Dussik had worked. Their apparatus was rather rudimentary, but it was based on the idea of scanning an object to build up a representation of it. They worked with great difficulty under wartime conditions and in the turmoil of the early postwar period. Karl Dussik published a paper in 1942 and another in the *Wiener Medizinische Wochenschrift* in 1947 (Dussik et al. 1947). In 1949 he established his own private clinic at Bad Ischl.

In the 1947 paper, Karl Dussik also mentioned the possibility of using ultrasound for surgical purposes, by focusing beams from different directions onto tumors in order to destroy them. This idea was later developed in the United States by William J. Fry. Essentially, though, Dussik seems to have regarded what he called hyperphonography as a research and diagnostic technique in neurology that did not require the introduction of air or of radio-opaque dyes into the brain and that was thus likely to be safer than the existing procedure using x-rays.

Mapping the Ventricular Space

The intention was to create a two-dimensional representation of the shape of the fluid-filled ventricles in the brain, because abnormality of this space would suggest abnormal growth of brain tissue and the presence of a tumor. In effect, it was a way of mapping interior surfaces of the brain. The Dussiks were trying to develop a particular kind of representation. It is important to recognize the principles and graphic conventions involved.

To some extent the representational conventions resembled those of radiography. But there were also differences. Rather than registering the outline of a tumor as a shadow, indicating the presence of opaque tissue, the hyperphonogram purported to reveal distortions of the interior surface of a fluid-filled space. Such structural changes were to be deduced from deviations from normal values of the transverse dimensions of that space. This dimension was measured by registering the smaller absorption of energy when the ultrasound passed through the ventricular fluid rather than simply through brain tissue. The energy absorption was detected by the shading of heat-sensitive paper. The darker the patch, the less the attenuation and thus the bigger the ventricle at that point. Shade stood for transverse cross section, and the shaded patches were added together by the eye to form a shape roughly similar to the longitudinal section of the ventricle. Looking at the hyperphonogram one could "see" the ventricle in three dimensions, even though the dark patches in the image had extension only in two. That at least was the theory. The value of the method rested on the accuracy with which one could delineate the shape of the ventricles and compare them with what was believed to be the norm. An example is shown in figure 1.

Uptake of Dussik's Work

Karl Dussik's work came to the attention of the American physicist, Richard Bolt, who was director of the Acoustics Laboratory at MIT from 1946 to 1957. In those years Bolt built up the laboratory into a major center of research in acoustics, much of it financed by the US Navy. By 1951 there were thirty-one active projects under way. A breakdown of funding from 1945 to 1952 shows that some $1,757,000 had been received over this period, of which about 83 percent had been supplied by the navy, about 8 percent by the air force, and the remaining 9 percent by some nine different sources. The number of faculty members was two or three from 1945 to 1950, expanding to ten in 1951. There were eleven research assistants in 1946, twenty-two in 1951. The Acoustics Laboratory was then a rapidly growing center, with a strong interest in underwater sound and architectural acoustics. The medical acoustics project was just one of a number of projects, although it is the one with which Bolt was involved.

In April 1949 Bolt, his colleague Leo Beranek, and their collaborator at the Massachusetts General Hospital, the brain surgeon

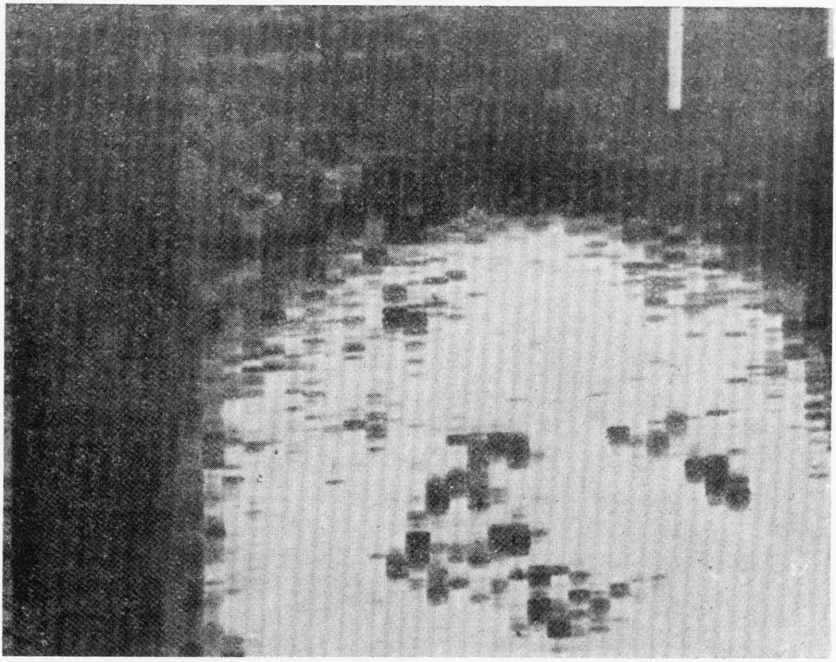

Figure 1
A hyperphonogram of a normal cranium. From Dussik et al. (1947).

