CHAPTER 7

In Search of Space: Fourier-Spectroscopy, 1950-1970

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Introduction

In the large grey area between science and technology, specialisms emerge with associated specialists. But some specialisms remain 'peripheral sciences', never attaining the status of 'disciplines' ensconced in universities, and their specialists do not become recognised 'professionals'.[1] A major social component of such side-lined sciences – one important grouping of technoscientific workers – is the 'research-technology community'.[2] An important question concerning research-technology is to explain how the grouping survives without specialised disciplinary and professional affiliations. The case to be discussed below illustrates the dynamics of one such community.

The specialists surrounding the technology of scientific instruments have been cited as major contributors to research-technology activities.[3] Their professional posts, social status and monetary support are more uncertain than is the case for either academic scientists or qualified engineers. Instrument specialists and their products are less established and often more transitory than their counterparts on either side of the science/technology divide.

The volatility and survival tactics of such interstitial communities can be illustrated by the case of Fourier spectroscopy (also known as Fourier transform spectroscopy, FT spectroscopy, FTS or FTIR). This technique, described briefly below, was developed after the second world war. It attracted a fairly robust coterie of participants for about two decades before becoming successfully commercialised and supplanting older technology in the 1970s and 1980s.

To a great extent, all spectroscopies (and, more generally, instrumental techniques and indeed measurement technologies) are 'peripheral' to academic science, which they supply.[4] Fourier spectroscopy, though, is in some respects an extreme case. It existed for a very long time without convincingly 'serving' academics, and without generating uncontentious results for all of its proponents. As a result, this small community employed several tactics to enhance its credibility. These were truly survival *tactics* rather than *strategies*, because they consisted of what can be seen as disciplinary border skirmishes without any overarching plan of attack.

The result was a transformation of the *discipline* of spectroscopy, but a continued *professional* invisibility of the community which achieved it. I contend that this is the norm rather than the exception for research-technologies. Moreover, this fluid, transdisciplinary behaviour is increasingly common in modern technoscience; a fluid community, indeed, can increase its enrolment by drawing upon practitioners in cognate fields.[5]

In the following sections, the principal players – including the concept, its proponents, sponsors and opponents – will be introduced. Their various attempts to influence the adoption of the technique will then be discussed.

The technology and its proponents

Many of the technical details of this technology can be by-passed for the social features that I want to stress, but it is necessary to sketch a few distinctive features to set the context and to make subsequent events understandable.

Devised in large part by Albert Michelson in the 1890s, Fourier spectroscopy was developed after the second world war by researchers in Britain, France and America. The name Michelson produces a ‘knee jerk’ response by philosophers and historians of science as the man who tested the hypothesis of aether drift, or perhaps for his precise measurement of the speed of light. But he was much more than that: Michelson was in many respects a prototypical research-technologist.

A professor of physics at a time in America when physics was subservient to engineering, Michelson spent most of his career perfecting and applying his version of an optical device dubbed an *interferometer* to numerous problems. In adapting his generic device to a myriad of uses, he found himself participating with astronomers, chemists and naturalists; interacting with standards institutions, universities and government – but never entirely embraced by any community. Through his career, Michelson stressed the multifarious applications of his instruments rather than any scientific objective. With his interferometers, Michelson measured differences in the speed of light; the separation of binary stars; the length of metrological standards; and the spectral components of coloured light.[6] This latter spectroscopic application was analytically the most complex.

An interferometer is a simple but unintuitive instrument (see Fig. 1). It consists of a detector of light intensity and three mirrors, one of which typically moves, and another of which is partially reflecting. When using an interferometer, the spectrum of the source of light turns out to be encoded. To determine the relative intensity of the wavelengths making up the light source (that is, to measure its ‘spectral distribution’) the experimenter moves one of the mirrors by infinitesimal amounts, while recording fluctuations in the *brightness* of the light – not the colour – leaving the device. These intensity variations are related to the spectrum by the complex mathematical relationship known as the Fourier transform (hence ‘Fourier spectroscopy’). The measurement of the mirror position and the light intensity must be made with exquisite precision.

So, this method is perceptually and experimentally difficult. It required Michelson to be not only adept at precision mechanics and meticulous optical observation – which a handful of other optical scientists also were, at the time – but to devise a method of decoding the spectrum. He eventually designed a mechanical calculating device – a form of harmonic analyser – to perform the mathematical operation of Fourier synthesis. This was a laborious and approximate procedure even with the analyser, and very few others took up his technique.[7] Michelson himself invented more convenient forms of precision spectrometer in the next decade, and did not pursue the method. By the end of the first world war, what Michelson called ‘interferential spectroscopy’ had been relegated to the status of an ingenious but outmoded technical curiosity.[8]

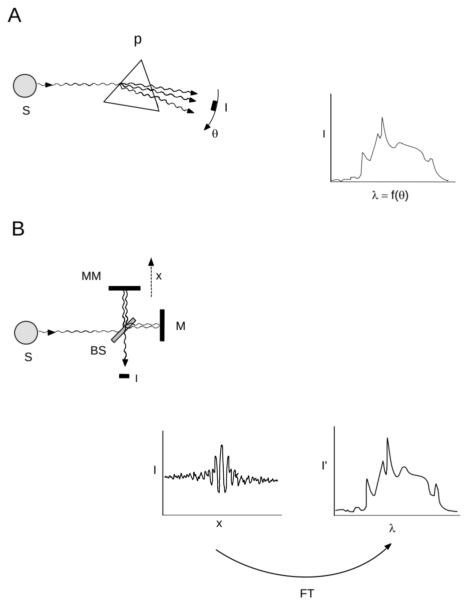

Figure 1. Distinguishing features of optical spectroscopies. A. Dispersive spectroscopy: *S*, light source; *P*, prism or diffraction grating; *I*, intensity measured by detector swept through angle θ. In the resulting spectrum I(λ), the wavelength λ is a function of θ. B. Fourier spectroscopy: *S*, light source; *BS*, beam-splitting mirror; *M*, fixed mirror; *MM*, moving mirror, translated through distance *x*; *I*, intensity measured by detector. The resulting record of *I* versus *x*, or 'interferogram', is related to the spectrum *I'*(λ) (or, more accurately, *I'*(1/λ)) by the Fourier transformation *FT*. Both techniques involve specific complications to calibrate the wavelength scale and instrumental response.

Through the interwar period and the second world war, the analysis of long-wavelength light (so-called infrared spectroscopy) became increasingly widespread. Infrared spectroscopy allowed chemists to identify, quantify and determine the chemical structure of molecules. It was particularly useful for wartime analyses of synthetic rubber and fuels. To satisfy a growing demand, a few commercial instruments appeared.

Like all spectrometers at that time, this conventional technology relied upon a prism or diffraction grating to disperse light into its component wavelengths and then to measure the intensity of each of them separately. It is relatively easy to comprehend, and, just like the spectrum itself, the engineering has the advantage of being separable into distinct parts that can be individually analysed. By contrast, 'Fourier spectroscopy' which developed from Michelson's 'interferential spectroscopy' is different in nature as well as detail: it requires a more 'integrated' theory and yields a more 'holistic' instrument.

