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PHYSICS IN OXFORD, 1839-1939

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In 1939, Oxford's Dr Lee's professor of experimental philosophy, Frederick Lindemann, delivered a devastating retrospective judgement on the state of the laboratory – the Clarendon Laboratory – as he had inherited it twenty years before. On his arrival in Oxford, as Lindemann recalled, the reputation of the Clarendon had "sunk almost to zero". Lindemann identified his predecessor in the chair, Robert Bellamy Clifton, as the man responsible for this state of affairs. Clifton had been appointed in 1865 and had served until his retirement at the age 80 in 1915. He had arrived as one of the ablest young physicists of his generation, a distinguished product of the Cambridge Mathematical Tripos and with five years' experience as professor of natural philosophy at Owens College, Manchester (the future University of Manchester). But long before his retirement, Clifton had become a notorious problem in Oxford. He had published virtually nothing for almost forty years and had presided over a laboratory in which students specializing in physics had become ever rarer birds.

In these circumstances, Lindemann's criticism appeared all too plausible, and it would be hard to defend Clifton against the core charge that the Clarendon in 1919 was in a "moribund" state. What we *can* hope to do, however, is to understand why things looked so bleak to Lindemann. Some explanations are obvious. In the first place, Lindemann arrived after four years in which the laboratory had been without a professor, since the university had taken the perfectly reasonable decision not to postpone the filling of Clifton's post until after the end of hostilities. The Clarendon, in fact, had been almost empty since Clifton left: the war had seriously impeded such activity as there still was and had claimed all but a tiny handful of students and an even tinier skeleton staff of men too old to be drafted. The scene was perforce a dismal one. But, in the exceptional circumstances, it called for sympathetic understanding rather than dismissive denigration.

The problem for Oxford's contemporary reputation was that the state of the Clarendon on (and for some years before) Lindemann's arrival served to reinforce the already widely held belief that Oxford was a university for the arts, while Cambridge was the place for science. Although that belief did not rest on the Clarendon's poor reputation alone, the high standing of physics among the scientific disciplines of the early twentieth century meant that a weak physics laboratory carried special weight in the assessment of any university's scientific effort. Hence Oxford's standing in the sciences as a whole suffered, despite manifest strengths in other disciplines, notably in the life and earth sciences.

Should we conclude, therefore, that Oxford physics had in some way let the side down? In the collective volume, *Physics in Oxford, 1839-1939. Laboratories, Learning, and College Life* (Oxford University Press, 2005), my fellow editor Graeme Gooday and I and the four other contributors – Tony Simcock, Jack Morrell, Benoit Lelong, and Jeff Hughes - did what we could to address this question but to do so without writing an *apologia* for the discipline in Oxford. One key to the approach we took lies in the title. Our decision to write about physics *in Oxford*, rather than in the Clarendon for example, reflected a shared belief that a good deal of teaching and research in physics went on, in our period, outside the laboratory. College laboratories, in particular, were important settings, and some of them developed strong specialities: the jointly run laboratory of Trinity and Balliol Colleges, where H. G. J. Moseley worked before and soon after his graduation from Trinity in 1910, was especially distinguished as a focus for work in areas of physical chemistry that in many universities would have fallen within the domain of physics. Among other alternative settings, through until after the second world war, were the private laboratories of a number of independently wealthy men with more or less close associations with the university. Hence the setting for Oxford physics, as several contributions to the book show, was far more diverse than has commonly been supposed.

The fact remains, of course, that physics in Oxford was dispersed in its locations and fragmented in its intellectual focus. To Lindemann, who had taken his doctorate with Nernst in Berlin (in 1910) and who aspired to recreate in Oxford a single powerful laboratory on the Nernst model, such fragmentation was a mark of weakness. But was fragmentation necessarily a failing? And whether or not it was, how had it come about in a university that still enjoyed enormous prestige both within Britain and internationally?