H. Thomas Ballantine, wrote to J. R. Killian, the president of MIT, seeking funds for their proposed research.

A group of officers from Headquarters European Command, USA, recently visited Dr. Dussik's hospital in Bad Ischl, Germany. Their report has been received and indicates that Dr. Dussik, a man of forty years of age, was on the staff of the University of Vienna until 1938 when he was displaced by the Nazis. He is a neurologist, at present on the staff of one of the Tyrolean universities and in charge of the Austrian State Neurological Institute. He developed his ultrasonic apparatus with the aid of his brother, who is a physicist. Dr. Dussik told the American medical officers that he fully realised his apparatus was crude, having been made in a local shop under rather adverse circumstances, but maintained that the principle is sound although the machine needed considerable refinement. (Bolt 1949)

They go on:

During the past year, another investigation into the possible use of ultrasound has been underway at the Naval Medical Research Institute, Bethesda, Maryland. This development has been undertaken by Dr. George Ludwig primarily as an aid to surgery of gall bladder disease. Briefly this project was an attempt to produce an instrument which by utilisation of apparatus quite similar to that employed by industry would give evidence as to the presence and location of gall stones. Dr. Ludwig has, with the aid of General Precision Laboratories, Pleasantville, New York, developed such an ultrasonic probe. He has been successful in localizing gall stones in dogs and this localization has been precise in a three-dimensional manner. He has also assembled basic data relating to the behaviour of high-frequency sound waves in human tissue. Dr. Ludwig will be on the staff of the Massachusetts General Hospital, July 1 1949. (Bolt 1949)

In October 1949 Bolt and Ballantine visited Dussik's clinic. Despite some doubts about his research strategy, they endorsed his work but concluded that it was unlikely to be refined as it needed to be, as Dussik himself acknowledged, under the conditions in Austria. Bolt and Ballantine were accompanied on this trip by a young German engineer, Theodore Hueter, whom they recruited to work at MIT.

By the beginning of 1950 the project was underway. One of the first tasks was to investigate the pain threshold and, using a continuous EEG recording, to determine whether ultrasound appeared to affect the functioning of the brain. At the power levels Bolt and Ballantine planned to use, there was no pain and no detectable change in EEG. At higher levels they were able to cause histological damage to the nerves of cats.

Testing the Representational Strategy

The course of the MIT–Massachusetts General work on ultrasound can be followed in some detail from the annual reports of the president of MIT, the quarterly reports of the Acoustic Laboratory, and the various publications that appeared. In the spring of 1950 a scanner was produced that more or less resembled that used by the Dussiks; it was used to examine fixed brain sections and two living subjects, one a member of the research team, the other a neurological patient with a brain tumor, whose heads were surrounded by a water bath. Almost immediately it became apparent that variations in skull thickness would be a problem because the acoustic signal was attenuated in passing through bone.

Nonetheless, papers were given at professional meetings that summer, and a paper appeared in *Science* in November 1950 (Ballantine et al. 1950). Also, Ludwig published an important paper on the velocity of ultrasound in various kinds of tissue and their "acoustic impedance" in the *Journal of the Acoustical Society of America* (Ludwig 1950).

The problem with the variation of bone thickness endured, and various ingenious attempts were made to get around it by varying the gain, comparing signals at different frequencies, and so on. One would imagine that highly trained electronic engineers used to processing noisy signals to extract information from them would not have been too daunted by this. At the beginning of 1951 the Massachusetts collaborators reported work on sound velocity in tumor tissues.

> A value for sound velocity in this type [meningioma] of 1.54×10^5 cm/sec was obtained. This value is practically identical with the sound velocities in normal tissues reported by Ludwig. Since there is little change in the densities of various tissues, no change in sound impedance between normal brain tissue and this type of tumor tissue can be expected and no reflections would occur. (*Quarterly Progress Report of the Acoustic Laboratory*, 1951, p. 24)

This is an interesting passage because it suggests that they were thinking about specular reflections from tissue boundaries as a means of diagnosing tumors. As I describe later, this possibility was being studied by two other groups in the United States at around this time, and one question that suggests itself is why the group at MIT did not switch from transmission studies to reflection work when the former began to prove difficult. After all, radar, sonar, and flaw detection all work using echoes. Perhaps the experts on sound velocity in biological materials (Hueter and Ludwig) "knew" that reflection would not work with brain tumors? Another explanation might be their commitment to working on studies of the head, where to this day it has proved almost impossible to get useful information from the immense complexity of reflected ultrasound signals. Whatever the reason, the fact that they stuck to their original research strategy is something to be explained.

Over the next two years attempts were made to use the scanner in diagnosis at the Massachusetts General Hospital, but the results were largely inconclusive. In the April–June 1951 issue of the *Quarterly Report*, they published their decision to discontinue the research, concluding that they were unable to distinguish the information in the transmitted signal from the "noise" introduced by medically irrelevant variations in skull thickness.

When compared with x-rays, diagnostic ultrasound in neurology had failed its clinical test. Bolt and Ballantine also compared ultrasonograms created from the skull of a living patient and from an empty cranium immersed in a water bath. In this comparison, essentially the same picture was obtained, suggesting the pattern produced was better understood as a representation of the variations in bone thickness over the skull than as a representation of the form of the brain. The ultrasound methodology was then tested in two separate comparisons, and in each a reference image was used to set standards of reliability. But it was the reliability of the way of generating an image that was being put to the test, not the representational conventions as such.