Apart from the community of analytical chemists beginning to adopt infrared spectrometers after the second world war, there were other groups engaged in less routine research. All of these other groups were trying to extend spectroscopy into more difficult domains. And for all of them, the overriding concern was one of sensitivity – to measure weak signals in difficult conditions. This concern led some to explore the nature of the instrument itself.

This new cognitive basis for spectroscopic design infused a new generation of investigator. According to one of them, Peter Fellgett, 'the real trigger' for development was *instrument science*. 'The basic idea of "instrument physics" is to understand in a full scientific sense why an instrument has a particular performance', he argued:

> In most cases, a scientific instrument is devised in the first place as a means to the end of making some physical phenomenon or quantity susceptible to observation or measurement, and once it has served this purpose nobody thinks very deeply about it again. Consequently, it is often tacitly accepted that 'in theory' an instrument should have a particular performance, but 'in practice' it does not. This however is not good science, which demands that if theory and practice differ, then one or both must be improved. Had Adams and Le Verrier been content to say that 'in theory' Uranus moves in a particular orbit but 'in practice' in a slightly different one, the planet Neptune would never have been discovered.[9]

This was hardly a new idea in Germany, where instrument science had been on the agenda since the 1870s.[10] It was, however, an epistemic basis little employed in Britain (except, perhaps, at a handful of sites such as the Cambridge Scientific Instrument Company, founded in 1878).[11] Such a hybrid was unappealing both to academics, who frequently disdained engineering sciences, and to British engineers who, in most branches of the subject, were still trained by apprenticeship more often than by academic studies.[12]

The need to concentrate on instrument design was a theme repeatedly broached by post-war spectroscopists, because all were seeking to make measurements at the limits of practicality. Several of them, though, were diverted from their original research to instrument science itself. I will briefly give a narrative of the emergence of the subject through these individuals, and then relate this to a more analytical description of their activities. A prosopography of the group that emerged, which will be termed the 'Fourier community', reveals some distinct career features that, I would argue, extend to other collectives of research-technologists.

During the second world war, Peter Fellgett had worked as part of a group analysing aviation fuels, using prism spectrometers. At that time, he says,

> I had become interested in why infra-red detectors . . . were so relatively insensitive; or, more generally, whether it were possible to identify some physical limit of radiation detectors.[13]

As a PhD student from 1948 at Cambridge, Fellgett realised that most of the radiation passing through a prism spectrometer is wasted because only a narrow band of wavelengths passes through a slit to be measured by the detector at any one time. He consequently looked for some means of measuring all wavelengths simultaneously. This would provide a stronger detector signal that was less obscured by sources of noise. His initial thoughts were for a rather complicated arrangement of rotating optical disks that would 'multiplex' different infrared wavelengths on a single detector, combined with an undefined scheme to somehow separate the encoded wavelengths from the detector signal afterwards.[14]

Within a year, however, Fellgett concluded that a Michelson interferometer could quite elegantly achieve what he wanted: to encode the entire spectrum of light into a single time-varying electrical signal. This would yield a much more sensitive form of spectrometer. This principle of 'multiplexing' eventually became known as the 'Fellgett advantage'. The drawback for Fellgett, as for Michelson, was that the signal was encoded in the form of a Fourier transform. Fellgett adopted a manual calculation aid developed before the war for analysing X-ray diffraction patterns. This was sufficient to demonstrate the principle.

Fellgett did not publish his results, but publicised them by word of mouth. John Strong, an infrared spectroscopist at Johns Hopkins University in Baltimore, who had been designing infrared instruments since the early 1930s, visited Cambridge in 1949 and saw some of Fellgett's apparatus and results. He misunderstood the principle and the multiplex advantage, however, and decided to build his own version simply as a means of improving conventional spectrometers.[15] Strong's post-graduate students and technicians at Johns Hopkins constructed and used the device as a sort of optical filter for a grating spectrometer. He was thus adapting this quite novel technology to an old problem. When Peter Fellgett visited Johns Hopkins the following year, however, one of Strong's students, George Vanasse, understood and rekindled interest in Fellgett's original ideas.[16]

Fellgett also presented some of his results in a 1952 meeting of the Optical Society of America.[17] A few commercial designers were immediately intrigued. Indeed, designers in the engineering department of Perkin-Elmer Corporation in New Jersey, producers of one of the few commercial prism-based infrared spectrometers, for a time investigated the practicality of interferometer designs.[18]

Strong invited a British physicist, H. Alistair Gebbie, who shared his interest in atmospheric measurements, to join his group on an Air Force contract in 1954. Gebbie proposed using a digital computer to transform the measurements into spectra.[19] Strong arranged for an IBM 605 computer at Binghampton, New York, to be programmed for the Fourier transform, and a single spectrum was calculated. Owing to the high cost of computer calculation (some $25,000 per spectrum), it remained the only published transformed spectrum until 1956, appearing in no fewer than four papers and a book by Strong by 1959.

In France, Pierre Jacquinot, who had employed large and high resolution spectrographs for his research, found that he needed a cheaper form of instrument when he moved to a new French university post in 1942 under the Vichy government. Through the mid-1940s he began to experiment with other forms of instrument and eventually discovered that the Michelson interferometer had another advantage: unlike dispersive instruments, it did not require a slit which inevitably limited the amount of light that could pass through it (and which thereby reduced the so-called optical 'throughput' or 'étendue'). This attribute later became known as the 'Jacquinot advantage'. Jacquinot judged, however, that 'the precision requirements to obtain good spectra were so severe that is was almost unbelievable that they could ever be met'.[20] He did not publish his work until 1954, and even then focused on other kinds of instrument.[21] Like Strong, Jacquinot directed his students to instrument research. Two of them, Janine and Pierre Connes, began studying the theory and practice of Fourier spectroscopy in 1954. Pierre Connes later wrote of Jacquinot's 'will and ability to treat menial instrumental matters as parts of physics just like more high-flown subjects'.[22]

In 1954 another American, Lawrence Mertz at Baird Associates, was working on a contract for the Army Signal Corps because two of his superiors had heard of Fellgett's work. Mertz developed a Fourier spectrometer of his own design, but again encountered the problem of calculation, and did not obtain a spectrum from his original measurements for over a decade.

By the late 1950s, then, there was a collection of perhaps a dozen persons in three countries pursuing this new instrument technology. But how self-conscious was the collectivity of these groups? Commonality was not immediately obvious: according to Mertz 'various clans coagulated to develop and exploit' the techniques, centred on the concepts of either Fellgett and Jacquinot.[23] Fellgett later remarked that Jacquinot's work was largely irrelevant to his own studies of stars because the 'Jacquinot advantage' related only to large light sources.[24] If, then, the group interests were not strong, by what mechanisms did this new subject survive?

Finding a common line: the 1957 Bellevue Conference

In 1957, Pierre Jacquinot promoted a merging of interests by organising a conference entitled ‘interferometric spectroscopy’ to cover the non-traditional but expanding list of techniques being investigated by his laboratory at Aimé Cotton. Only five of the papers dealt with Fourier spectroscopy. The conference ‘served to bring all Fourier spectroscopists. . . together and to provide an arena for exchanging their ideas’ and to share their meagre results.[25] Fellgett showed some extensions of his thesis work; Strong, Vanasse, and Gebbie, their single spectrum calculated the previous year; Mertz, his still undecoded data of a star; and Janine Connes, a theoretical analysis of the underlying mathematics. The conference was nevertheless seen by contemporaries as important. This first joint meeting provided encouragingly complementary information. Within the jumble of scanty results, the proponents saw a pattern that seemed to support what had been separate theoretical derivations: Fourier spectroscopy appeared able to give recognisable spectra, and promised to be orders of magnitude more sensitive than conventional methods. The ensemble of papers also provided a critical mass that gained attention for them in the specialist press.[26] This potential for opening hitherto impracticable domains such as far infrared, atmospheric and stellar spectroscopy was reason enough to tackle the serious problems of computation and demanding mechanics.