The straightforward answer is that physics in Oxford had evolved as a discipline within the peculiar setting of an institution that was at once collegiate and, even in the late nineteenth century, still strongly coloured by the overriding objective of fashioning broadly educated young men imbued with the values of Anglican Christianity and gentlemanly refinement. Professional training had no part in this objective: future doctors, for example, might study the life sciences as undergraduates, but they would then go on to a London hospital for their clinical training. It was entirely in keeping with such a conception of education that the holder of the university's first independent, salaried post in experimental philosophy was the Revd Robert Walker, a clergyman and Oxford graduate who came to the post in 1839 after eleven years as a tutor in mathematics at Wadham College and a period (from 1826 to 1831) as college chaplain. Walker was a competent physicist who lectured across the whole range of the subject and did so attractively. Indeed, at a time when the sciences, though taught in Oxford, had no place in the undergraduate curriculum, Walker managed to secure audiences of a size that few of his peers in the Oxford scientific community were able to match. But he was above all a teacher, and (despite being elected a Fellow of the Royal Society in 1831) only secondarily a researcher.

The priorities of Walker's intellectual life are reflected in the provision for physics that was made in the new University Museum, inaugurated in 1860.

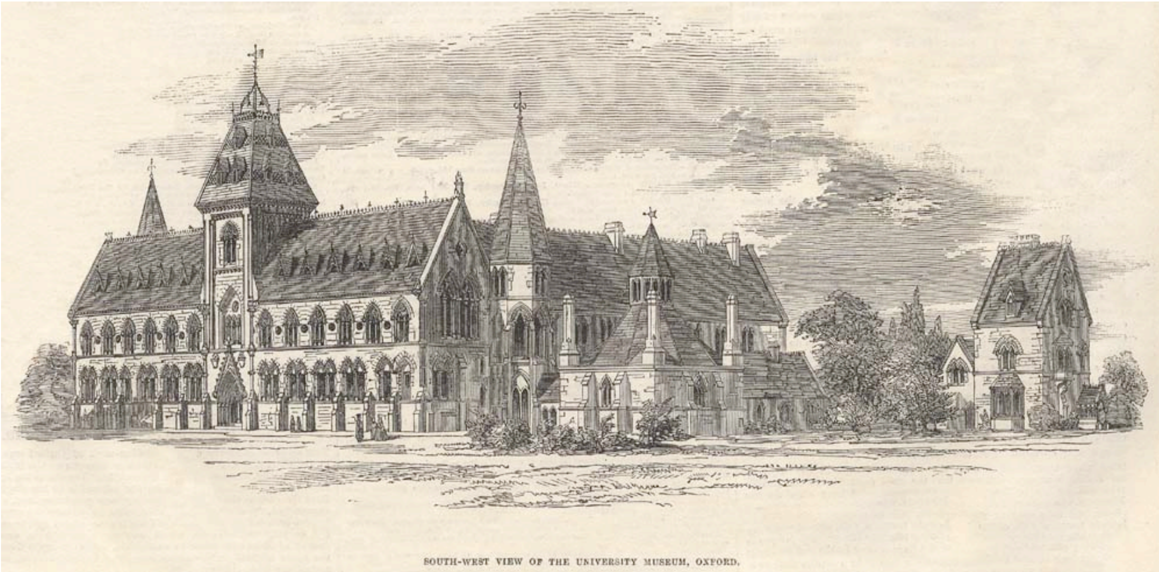

Figure 1. The University Museum, inaugurated in 1860 to house the university's collections and facilities for teaching and research. The building of the Museum was made possible by a grant of £30,000 from the university's publishing house, the Clarendon Press. By 1867, however, the total cost had risen to £87,000. This illustration, published in the year of the laying of the foundation stone, shows the building as it was originally conceived by the architects, Thomas Deane and Benjamin Woodward. From *The Illustrated London News*, 30 June 1855, 652. Private collection.