Hueter and Ballantine went on to work on the surgical applications of higher-power ultrasound, but there are clear signs that the impetus had gone out of the medical acoustics initiative at MIT. Hueter left MIT in 1956 to work for the Submarine Signalling Division of the Raytheon Corporation, before moving on to work for Honeywell, of which he became a vice-president. That they should have abandoned their work to apply ultrasound in neurology is not surprising, but it does need fuller explanation. At first sight it is more puzzling that they did not consider other areas of medicine to develop diagnostic applications, as two other groups in the United States in the early 1950s had published papers indicating that ultrasound scanners could detect tumors in other parts of the body. Perhaps they felt restricted to neurological patients?

Wild and A-Mode Scanning

The origins of diagnostic ultrasound lie in neurology, where it proved difficult to use. While the work still continued at MIT, other researchers explored different ways of using ultrasound more in line with its engineering and naval uses, which employed reflection. This much more successful approach called forth another set of graphic conventions for displaying an image.

One research group was based in Minneapolis and centered on Dr. John Wild in the Department of Surgery at the University of Minnesota Medical School. He had qualified as a doctor in Great Britain in 1942 and took up this post in America in 1946. He began using an ultrasound generator that had been employed to train naval pilots to recognize the kinds of radar echoes that they would see flying over the islands of Japan. The generator worked at 15 MHz, an order of magnitude higher than the frequencies employed in flaw detection. This improved the resolution of ultrasound images but reduced the penetration through tissue. The ultrasound was transmitted in short pulses rather than continuously, as was the case with Dussik's equipment.

Wild's initial apparatus was simple in design. It consisted of a crystal mounted inside a water-filled chamber, at one end of which was a rubber membrane. The tissue was not scanned as such; the

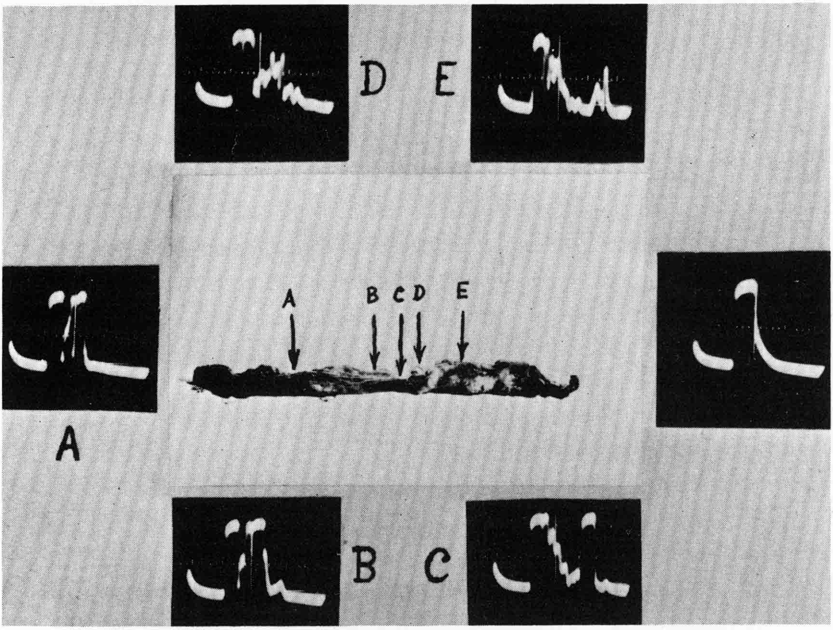

Figure 2
Results of experiments with a strip of human stomach containing a carcinomatous ulcer. The letters indicate the region of the specimen (shown in the center) being tested. The tracing to the right is of a control rubber membrane. From Wild (1950).

acoustic beam was sent into it, and the echoes from within the tissue were picked up and displayed on an oscilloscope screen. This gave an indication of the nature of the tissue from the kind of echoes that were returned. Wild compared his early method to a needle biopsy. Originally he had been interested in measuring the thickness of the intestinal wall, in the belief that an increase in thickness would represent cancerous tissue.

Wild's first paper, which describes experiments on tissue samples from dogs and human patients, shows photographs of the images obtained. One is shown in figure 2. Again there is a convention at work here. Vertical amplitude of the trace stands for the strength of the signal, and horizontal displacement across the screen is a measure of the time to detect the echo. The baseline interval is then a measure of the thickness for tissues of known density or a measure of density for tissues of constant thickness. Wild believed that tumors could be picked up as abnormally dense or abnormally thick regions. But he also noticed that tumor tissue was abnormally reflective, and he believed that this could be used in the early detection of tumors.

Whereas the graphic conventions underlying Dussik's approach bore some relation to those in a television camera—building up a two-dimensional image using points or patches of light arranged in lines—Wild's conventions were like those in metallurgy, in which changes in one dimension through a sample are rendered as a two-dimensional image. This kind of analysis came to be known as A-mode. Although A-mode imaging continues to have a specific use in fetal cephalometry—and was also used in the 1950s in studies that sought to pick up reflections from the midline of the brain in order to see if it had been displaced by injury—it has given way to other more complex forms of representation. Its advantage is that it can be used to make accurate positional measurements, such as those involved in calculating the diameter of the fetal skull at its widest point, an indicator of fetal maturity. But at the same time, because measurement is restricted to one dimension, the technique can give no direct representation of form. In 1951 Wild and his collaborator, Donald Neal, an aeronautical engineer, published a paper in *The Lancet* describing work with postmortem material to detect brain tumors and on the diagnosis of breast tumors in living patients.

