Knowing Cases: Biomedicine in Edinburgh, 1887—1920
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ABSTRACT This paper examines the scientific work of the Laboratory of the Royal College of Physicians of Edinburgh from its foundation in 1887 to 1920. It looks in particular at the pivotal role of clinical cases in the work of the Laboratory, using the concept of ‘triangulation’ to analyse how cases served both as objects of scientific knowledge and as sites for articulating and aligning the concerns of medical practitioners and career scientists. It goes on to propose a general model for thinking about the role of cases in scientific knowledge production, based on a rereading of Kuhn as seen through the lens of the sociology of scientific knowledge. It concludes with some general reflections on how this analysis of the work of the Laboratory helps us to rethink the relations between basic and applied medical science in the period before the emergence of modern biomedicine.

Keywords cases, Edinburgh, laboratories, medicine, sociology of scientific knowledge, triangulation

Knowing Cases:

Biomedicine in Edinburgh, 1887–1920

Steve Sturdy

Shortly after noon on 18 April 1908, a 33-year old man was brought to the Royal Infirmary of Edinburgh with severe neurological symptoms. He was admitted to one of the medical wards run by Alexander Bruce, physician to the Infirmary and one of Scotland’s most respected consulting neurologists (Woodhead, 1912). Bruce conducted a thorough physical examination of the patient, as well as a number of tests in the small ‘side room’ laboratory attached to his ward. These included chemical tests for albumin and other substances in the urine, and a lumbar puncture that revealed the presence of blood in the cerebro-spinal fluid. On the basis of these clinical observations, Bruce concluded that the patient was suffering from a ruptured cerebral aneurysm (Bruce et al., 1908).

Contrary to the normal course of events in such cases, however, the patient made a partial recovery and survived for a further 10 days before finally relapsing and dying. This led Bruce to comment that, seen in its ‘clinical aspect’, the case was ‘of unusual interest in several respects’. In particular, ‘The duration of life after the onset was very remarkable’ (Bruce et al., 1908: 459–60). Given these unusual features, Bruce therefore requested that an autopsy be performed by the hospital pathologist. Naked-eye examination of the brain...
again confirmed the diagnosis of a ruptured cerebral aneurysm. But the investigations did not end there. Following the autopsy, Bruce took the dissected brain to another laboratory, run by the Royal College of Physicians of Edinburgh, where facilities were available for more sophisticated biological and medical investigations. Working with two younger colleagues, he subjected the brain to detailed histological examination, concentrating on the microscopic appearance of the wall of the cerebral artery and on the distribution of blood clots in the surrounding tissues. On the basis of their observations, backed up by additional reading in the medical literature, Bruce and his collaborators were able to infer a series of vascular and cerebral events that in turn could be correlated with the different clinical stages of the patient’s illness.

Initially, they declared, a localized weakness of the artery wall had led to the formation of an aneurysm that had partially ruptured, spilling blood into the brain tissues and ventricles and raising the pressure of fluid within the brain. The aneurysm had then stabilized, while the brain had begun to adapt to the rise in pressure, enabling the patient to recover some of his damaged functions. However, the recovery had been short-lived. The artery wall was too weak to remain stable for long, and had ruptured again, this time catastrophically, resulting in the patient’s death.

The anomalous aspects of the case were thus explained. Seen in its ‘clinical aspect’, the case was highly unusual. But the various pathological and physiological events that were held to account for the clinical phenomena – including the ruptured aneurysm, and the ability of the brain to adapt to the rise in intra-cranial pressure – were no different from what might be seen in other cases of brain injury. Biologically speaking there was nothing peculiar going on. The clinical peculiarities of the case were simply a consequence of a chance combination of otherwise ordinary pathological phenomena.

The role of the Laboratory of the Royal College of Physicians of Edinburgh in investigating this and other clinical cases is of interest for the light it throws on the relationship between laboratory science and clinical medicine in the years around the turn of the 20th century. Those years witnessed a rapid expansion in the number and variety of laboratories devoted to medical investigation of one sort and another, in Britain as elsewhere in Europe and North America. Historians of the medical sciences have done much to document such developments. However, much of their work tends to assume, implicitly if not always intentionally, that the growth of the laboratory sciences at this time brought with it a deepening division – social, technical and epistemic – between medical science and medical practice.

Thus historians of medical science have been particularly interested in the emergence of ‘the laboratory’ (frequently reified as such, despite growing awareness of the diversity of laboratories and practices at that time) as a physical and social space of scientific knowledge production quite distinct from the practically oriented activities of clinical medicine, and characterized by its own disciplinary institutions and academic career structures, its own epistemic goals and criteria of knowledge production, and its own
bodies of theoretical and technical practice.\textsuperscript{1} Insofar as laboratory science and clinical practice are seen to have interacted with one another, moreover, this is commonly conceived in terms of a more-or-less clear-cut division of epistemic labour, mediated and maintained by processes of exchange: practical problems encountered in the clinic provided a spur to technical and scientific innovation in the laboratory, which in turn yielded new knowledge and new technical services that could then be put to use in the work of clinical problem-solving.\textsuperscript{2} Such a view, it might be noted, implicitly reproduces the distinction, commonly articulated at the time by laboratory scientists themselves, between the production of scientific knowledge in the laboratory and its application in spheres of practical endeavour such as the clinic (cf. Kline, 1995).

This view of the relationship between the laboratory sciences and clinical medicine during the late 19th and early 20th centuries is in marked contrast to the way that historians and sociologists have tended to think about the work of medical laboratories in the later 20th century. Analysts of post-war ‘biomedicine’ are particularly interested in the large-scale, complex and often relatively seamless alignment of laboratory-based and clinical activities that has come to characterize much modern medical practice (for example, Löwy, 1996; Gaudillière, 2002; Keating & Cambrosio, 2003). They also emphasize how this has transformed medical knowledge production more generally, to the extent that it is now often impossible to distinguish between the work of solving concrete problems arising in the course of clinical practice and the generation of more general biological knowledge, or between ‘routine’ diagnostic testing and innovative biological research. Some of these historians and sociologists, moreover, are inclined to suppose that this situation is peculiar to the post-war period, and is consequent on the increasingly close alignment of the hitherto separate spheres of laboratory research and clinical medicine that occurred at that time. That being the case, they argue that the term ‘biomedicine’ cannot legitimately be applied to the alliance of medical science and medical practice before World War II (for example, Keating & Cambrosio 2003: 49–50, 330–32).

The Laboratory of the Royal College of Physicians of Edinburgh provides an opportunity to reassess this claim. As we shall see, the Laboratory was notable both for the diversity of practitioners who worked there – including eminent local clinicians and successful career scientists – and for the range and variety of its investigative activities. These activities included detailed studies of single clinical cases such as the one described above, but also more routine forms of diagnostic testing on the one hand, and more obviously biological research into physiological and pathological processes on the other. As in late 20th-century ‘biomedicine’, much of the work conducted in the College Laboratory defies any attempt to demarcate between clinical puzzle-solving and the production of more generalized forms of biological knowledge: in many instances the same investigations could contribute both to the diagnosis or elucidation of particular cases and to the production of more general biological knowledge. As we shall see,
clinical cases – actual instances of disease encountered in the course of clinical practice – were a particularly important site around which these different epistemic activities, and the different medical and scientific interests they served, were aligned and articulated.

The present paper accordingly looks in some detail at the way that clinical cases were investigated and understood in the College Laboratory. It draws in particular on the exemplary work of Susan Leigh Star (1989) and L. Stephen Jacyna (2000), both of whom have looked at the construction of scientific knowledge of clinical cases around the turn of the 20th century – both, coincidentally, focusing on the field of neurology – and both of whom show how clinical and laboratory observations were combined to produce knowledge that simultaneously served to explain the case at hand and contributed to the development of more general theories of biological function. Star, in particular, coined the term ‘triangulation’ to describe the work of combining clinical with laboratory observations (Star, 1989: 96–117). The present paper follows Star in employing the concept of triangulation to analyse the work of the College Laboratory, and especially the role of clinical cases in that work. It shows how the concept of triangulation can help us not only to understand how scientific knowledge of individual cases was constructed, but also to explain the reciprocal connections between clinical puzzle-solving and the generation of more general knowledge of the biology of disease, and between routine diagnostic testing and more ‘basic’ research. Throughout, I argue that clinical cases provided a primary point of reference around which these diverse investigative activities, and the different professional groups and practical interests with which they were associated, were articulated.