The building was a product of the university's decision of 1850 to introduce science as an examination subject. The decision conveyed Oxford's readiness for reform, albeit reform of a characteristically cautious kind. In order to graduate in the School of Natural Science, it was necessary first to pass or take honours in the classical school, Literae Humaniores. This meant spending four years on the study of the Latin and Greek language, literature, and philosophy, with strong doses of theology and mathematics, before the study of science could begin. And even then it could only begin in the context of a very general (and strikingly elementary) syllabus embracing physics, chemistry, and biology. The University Museum, which encapsulated this essentially humanistic ideal of scientific instruction, can be read as in every sense a building of its time and place. Its neo-gothic architecture responded to a wish to integrate aesthetically with Oxford's gothic past and with the ethos of college life. And the disposition of the rooms for the various scientific disciplines reflected an ideal of the unity of the sciences that found expression in the breadth of the undergraduate syllabus and in the guiding philosophy of the great pioneers of both the syllabus and the museum: Walker and, more particularly, Charles Daubeny, at various times professor of chemistry, botany, and rural economy, and Henry Acland, the Regius professor of medicine.

The core of the design was the central court, around which the various laboratories and lecture-rooms were arranged along connecting colonnades that facilitated easy contact between one science and another.

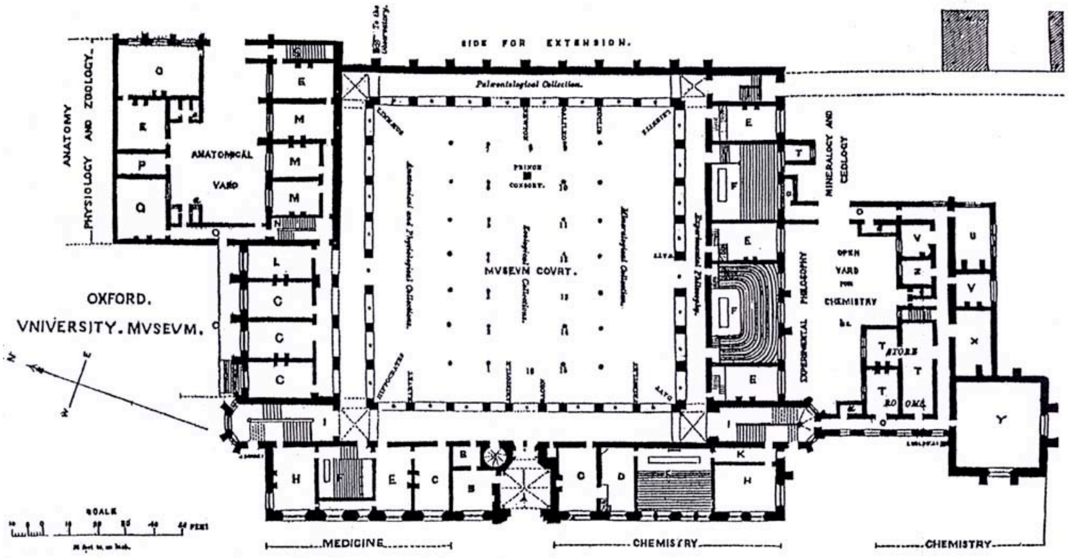

Figure 2 The ground floor of the University Museum. The core of the arrangement was the central court, covered with a glass roof, around which the facilities for the various sciences were arranged. The space allocated for physics, traditionally known in Oxford as "experimental philosophy", consisted of a lecture-theatre on the south side of the court with two rooms on its west side, one above the other and each of about 15 square metres. By the later 1860s, the two similar rooms on the other side of the lecture-theatre had also been commandeered for physics, as the floor-plan shows. From Henry Acland, *The Oxford Museum. The Substance of a Lecture*, 3rd edn (Oxford, 1866), frontispiece. Private collection.

Physics (marked as "experimental philosophy" on the ground plan) was provided for with a large lecture-theatre, accommodating about a hundred people, with two small rooms for the professor's use on the theatre's western side. Since only one of these small rooms was designated for experimental work (the other room being the professor's sitting room), the disparity between the space for lecturing and that for practical instruction and experimental research was striking. Physics, as Walker conceived it, was a subject for demonstration and oral explication, not one in which laboratory practice had any significant place.