New communities and their patrons

After the Bellevue conference, these individuals were linked collectively by a growing sense of community – or at least an awareness of common goals. The groups and the claims they made expanded, and as they did so, began to attract both allies and foes. The directions of expansion were quite distinct in each country.

Fellgett remained in academia, and from the end of the 1950s was relatively inactive in Fourier spectroscopy.

In France, Jacquinot continued to supervise workers, particularly the Connes, at the Laboratoire Aimé Cotton (LAC) until 1964.[27] This research site was arguably the most stable one for the emerging subject. There, a tradition and acceptance of such research-technology activities had already been established.[28] Moreover, they were able to extend their influence. The bargaining chip, in negotiating recognition, was the promise of dramatically improved instrumental sensitivity. From late 1963, the Connes took a sabbatical year at the Jet Propulsion Laboratory in Pasadena, California, where a development project for a Mars spacecraft was being planned. There they developed a very successful Fourier spectrometer to be used on an earth-based telescope to study the atmospheres of planets. After using it at an Arizona observatory to observe Venus, they brought the instrument back to France in 1965 for studies of Mars. The number of investigators at the LAC grew and developed a widening range of instruments. A number of French spectroscopists were trained at the LAC and have subsequently populated other laboratories at the CNRS and elsewhere.[29]

Vanasse, and Strong’s other students, made Fourier spectroscopy the focus of their PhD dissertations. Strong, however, was not closely involved in the research. With the waning of his personal influence came a decline of the Baltimore research programme.

Vanasse eventually went to the Air Force Cambridge Research Laboratory (AFCRL) in Massachusetts, with which Strong had developed links through earlier consulting and research contracts. Along with him went a number of other former Johns Hopkins students, forming a development team in the 1960s concerned with atmospheric research. Thus a second generation of research technologists – the students of Strong and Jacquinot – were populating this interstitial science outside universities but within government-sponsored laboratories.

The second wave of practitioners also filled another occupational niche: product-oriented research and development in commercial firms. Alistair Gebbie was largely responsible for starting this. He joined the British National Physical Laboratory (NPL) in 1957. By 1960 he had developed his own design of Fourier spectrometer, and benefited from a faster (and effectively free) 'in-house' computer to calculate the spectra. His design was truly a 'generic device', in that the prototype was reworked and essentially resold to three companies over a period of twenty years.[30]

Lawrence Mertz, too, promoted commercial applications. He became Vice President of the fledgling Block Associates (later Block Engineering), a Cambridge, Massachusetts instrument consulting company, from 1960. The company provided special purpose Fourier spectrometers to the Air Force, including one for a 1962 satellite.[31] Here, again, the promise of highly sensitive instruments was promoted, but on rugged air-borne or space-borne platforms rather than on the ground. Both Gebbie and Mertz thus indoctrinated new and experienced instrument designers in Fourier technology while encouraging its transformation.

Provoking opposition

The first advocates of the technology thus found allies at the AFCRL, CNRS and NPL – large, well-funded organisations that allowed considerable leeway in research and which were relatively undemanding of immediate results.[32] Each of these sponsors was government-supported and had academic and industrial links.

But the advocates of Fourier spectroscopy found rising opposition closer to home. From the late 1950s their increasing visibility among colleagues provoked disapprobation. Their critics were spectroscopists and optical physicists employing conventional prism and grating spectrometers. The ostensible technical issues were that (a) Fourier spectroscopy was no better, and probably worse, than existing techniques; (b) it was prone to poorly understood errors which were difficult to discern or resolve because (c) the technique was unintuitive; and, (d) impracticable because of computational difficulties and unrealistic demands for mechanical precision.

As Latour, Galison and others have shown, however, scientific disputes are seldom resolved by objectively agreed cognitive evidence.[33] Many of the critics, for example, were to be found working in similar environments: under government contracts, in national laboratories, or as associated academic staff at universities. Indeed, the 'Fourier community' was constructed by the system of alliances and oppositions which developed. In a very real sense, the nascent Fourier collective was threatening conventional spectroscopists and instrument designers on both intellectual and social territory, by vying for publication space, development contracts and employment openings. Intellectual disagreements consequently were accompanied by other, non-cognitive, arguments. The skirmishes between the old guard and new took place in various locales and employed a variety of tactics. For over a decade, they failed to be decisive.

Tactics of the Fourier community

For the first young Fourier spectroscopists, gaining acceptance by their peers for the technology went hand in hand with their professional accreditation. Fellgett found his PhD advisor unreceptive to his plans to test his notions of the multiplex advantage experimentally, and was advised to downplay his Fourier spectroscopy research.[34] Similarly Mertz, pursuing a PhD at Harvard after his time at Baird Associates, found his work in Fourier spectroscopy rejected as a subject unsuitable for a thesis topic. He later complained that 'the academic community would have nothing to do' with the technique.[35]

On the other hand, those students with an influential academic patron fared rather better. John Strong had a firm reputation as a designer and spectroscopist, as well as influence in industry and government as a consultant.[36] The AFCRL, which had supported research contracts for Gebbie and others, also employed Strong's former students.[37] In the same way, Janine and Pierre Connes found guardianship under Jacquinot both as students and research workers. Both Strong and Jacquinot were securely placed instrument designers able to shepherd their students across the bridge from personal study to academic certification to employment.

The ostensible criterion of scientific validity is to demonstrate convincing experimental evidence. The criteria of convincing evidence, however, are frequently difficult to negotiate between advocates and their opponents. For Fourier spectroscopy, direct confrontation with the prevailing technology proved ineffective.

Not only the nature of evidence, but its manner and sequence of presentation, can be crucial in enrolling support. At the 1957 Bellevue conference Fellgett, and Strong's group, presented only scanty data demonstrating that the method could yield a spectrum of trivial light sources. Such evidence in no way threatened conventional instruments which had been producing copious results routinely for a quarter-century, and did little to convince skeptics either of its potential or justification. The presentation by Mertz of other, untransformed data only underlined the suffocating burden of calculation carried by the technique. Strong himself was biased against the technique by 'an experimentalist's natural distrust of anything involving such a prodigious calculation'.[38]

Other investigators entertained more serious doubts. In 1959 Franz Kahn published a note in *Astrophysics Journal* concluding that Fellgett's 'advantage' was in fact a serious disadvantage when the light source was not extremely stable.[39] Another notable early critic was Gerard Kuiper of the Lunar and Planetary Laboratory at the University of Arizona. A prominent planetary astronomer, Kuiper published an analysis in 1962 which, he argued, proved conclusively that Fourier spectroscopy could not deliver on its promise even in principle.[40] Mertz suggests that Kuiper was converted to Fourier technology during a planetary observing experiment by the substitution of a Fourier spectrometer for Kuiper's malfunctioning dispersive instrument, although this change of faith probably involved other factors.[41] Kuiper subsequently made links with the Connes group at the CNRS; they initiated one of the members of his group in Fourier spectroscopy, and gave him the instrument they had developed at JPL to begin his own programme back in Arizona.[42]

Among the few vocal critics were designers of optimised conventional instruments, some of whom undertook their own comparisons, such as Fritz Kneubühl at the Swiss Federal Institute of Technology.[43] Kneubühl's conclusion that the Fourier community had made exaggerated claims were countered by their criticisms of his expertise in operating the new technology.