Howry and the Idea of Sector Scanning

Someone else who took up the diagnostic use of ultrasound was Dr. Douglass Howry. In 1948, while an intern at the Veterans' Administration Hospital in Denver, Colorado, Howry began a literature review and some theoretical studies on how ultrasound might be used. His first experiments were done in 1949, somewhat after Ludwig's work on gallstones at the Naval Research Institute in Bethesda. Howry used a reflectoscope of the kind that Firestone had invented. Although Howry and his collaborator, Rod Bliss, an electronics engineer, found that they could get echoes from tissue interfaces and foreign bodies embedded in tissues, the results were too erratic to be useful. Thus they built an oscillator that would work at higher frequencies and a system for scanning a tissue specimen by moving the acoustic beam through an arc. They called this equipment a Somascope. In other words the beam was swept through a narrow angle, creating a two-dimensional pattern of echoes. By timing the echoes electronically, they could build up a representation of a slice through an insonated object. The first results were obtained in 1950.

In their first paper, published in 1952, they showed pictures of the oscilloscope patterns created using a series of objects immersed in water: first, a water-filled condom containing a glass rod; then a normal gall bladder and one with gallstones; a slice of liver with a match, a nail, and a plastic rod stuck in it; and, finally, an arm held in a water bath. They claimed that the reflections from the radius, the ulna, the extensor tendons, the muscle-fat junction and the skin surface could be seen on their "somagram." The pictures they published were taken in the spring and summer of 1951. More particularly they claimed that their equipment was capable of revealing details of soft tissue of a kind that was completely unavailable from an x-ray. They felt confident enough to circulate their results widely and to send them to the National Research Council for review in August and September 1951 (Howry and Bliss 1952).

Howry's apparatus created a rather different kind of image from those generated by the MIT group and by Wild and Neal in Minneapolis. In effect it is A-scanning, repeated many times by sweeping the ultrasound beam through an arc. The geometry of graphical space is polar rather than Cartesian. What appears on the screen is a series of bright spots, each of which registers a reflection from a discontinuity. As the beam moves through the arc, these points on the screen merge into a pattern that appears as if it were a slice through the insonated object. The partial rotation of the beam and the electronic recording of the echoes as spots of light thus "renders," in Lynch's sense, the internal two-dimensional structure of an organ or a limb or a test object in a given plane. The resulting image is certainly not artifactual. It registers features, like the fat-muscle interface, that really exist. Yet it picks out only those features that reflect ultrasound.

An example is shown in figure 3, along with a drawing of a transverse section through the mid-forearm showing the two bones of the arm, various tendons, membranes, and arteries, the subcutaneous fat, and the skin surface, which serves as a reference image. In Howry's 1952 article the two images were juxtaposed, and the reader was tacitly invited to compare the position of bright patches on the ultrasound image with elements in the anatomical drawing. Anomalous bright patches are also identified as reference reflectors held behind the arm. Furthermore, the lack of detail in the lower half of the image is explained as the result of absorption of the ultrasound by the bones of the arm. Within the image there are areas in acoustic shadow.

With these directions the image in figure 3 is now fairly easy to read. However, if one tries to compare it with the exterior form of an object, then it is in fact much harder to work out what one would be seeing at any point on a slice through that object. There is both a cognitive and a perceptual barrier to be surmounted. One needs to