Additionally, the present paper goes on to assimilate Star’s concept of triangulation to a more generalized account of scientific knowledge production that gives due weight to the role of cases in the generation of new scientific knowledge, and that enables us to rethink the relationship between the production and the application of such knowledge. The account in question derives from a sociologically informed reading of the work of Thomas Kuhn, according to which scientific knowledge production proceeds on a case-by-case basis, using previously solved puzzles as exemplars for the solution of new puzzles. Seen in this light, the diverse investigative activities undertaken in the College Laboratory can all be regarded as different instantiations of the same basic processes of knowledge production, while distinctions between routine testing and innovative research, and between laboratory science and its clinical applications, appear not as fundamental epistemological divisions, but as the results of pragmatic, situated and ultimately revisable judgements regarding the epistemic value attributable to any given scientific puzzle-solution. Such a conclusion is consonant with, and helps to enrich, other recent work in science and technology studies that argues that the demarcation of basic from applied science is a rhetorical achievement rather than an epistemological distinction (for example, Calvert, 2006).
Applied to the work of the College Laboratory, this perspective serves both to problematize the prevailing view of the relationship between laboratory science and clinical medicine in the late 19th and early 20th century, and to offer a solution to that problematization. In particular, it makes clear that clinical as well as laboratory investigations could contribute to the work of scientific knowledge production, in ways that had much more in common with later 20th-century biomedicine than some analysts have been prepared to acknowledge. Beyond this specific historiographical focus, it may also provide a way of rethinking the role of cases in the production of scientific knowledge more generally, and indeed in other fields of intellectual endeavour including the law and the social sciences (cf. Forrester, 1996). Exploration of these wider implications lies beyond the scope of the present paper, however, which is concerned with the more specific task of analysing how clinical cases figured in the work of the College Laboratory. In order to set the scene for this analysis, it is necessary first to provide some context, in the form of a brief account of the origins and organization of the Laboratory and the variety of scientific investigations undertaken there.

The College Laboratory in Context

The Laboratory of the Royal College of Physicians of Edinburgh was very much a medical institution. Established in 1887 by a vote of the elite medical practitioners who made up the Fellowship of the College, the Laboratory was intended to provide research facilities and diagnostic services for the Fellows themselves, and for the local medical community more generally. Fellows and Members of the College, and subsequently also of the sister Royal College of Surgeons of Edinburgh, were given free access to the Laboratory and its facilities to undertake any investigations they wished to pursue, while others not affiliated to either of the Colleges could apply for permission to work there (Ritchie, 1953: 5–10, 29). It quickly established itself as one of the most active and productive centres of medical research in the Scottish capital, and would remain so for at least the next 35 years. In addition, the Laboratory also offered routine diagnostic testing services, and was soon conducting large numbers of histological and bacteriological tests for medical practitioners from around Scotland. In view of the ambivalence that clinicians of that period often expressed towards the growth of the new laboratory sciences, it is worth saying a little about the motivation behind this initiative, and the way that it developed thereafter.

Many among the Edinburgh clinical elite were keen to associate themselves with the new sciences, and to be seen to contribute to the advancement of those sciences. This attitude had its roots in the distinctive medical economy of the Scottish capital. Compared with London, Edinburgh was a small town with a limited supply of wealthy private patients. Consequently, the Edinburgh medical elite relied far more heavily than their metropolitan counterparts on teaching – including clinical teaching on the wards of the...
Royal Infirmary, but also lecture courses offered through the so-called ‘extra-mural’ school of medicine that ran alongside the University medical school – as a source of both income and reputation. By the same token, with only one major teaching hospital, Edinburgh offered students far less opportunity to gain practical hands-on experience of medicine and surgery than was available in London. That the Scottish capital was nonetheless able to compete effectively for students with London and latterly with the rapidly expanding English provincial medical schools was in large part due to its long-standing reputation for providing a distinctly scientific introduction to medical practice (Jacyna, 1995; Lawrence, 2006). The Edinburgh medical elite actively cultivated this reputation, not least through their own frequent contributions to the medical literature. The establishment of the College Laboratory was intended to further this aim by furnishing practitioners with access to the most up-to-date facilities for prosecuting a scientific approach to medical research and practice.

Edinburgh doctors were quick to make use of these new facilities. Though committed primarily to careers in medical practice, many were nonetheless prepared to devote a significant part of their time to research, particularly but not solely in the early stages of their careers. From the early years of the Laboratory, more than 30 individuals per year worked in the Laboratory on a part-time or occasional basis, growing to almost 50 per year in the first decade of the 20th century. Some of these would occupy positions of eminence within the world of Edinburgh medicine, including several who became professors of medicine and surgery in the University, and three presidents of the Royal College of Physicians of Edinburgh. The conduct of research in the College Laboratory was thus at least compatible with, and more probably helped to secure, advancement within the ranks of the local medical profession.

The facilities of the College Laboratory were not confined solely to the advancement of careers in medical practice, however. Work undertaken there could also be used to advance other professional roles and identities, most notably in academic medical science. From the beginning, the College employed a full-time ‘scientific man’ as Superintendent of the Laboratory, and this scientific staff was later enlarged with the appointment of a bacteriologist in 1909 and a histologist 1913 (Ritchie, 1953: 8, 50, 53). According to their terms of employment, these full-time scientists were in effect College servants, employed to see to the day-to-day running of the Laboratory, and ‘to assist, if required to do so, in the work of other investigators’ – meaning, principally, the clinicians who worked there part-time (Ritchie, 1953: 8). The full-time staff was also responsible for performing and reporting on the growing numbers of routine tests on pathological specimens that the Laboratory provided free-of-charge for medical practitioners from Edinburgh and beyond. In key respects, then, within the College Laboratory, full-time scientists were cast in a service role, supporting the work of the clinicians who employed them.

In addition, however, the Superintendents and the more junior scientific staff were also expected to ‘undertake the prosecution of Original
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Research’, albeit ‘under supervision’ by a committee of Fellows set up specifically to oversee the work of the Laboratory (Ritchie, 1953: 8). In the event, the scientists were permitted to develop their research in ways that were compatible with their own ambitions to pursue careers not just as scientific service workers but as members of an emerging community of primarily academic medical scientists. The first two Superintendents were local men just starting out in careers as teachers and researchers, but the work they conducted in the College Laboratory did much to establish them on the road to academic eminence. German Sims Woodhead, who served as Superintendent from 1887 to 1890, would later occupy the Chair of Pathology at the University of Cambridge (Ritchie et al., 1922); while his successor, Diarmid Noël Paton, would leave the College Laboratory in 1906 to become Regius Professor of Physiology at Glasgow (Cathcart, 1928; Smith & Nicolson, 1989). Under Woodhead and Paton, moreover, the College Laboratory acquired a reputation for research that rivalled anything achieved by the Scottish universities.7 Following Paton’s departure, the College was able to recruit a well-established academic medical scientist – James Ritchie, Professor of Pathology at the University of Oxford – to replace him (Muir, 1923). Meanwhile, a number of more junior men who conducted research in the College Laboratory would also go on to pursue careers as full-time medical scientists in academic and other institutions.8 The scientific culture of the Laboratory was thus plainly compatible, not just with the concerns and interests of medical practitioners, but also with the advancement of medical science as a distinct profession.

The research conducted in the College Laboratory addressed much the same spread of scientific and medical concerns as was apparent in the career paths of those who undertook that research. At least half of the publications to appear from the Laboratory in the years to 1920 met the standards required for inclusion in journals such as the Journal of Physiology and the Journal of Pathology and Bacteriology, which served primarily as outlets for the kind of work that characterized the new academic science disciplines. In keeping with the overall content of these journals, the Edinburgh contributions were commonly based on animal experiments, facilities for which were included in the Laboratory from the start (Woodhead, 1889; Paton, 1897), or on in vitro bacteriological and immunological studies. The remaining publications appeared in a variety of national and local journals for medical practitioners. Such publications tended to report investigations prompted by puzzles arising in the course of clinical practice, and included studies of individual cases of illness as well as other kinds of research into human and animal diseases.9