But even direct comparison could prove remarkably unpersuasive. In 1966 Janine and Pierre Connes demonstrated remarkable spectra of planets demonstrably better than prior results which, they admitted, had been inferior or at best comparable to those obtained using conventional instruments.[44] One sympathetic commentator wrote that the results were 'far superior to that attained by conventional means, and all doubts about the importance of Fourier spectroscopy should be laid to rest by this work'.[45] Such evidence, deemed 'conclusive' by proponents, was nevertheless ignored by many spectroscopists still mistrustful of the indirectness of the technique. The opponents disputed the very definition of 'evidence', some of them arguing that the claimed improvement of spectral resolution was in fact due to instrumental artefacts. Gebbie later reported that he, too, had long worried over unexplained spectral features in his first spectrum of the atmosphere, suppressing the data while trying to rule out the possibility of some unexplained instrumental effect.[46] Thus the influence of the public demonstration of the technique was tempered by a seeming morass of tacit considerations.

Experimental evidence arguably weaves a convincing tapestry only with the supporting thread of theoretical justification. Here too, however, obtaining consensus on what constituted adequate and convincing evidence was difficult.

Janine Connes published her PhD thesis in 1961. This was, for several years, the most careful and complete analysis of Fourier spectroscopy in print. Published in French, it was translated by the AFCRL for its own use.[47] Rather than attracting praise by advocates, however, it was simply ignored by critics of the technique. Those that did take note called attention to another worrying 'artefact': the appearance of a transformed spectrum could be altered dramatically by the kind of 'apodisation', or mathematical fine-tuning, employed with the Fourier transformation. The Fourierists countered that such 'filtering' was an inevitable feature of any optical instrument, but their opponents, largely unversed in, and mistrustful of, such mathematical niceties, rejected such 'sophisticated' arguments.[48]

A thematic issue of *Applied Optics* in 1969 rehearsed the disputes concerning the superiority of conventional versus Fourier spectroscopy. Jacquinot, no longer active in the subject and cast as impartial arbiter, strove to 'objectively survey both methods (i.e. interferometry and grating spectroscopy)'.[49] Gebbie recast the mathematics to highlight similarities with conventional spectroscopy, but still failed to convince skeptics, who now cited experimental evidence to back up their claims.[50] Critics and proponents, experimentalists and theorists, were speaking largely incomprehensible languages.

Part of the problem was that there was no truly 'neutral' venue for discussing claims. The publication of results was thus of mixed benefit to the early Fourier community. On the one hand, it communicated the members' work to a broader audience and promoted the technology. On the other, it provoked attacks of the still-contentious results.

For at least a decade, too, publications were incoherent and sparse. Fellgett's thesis remained unpublished, and most of his papers were difficult to obtain. After Fellgett's 1952

Optical Society abstract, the next publication was a brief note by John Strong in 1954. He reached a much wider audience with a lengthy description of the technique as an appendix to a popular optics textbook in 1958.[51]

As a means of articulating group interests, such publications were unsuccessful. The community was small and already better served by direct communication through meetings or correspondence. Moreover, its members found their interests poorly served by existing journals. A major vehicle for papers on spectroscopic instrumentation had, since 1919, been the *Journal of the Optical Society of America* (*JOSA*). The Fourier community, apart from John Strong's group, found the *JOSA* editor, Wallace Brode, reticent to publish papers on the technology. Mertz, for example, whose papers were repeatedly rejected, subsequently employed the tactic of submitting a series of advertisements to *JOSA* consisting of technical abstracts.[52] As a result, he said, 'there were no hassles with referees, they were inexpensive. . . and publication was swift'.[53] Such non-traditional beacons of communication may revert to historical invisibility, however: advertisements are frequently stripped from archived journals. *JOSA* was not unique in opposing publication. Other optics journals, too, were unenthusiastic about a technology that relied so intimately on mechanical design, electronic components and mathematical manipulation. The Mertz episode has nevertheless been credited with fostering, in 1960, the launch of a new OSA journal, *Applied Optics*. Significantly, the editor was employed at AFCRL.[54] This proved a more welcoming repository for papers on the subject as did, from 1961, the new British journal *Infrared Physics*. Thus, the 1969 'debate' was conducted on the home ground of the Fourierists under a sympathetic editor who had already published a plethora of papers on the technique. On the other hand, traditional designers' periodicals such as the British *Journal of Scientific Instruments* and the American *Review of Scientific Instruments* were infrequent vehicles for publication.[55]

Books, which necessarily presented more accomplished results, also were insignificant for the first decade and were devoted to the technology only from about 1970.[56] Those books that did appear, such as Strong's *Fundamentals of Optics* (1959) and Mertz's *Transformations in Optics* (1966), referred to Fourier spectroscopy as a side topic.

The seminal Bellevue conference has been mentioned above. Other important stages for uniting the Fourier community were a conference at Orsay, France, in 1966, again organised by the CNRS, and at Aspen, USA in 1970. At Orsay 23 papers on Fourier spectroscopy were presented, including the widely cited Connes' work on planetary spectra.[57]

Aspen, four years later, was sponsored by the AFCRL and was the first full conference on the technique.[58] If Bellevue had given a sense of collectivity to the workers, Aspen provided evidence of a burgeoning community having autonomy over an instrumental technology. Attendance swelled to over 400. Members of the community achieved a transient visibility: photographs of prominent participants were published over the legend 'some pioneers of Fourier transform spectroscopy'.[59] A sense of collective history also developed. Twelve years later, a session of another conference at Durham, England was devoted to historical reminiscences.[60]

Such conferences were successful largely because they preached to the converted, attracting audiences already disposed towards the technique or in fact already studying or using it. Joint conferences, sponsored by optical societies and others, were considerably less successful during this period for precisely the same reason. Such conferences, such as the annual Optical Society of America meetings, brought together experts on conventional techniques including a large number involved with applying conventional technology. This was the worst of all possible worlds: young Fourier advocates, employing relatively exotic but unexplored devices, faced older practitioners who had decades earlier moved beyond 'proof of concept' to applications. A secondary problem was that such conferences, while covering a broad range of topics, still generally limited those topics to optics or spectroscopy. Their attendees had less familiarity with either electronics or computation, both of which were integral to the new technology. The reasons for the relative failure to communicate at annual conferences, then, were similar to the problems with publishing in established journals: Fourier spectroscopy was seen by organisers and a majority of participants as too far removed from the traditional audiences, and too much a 'hybrid' of disciplines to be comprehensible to them.