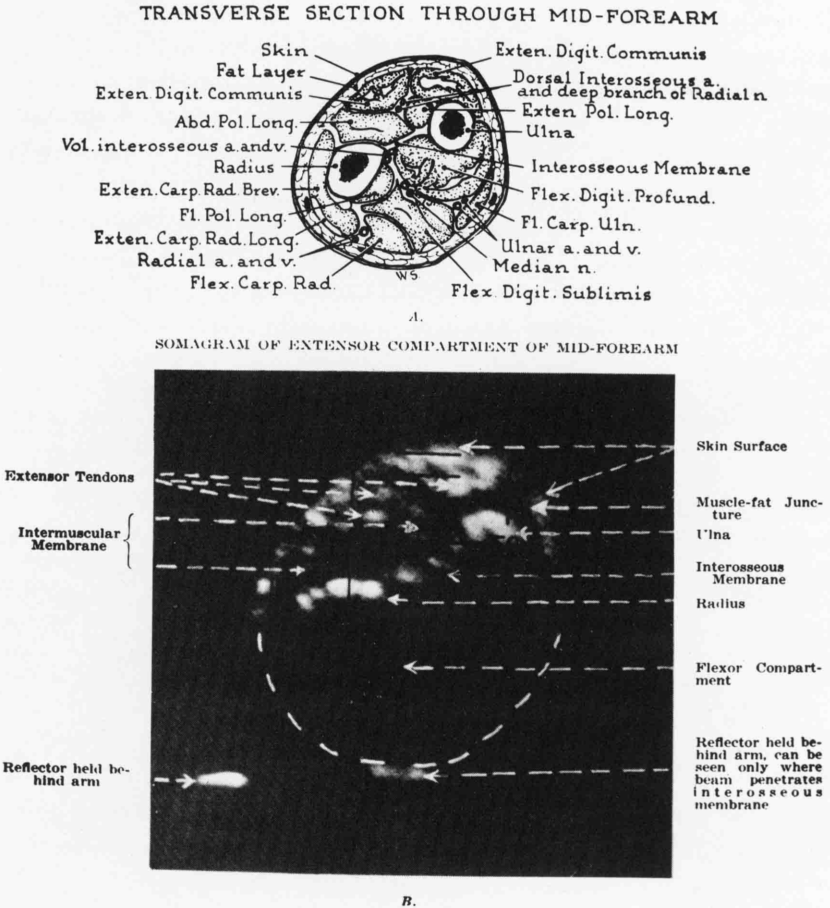

Figure 3
A sample somagram. From Howry and Bliss (1952).

know what should be where, and one needs to be able to visualize how any given slice will appear. Thus the ultrasound images of normal and calcified gall bladders in the same article, for which the reference image is an exterior view of each object, are much harder to decode. Moreover, this is precisely the perceptual transformation that has to be made with obstetric ultrasound scans.

This point about ease of visualization was made in a report for the US Atomic Energy Commission, published in 1955, although the contract work on which it was based was done in 1952 through 1953.

> Probably the most difficult aspect of the problem is to interpret the data yielded by the echo-ranging system. The familiar "A-scope" presentation, which is a plot of reflected sound amplitude versus distance along the sound beam, is quite seriously limited in its information. It is necessary to integrate or scan a whole series of probe positions in order to consolidate the data so that any analysis can be performed on it. The reflection patterns are so complex that only a comparative analysis against a normal pattern can be interpreted. The problem is so difficult because we are not used to mentally interpreting data which shows not only the exterior details, but all the internal configurations of an opaque solid presented in three dimensions. An analogy would be to observe a series of cross-sectional x-ray planigrams of an opaque object oriented such that the original spatial references are retained. (USAEC, 1955, p. 23)

The report goes on to consider various other display systems that would be perceptually easier to deal with but more technically complex to create, including Howry's sector scanning method. This section of the report concludes:

> In conclusion therefore it must be realised that there is no prospect of adapting this tool to detecting intracranial lesions, for even if the skull did not have high absorption properties of its own, the distances involved would make the application foolish. The use of ultrasonics in this manner or anywhere on the body would have to be limited to very shallow penetrations. Such an instrument must have an intregrating scanning system and reduced aperture transducers. (USAEC, 1955, p. 45)

No author is given for the report. Its unenthusiastic attitude toward the neurological work is said to have inhibited research in the United States in other areas as well. Interestingly, the first section deals with imaging methods based on isotopic labeling of blood proteins. These are favorably reviewed, and work at the Department of Surgery at the University of Minnesota, although not by Wild, is used to illustrate what is possible.

Both Howry and Wild began to refine their equipment and to extend its versatility. Both groups eventually obtained funding from the NIH. For the Denver group the geometric complexity of the scanning was increased to remove some of the artifacts from the image and to improve its definition. One of the problems they discovered was that significant reflection occurs only from an interface when the beam crosses it at right angles or close to it. Rotating the transducer around the object being examined was one way of tackling that problem. But to make this possible, subjects were immersed in water while the scanner ran on a circular track around them. One version of their equipment was made from a B-29 gun turret, filled with water in which the subject sat with a lead weight around the waist to prevent him or her from floating to the surface. This arrangement could be used to scan the liver or other internal organs. It gave remarkably clear pictures but was obviously unsuitable for sick patients. The next development was a scanner with a semicircular water bath, the rotating transducer head being immersed in the water with the patient sitting against a window in the flat surface of the bath, out of the water. This kind of equipment gave clinically useful images, and the impetus in Denver to develop new forms lessened through the 1950s, even though the equipment was rather bulky and not at all portable (Holmes 1980).

By 1955, then, the experience with diagnostic ultrasound was mixed. Some people had found it hard to use. By concentrating on soft tissues and internal organs not surrounded by bone, some researchers had found that the complexity of echoes could be simplified by modifying the equipment to complexify the scanning. The resulting transformations of the internal structures of insolated objects were then somewhat simpler to comprehend. But this perceptual assistance was bought at the price of mechancial or electronic complexity. But usable systems could be engineered and results obtained that were at least promising, if one chose to spend the time building the equipment to generate them and had the confidence to decode them.

This is not an exhaustive account of all the work in ultrasound in the late 1940s and early 1950s. More detail is available from other secondary sources (Wells 1978; Hill 1973; Holmes 1980; Oakley 1984). I have deliberately ignored other work in the United States, France, Sweden, and the United Kingdom for the sake of brevity. I am not able to consider the significance of work in cardiology, ophthalmology, and physical medicine for the development of imaging technologies. But it should already be clear that there was a diversity of approaches to the technical and representational problems of using sound to visualize internal organs. Different groups pursued different strategies and explored the utility of different graphic conventions, even though a common aim was improved diagnosis. What seemed an acceptable engineering solution was somewhat variable, although in each case the basic challenge to be faced was the validation of the resulting image through some sort of visual comparison. For the Denver group, by the mid 1950s they believed that they had what one could call in the idiom of the new sociology of technology a "stable" artifact that generated usable images and was not thought to need radical design modifications. The quality and reliability of the images was seen as a major problem, however, and attention eventually turned to the question of how to standardize the equipment with test objects (Holmes 1967). In Minneapolis the apparatus was much more compact, but there remained considerable skepticism as to whether it was capable of the kind of diagnostic sensitivity that Wild claimed for it. Ultimately this difference of judgment led to the termination of Wild's research funding and massive litigation between him and the agency managing his research.