In view of the different career tracks pursued in the College Laboratory, it is not surprising to find that the aspiring scientists were most likely to publish in the more academic outlets, while medical practitioners aimed to publish their more practical investigations in the medical journals. But the divergence was by no means absolute. On the contrary, there was a striking degree of crossover between the two occupational groupings and the kinds of work they undertook. Thus while the majority of the animal experiments
published in the *Journal of Physiology* were performed by members of the full-time science staff, aspiring clinicians also undertook such work, usually in collaboration with the full-timers but sometimes as sole authors in their own right. By the same token, the full-time staff frequently assisted or collaborated with their clinician colleagues in the more obviously practical or clinical investigations for the medical press. Moreover, as we shall see when we look more closely at some of the research on which these publications were based, it is not actually possible clearly to demarcate publications in more academic scientific journals from papers in more obviously medical periodicals. Rather, what we find is a continuum of styles of scientific research in which animal and in vitro research overlapped with work on human diseases, while a single series of closely related investigations could span a range of styles and result in publications in academic and medical periodicals. Nor, it might be added, can we even draw a principled distinction between research concerned with generating novel scientific knowledge or insights, and routine testing procedures that merely applied the fruits of previous research. Even the most routine forms of diagnostic testing could sometimes provide the basis for genuinely new knowledge of disease, as we shall see. The College Laboratory is thus notable for the sheer eclecticism of the scientific work undertaken there, and the range of interests it addressed: from the ‘basic’ research agendas defined by academic scientists, through the more clinical concerns of elite practitioners, to the immediate practical needs of ordinary general practitioners. Equally striking is the lack of demarcation between these different kinds of work, which are better seen merely as different facets of a single if diversified scientific enterprise. Motivated by a common commitment to advancing the scientific approach to medicine with which they associated Edinburgh’s success as a centre of medical education, clinicians and scientists shared a mutual respect for the different skills and diverse lines of investigation they brought to the Laboratory, collaborating in each other’s investigative endeavours and sustaining a genuine dialogue between more academic programmes of research and more clinical forms of medical puzzle-solving. The study of clinical cases, in particular, provided an important focus for such collaboration and dialogue. I therefore now turn to a closer examination of how clinical cases were investigated and understood within the larger context of the activities of the College Laboratory.

**Triangulating ‘Rare and Interesting Cases’**

One place where cases loomed large in the work of the College Laboratory was in published reports of what a later Superintendent (Ritchie, 1953: 33) called ‘rare or interesting cases’ – single human cases of disease, encountered in the course of medical practice, that differed in unusual or puzzling ways from the kinds of cases that normally passed through the clinic. The case of ruptured cerebral aneurysm with which I opened this paper was just such a case, and it is instructive to look a bit more closely at exactly what role the College Laboratory played in elucidating it.
First, it is important to recognize that a considerable amount was already known about the case before it was ever brought to the laboratory for investigation. On admission to the hospital, the attending physician subjected the patient to the usual procedures of clinical assessment, including a full case history and a thorough physical examination. Additional tests, including a lumbar puncture, served to extend the range of observations beyond what was possible using only the unaided senses. This was an implicitly comparative procedure: the signs and symptoms revealed by the examination were compared to those that were considered to characterize particular categories of disease. In order to facilitate such comparison, clinical examiners typically followed a more or less standardized format that ensured that the same basic information was collected in all cases, while allowing for more open-ended explorations whenever that information proved in some way to be unusual or anomalous. All this served to confirm the clinical diagnosis of a ruptured cerebral aneurysm. It was only the prolonged survival of the patient after admission that differed significantly from what would otherwise have been regarded as a typical case. That departure from the norm was not sufficient to challenge the original diagnosis, but it was enough to make the case one of ‘unusual interest’. In this instance, then, the interest of the case derived, at least initially, from the way that it was perceived and understood in its ‘clinical aspect’. The published report of the case accordingly began with a full account of the case history – including a quite detailed narrative of the circumstances surrounding the patient’s initial collapse – and of the clinical examination, following much the same format as was used for recording case notes in the clinic itself, in order to ensure that other practitioners were able to judge that interest for themselves (Bruce et al., 1908: 449–53).

Second, what the Laboratory investigations added to consideration of the case was an additional, biological frame of reference within which different aspects of the case, inaccessible to ordinary clinical observation, could be perceived, evaluated and judged to be normal or anomalous. This too was a matter of seeing the case in comparison with other cases. But the kinds of comparison that were now possible were greatly extended by the technologies available in the Laboratory. Microscopic features of the case could be brought into view, and compared with what had been seen, not only in other aneurysm cases, but also in other kinds of brain injuries. Thus, consideration of the biological processes revealed by laboratory study led Bruce and his colleagues to search the neurological literature for cases of recovery from intra-ventricular haemorrhage. The cases they found were taken as evidence that the brain was physiologically capable of adapting to the increased pressure caused by internal bleeding, and that the same processes could therefore be imputed in the present case of ruptured cerebral aneurysm (Bruce et al., 1908: 459–60).

Third, the resolution of the investigations depended on the way that these two frames of reference were then combined to create a single account of the case. By retrospectively imputing chains of causal mechanisms that might account for the histological and clinical aspects of the case, the
investigators were able to argue that both sets of observations could be traced to a single sequence of biological events. In effect, the clinical peculiarities of the case could be reduced to mere instances of otherwise normal biological processes. Though necessarily hypothetical, the credibility of these claims gained much from the fact that they made possible a single coherent narrative that combined the clinical and histological observations in ways that rendered them more significant and meaningful than when treated by themselves. In her study of clinical and physiological research on the human brain around the turn of the 20th century, Susan Leigh Star employed the term ‘triangulation’ to describe this process of combining different kinds of observation of what were deemed to be the same phenomena, as a means of reducing the uncertainty that attended each set of observations when considered independently (Star, 1989: 96–117). The concept serves equally well in the present case of cerebral aneurysm.

Fourth, while this work of explanatory triangulation turned on the claim that particular clinical events were merely specific instances of more general biological phenomena or principles, it involved far more than just a series of deductive inferences. It also involved a large measure of hermeneutic reasoning and inductive generalization. It was necessary to consider the case and its comparators holistically and analogically, in order to identify meaningful dimensions of similarity and dissimilarity. Moreover, the very imputation of such meaning involved imagination; specifically, it involved the construction of a coherent narrative that tied the various facts of the case together in a web of biological cause and effect. In this regard, then, the present case of cerebral aneurysm amply bears out observations about the narrative, hermeneutic and holistic character of medical knowledge (for example, Hunter, 1991).

Fifth, the wider significance invested in this scientific narrative, including the judgement that it warranted publication in a medical journal, turned upon the way that it simultaneously demonstrated both the anomalous character of the case at hand, and the fact that that case was nonetheless just the same as other relevant cases. The balance that was struck between these dimensions of similarity and difference was central to the way the present case of aneurysm was presented and, as we shall see, it was central to how other cases came to be judged as significant. In the present narrative, it was the idiosyncratic character of the case that was placed at centre-stage, while the rest of the action revolved around it. The import of the narrative lay in the demonstration that even the most clinically anomalous of cases could ultimately be explained in biological terms, provided one had the requisite scientific skills and access to appropriate laboratory facilities. In effect, the case served to exemplify the power of the scientific physician to bring order to the chaotic world of disease. Beyond this, no further conclusions were drawn. Specifically, no attempt was made to argue that the biological elucidation of this particular case had any practical implications for the way that future cases of illness should be diagnosed or treated.

That was certainly not the only use to which narratives of the investigation of ‘rare or interesting cases’ could be put, however. Such
investigations could also be used to generate more general knowledge of the disease categories to which those cases were assigned, as we can see if we consider some of the other case reports that issued from the College Laboratory. Take, for instance, the case of a child who was admitted to hospital with severe neurological symptoms. In this case, the clinical diagnosis itself was uncertain. Though the symptoms were consistent with an acute infection of the middle ear, clinical examination failed to elicit the more usual signs of such a condition. Bacteriological tests carried out shortly after admission also yielded results suggestive of infection, but in the absence of the usual clinical signs this diagnosis remained only tentative. As in the case of aneurysm, explanation of the clinical phenomena had to wait until after the child’s death, when autopsy followed by further bacteriological and histological investigations in the College Laboratory confirmed the existence of a middle ear infection. Again, triangulation between laboratory and clinic served to elucidate the clinical puzzle posed by the individual case, but in this instance it was also seen as leading to more general conclusions. The case was deemed ‘worthy of record, because it shows that a child may suffer from acute middle ear suppuration, acute purulent labyrinthitis and leptomenigitis without having any discharge from the ear’ (Fraser, 1914: 417). In other words, the constellation of clinical signs and symptoms that was generally considered indicative of middle ear infection now needed to be modified to allow for the occurrence of cases such as the one just investigated.

This conclusion was in turn reinforced by a further act of triangulation – not, in this instance, by correlating clinical with laboratory observations of a particular case, but rather by correlating clinical observations made in a number of different cases. In the course of writing up his account of the otitis case, Fraser conducted a review of the clinical literature that turned up a number of other cases with what he considered to be similar histories. He took this to indicate that the same pathological phenomena occurred in all of these cases, and reiterated his conclusion that neurological symptoms could ‘occasionally occur in acute suppurative otitis media before rupture of the tympanic membrane’ – though the present case had the distinction of being ‘the first in which these conditions have been proved to be present by microscopic examination of the ear’ (Fraser, 1914: 424).