Yet communication at non-specialist conferences *did* finally improve from the 1970s for two reasons. First, the number of practising Fourier spectroscopists had by then risen sufficiently to support sessions devoted to the technology; the phenomenon of a 'conference within a conference' appeared. Secondly, a prominent advocate of Fourier spectroscopy papers and conferences emerged in the guise of the Society of Photo-optical Instrumentation Engineers (SPIE). The SPIE had begun in the 1940s as the 'Society of *Photographic* Instrumentation Engineers', a specialist organisation devoted mainly to the design of still and motion picture cameras. These devices involved precision mechanics, optics (although for imaging, rather than high-precision interferometric optics) and, increasingly, electrical and electronic components. The SPIE had also become a vehicle for the work of American military- and civil government-funded investigators, the very contractors that had become prominent advocates of American Fourier spectroscopy research. By the mid-1970s the SPIE had recast itself as an organisation devoted to optical engineering, which itself was understood to include expertise in mechanics, electronics and computing. On the other hand, the conferences devoted entirely to Fourier spectroscopy after the 1970 Aspen conference were populated substantially by spectroscopists and analytical chemists, not the designer-researchers of the original Fourier community.[61]

Beyond meeting at conferences, there was a very limited amount of assimilation of new ideas by the sharing of researchers and equipment, but not post-doctoral workers or technicians, between groups. Pierre Connes notes, for example, that the research students at the Laboratoire Aimé Cotton were 'sadly innocent of digital techniques' and unaware of any practical details of other research, during their first years there.[62] The Connes' sojourn at JPL, and their subsequent loan of their instrument to the University of Arizona, are exceptions to the general isolation of workers.

While a disciplinary presence of sorts thus emerged in America, academic posts for the Fourier community failed, on the whole, to materialise. Instrumentation development was seen as too application-driven to serve as a suitable subject for a permanent post. Those few who did settle in academia in the 1960s gained posts in electrical engineering departments more often than in physics departments.[63]

The situation changed in the late 1970s for two reasons. First, 'optical engineering' became an increasingly recognised academic specialism at a few centres, notably the University of Rochester (New York) and the University of Utah.[64] Second, analytical chemists were gaining interest in Fourier spectroscopy as a viable and sensitive technique.[65] Academic-based chemists such as Peter Griffiths began to make the development of new

Fourier-based measurement techniques the basis for their work. Even Griffiths, though, had had an earlier career in instrument development.[66]

Acquiring sponsors proved to be the most socially effective tactic for the these research-technologists. Early British and American companies have already been mentioned. According to one participant, British spectrometer manufacturers ‘were interested rather than wildly excited by the development because there was not much market’, and mechanical accuracy problems ‘made it look like an uneconomic proposition’.[67] The first American companies survived by development contracts for government departments intrigued by the promise of measurements in difficult circumstances: to observe aircraft, to scan terrain or to probe the atmosphere from satellites and space probes. Indeed, the region of Massachusetts surrounding the Air Force Cambridge Research Laboratory (later the Air Force Geophysical Laboratory at Bedford) and populated by its contractors has subsequently become the principal American centre of so-called ‘electro-optical’ technology. It is noteworthy that acquiring this sponsorship required the instrument to mutate, to become more ‘generic’: it had to become portable, more robust and capable of employment with different kinds of light source.[68]

As late as 1978, about one-third of commercial spectrometers were still sold to government.[69] The AFCRL was a major early patron, but was joined by other significant research and development sites in America: the Jet Propulsion Laboratory in Houston, and the NASA Goddard Space Flight Center in Maryland.[70] The Aerospace Corporation, in Los Angeles, also began developing and purchasing Fourier spectrometers in the early 1960s, maintaining informal communications with John Strong and his former students at AFCRL.[71]

As at the CNRS and NPL, groups at these American government institutions enjoyed the luxury of relatively good funding, an absence of commercial pressures and considerable freedom to pursue engineering innovations. Its designers maintained close links with industry.[72] Moreover, further funding for both the Connes and Mertz came from the American Air Force, primarily through George Vanasse and Alistair Stair, an associate.[73] This financing of fundamental research was both possible and common in the USA until the Mansfield Amendment prevented such military spending.

More companies were founded from the late 1960s, generally with Fourier spectrometer designers as instigators or consultants. Such individuals were often quite anonymous; the fledgling companies strove to market innovations and were consequently reticent to publish. Moreover, the commercialisation of Fourier spectroscopy has been marked by numerous small and often short-lived companies.[74] A small number of persons broadcast the ‘seeds’ of the technology widely. Besides Alistair Gebbie, for example, Ray Milward, a Briton who did postdoctoral research at MIT in the late 1950s and then went to the Royal Radar Establishment in England, where he borrowed Gebbie’s instruments, was a proselytiser. Milward subsequently joined the RIIC company in England, then the French company Coderg in 1973, and then another French company, Polytec, at each of which he created a product line of Fourier spectrometers.[75] He afterwards founded the British branch of an American spectrometer company (Mattson) and subsequently continued to hop between manufacturers of the instruments as a manager or consultant.[76]

Those publications that did appear fulfilled the role of publicity and marketing as much as technical content. In the company brochures, just as in the literature and conference papers, there was an awkward coexistence of publicity hype and reticence about technical details.[77] Moreover, the skills demanded of such instrument designers were unusually broad (optics, electronics, mechanics, and computing expertise). Most had been trained as physicists or electronics engineers. Owing to the small size and financial fragility of the small instrument manufacturers and the uncertainty of the market, the spectrometer designers in industry not infrequently added marketing and administrative skills to their already broad technical backgrounds.

The technology was made more marketable in the late 1960s by the publication of a much more efficient calculating algorithm (the Fast Fourier transform, or FFT), the development by Mertz and associates of a rapid-scanning form of the instrument, and the commercial availability of minicomputers. These increased the speed and economy of the instruments dramatically. Companies also provided an important vector for change. Attempting to increase their markets, spectrometer companies tried from the late 1960s to interest chemists in the technology. These attempts largely failed at the time because Fourier spectrometers were less reliable and more demanding of technical knowledge than the, by then, highly automated dispersive instruments.[78] A growing trend towards computerising measuring instruments from the 1970s, however, allowed its advocates to present Fourier spectroscopy in a better light. The technology had the disadvantage of demanding computers, but this could be portrayed as an advantage in itself: digital manipulation of data from a Fourier instrument could be much more informative than that from a dispersive machine because of the precision of the scale of wavelength – which became known as the 'Connes advantage'. What physicists had identified as the serious drawbacks of the technology (its reliance on unintuitive and expensive equipment, which necessitated waiting to obtain measurements rather than observing them in 'real time') were increasingly down-played for its putative advantages (the ability to measure more 'difficult' samples with less preparation, and to undertake more elaborate analyses of the data).[79] This recasting of the criteria of judgement by chemists substantially disregarded the criticisms of earlier critics. Not surprisingly, they renamed their appropriated and remoulded technology ('FTIR', for Fourier transform infra-red). It is significant, too, that these instruments became more generic – more versatile – when applied to the wide variety of new applications that chemists, and their commercial markets, provided.[80] The instruments were taken up in routine testing labs and then the shop floor in the process industries. The instruments only became 'black-boxed' in this way in the early 1980s when the technology had been rendered reliable and automated, and when adequately powerful microcomputers rendered the technology less expensive and thus competitive. Chemists, inexperienced in such instrumentation, neither wanted nor needed to take on the peculiar culture of the research technologists: it was now embedded in their instruments.