As far as I can discover, although electronics and engineering firms were involved with this research in various ways, funding some work, lending equipment, and following the results, there were no actual plans to develop and market diagnostic as opposed to therapeutic equipment in the mid-1950s. By the end of the decade General Precision had an interest in ultrasound equipment in ophthalmology, and Smith Kline marketed in 1964 an echocardiograph based on work done outside the United States ten years before. By the late 1950s the Glasgow firm of Kelvin-Hughes, who made weld-flaw detection equipment was investing in the development of a diagnostic scanner. Their Diasonograph was put on the market in the early 1960s. Aspects of the development of their product are described in what follows, but against the background of difficulties in application their judgment that this technology was worth a significant investment over a number of years is quite remarkable. A convincing explanation of it requires more research. What is interesting about it is that Kelvin-Hughes's interest came in two stages: initial low-level assistance and then a much more substantial commitment to development of a product. The move from the first to the second stage was accompanied by a major design change to create a new mode of representation, and the commitment to market a scanner went ahead despite a low level of interest from doctors.

Ultrasound in Obstetrics: Early Interpretative Labor

The next set of developments arose through the work of Ian Donald, Regius Professor of Midwifery in Glasgow from 1954 to 1976. Donald had qualified as a doctor before the war and had served in the Royal Air Force from 1942 to 1946. He met John Wild in London in the early 1950s and was stimulated to try to use ultrasound himself. When he obtained his chair in 1954, his opportunities outside the rather conservative ethos of medicine in London and his power as a Regius Professor in Scotland increased and he began to consider how he could realize his plans. He had a patient whose husband was a director of the engineering firm of Babcock and Wilcox, and through this contact he obtained an ultrasound flaw detector. This led him to the manufacturer Kelvin-Hughes, and Donald's first experiments were done on their premises in 1955, using the equipment on an ovarian cyst, fibroids, and a large piece of steak. Their initial results, recorded by the company artist, were promising. As Wild had found, different tissues could be distinguished ultrasonically. The production of recognizable images in the hospital turned out to be much more difficult, and it was another three years before Donald, his junior colleague, John MacVicar, and an engineer, Tom Brown, published a paper on their work in *The Lancet* (Donald et al. 1958).

Several themes stand out in Donald's recent accounts of this development, of which there have been several (Donald 1969, 1974a, 1974b, 1976, 1980). First, the problems of producing clinically useful images in the 1950s were considerable, and many hours were spent in modifying the equipment, changing the procedures, and trying new approaches. One could not see this as a marginal activity occasionally taken up nor as yet a routine task that could be entrusted to a subordinate. Equally, the facts that funds were made available by the Scottish Hospitals Endowment Trust and that Brown was seconded from Kelvin-Hughes indicate that ultrasound was also taken seriously by others as something that would pay off. When Smiths Industries withdrew, having bought up Kelvin-Hughes, the National Research Development Corporation also took an interest. Second, moving into the clinical setting was enormously helpful, although it exposed women and their babies to a highly experimental research technology. Certainly Donald saw their early rejection of postmortem material and laboratory experimentation as decisive (Donald 1969).

Third, this work was undertaken knowing that others had published promising results. Even if some of these earlier claims looked rather dubious, nonetheless it must have been reassuring to know that the technique could be made to work. Donald maintains that the principal stimulus for him came from Wild and that initially he was unaware of most of the early work. After making contact with Kelvin-Hughes, he went to visit Professor Mayneord at the Royal Marsden Hospital in London; Mayneord was using some of Kelvin-Hughes's equipment to extend Dussik's work. When Mayneord gave up, Donald was lent that equipment, through the intervention of Dr. Donald Gordon, a radiologist interested in ultrasound himself (Gordon 1959). There were therefore several individuals in Great Britain able to give Donald some initial practical support.

Gradually the group in Glasgow increased their skill at generating and interpreting A-mode images. Interestingly one of the crucial cases, as it came to seem afterward, involved a disagreement between MacVicar and Donald over what the image meant. Clinical and visual judgments were inconsistent. Subsequent surgery showed that MacVicar was in fact correct and that the prior diagnosis of a tumor, which Donald had been asked to confirm, was wrong.

Compound Contact Scanning: The Most Complex Graphic Space

By 1957 the Glasgow group came to the conclusion that more complex forms of representation were necessary. They set about designing equipment in which the probe was in contact with the surface of the abdomen while being moved across its surface. Their early A-mode work had used a small water column to "couple" the ultrasound transducer to the patient's body, which had been difficult to use in practice and produced many wet beds. The problem was somehow to allow movement across the abdomen—or longitudinally down it—and to use the sector-scanning technique employed by Douglas Howry without immersing the patient in water. The solution was ingenious and created an exceedingly complex graphic space, which is shown in figure 4. This figure is taken from an article for electronics engineers (Brown 1960).