Sadly, Walker enjoyed the facilities of the University Museum for only a short time. By the early 1860s his health was failing, and in 1865 he died. The election of Clifton in November of that year marked a passage from an older to a younger generation: Walker was in his mid-sixties at his death, while Clifton (even after five years in the Manchester chair) was still only 29. It also marked a movement to a new conception of physics that placed far greater emphasis than Walker had done on laboratory teaching. For a professor of Clifton's generation and background, the facilities of the University Museum were palpably inadequate, and he embarked immediately on the quest for funding that resulted in the construction of the purpose-built free-standing physics laboratory that came into use from 1870.

In a manner that was typical of much of the development of Oxford physics in the century preceding the second world war, chance played a large part in the decision to build the new laboratory. It so happened that by the late 1860s the trustees of a fund left to the university in 1751 by Henry, Lord Hyde, the great grandson of the first earl of Clarendon, were resolved to divest themselves of an accumulated sum of about £10,000, which corresponded exactly to the cost of the proposed building. It is inconceivable that the university would have found such a sum from its own resources, but after some competing claims on the money (for the construction of premises for examining, for example) had been dispelled, the £10,000 was allocated to the realization of Clifton's ideal laboratory, which almost immediately became known as the Clarendon Laboratory.

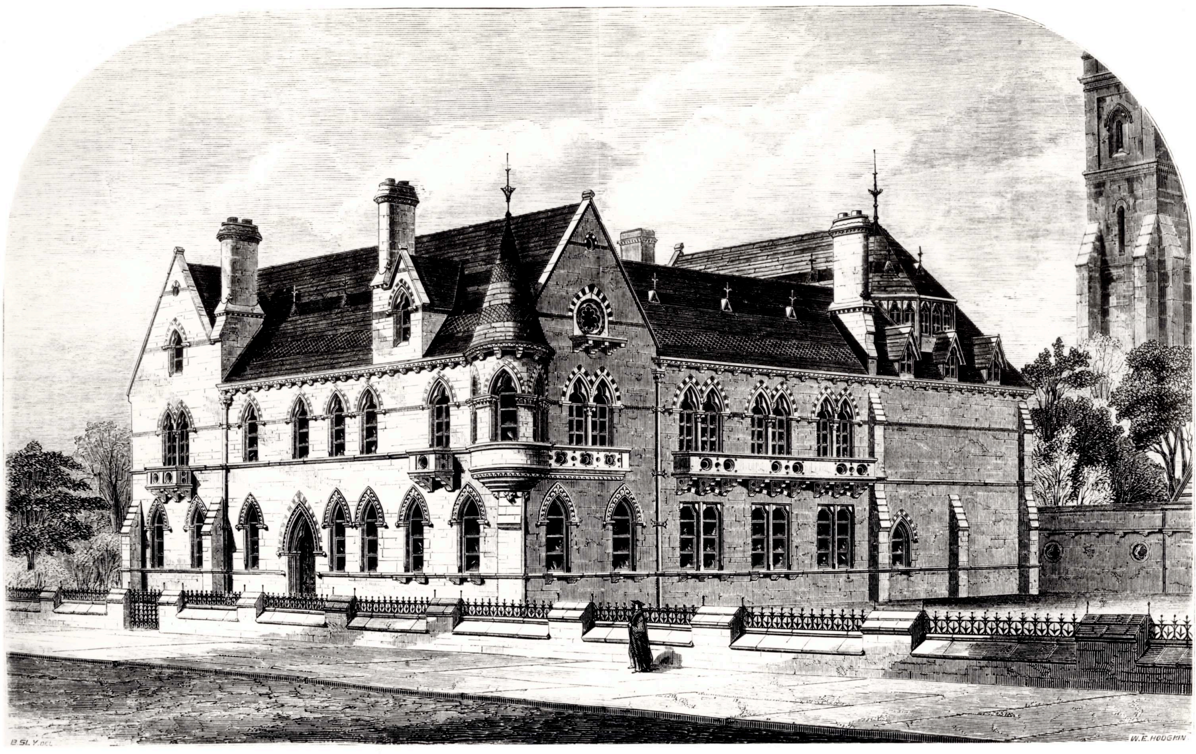

Figure 3 The Clarendon Laboratory. Built and fitted out at a total cost of about £12,000, the Clarendon was first used as a setting for laboratory instruction in October 1870. From *The Builder*, 27 (8 May 1869), 367. Private collection.