The enrolment of support from this new community was not universally applauded, however. Connes later complained that 'if you are an instrument builder, your viewpoint differs greatly from that of the person who buys a ready-made interferometer, and I personally have some doubts about Fourier spectrometers being used properly even when producing indisputably fine results'.[81] An American contemporary emphasised the distinction and desire for continued independence between communities:

> the ideal operation of an FT-IR system should be a closed shop with one key operator. Furthermore, the key operator should be electronically oriented with a background in both machine language and high order programming. This type of key operator can easily be trained in infrared sample handling and would provide an ideal interface between the analytical chemist and the system. . .[82]

The original opponents of Fourier spectroscopy were not widely converted to the new technology; they continued to employ conventional instruments in proven applications. Eventually, however, Fourier technology and its development community became more numerous than conventional spectroscopists. This was due both to the inevitable retirement of older practitioners and to the Fourier community's success in recruiting new adherents, particularly in the form of contract sponsors and analytical spectroscopists. The old guard was merely superseded. The Fourier community itself remained stable but not sizeable: by the early 1970s it included perhaps 500 investigators, with certainly fewer than 100 making it a full-time occupation.

Fate of the community

The Fourier community thus nurtured its nascent technology from the early 1950s over two decades, before introducing an academic community, the analytical chemists, to it. In this time, the Fourier community managed not only to survive, but to find influential patrons in the American military and space programmes, at national laboratories and in industry. Many of them (e.g. Gebbie, Mertz and other less familiar names) were funded by government contract and a succession of companies.

The members of this fluid community were disciplinary hybrids who had difficulty in establishing academic homes. Their visibility to colleagues in science and engineering was mixed. Those in industry published relatively little; they skirted recognition just as the community itself fell between the domains of academic science and commercial engineering. Much of their expertise remained tacit knowledge, embodied in a handful of practitioners fertilising new companies. The sense of community was fading, too, by the mid-1970s, when the number of designers employed by commercial firms began to dominate those in relatively open and large institutions. Moreover, the 'external threat' of conventional technology was ebbing as Fourier spectroscopy became commercially established, which also diminished the strength of collective identity. While the interstitial specialism persisted and indeed prospered, its technological shepherds lost their social coherency.

Conclusion

The early survival success of the Fourier community was achieved principally through two tactics: affiliation with generous sponsors in the government and military, and by association with companies, which then largely proselytised the separate chemistry community. It is noteworthy that the early success was largely contingent on a particular political context: the uncritical cold-war funding for high-status and militarily promising projects. The community's employment of technical literature relied on new forms of journal that stressed 'applied science' or hybrid specialisms such as 'optical engineering' or 'electro-optics'. New modes of communication such as advertisements became persuasive. All these features deviate markedly from models of scientific development that emphasise the importance of university research and open publication in disciplinary journals.

What is distinct from many of the other contributions in this volume is the existence of a true *community* forged around a specific device. The members of this fragile skill collective enhanced their own chances of professional survival by cooperating professionally. They socialised at conferences, freely exchanged and debated ideas on engineering approaches, shared equipment and, most importantly, presented a united front to sway conventional spectroscopists. Their collectivism and tactics were an evolutionary necessity to enable each of them to cling to their untenured and vulnerable posts.

The Fourier community was 'emergent' in the sense of appearing only above of certain scale of activity, and not being predictable from smaller-scale events. This group became coherent only when defined by the acceptance of the technology by its sponsors and by its opposition to other groups such as conventional spectroscopists. The members were defined, and 'emerged', not only through the technology they employed, but in relation to their social alliances and oppositions. The 'old guard' is, in fact, difficult to characterise in this episode because the principal form of opposition 'they' employed was to ignore the Fourier community. Thus, the small group of advocates for the new technology became more visible than the much more numerous supporters of the conventional technologies. This is intriguing because the Fourier community continued to survive in the cultural interstices: *between* science and engineering, *between* academia and industry, *between* design and application. The incongruity of this 'holistic' subject, like other research-technology specialisms that rely upon integration of multiple disciplines, is that its specialists have remained socially dispersed through industry, government and the fringes of academia.

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Biographical description

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Notes

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[1] For a more lengthy discussion and examples of other cases of peripheral science, see Sean F. Johnston, 'Making light work: practices and practitioners of photometry', *History of Science* Vol. 34 (1996), 273-302 and Johnston, 'The construction of colorimetry by committee', *Science in Context*, Vol. (1996), 387-420.

[2] E.g. Terry Shinn, 'Crossing boundaries: the emergence of research-technology communities', in: Henry Etzkowitz and Loet A. Leydesdorff (eds.), *Universities and the Global Knowledge Economy : A Triple Helix of University-Industry-Government Relations* (London: Cassell Academic, 1997), 85-96. Other communities associated with such sciences in the twentieth century include those of customers, salespersons and publishers.

[3] Bernward Joerges and Terry Shinn, 'Research-technology: instrumentation for science, state and industry' (this volume).

[4] See, for example, the contributions by Myles Jackson and Alexandre Mallard in this volume.

[5] This account, in at least its early stages, fits well with Bruno Latour's conceptions [e.g. *Science in Action* (Cambridge: Harvard University Press, 1987), esp. 146-53] of how science gets done. As he describes, proponents enrol support by marshalling a variety of resources, both technical and social. There are, however, some distinct divergences. For example, even when the 'new science' had replaced the old, its proponents remained a shadowy, peripheral group who merely passed 'power' (that is, subsequent control of technical development and application) to analytical chemists. Similarly, while the discussion of 'communities' developed here has resonances with Kuhn, Merton and others, it can most usefully be associated with recent work on the emergence of technical professions. See, for example, Andrew D. Abbott, *The System of the Professions* (Chicago: University of Chicago Press, 1988) and Keith M. MacDonald, *The Sociology of the Professions* (London: Sage Publications, 1995).

[6] For an autobiographical account aimed at general audiences, see Albert A. Michelson, *Light Waves and Their Uses* (Chicago: University of Chicago Press, 1902).

[7] Replication of Michelson's results was difficult even for experts, and later was to dog his claims concerning the absence of an aether drift (see Lloyd S. Swenson, *Ethereal Aether: A History of the Michelson-Morley-Miller Aether-Drift Experiments, 1880-1930* (Austin: University of Texas Press, 1972). The tacit knowledge required to perform interferometry, and Michelson's own reticence to support close collaborators or graduate students (as opposed to associates from other fields), militated against its development in his hands as a viable research-technology.

[8] For more on the technology and its history, see Sean F. Johnston, *Fourier Transform Infrared: A Constantly Evolving Technology* (London: Ellis Horwood, 1991); Pierre Connes, 'Early history of Fourier transform spectroscopy', *Infrared Physics*, Vol. 24 (1984), 69-93.

[9] Peter Fellgett, 'Three concepts make a million points', *Infrared Physics*, Vol. 24 (1984), 95-8.

[10] For the fortunes of German *instrumentenkunde*, see Terry Shinn, 'The research-technology matrix: German origins, 1870-1900' (this volume). On the other hand, such an emphasis was sufficiently unusual even in 1960s Germany that another Fourier pioneer, Günther R. Laukien, a co-founder of the successful Bruker Instrument company (specialising in Fourier and NMR spectrometers from the 1970s) was praised by his company upon his death for having 'recognised the need to apply scientific knowledge to commercial products in order to foster research and development', a 'highly unusual' idea 'only achievable through confrontation and reluctance within the academic community.' [*Spectroscopy Europe*, Vol. 9 (1997), 6].