The resulting oscilloscope image is thus the integration of a whole series of sector scans, which are themselves summations of axial probes into the body. By 1958 Donald and and his colleagues felt that they had enough material to publish a paper in *The Lancet*. This paper describes 275 scans on 100 female patients at the Western Infirmary in Glasgow. They show scans of the bladder, the femur, ovarian cysts of varius kinds, a normal abdomen (MacVicar's in fact), an abdomen distended by ascites, the fetal skull, showing reflections from the

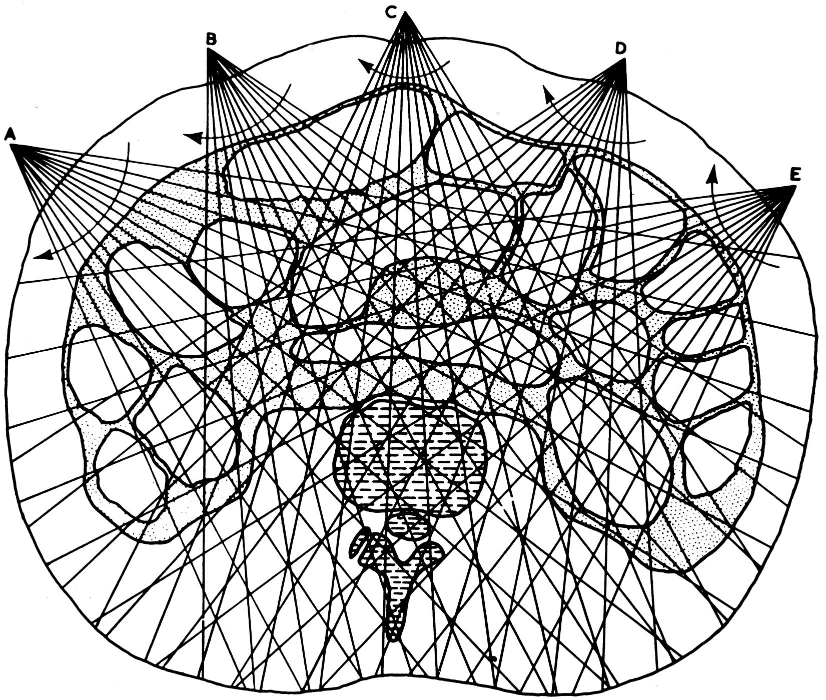

Figure 4
Diagram illustrating the principle of compound sector scanning. Here A through E are the origins of typical sector scans. From Donald and Brown (1961), who reproduced it from *Medical Electronics.*

midline of the brain, hydramnios, and two twins in the breech position. They then cover a range of obstetric complications and related clinical conditions. The original purpose of the paper was the differential diagnosis of abdominal distension and the exclusion of malignant disease. The concentration on pregnancy came only as the work progressed, and interest in fetal abnormality and rates of growth, with which obstetric ultrasound is now often associated, came even later.

They concluded:

Our experience of 78 cases in which diagnosis was quickly verified by laparotomy and subsequent histology indicates that ultrasonic diagnosis is still very crude, and that preoperative diagnosis of histological structure is still far off, although such a possibility in the future is an exciting prospect. The fact that recordable echoes can be obtained at all has both surprised and encouraged us, but our findings are still of more academic interest than practical importance, and we do not feel that our clinical judgement should be influenced by our ultrasonic findings. Our most spectacular results have been obtained in dealing with fluid-filled cavities, which certainly show up well; but it is only fair to point out that the illustrations shown herewith are among the very best that we have so far been able to produce out of about 450. They do however encourage great efforts to refine our technique. (Donald et al. 1958)

Great efforts were needed, for it was three years before Donald and his co-workers published another paper. Although two papers on obstetric and gynecological ultrasound in 1959 by other workers and one in 1960 had appeared, it was five years before others began to publish papers replicating Donald's work (Taylor et al. 1964). Certainly one cannot say that Donald's was a technique that others immediately sought to copy, even though there was considerable interest in the *Lancet* paper. Comparative data from the *World Bibliography of Ultrasound*, which is supposed to be an exhaustive compilation of all papers on all aspects of the subject published this century, shows that publications on the use of ultrasound in obstetrics lagged those relating to its use in other fields, such as cardiology and ophthalmology, although obstetric publications are now vastly more numerous.

Conclusions

What implications does this case study have for the sociology of technological innovation? Is it consistent with a social constructivist view of technology? I believe it is, in the sense that the process of development described here is one in which different strategies were pursued with mixed results; at various stages in the work of each group, significant appraisals of overall process took place, and the decisions to continue as before, to diversify, or to abandon the work were made by reference to a whole set of factors. One could not say that some intrinsic "best solution" guided the success of particular groups or that some set of intrinsic difficulties can explain the failure of those who pulled out. It is, of course, the case that those working in neurology had to cope with a complexity of reflections and other problems that did not exist in studies of other parts of the body. But that is not in itself a sufficient explanation of the particular moves that they made.