The difference between the provision for physics in the University Museum and that in the Clarendon is striking. In the Clarendon, as in the Museum, there was a large lecture-theatre and a central glass-covered court. But most of the space was distributed between small rooms, each of them devoted to a particular branch of physics. On the ground floor, for example, there were rooms for work on radiant heat and other thermal phenomena, static electricity, and spectroscopy while rooms on the first floor were devoted to other areas of optics, current electricity, and acoustics.

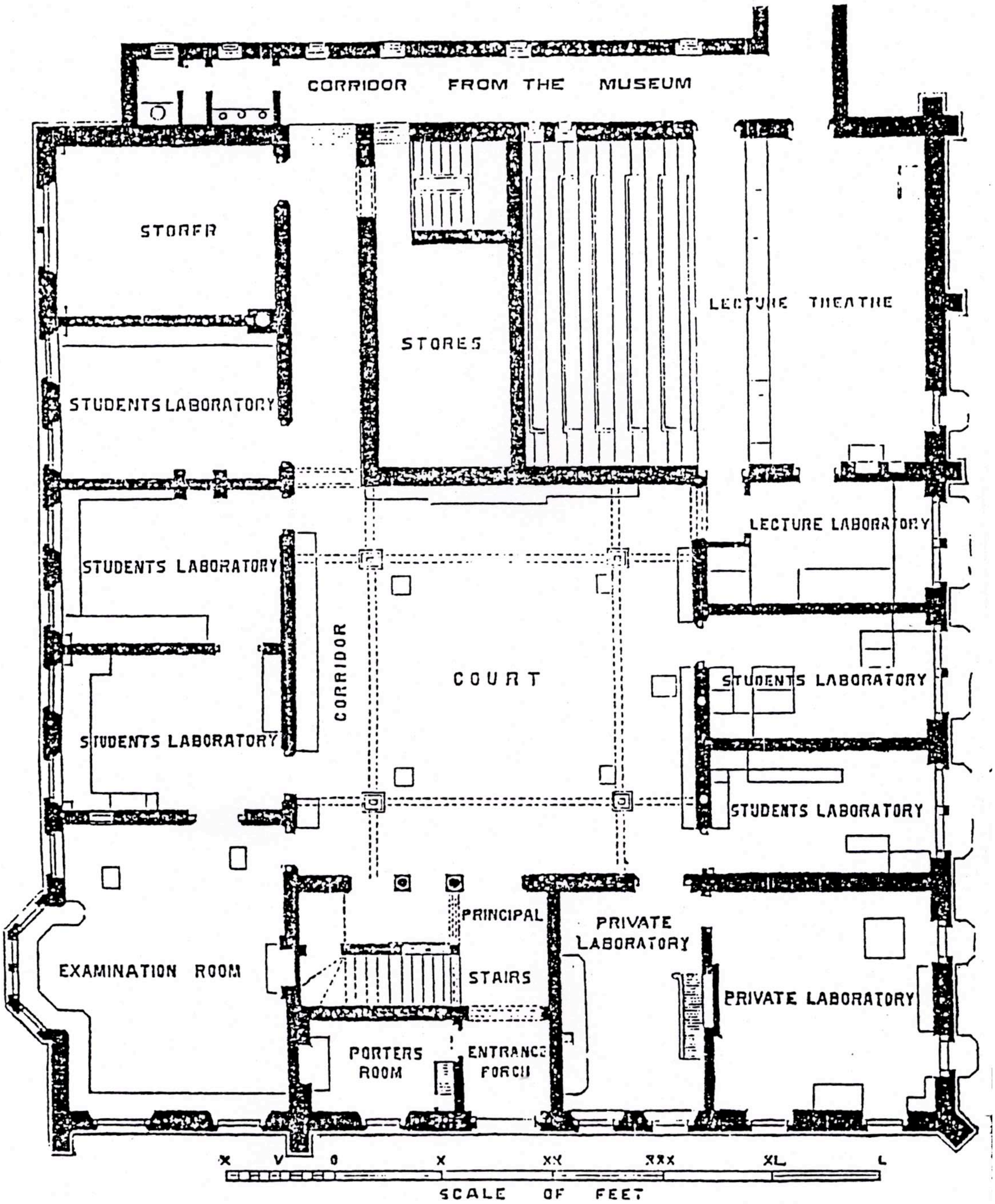

Figure 4 The ground floor of the Clarendon Laboratory, showing the arrangement of small rooms, each devoted to a particular branch of physics, round the central court, which provided light (through its glass roof) and the possibility of performing experiments requiring the full height of the building. A similar arrangement of small rooms was repeated on the first floor, with other space, notably for optical experiments and photography, in the roof. From *The Builder,* 27 (8 May 1869), 367. Private collection.

Clifton's idea was that specialized apparatus should be set up in the various rooms and that, in the course of their studies, students would move from one room to another. In this way, students could be given access to the delicate high-quality instruments that Clifton had acquired, mainly at the International Exhibition in Paris in 1867, while the instruments themselves were protected from the harmful effects of frequent moving. It was an exciting new departure for Oxford, and Oxonian pride was fed still further by the knowledge that the university had stolen a march on Cambridge, whose Cavendish Laboratory only came into use in the autumn of 1873 and spring of 1874.

Through a mixture of Clifton's ambition for the subject and the fortuitous availability of the money from the Clarendon fund, Oxford found itself endowed with one of the finest laboratories of the day. The task now was to ensure that the laboratory was used. Clifton himself began vigorously enough, and some able students (including a number who went on to chairs in other universities) worked with him. But when, in 1877, Clifton published a paper on the source of the electro-motive force of a dry cell, the effect on the gathering momentum was devastating. The London physicists, William Ayrton and John Perry, responded with the charge that Clifton had merely repeated experiments of their own which they had described in print a year before. Clifton's confidence took a severe blow, and (even though he was still barely forty) he published no further scientific papers.