Again, the conclusions to be drawn from this case turned upon a judgement of relevant dimensions of difference and similarity between the case at hand and other cases. As in the case of cerebral aneurysm discussed above, the case of otitis derived much of its interest from the fact that it differed in significant ways from other more typical otitis cases. In this instance, however, the aim of the case narrative was not simply to show how clinical idiosyncrasy could be explained in terms of underlying biological processes. In addition, the author also chose to identify new ways in which the case could be seen to resemble other cases. In effect, comparison with other cases provided an opportunity to reconfigure the network of similarities and differences that constituted the diagnostic categories to which cases were to be allocated.
In the case of otitis just considered, this reconfiguration of disease categories referred primarily to how ailments should be diagnosed and categorized in the clinic. But investigation of ‘rare or interesting cases’ could also have implications for the way that diseases were defined in the laboratory. This is apparent, for instance, in the case of a patient who died in hospital after suffering for several years from spastic paraplegia. This clinical condition was widely considered to be pathognomonic of a degenerative disease of the central nervous system called disseminated sclerosis. But, though the patient ‘was again and again examined in the hope of detecting some of the characteristic symptoms of disseminated sclerosis’, those symptoms could not be elicited. Again, resolution of this anomaly had to wait until after the patient’s death from an unrelated ailment, when autopsy followed by histological examination in the College Laboratory uncovered ‘the characteristic lesions in the spinal cord of disseminated sclerosis’ (Bramwell & Dawson, 1916: 412). The absence of the expected symptoms was explained in this case by the fact that the distribution of the lesions within the central nervous system differed somewhat from what was considered to be the usual pattern in disseminated sclerosis. Consequently, while from the clinical perspective the case was ‘one of quite exceptional interest and rarity’, from the pathological point of view it was simply ‘a typical one of disseminated sclerosis’ (Bramwell & Dawson, 1916: 415).

There was rather more at stake in this statement than immediately meets the eye, however. In fact, the existence of disseminated sclerosis as a single neurological category was the subject of much debate at the time this case was investigated. Clinical uncertainty was compounded by disputes over whether the variable distribution and appearance of the pathological lesions revealed by autopsy should be taken as indicating one or a number of disease entities. The very possibility of talking about the ‘characteristic lesions’ of disseminated sclerosis was thus open to doubt. However, doctors and pathologists connected to the College Laboratory took the view that these uncertainties could be resolved by histological examination of the brain and spinal cord. To that end, they had conducted detailed histological studies of a series of cases that were seen to be typical of disseminated sclerosis to a greater or lesser extent. As a result of such studies, they had proposed an alternative set of criteria, based on the identification of particular patterns of cellular degeneration, for defining disseminated sclerosis in the laboratory (Dawson, 1916). In effect, they had formulated a new view of what should be seen as the ‘characteristic lesions’ of the disease, which in turn enabled them to argue that, despite its diverse clinical and pathological manifestations, disseminated sclerosis was indeed a single disease, with a unique aetiology and a distinctive histological appearance. This work is now widely regarded as a key step in the characterization of what subsequently came to be called ‘multiple sclerosis’ (Murray, 2005: 189–94).

The particular case of spastic paraplegia discussed above appears to have been singled out for publication in its own right because it was seen to provide a particularly strong argument in favour of adopting this new histological definition of disseminated sclerosis. From the clinical point of
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view, the case could be seen as especially problematic, because it called into question the association between the diagnostic category of disseminated sclerosis and what had hitherto been seen as the most reliable clinical signs of that condition. By demonstrating that the case exhibited what they now proposed should be seen as the typical histological appearance of disseminated sclerosis, the Edinburgh pathologists were able to reassert the validity of that association, thereby preserving the pathognomonic status of spastic paraplegia. In so doing, they also provided good practical reasons for why clinicians should accept the Edinburgh pathologists’ claims to have elucidated the histological characteristics of disseminated sclerosis. In this instance, then, triangulating between the clinic and the laboratory effectively provided a means of consolidating knowledge and practice in the laboratory as much as in the clinic.

Routine Testing and ‘Basic’ Research

It was not only ‘rare or interesting cases’ that could be mobilized to this effect. Other kinds of cases also figured in the Laboratory’s work of knowledge production. This is apparent, for instance, if we examine some of the research into tuberculosis that was undertaken there. By the early 1910s, evidence was beginning to mount up, in Britain and elsewhere, that humans might be susceptible to infection by the bovine as well as the human form of the tubercle bacillus. However, the number of individuals positively identified as suffering from the bovine form of the disease remained small, and there was little agreement over the implications of bovine infection for public health. Prompted by worryingly high levels of tuberculosis in Edinburgh, clinicians and pathologists working in the College Laboratory began investigating whether locally occurring cases of the disease could be attributed to the bovine form of the bacterium.

One line of research involved examining tuberculous lymph glands taken from children admitted for treatment at a local hospital (Mitchell, 1914a,b).16 From a clinical point of view, these cases were only of limited interest. They had all been diagnosed as more or less typical cases of tuberculosis, and had been admitted to hospital for routine excision of their diseased neck glands. Otherwise, they were all clinically unexceptional, with little to distinguish one from another. When their excised glands were subjected to a series of bacteriological tests in the College Laboratory, however, more than half the cases yielded evidence of infection with the bovine form of tuberculosis. The investigator – A. Philp Mitchell, a local clinician – concluded that bovine tuberculosis was a far more prevalent cause of disease among Edinburgh schoolchildren than had hitherto been realized. He also correlated his bacteriological findings with the answers to a series of questions that had been inserted into the clinical examination expressly for the purposes of his present research. Insofar as doctors suspected that bovine tuberculosis might be an epidemiologically significant cause of infection, they were inclined to suppose that it came from drinking milk from infected cattle. Consequently, when the cases selected for investigation were examined in the clinic, they
were asked where the family obtained milk and about any history of tuberculosis among other family members. By triangulating these findings against the results of bacteriological examination of the excised glands, Mitchell was able to argue that bovine infection did indeed appear more likely to result from milk than from transmission within the family.

Further research provided additional points of reference – both clinical and laboratory-based – from which to consolidate claims about the nature of childhood tuberculosis in Edinburgh. Thus, bacteriological investigation of samples of milk obtained from local dairies indicated that many were contaminated with the tubercle bacillus (Mitchell, 1914c). Finally, histological and bacteriological study of tonsils removed from tuberculous and non-tuberculous children admitted to hospital for routine tonsilectomy indicated that those with bovine tubercular infection of the neck glands also commonly showed evidence of primary infection in the tonsils (Mitchell, 1916–17). Taken together, these findings provided the material from which Mitchell was able to construct an account of how the bovine bacillus typically invaded the body: passing from contaminated milk into the tonsils where it established a primary and usually non-symptomatic site of infection, before progressing into the lymph glands in the neck and elsewhere.

Once again, the interpretation and presentation of these findings involved identifying significant similarities and differences between clinical cases, which in turn involved constructing a coherent etiological narrative that helped to define what should actually be regarded as significant. In this instance, however, this hermeneutic work was clearly not directed to highlighting any idiosyncratic aspects of the individual cases investigated, which were simply presented as typical cases of either tuberculosis or hypertrophied tonsils. Thus, when writing up the cases for publication, Mitchell saw little need to provide more than the most cursory clinical information about each case, simply listing the diagnosis, along with the age, previous health and duration of present illness in each case. Rather, he focused on showing how the systematic employment of certain laboratory tests made it possible to reorganize these cases into two quite new groups: one characterized by infection with bovine tubercle bacillus of probably alimentary origin, and the other by the absence of such infection. As a result, hitherto routine cases now acquired new and much greater interest – albeit collectively rather than individually – not just for clinicians and pathologists but also for public health doctors and legislators.