It would also be difficult, and I believe rather perverse, to see this technology as emerging through a unilinear process of development from some initial scientific breakthrough. Although one could make much more of developments in applied science, particularly those in electronics in the late 1930s and early 1940s that produced fast switching circuits that allowed transducers to be switched from sending to receiving rapidly, it is apparent even from the restricted data that I have presented here that each such technical breakthrough was mediated through all kinds of different applications. In other words, this case conforms to the model of overlapping and multilinear development, discussed by Bijker (this volume), with inputs from applied science at various stages and not just at the origin. Given the time frame that I have chosen, from the late 1930s to the late 1950s, this study is not like those of large technological systems discussed in this volume by Hughes, Constant, and MacKenzie. My focus has been much more on invention and early development to the prototype stage, rather than on the modification of elements within a mature system, in which control of the commercial or political environment may be of crucial importance.

This case study limits itself to the inventive activity of various medical men, assisted by applied scientists and engineers, around the time of the Second World War. It comes, then, after the scientization of medicine in the early years of the twentieth century, within what Reiser has called "the reign of technology" (Reiser 1978). All the doctors here clearly believed it appropriate and worthwhile to devote time to producing a diagnostic technology. Their full motivation I am not able to describe, but it is striking in the case of all but Dussik that the experience of the war and acquaintance with radar or sonar precipitated their interest. One could certainly link this story to other analogical and practical borrowings from wartime work with radar that led to new research techniques, technologies, and models. The examples include Huxley and Hodgkin (nerve conduction), Porter (flash photolysis), and Ryle and Lovell (radio astronomy). My point is that it was not just a technical familiarity with electronics that was helpful; it was the idea of propagating a signal or a transient perturbation that could reveal characteristics of the system being analyzed that was of heuristic value. For ultrasound the ideas of reflection and image composition seem to have been as important as an appreciation of what the electronic equipment could and could not do.

This leads to consideration of the attributes common to the researchers involved. Bijker has proposed the notion of a technological frame in order to characterize commonality of approach or perception, without the more usual reference to specific disciplines or research traditions (Bijker, this volume). He also writes of the degree of inclusion within the frame as a determining variable in explanations of inventive strategy and performance. I myself am not persuaded that these terms are sufficiently precisely defined to move the debate forward, but the phenomenon to which they are directed is an interesting one, namely, the influence of professional socialization or technical facility on imagination and inventiveness. If one speaks of a shared commitment to the application of ultrasound in diagnostics as a technological frame, then it is apparent that in this case study the individuals concerned drew on their instititional and intellectual resources in markedly different ways. The groups that failed were those that stuck rigidly to an initial strategy: in Dussik's case with limited financial and technical resources, in the case of the MIT group with massive amounts of money and technical experience and support. The groups that were more successful tried a wide range of different approaches and built a range of different devices; they did not just modify one initial piece of equipment. If we treat inclusion as a synonym for rigidity and thus abandon its use as an explanatory concept, the more highly included individuals seem to have lacked the flexible puzzle-solving strategy of the more successful centers.

This is really a question of how different people defined the technical constraints to the use of ultrasound, given the goal of eventual clinical application. The events at MIT suggest that in technological development, as in more fundamental studies, there is no such thing as a crucial experiment, in the sense of a result that instantly terminates a line of research. Thus the difficulties with variations in skull thickness were recognized by them, if not by Dussik, early on, and they tried a variety of ways of getting around them. There were other problems that they might have pursued, such as studies of reflections from the midline of the brain, or studies of other organs. They did not do so because they took a particular result as indicative of more general problems with the technology, a view not shared by researchers elsewhere. This study supports the view that technical judgments are rarely, if ever, dictated by experimental evidence alone and that what has to be explained is how the interpretative flexibility inherent in research is actually used.

Finally one could group the various devices produced in Minneapolis, Denver, Cambridge, Glasgow, and Bad Ischl and elsewhere in Sweden, France, Germany and Japan, into some kind of evolutionary tree. If one chooses to construct a tree, there is a kind of trajectory or pattern of relation and descent from bulky lab equipment closely modeled on or built with industrial flaw detectors with awkward systems for "coupling" the transducer to the patient to the machines with solid-state circuitry and much more complex projective geometry. One can find closure of inventive cycles and stabilization of design thinking around particular forms. Clearly, these are phenomena within the innovation process that need to be explained, and, as MacKenzie's work shows, the variable that comes to be decisive is only one of several possible (MacKenzie, this volume). I am not going to comment on this for the case of ultrasound, except to say that my hypothesis is that ultrasound technology stabilized when doctors came to the view that the generation of images on which they could rely for making clinical decisions could be safely entrusted to others, such as radiographers and other subordinate personnel.

I began with a reference to the contemporary debates on the value of ultrasound and the way it is used in the hospital environment. I believe that such discussions have much to gain from taking account of the fact that scanning now is work. The job of the person performing the scan has a history. The tasks involved have been designed, negotiated, and defined in relation to the work of others and depend on the exercise of specific skills. Who has these skills and how they are valued by others has changed through time. Thus the experience of having an ultrasound scan depends on how various individuals are able to work, how they are intended to work, and how their constantly shifting relations with doctors are managed. Explanations of the stability of technologies must take account of the social relations of work as one aspect and, in the case of ultrasound, of the reliability of the images produced.

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