The year 1877 marked a turning point in Clifton's career and the history of the Clarendon in other respects as well. From then, both the number of students and the general level of activity in then laboratory stagnated, and the promise of Clifton's first decade or so in the chair was never recovered. It is a matter of speculation why this should have happened. But Clifton increasingly laid the blame at the door of the university, which he saw as unwilling to give physics proper support. Certainly, requests for the creation of a second chair, devoted primarily to electricity and magnetism (an area in which Clifton, a specialist in optics and acoustics, never felt at home), fell on deaf ears, as did his complaints about the inadequacy of the space available to him in the Clarendon. Such a response is, in fact, hardly surprising and it cannot be attributed to a systematic opposition to science in the university. The hard facts were that few undergraduates chose to undertake advanced work in physics and, crucially, that the agricultural depression of the later nineteenth century placed the university's finances in a precarious condition. The good will that had benefited science in the 1860s was something that even those sympathetic to science could not hope to see repeated twenty years later.

The irony for Clifton was that when money was eventually found for a second chair (the Wykeham chair, established in 1900 from funds made available by New College), he and his professorial colleague simply could not get on. The core problems were a generation gap and a divergence in style that separated Clifton (now in his mid-sixties) from the 32-year-old newcomer, John Townsend, a gifted product of the Cambridge school that had formed around J. J. Thomson. Townsend, a specialist in ion physics who shared none of Clifton's concern with meticulous experimentation, came with the clear intention of transferring the "Cavendish style" to Oxford. On his arrival, however, he was immediately aggrieved by being given inadequate premises in the University Museum, and it was only with the opening of a new electrical laboratory in 1910 that he felt he could properly pursue his conception of the discipline. Built with the aid of a donation of £23,000 from the Drapers' Company, one of the London livery companies, the electrical laboratory was inaugurated amid high hopes for a new start for physics in Oxford.