Not only routine forms of clinical practice, but also routine laboratory procedures, could provide material for scientific knowledge production. As we have seen, a significant proportion of the resources of the College Laboratory were devoted to preparing routine reports on pathological samples sent in by general practitioners. The cases from which such samples were taken were usually only of limited and local clinical interest, and the same was generally true of the results of the tests conducted on those samples. Nonetheless, even such routine laboratory work could sometimes be used as a basis for drawing conclusions of interest to a wider audience. Two examples might briefly be mentioned.
The first follows directly from the work just discussed. General practitioners commonly sent sputum samples from suspected cases of tuberculosis to the Laboratory for routine diagnostic testing. These samples provided material from which researchers were able to conduct further investigations into the incidence of bovine tubercle infection in sections of the population not covered in the previous work (Wang, 1916a,b,c, 1917).17

The second was based on a large number of moles and melanomas sent in to the Laboratory over a number of years for histological examination and assessment of their likely malignancy. These provided the material from which to construct a comprehensive taxonomic and aetiological account of this class of affections (Dawson, 1925). This work demonstrates with particular clarity the ongoing nature of the ordering and reordering of the diagnostic categories employed in the clinic and the laboratory. In this latter instance especially, it is difficult to draw any clear demarcation between the work of diagnosing clinical cases and the construction and reconstruction of the diagnostic categories to which those cases were themselves assigned. It would seem that the ‘interdependence of reporting and research’ noted by a later Superintendent (Ritchie, 1953: 14) was more than just a rhetorical device to justify the pursuit of scientifically unrewarding service work; in this area of the Laboratory’s activities at least, routine reporting and pathological research were indeed two sides of the same coin.

Finally, it is worth asking how all this work on clinical cases related to aspects of the Laboratory’s research that concentrated on different kinds of empirical phenomena, particularly experimental work on animals or in vitro. Much of that research was clearly devoted to addressing agendas and questions defined by academic scientists working in the new laboratory disciplines. Moreover, the published accounts of that research commonly made no obvious reference to concerns of immediate clinical relevance. Is it therefore possible to draw any connections between this kind of research, as undertaken in the College Laboratory, and the constellation of more obviously clinically oriented and case-based activities that I have recounted above? At least in some instances it is.

In the course of his research into the physiology of protein metabolism, for instance, the Laboratory Superintendent, Diairn Noël Paton, developed surgical techniques that enabled him to remove rabbits’ spleens without otherwise seriously harming the animals (Paton, 1900). His work caught the attention of a small group of local clinicians who were interested in the role of the spleen, not in metabolism, but in relation to the constitution of the blood. Paton and his clinician colleagues consequently embarked on a series of experimental studies of the effects of splenectomy on the blood of rabbits. Their findings were largely negative. There was little indication that the spleen had much to do with the constitution of the blood. At the most, it might act as a site for storing iron liberated during the destruction of effete blood cells. Other than this, however, its functions remained obscure (Paton et al., 1902; Paton & Goodall, 1903; Goodall et al., 1903; Goodall & Paton, 1905).18
Conducted on rabbits and published in the *Journal of Physiology*, there was little in the accounts of these experimental investigations to indicate that they had any bearing on questions of clinical importance. At first sight, their aims were purely abstract, concerned with elucidating the biological functions of certain vital organs. Indeed, they were accepted as such by other physiologists, who welcomed their findings as a contribution to their own disciplinary research programmes. Looked at in context, however, this work was plainly also motivated by more practical concerns. While the function of the spleen remained physiologically uncertain, it was widely suspected that it played a vital role either in the generation or the destruction of red blood cells. Consequently, many doctors were inclined to suppose that certain kinds of anaemia might be attributable to dysfunction of the spleen. These included some surgeons, who argued that pernicious and other anaemias were a consequence of excessive haemolysis in the spleen, and could therefore be treated by splenectomy (Wailoo, 1997: 46–72). Edinburgh physicians were generally sceptical of such theories, however, and favoured the view that at least some forms of anaemia could be attributed in whole or in part to defective diet or excessive menstruation; indeed, some had already had recourse to the College Laboratory as a means of corroborating this view (for example, Stockman, 1895a,b, 1896).  

Studies of splenectomized rabbits served further to confirm the Edinburgh view by calling into question the supposition that the spleen played a significant role in haemolysis and hence in anaemia. Indeed, experiments to test the effects of splenectomy on animals in which anaemia had been produced by feeding them iron-deficient diets indicated that if anything the operation merely exacerbated their condition.  

Meanwhile, in parallel with these animal experiments, Paton’s clinical collaborators also used the facilities of the College Laboratory to investigate a series of clinically and pathologically typical cases of pernicious anaemia in patients who had died in the Royal Infirmary. After removing ‘all the more important organs’ from these cases at post-mortem examination, they examined the organs in the Laboratory for histological evidence that they were involved in the destruction of blood cells. Their findings indicated that haemoloytic processes were equally distributed throughout all the organs studied, and were not localized specifically in the spleen. They concluded that there was no evidence that pernicious anaemia could be attributed to excessive haemolysis in the spleen, or indeed in any particular organ. They reinforced this conclusion by citing their own work with splenectomized rabbits, and specifically the failure of that work to produce any evidence that the spleen was implicated in the ‘normal haemopoietic processes’ (Gulland & Goodall, 1905: 140). Again, this was an act of triangulation, through which the same physiological functions – normal haemopoiesis, and the neutrality of the spleen as regards the constitution of the blood – were invoked to explain and thereby consolidate phenomena observed in human anaemia cases and experimental rabbits. By removing rabbits’ spleens, Paton and his colleagues had effectively created animal models, not of anaemia as such, but rather of what anaemia was not. As a
result, the network of similarities and differences between cases that constituted diagnostic categories was effectively extended to include not just naturally occurring human cases of disease, but also artificial cases of surgically modified experimental animals.

Seen in this light, it is evident that the more discipline-oriented research undertaken in the College Laboratory did not ignore more clinically relevant work, and vice versa. On the contrary, the various investigative activities accommodated within the Laboratory proceeded in constant dialogue and interaction with one another, from routine diagnostic work at one end of the spectrum to the most arcane of animal experiments at the other. Indeed, it is impossible to draw any clear lines of demarcation between these different kinds of work; rather, there existed a continuum of scientific activities, employing a diversity of investigative techniques and methodologies that generated knowledge of interest to a variety of scientific and medical constituencies. If there was one thing that served to bind together these eclectic and promiscuous approaches to medical science, however, it was a common interest in clinical cases as objects of scientific knowledge. Clinical cases figured in many of the investigative activities undertaken in the Laboratory, while the great bulk of its epistemic output featured cases, seen either individually or classified into particular disease categories. Even when other kinds of research – for instance on experimental animals – were pursued without express reference to clinical matters, practitioners were always alert to how that work could be brought to bear on the way that clinical cases were examined and understood. Knowledge of cases was at the heart of the collaborative enterprise that was the College Laboratory, and provided a common focus for the diverse social and scientific interests that came together there.

Knowing Cases

Thus far, I have looked in some detail at the ways in which clinical cases were constituted as objects of scientific knowledge within the College Laboratory, and at how the elucidation of cases articulated with routine forms of diagnostic testing on the one hand and with basic scientific research on the other. It is worth thinking further about the implications of this analysis, however. As John Forrester has recently argued, the role of cases in the construction – as opposed to the practical application of scientific knowledge – remains notably under-appreciated, not least because of a lingering tendency to suppose that scientific knowledge properly consists of abstract, deductive and universally valid generalizations, while knowledge of cases is concrete, particularistic, holistic, inductive, evaluative and hermeneutic in form (Forrester, 1996). However, as Forrester recognizes but does not pursue in detail, this view of cases is perfectly consistent with an account of scientific knowledge production that originally derives from Thomas Kuhn’s conception of ‘normal science’ as a ‘puzzle-solving’ activity (Kuhn, 1970 [1962]: 35–42), and that Barry Barnes has subsequently reformulated within the framework of the sociology of scientific knowledge (Barnes, 1982).
Kuhn famously showed how the work of scientific knowledge production could be explicated in terms of scientists’ use of ‘paradigms’ to solve new empirical or theoretical puzzles. Paradigms, as Kuhn defined them, do not consist solely of scientific laws or other symbolic generalizations. Rather, they are made up of ‘accepted examples of actual scientific practice’ – concrete cases of successful puzzle-solving that serve to exemplify how science is actually done, including the technical, methodological and intellectual resources peculiar to a particular scientific community, as well as the practical skills and other kinds of tacit knowledge necessary to put those resources to use (Kuhn, 1970 [1962]: 10). Kuhn later coined the word ‘exemplar’ to denote specifically those ‘concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic’ (Kuhn, 1970 [1962]: 187–91; Kuhn, 1977: 298). ‘Normal science’, according to Kuhn, proceeds by seeking out further puzzles that appear likely to yield to the same resources as have proved successful in previous exemplary cases of puzzle-solving. Should such a solution be attempted, and should it be deemed successful by the relevant community of scientists, then the new puzzle-solution will be accepted as an addition to scientific knowledge. As such, it will itself come to exemplify the kinds of resources that might be mobilized in solving further scientific puzzles.