[11] Michael J. G. Cattermole and A. F. Wolf, *Horace Darwin's Shop: A History of the Cambridge Scientific Instrument Company 1978-1968* (Bristol: Hilger, 1987).

[12] An extreme case is chemical engineering, arguably the most 'academicised' engineering occupation in Britain. By the end of the second world war, there were departments in only two British universities, both with technological connections (London and Glasgow). About half of the qualified practitioners held degrees, mostly in chemistry or mechanical engineering. Departments and degree-holders increased rapidly after the war, a department and professorship being created at Cambridge, for example.

[13] Peter Fellgett, personal communication, Jan 18-30, 1991.

[14] The purpose of encoding the signal was to create the largest possible signal so that it was stronger than the relatively constant electrical noise, thus improving sensitivity. See Peter Fellgett, *Theory of Infra-Red*

Sensitivities and its Application to Investigations of Stellar Radiation in the Near Infra-Red, (unpublished PhD Thesis, Univ. Cambridge, UK, 1951). Another encoding scheme, with a less articulated theoretical justification, was Marcel J. E. Golay, 'Multislit spectrometry and its application to the panoramic display of infrared spectra', *Journal of the Optical Society of America*, Vol. 41 (1951), 468-72.

[15] John Strong, 'Fourier transform spectroscopy reminiscences', *Infrared Physics*.Vol. 24 (1984), 103.

[16] George A. Vanasse, 'Infrared spectrometry', *Applied Optics*, Vol. 21 (1982), 189-95.

[17] Peter Fellgett, 'Multi-channel spectrometry', *Journal of the Optical Society of America*, Vol. 42 (1952), 872.

[18] L. B. Scott and R. M. Scott, 'A new arrangement for an interferometer', *Perkin-Elmer Corporation Engineering Report No. 246*, May 22, 1953. The design they later patented was not commercialised.

[19] H. Alistair Gebbie, 'Fourier transform spectroscopy – recollections of the period 1955-1960', *Infrared Physics, Vol. 24* (1984), 105-9.

[20] Pierre Jacquinot, 'How the search for a throughput advantage led to Fourier transform spectroscopy', *Infrared Physics*, Vol. 24 (1984), 99-101.

[21] Pierre Jacquinot, '‘The luminosity of spectrometers with prisms, gratings or Fabry-Pérot etalons', *Journal of the Optical Society of America*, Vol. 44 (1954), 761.

[22] Pierre Connes, 'Pierre Jacquinot and the beginnings of Fourier transform spectrometry', *Journal de Physique II*, Vol. 2 (1992), 565-71.

[23] Lawrence Mertz, 'Fourier spectroscopy, past, present, and future', *Applied Optics*, Vol. 10 (1971), 386-89.

[24] Fellgett, note 13.

[25] Hajime Sakai, unpublished notes, Dept. of Physics and Astronomy, Univ. of Massachusetts at Amherst, 1991.

[26] For the conference proceedings, see 'Les progrès recents en spectroscopie interférentielle', *Journal de Physique et Radium*, Vol. 19 (1958), 185.

[27] See Pierre Connes, 'Fourier transform spectrometry at the Laboratoire Aimé Cotton 1964-1974', *Spectrochimica Acta*, Vol. 51A (1995), 1097-104. Jacquinot subsequently moved to another division of the CNRS.

[28] Terry Shinn, 'The Bellevue grand électroaimant, 1900-1940: birth of a research-technology community', *Historical Studies in the Physical and Biological Sciences*, Vol. 24 (1993), 157-87.

[29] See, for example, a special issue of *Spectrochimica Acta*, Vol. 51A (1995) on 'High resolution Fourier transform spectrometry in France'. The authors, several former members of the LAC, are affiliated with the Institut d’Astrophysique de Paris, CNRS; Laboratoire de Physique Moléculaire et Applications, Université de Paris Sud; Groupe de Spectromètrie Moléculaire et Instrumentation Laser, CNRS. Connes himself left the Laboratoire Aimé Cotton for the Service d’Aeronomie of the CNRS in 1974. Note, however, that these sometimes academic off-shoots were concerned primarily with *application* of the technology rather than its development as such.

[30] Sir Howard Grubb-Parsons & Co. c1960-62; Research and Industrial Instrument Company (RIIC) c1963-8; Lloyd Instruments c1983-8. Gebbie also approached manufacturers such as Perkin-Elmer, where he was informed that ‘no-one will ever depend on computers to get their spectra’ [H. Alistair Gebbie, personal communication, 21 Nov 1990]. Grubb-Parsons and RIIC ceased trading in the late 1970s; Perkin-Elmer began manufacturing Fourier spectrometers then, and by the 1990s was the chief commercial producer.

[31] L. C. Block and A. S. Zachor, 'Inflight satellite measurements of infrared spectral radiance of the earth', *Applied Optics*, Vol. 3 (1964), 209. Note that Mertz’s instrument – a simple fist-sized device – was very dissimilar to the Connes’ large and multiply-compensated instrument intended for telescopic use, but incorporated similar underlying principles.

[32] The more demanding, poorer-funded exceptions were Grubb-Parsons and RIIC in England. Their first commercial products were, however, acceptable to the small number of versatile physicists purchasing them, who were able and willing to engage in their own ‘instrument science’ activity.

[33] Bruno Latour, op. cit. note 5; Peter Galison, *How Experiments End* (Chicago: Chicago University Press, 1987).

[34] Fellgett, note 13.

[35] Mertz received a PhD some eight years later: 'eventually, after Pierre Connes so convincingly demonstrated the capability of FTIR, Harvard conceded that I might receive a PhD for my part' [personal communications, 29 Jan and 19 Feb 1991].

[36] See Vanasse, op. cit. note 16, 190-1.

[37] George Vanasse, Ernest Loewenstein and Hajime Sakai, by 1963.

[38] Strong, op. cit. note 15.

[39] Franz D. Kahn, 'The signal: noise ratio of a suggested spectral analyzer', *Astrophysical Journal*, Vol. 129 (1959), 518.

[40] Gerard Kuiper, *Communications of the Lunar & Planetary Laboratory of the University of Arizona*, Vol. 1 (1962), 83.

[41] Mertz,op. cit. note 35.

[42] See Connes, op. cit. note 27, 1102.

[43] See, for example, Fritz K. Kneubühl, J.-F. Moser and H. Steffan, 'High-resolution grating spectrometer for the far infrared', *Journal of the Optical Society of America*, Vol. 56 (1966), 760-4.

[44] Janine Connes and Pierre Connes, 'Near-infrared planetary spectra by Fourier spectroscopy. I Instruments and results', *Journal of the Optical Society of America*, Vol. 56 (1966), 896-910.

[45] Ernest V. Loewenstein, 'The history and current status of Fourier transform spectroscopy', *Applied Optics*, Vol. 5 (1966), 845-54. Loewenstein later worked in ophthalmic optics.

[46] Gebbie, op. cit. note 19.

[47] Janine Connes, 'Recherches sur la spectroscopie par transformation de Fourier', *Revue d'Optique*, Vol. 40 (1961), 45, 116, 171, 231, translated by C. A. Flanagan as AD 409 869 (Defense Documentation Center, 1963).