Scientific knowledge, on this view, is knowledge of cases – specifically, of exemplary cases of scientific puzzle-solving – while the generation of new scientific knowledge proceeds on a case-by-case basis, working from solved to unsolved puzzles. As Kuhn makes clear, this is essentially an open-ended and inductive process. Every new puzzle-situation inevitably differs from everything that has gone before. Consequently, the business of exemplar-based puzzle-solving cannot proceed mechanically, through the unreflective application of predetermined methodological rules. Rather, it must necessarily involve an element of judgement and discrimination: first to determine whether any particular puzzle is sufficiently similar to previously solved puzzles to yield to the same kinds of technical and intellectual approaches; and subsequently, once a solution has been proposed, to determine whether that solution resembles previous puzzle-solutions sufficiently closely to be accepted as an addition to scientific knowledge. Such judgement is not simply a matter of deductive reasoning from general principles to specific instantiations of those principles. Rather, it depends upon a holistic appraisal of the current and previous puzzle situations, on reasoning by analogy, and on the creative and imaginative construal of relevant dimensions of similarity between them (Kuhn, 1970 [1962]: 189–90; Barnes, 1982: 45–53, 70–83; Nickles, 2003).

By the same token, the grouping of cases into more general categories of knowledge – including for instance disease categories, but also biological processes and scientific laws – is equally a matter of inductive judgement rather than deductive reasoning. Since scientific knowledge is knowledge of cases, the content or meaning of any scientific generalization is no more than the finite set of cases that are taken to be instances or exemplifications of that generalization. Here too, decisions about what are to count as instances of
more general categories depend not upon any predetermined relationship between those instances or between instances and the generalizations they are held to exemplify, but upon the holistic evaluation of relevant dimensions of similarity and difference between cases. Scientific categories are thus matters of family resemblance, not logical identity. Moreover, it is a corollary of this ‘finitist’ view of scientific generalizations that the meaning of any category, and the family resemblances that characterize it, may alter as new instances of that category are identified and elected (Barnes, 1982: 27–30; Barnes et al., 1996: 53–73). ‘Normal’ exemplar-based scientific puzzle-solving thus has the capacity to generate novelty at the more general conceptual level as well as at the level of individual puzzle-solutions.

This view of scientific knowledge production as exemplar-based puzzle-solving fits remarkably well with what we have seen taking place in the investigation of clinical cases in the Laboratory of the Royal College of Physicians of Edinburgh – including both relatively routine and more obviously ‘rare or interesting’ cases. Implicitly or explicitly, such investigation always involved an element of comparison, as puzzling cases were weighed against previously accepted examples, both typical and atypical, of particular pathological categories. It was also holistic and multidimensional, based on assessment both of the clinical presentation of the case and of the phenomena that could be elicited from it in the laboratory. Seen from a Kuhnian point of view, this is exactly how normal scientific puzzle-solving proceeds.

Of course, that is not to say that all the scientific puzzle-solutions generated in the College Laboratory were seen to be equally important, or of equal interest to the different scientific and clinical constituencies that revolved around the Laboratory itself. While some were judged worthy of publication in academic science journals, others were published in outlets chiefly aimed at medical practitioners, while yet others – the results of routine diagnostic investigation, for instance – were not usually considered worthy of publication at all, but only of distribution to those practitioners who had a direct medical interest in a particular case. However, we should not suppose that the different evaluations that practitioners placed on these different kinds of scientific work reflect an underlying epistemological distinction between, say, the production and the application of scientific knowledge. The Kuhnian view of science as exemplar-based puzzle-solving supports a rather different interpretation.

So far, I have stressed the conservative side of Kuhn’s view of science: if a new puzzle-solution is to be accepted as successful, it must be judged to be sufficiently similar, in scientifically salient ways, to previous ones. But equally, if a particular puzzle-solution is to be seen as adding something to scientific knowledge, and not merely as replicating a prior puzzle-solution, it must also be seen to differ in significant ways from what has gone before (Barnes, 1982: 87–90; Collins, 1975, 1985). In effect, it must add something new to the repertoire of exemplars available for use in future cases of scientific puzzle-solving. Such judgements of relevant similarity and difference are not predetermined by any inherent characteristics of the cases themselves; rather, they are informed by the particular technical, scientific and wider
Such commitments may of course vary from one community of practitioners to another, with the consequence that different groups will see different puzzle-solutions as yielding useful novelty.

Consider for instance the ‘rare or interesting cases’ brought to the Laboratory for investigation. The outcome of that investigation was commonly to show that, in terms of the phenomena rendered visible in the Laboratory, such cases were in key respects identical to other cases that had been observed previously and that were deemed to exemplify particular pathological processes or categories of disease. Cases of this kind were usually of only passing interest to laboratory scientists, since their solution was seen merely to repeat procedures and results that had already proved effective in previous cases: while they helped to solve the puzzle presented by the particular case at hand, they offered nothing new for incorporation into future pathological practices or judgements. At the same time, however, such cases were often seen to differ in their clinical presentation from other cases they seemed to resemble on pathological grounds; indeed, this was what made them puzzling in the first instance. Consequently, elucidation of such cases added something new to the matrix of clinically observable similarities and differences against which future cases could be compared and classified. In effect, these cases came to exemplify new ways in which particular categories of disease could manifest in the clinic. As such, they represented additions to the repertoire of exemplars for solving further diagnostic puzzles, and so were deemed worthy of publication in outlets aimed principally at an audience of clinical practitioners.

Interest in the elucidation of clinical cases was not necessarily confined solely to clinicians, however. As we have seen, some cases were also seen to be distinctive in their pathological as well as their clinical aspect. The pathological findings in such cases – the particular puzzle-solutions they yielded when subjected to laboratory investigation – differed from previous results in ways that were judged to have implications for future diagnostic practice in the laboratory as much as in the clinic. Reports on this kind of case might therefore warrant publication in journals aimed primarily at the relevant community of professional scientists.

Moreover, such scientific puzzle-solutions were not necessarily limited to clinical diagnosis. As we have seen, laboratory studies of clinical cases could also sometimes provide a stimulus for other kinds of scientific puzzle-solving activities, including attempts to produce analogous pathological phenomena in experimental animals. An example was the programme of work with splenectomized rabbits, which sought to produce anaemia in laboratory animals. This research effectively addressed two scientific communities. On the one hand, it provided clinicians with a way of drawing new lines of similarity and difference, not just between human cases of disease, but also between human cases and cases of biological abnormality created artificially in laboratory animals. By producing animal models for a particular kind of disease (or rather, in this instance, for an absence of a particular disease), these laboratory experiments served to redefine disease
as perceived in the clinic. On the other hand, the splenectomized animals provided laboratory scientists with new exemplars for use in advancing their own more biological lines of research; as such, these experiments also warranted publication in academic journals aimed at the community of professional medical scientists.

It thus makes little sense to try to draw a firm epistemological distinction between ‘basic’ scientific knowledge production and the ‘application’ of such knowledge to problems encountered in the clinic; depending on circumstances, solutions to clinical puzzles might be seen as serving to advance the disciplinary ambitions of career scientists as much as the practical needs of clinical practitioners. Moreover, such judgements are not fixed, but are open to revision; the interest accorded to any particular instance of clinical puzzle-solving can change as the context in which it is evaluated changed (Barnes, 1982: 87–90). Thus, for instance, the kinds of cases submitted to the College Laboratory for routine reporting were rarely judged worthy of publication, whether in a clinical or an academic scientific journal. Both clinically and in terms of the phenomena they exhibited in the laboratory, such cases were generally seen to differ little from countless previous cases, and were therefore of practical interest only to the doctors directly involved in diagnosing and treating those cases. But as we have seen, even these kinds of cases could acquire new and wider interest, for instance when the availability of new laboratory procedures or simply the accumulation of a variety of more-or-less similar cases provided an opportunity to re-evaluate and redraw the axes of similarity and difference according to which those cases had originally been classified and subdivided. Depending on how it was framed, the same investigative work could thus be construed simply as routine repetition of previous puzzle-solutions or as generating novel knowledge of interest to a much wider community of clinical and scientific practitioners. The distinction between routine diagnostic testing on the one hand, and research leading to the production of new scientific knowledge on the other, is on this view a contingent and revisable one, which depended on whether scientists and medical practitioners decided at any time to retain existing configurations of similarity and difference between cases, or to review and revise those configurations.