[48] The central issue was the subjective interpretation and judgement of distortion of the spectrum that was caused by applying a Fourier transform to a limited quantity of data instead of an infinite range of frequencies. Proponents 'solved' the problem by applying various versions of mathematical smoothing, or apodisation (literally 'foot removal'). Their opponents argued that such 'artificial' and 'cosmetic' manipulation was disturbingly sensitive, and consequently labelled the entire technology as *ad hoc* and scientifically unsound.

[49] Pierre Jacquinot, 'Interferometry and grating spectroscopy: an introductory survey', *Applied Optics*, Vol. 8 (1969), 497-9.

[50] H. Alistair Gebbie, 'Fourier transform versus grating spectroscopy', *Applied Optics*, Vol. 8 (1969), 501-4.

[51] John Strong, 'Interferometric modulator', *Journal of the Optical Society of America*, Vol. 44 (1954), 352 (A); George A. Vanasse and John Strong, 'Applications of Fourier transformation in optics: Interferometric spectroscopy', in John Strong (ed.), *Concepts of Classical Optics* (San Francisco: W. H. Freeman, 1958), Appendix F.

[52] E.g. *Journal of the Optical Society of America*, Vols. 50 (1960), (3) and 51 (1961) (8).

[53] Mertz, op. cit. note 35.

[54] See, for example, John N. Howard, editor of *Applied Optics*, quoted in R. A. Hanel, 'International conference on Fourier spectroscopy, Aspen, 16-20 March 1970', *Applied Optics*, Vol. 9 (1970), 2212-5.

[55] These journals both stressed non-mathematical, non-precision metrologies. The papers that were accepted, and their readership, emphasised relatively unsophisticated mathematical analysis.

[56] This first wave of books, notably George W. Chantry, *Submillimetre Spectroscopy*, (London: Academic Press, 1971) and Robert J. Bell, *Introductory Fourier Transform Spectroscopy* (New York: Academic Press, 1972), came from physicists, and stressed applications in far infrared and solid state research.

[57] For conference proceedings see 'Méthodes nouvelles de spectroscopie instrumentale', *Journal de Physique*, Vol. 28, suppl. C2 (1967).

[58] See *AFCRL Special Report 114* (Jan. 1971). At least seven AFCRL designer/researchers were there, several of them former students of John Strong.

[59] *Applied Optics*, Vol. 11 (1972), 1673.

[60] Published in *Infrared Physics*, Vol. 24 (1983).

[61] These conferences were at Columbia, SC (1977 and 1981); Durham, England (1983); Ottawa, Canada (1985) and Vienna (1987).

[62] Connes, op. cit. note 22.

[63] H. Alistair Gebbie, at Imperial College, and Günther R. Laukien, at Karlsruhe, were both trained in physics and became professors of electrical engineering. Similarly, until the early 1970s, most practitioners of the subject had backgrounds either in physics or electrical engineering.

[64] Rochester had long associations with optics through its links with Eastman Kodak. The Utah connection grew from its links with planetary astronomy. On the other hand, technological universities such as the Massachusetts Instiute of Technology and the California Institute of Technology developed no particular connection with Fourier spectroscopy.

[65] See, for example, Arthur Finch et al., *Chemical Applications of Far Infrared Spectroscopy* (New York: Academic Press, 1970).

[66] Griffiths worked as a designer at Digilab, a successor to Block Engineering, in the early 1970s.

[67] Terry Threlfall, personal communication, 25 Sep 1990.

[68] The physical transformation of Fourier spectrometers, based on local experience and particular applications, was profound. At the NPL, smooth operation relied on precision-ground mechanical guides; at Block Engineering, the small and simple instruments were scanned using converted loudspeaker voice coils; at the LAC, complex optical and servo-mechanically controlled devices evolved. By the late 1970s, however, all such instruments incorporated a laser as a precise reference of wavelength, and employed the fast Fourier transform (FFT, first publicised in 1965 by a mathematician but exploited by the Fourier community a year later), which speeded computation by orders of magnitude.

[69] S. T. Dunn, 'Fourier transform infrared spectrometers: their recent history, current status, and commercial future', *Applied Optics*, Vol. 17 (1978), 1367-73.

[70] GSFC developed Fourier spectrometers that flew on Nimbus satellites and the Voyager and Mariner spacecraft, and JPL applied the technology to atmospheric remote sensing.

[71] Charles M. Randall, personal communication, 22 Apr 1991. The Aerospace Corporation was founded in 1960 specifically as a contractor for the Air Force.

[72] Companies such as Block Engineering (founded 1960), Idealab (c1962), Midac and Bomem (1973) were maintained by such contracts in their early years.

[73] Mertz, op. cit. note 35.

[74] Johnston, op. cit. note 8, Chap. 15. The companies evolved rapidly in an increasingly competitive market. Some seeded new companies or product lines (e.g. Digilab (later Bio-Rad), and Nicolet from Block Engineering; Mattson Instruments from Nicolet); others merged with larger firms (RIIC with Beckman; Analect with Laser Precision Analytical; Bomem with Hartmann & Braun and then Elsag Bailey; Mattson with Philips); still others failed or left the market (Beckman; Coderg; IBM Instruments; Lloyd Instruments; Spectrotherm). During the 1960s, a good sales volume was of the order of two dozen instruments per year. By the mid 1980s, this had risen to several hundred per year for the largest companies.

[75] All three firms ceased manufacturing Fourier spectrometers by the mid 1970s.

[76] Ray C. Milward, personal communication 15 Nov. 1990. For one of his more widespread designs, see 'A small lamellar grating interferometer for the very far-infrared', *Infrared Physics*, Vol. 9 (1969), 59-74.

[77] This was, in fact, a replay of the public demonstration/tacit knowledge conundrum that had mired the first presentations of ‘convincing evidence’.

[78] Through the 1960s, physicists were the primary purchasers of Fourier spectrometers, which were applied to problems where the intensity of optical radiation was impracticably low for conventional instruments. Investigations included far infrared spectroscopy, atmospheric studies such as airglow and auroral emission, telescope-based observations of stars and planets, and studies of very weak optical absorption such as by impurities in semiconductors or dilute liquids. Somewhat later, the inherent sensitivity was used for measurement of short-lived phenomena such as transient chemical species and magnetically-confined plasmas.

[79] Chemists and industry were attracted to the opportunities for energy-wasting optical sampling techniques provided by this efficient form of spectroscopy. Techniques such as diffuse reflectance, gas chromatography/infrared (GC/IR) and Fourier transform-Raman spectroscopy were made practicable by Fourier techniques, and considerably extended the precision, speed and versatility of analytical chemistry.

[80] The evolution of Fourier spectrometers along a distinctly nonlinear path as they were appropriated, refashioned and extended by different groups, supports what Michel Callon and others have termed a 'model of translation'. See, e.g., Michael Callon and John Law, 'On interests and their transformation: enrolment and counter-enrolment', *Social Studies of Science*, Vol. 12 (1982), 615-26; Latour, op. cit. note 5, 133-44.

[81] Pierre Connes, 'Of Fourier, Pasteur, and sundry others', *Applied Optics*, Vol. 17 (1978), 1318-21.

[82] Dunn, op. cit. note 69.