Conclusion

A number of general implications follow from this Kuhnian interpretation of the work of the College Laboratory. First, the elucidation of clinical cases in the Laboratory was as much a matter of ‘normal’ scientific knowledge production as was the more programmatic experimental work performed on animals and bacteria. This point is worth emphasizing in view of claims sometimes made by critics of ‘scientific medicine’ that, while scientific knowledge may help to frame or inform the way doctors think about clinical cases, knowledge of cases cannot be scientific. The particularistic, holistic and hermeneutic character of case narratives is often held up as evidence that knowledge of cases differs in form from genuinely scientific knowledge.
Indeed, Kathryn Montgomery Hunter, one of the most insightful of commentators on the narrative dimensions of medical knowledge, denies the applicability of Kuhn’s account of science to clinical cases on the grounds that the degree and variety of differences between clinical cases is too great to sustain any systematic judgements of their similarities: ‘If Kuhn’s model of “ordinary science” [sic] as working within a paradigm were applied to clinical nosology ... the result would be biennial [sic] revolution as “new” diseases are identified and old ones subdivided or combined.’ Consequently, disease classifications are based on family resemblances between cases rather than universal diagnostic criteria, such that ‘each patient, each instance of illness, is uncharted territory’ (Hunter, 1991: 18). In fact, this is perfectly consonant with the finitist reading of Kuhn’s account of normal science outlined above. Moreover, as the clinical cases discussed in this paper make clear, narrative reconstruction often played an important role in the work of triangulating clinical and laboratory observations of the cases considered above (cf. Jacyna, 2000: 167–70, 185–89), while a number of other scholars have shown that narrativity may loom equally large in other spheres of scientific knowledge-making (for example, Myers, 1990; Harré, 1991). There are therefore no grounds for claiming that knowledge of clinical cases cannot, as a matter of principle, be scientific.22

Second, the example of the College Laboratory helps to illuminate the relationship between research and more routine forms of laboratory practice such as diagnostic testing. This relationship has been discussed by a number of historians concerned in particular with the development of the biomedical sciences in the period since World War II. Exploring as they do the complex configurations of instruments, skill, knowledge and social organization that characterize modern biomedicine, these writers have tended to see the distinction between routine and innovation as a consequence, not of the kinds of technical procedures or materials with which scientists work, nor of any clear demarcation of spaces of knowledge production from those of its application, but rather of the arrangement and orientation of sociotechnical systems. Thus, for Hans-Jörg Rheinberger it depends on whether the various elements of such a system are articulated sufficiently loosely with one another to permit the appearance of epistemic novelty, in which case they constitute an ‘experimental system’; or whether instead they are so fixed and standardized that the system can only generate further instances of what is already known, in which case they constitute a technology (Rheinberger, 1997: 228–29). Peter Keating and Alberto Cambrosio prefer to think in terms of ‘biomedical platforms’ – arrangements of instruments, standards and conventionalized and regulatory practices that constitute the link between clinical practice and biological science. On their view, the innovative processes that distinguish research from the purely reproductive procedures of routine testing are a consequence of the economy of regulatory practices that is itself a key constituent of any particular biomedical platform (Keating & Cambrosio, 2003: 328–33, 455 n. 22).
Such views are consistent with what we have seen in the work of the Laboratory of the Royal College of Physicians of Edinburgh, namely, that the distinction between routine testing and innovative research resides in how far new cases of scientific puzzle-solving are judged simply to reproduce previous puzzle-solutions, and how far they are judged to generate significant novelty. However, the example of the College Laboratory takes us back to a period before scientific medicine had achieved anything like the degree of sociotechnical complexity and large-scale integration that characterizes modern-day biomedicine, and in which experimental and other laboratory systems were much more local, transient and transparent. In this earlier setting, the sheer contingency underlying judgements of similarity and difference becomes much more apparent, as does the fact that such judgements were not predetermined by the disposition of particular experimental or other sociotechnical systems, but rather were made by practitioners whose perceptions were informed by whatever use they might make of the results of scientific investigation. Indeed, as we have seen, depending on the social location and practical interests of the persons doing the judging, the scientific solution of, say, a puzzling case of human illness could simultaneously be construed both as an instance of routine diagnostic investigation and as a novel scientific finding. In this regard, the case of the College Laboratory sustains a more sociological account of the research/routine distinction than some historians of modern biomedicine have been inclined to favour.

Third – and related – this analysis of the work of the College Laboratory also helps to throw light on the relationship between ‘basic’ and ‘applied’ research. Historians of industrial and corporate research have for some time been aware of the difficulty of drawing any hard-and-fast distinction between basic and applied research, particularly insofar as that distinction has traditionally been articulated in terms of a distinction between scientific knowledge production and its employment in solving problems arising in some other sphere of activity (see, for example, Dennis, 1987 and Mulkay, 1979). Other historians have begun to recognize that similar difficulties may arise when looking at more academic institutions, particularly where those institutions profess to produce knowledge of relevance to other kinds of practical activities (for example, Harwood, 2005). The Kuhnian view of science as a puzzle-solving enterprise developed in the present paper enables us to rethink the basic/applied distinction in ways that help to illuminate the work of these kinds of institutions. For one thing, it confirms the absence of any fundamental epistemological difference between basic and applied research, both of which involve using previously established scientific puzzle-solutions to solve novel scientific puzzles. Consequently, insofar as any distinction can be drawn between basic and applied research, it must be based on how new puzzles are selected: ‘basic’ science revolves around the solution of puzzles selected by scientists in isolation from practitioners; whereas ‘applied’ science addresses puzzles arising in other fields of endeavour. In other words, the basic/applied science
distinction needs to be seen, not as an epistemological issue, but as a pragmatic matter of work organization.23

Moreover, the work of the College Laboratory makes clear that it is perfectly possible for an institution to combine more programmatic or disciplinary forms of research in which scientists select their own puzzles, with more problem-based research prompted by puzzles arising in the course of medical practice. Nor was this simply a matter of the co-existence of two otherwise distinct and separate approaches to scientific puzzle solving. On the contrary, problem-based investigations could and did provide the starting point for more programmatic lines of research, just as the results of programmatic investigations could provide technical and intellectual resources for solving practical problems arising in the clinic. What sustained this work was, first, the shared commitment of career scientists and clinical practitioners to pursuing a distinctly scientific approach to clinical medicine; and second, their success in formulating ‘do-able’ problems the solutions to which helped to advance the practical activities and interest of both groups of practitioners (cf. Fujimura, 1987; Löwy, 1996).

In this regard, it makes little sense to try to analyse the work of the College Laboratory in terms of some balance or combination of basic and applied research. Rather, it might more usefully be seen as embodying a particular ‘epistemic culture’ (Knorr Cetina, 1999) that brought together different kinds of practitioners in pursuit of an eclectic but mutually stimulating programme of scientific investigations. Central to that epistemic culture was a common concern with the elucidation of clinical cases, which were the pivotal class of ‘epistemic objects’ around which different practitioners were able to articulate and triangulate their various skills, knowledges and practical concerns. Meanwhile, the College Laboratory itself stands as an exemplary case study of a more general reconfiguration of scientific medicine at that time, in which basic research and routine clinical testing were both implicated, and which would help to pave the way for the increasingly complex integration of technical and epistemic practices that characterize modern biomedicine.

Notes

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1. This is particularly apparent in a number of now-canonical studies of the establishment of more-or-less independent pre-clinical science disciplines – most notably, physiology

2. Thus, work in the history of medical disciplines has tended to draw on Charles Rosenberg’s (1979) concept of ‘institutional ecology’ to explain how differing medical demands, particularly for clinical ‘service work’, determined the ways that laboratory disciplines such as biochemistry and pathology developed in different local settings. See for instance Kohler (1982) and Morman (1984). A number of other studies, particularly in the history of pathology and bacteriology, have drawn attention to scientists’ own commitments to developing clinically relevant forms of knowledge and practice: for example, Amsterdamska (1987), Sturdy (1993), Prüll (1998) and Worboys (2000). Finally, a number of historians have made clear the complex intellectual and social transformations involved in generating such knowledge and techniques and putting them into practice in the clinic. See, for instance, Latour (1988), Jacyna (1988) and Amsterdamska (1998), all of which make clear not only that medical problems were redefined in their passage through the laboratory, but that medical practice had in turn to be reorganized and reoriented if the cognitive and technical products of the laboratory were to be put to effective use there.

3. In 1889 some 31 part-time researchers used the facilities of the Laboratory, of whom 11 were Fellows of the College of Physicians, two were Members and 18 had no formal connection with the College. In 1901 the total was 35, comprising 16 Fellows or Members of the College of Physicians, seven Fellows of the College of Surgeons, and 12 others. The total number of part-time workers had risen to 47 in 1906 (Ritchie, 1953: 15, 34).

4. These were: Henry Alexis Thomson (Professor of Surgery 1909–23), Francis Darby Boyd (Professor of Clinical Medicine 1919–22), David P.D. Wilkie (Professor of Surgery 1924–38), Sir John Fraser (Regius Professor of Clinical Surgery 1925–44 and Principal of the University of Edinburgh 1944–48), George Lovell Gulland (Professor of Medicine 1915–28 and College President 1923–25), W.T. Ritchie (Professor of Medicine 1928–37 and College President 1935–37), and Alexander Goodall (College President 1937–40). In addition, Ralph Stockman went on to become Professor of Materia Medica in the University of Glasgow. Gulland and Goodall’s work in the College Laboratory is discussed below.

5. In addition, a single laboratory assistant was appointed in 1887, increasing to three assistants by 1900, including one in each of the departments of bacteriology and chemistry and one serving the departments of histology and experimental physiology (Ritchie, 1953: 11–13, 31).

6. From just 50 reports issued in 1889, the work undertaken grew rapidly, exceeding 1000 per year in 1898, 2000 in 1903 and 3000 in 1907. Thereafter, the load remained roughly level until about 1913, but expanded again when the Laboratory took on additional testing for the armed forces during World War I, then for the public health authorities under the Public Health (Venereal Disease) regulations of 1916. Initially the work was mostly histological, but became increasingly dominated by bacteriological tests from the late 1890s onwards. The appointment of a bacteriologist in 1909 was intended to relieve the Superintendent of some of the burden of this work (Ritchie, 1953: 26, 61, 63–64, 154).

7. Indeed, in 1903 the Carnegie Trust for the Universities of Scotland began underwriting and contributing to the costs of the Laboratory, and by 1921 was paying over £2500 per annum, while leaving responsibility for determining what went on in the Laboratory chiefly in the hands of the College Fellows (Ritchie, 1953: 38–40, 50, 69, 95–96).

8. Four men who worked in the College Laboratory went on to hold academic chairs of pathology: Theodore Shennan at the University of Aberdeen; Stuart McDonald at the Newcastle School of Medicine, University of Durham; James Miller at Queen’s University, Kingston, Ontario; and Chung Yik Wang at the Hong Kong Medical School. In addition, J.P. McGowan spent the rest of his career at the Rowett Research Institute in Aberdeen; George Marshall Findlay went on to the laboratories of the Imperial Cancer Research Fund and later the Wellcome Bureau of Scientific Research in London; J.H. Harvey Pirie became Superintendent of the Routine Division of the
South African Institute for Medical Research in Johannesburg; and J.C. Dunlop became Medical Superintendent of Statistics at the Registrar General’s Office and subsequently Registrar General for Scotland.

9. As a rough indication of the spread of work published from the College Laboratory, I have looked at the sites of publication of all papers published between 1887 and 1920 and listed in the fairly comprehensive and reasonably accurate bibliography included in Ritchie (1953: 109–53). Of a total of 329 papers published in this period, 29 were published in the Journal of Physiology and 57, plus eight shorter reports, in the Journal of Pathology and Bacteriology. A further 16 appeared in the Quarterly Journal of Physiology, the Biochemical Journal and the Pharmacological Journal. Roughly one-third of the published output of the Laboratory thus appeared in journals run and chiefly patronized by academic scientists, and closely linked to the emergence of new science disciplines at this time. A total of 168 papers appeared in professional medical journals of national importance including the British Medical Journal, the Lancet and the Edinburgh Medical Journal, or in more ephemeral and local medical outlets including Edinburgh Hospital Reports. The remaining 51 papers appeared in journals such as the Proceedings of the Royal Society of Edinburgh, the Proceedings of the Royal Society of London and the Journal of Anatomy and Physiology, which attracted contributions both from career scientists and professional clinicians.

10. Of the 29 papers published in the Journal of Physiology between 1894 and 1906, for instance, eight were by Paton alone and a further ten were co-authored by him.

11. See, for instance, the research that Alexander Goodall and George Lovell Gulland undertook with Noël Paton, discussed below.

12. The standardization and disciplining of clinical methods during the 19th century remains under-explored from a science studies perspective (though see Lachmund, 1999). It is worth noting that Harry Rainy, one of the clinicians who conducted occasional research at the College Laboratory, was also co-author of one of the best-selling British textbooks on clinical methods of the early 20th century (Hutchison & Rainy, 1897).

13. Additionally, Stephen Jacyna (2000) has explored the literary and representational techniques whereby clinical observations and laboratory experiments on aphasic patients were woven together to generate both particular knowledge of the sites of lesions in the brains of individual patients, and more universalistic knowledge of the location of functions within the human cerebrum.

14. John Smith Fraser was an eminent ear, nose and throat surgeon who practised and taught as Assistant Surgeon to the Royal Infirmary of Edinburgh from 1906 and full Surgeon from 1921 to 1936 (A.T., 1936).

15. Byrom Bramwell, the attending physician, was another eminent Edinburgh neurologist (Ashworth, 1986). James Walker Dawson was employed as full-time histologist to the College Laboratory from 1913 until his early death from a long-standing tuberculosis infection in 1927 (C.M'N., 1927).

16. A. Philp Mitchell was a member of what might be regarded as the second rank of the Edinburgh clinical elite; he served on the surgical staff of the Leith Infirmary but was never appointed to the staff of the city’s main teaching hospital, the Royal Infirmary (Anonymous, 1959).

17. Chung Yik Wang was an Edinburgh graduate who became a full-time bacteriologist to the Laboratory in 1917; in 1920 he was appointed professor of pathology at the Hong Kong Medical School (Anonymous, 1931).

18. George Lovell Gulland and Alexander Goodall were two of the most eminent Edinburgh physicians of the first half of the 20th century, both achieving prominence in the field of haematology. At the time they undertook the research discussed here, Gulland already held the coveted post of Assistant Physician to the Royal Infirmary, and was appointed Ordinary (that is, full) Physician in 1911. He became part-time Professor of Medicine at the University of Edinburgh in 1915 and served as President of the Royal College of Physicians of Edinburgh 1923–25 (Anonymous, 1941a).
Goodall became Assistant Physician to the Infirmary in 1913, Ordinary Physician in 1928 and President of the College 1937–40 (Anonymous, 1941b).

19. Ralph Stockman was appointed part-time Professor of Materia Medica in the University of Glasgow and Physician to Glasgow Western Infirmary in 1897 (Anonymous, 1946).

20. Their view of what was typical in such cases was based on clinical studies Goodall had conducted of the symptoms, blood changes, prognosis and post-mortem findings of a number of pernicious anaemia cases in local hospitals (Goodall, 1902). The microscopic work for this latter publication was performed in the hospital ‘side rooms’, and did not warrant recourse to the more sophisticated facilities available in the College Laboratory.

21. Kuhn coined the phrase ‘disciplinary matrix’ to denote the ‘group commitments’ – including methodological and metaphysical commitments and other values – that informed how a particular paradigm community evaluated and employed their chosen exemplars (Kuhn, 1970 [1962]: 181–87).

22. This is not to deny that doctors’ interactions with their patients often involve rather more than just exemplar-based puzzle-solving. But it is to deny that the case-based character of clinical practice precludes that activity.

23. It might be noted that Kuhn himself tended to equate ‘normal’ with ‘basic’ science. Thus he took it for granted that ‘normal science’ is typically a programmatic enterprise, in which scientists concentrate their efforts around extending the empirical scope of a handful of favoured paradigms, and proceed by seeking out puzzles that appear likely to yield to the intellectual and technical resources exemplified by those paradigms (Kuhn, 1970 [1962]: ch. 5: ‘The Priority of Paradigms’). This in turn requires that scientists are free to choose for themselves what puzzles to address. Consequently, Kuhn assumed that as a science matures, it will assume the character of ‘basic’ science, which he understood to be ‘an enterprise whose practitioners have ordinarily been relatively free to choose their own problems’, as distinct from the work of ‘the inventor and applied scientist’ whose choice of problems is ‘likely to be largely determined by social, economic or military circumstances external to the sciences’ (Kuhn, 1977: 237–38). As Barnes (2003) observes, this is an implicitly functionalist view of science, in which social organization evolves to ensure the most effective fulfilment of the epistemic potential of a new paradigm. It is also a normative vision of science, that says as much about Kuhn’s own ideas about how science should be organized as about the nature of science itself.

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