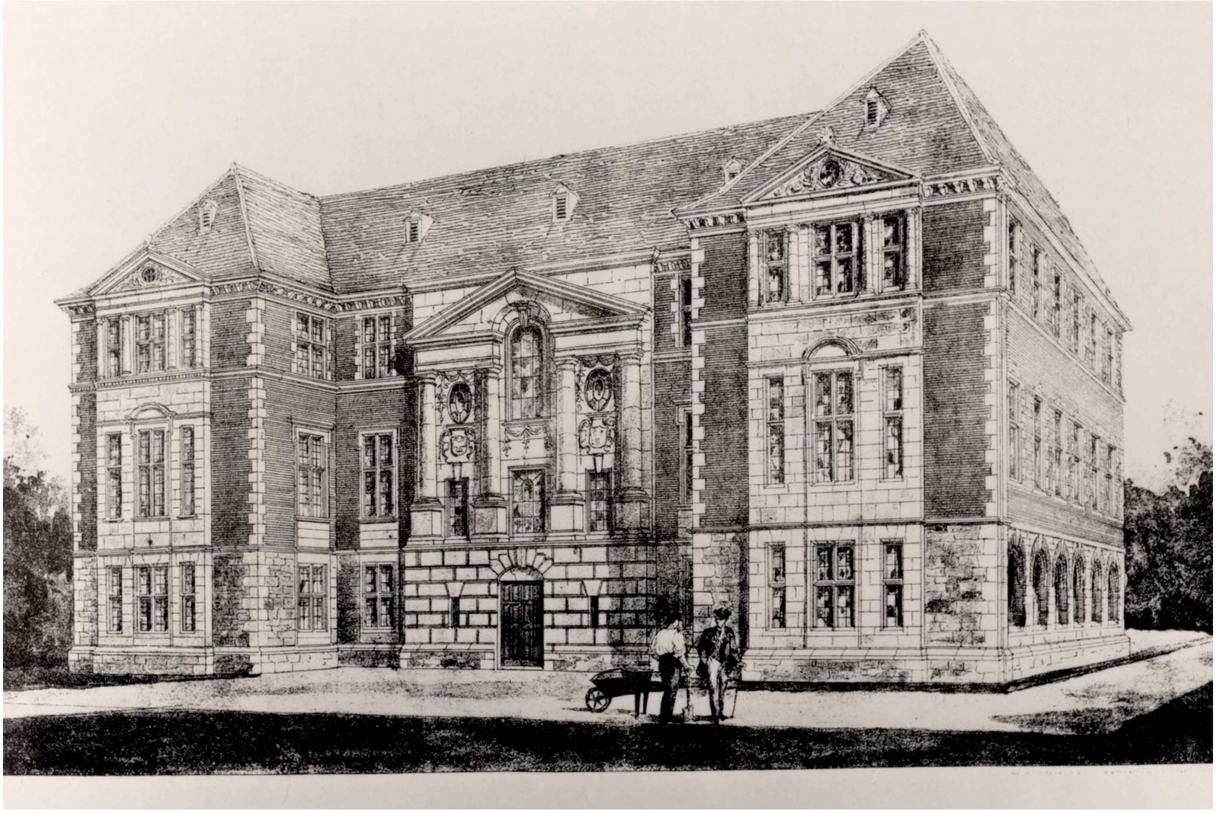

Figure 5 The electrical laboratory, inaugurated in 1910. Financed with the aid of a grant of £23,000 from the Drapers' Company, the laboratory became, for some years, the setting for research in ion physics and other areas of electricity under the supervision of the Wykeham professor of physics, John Townsend. Reproduced by courtesy of the Department of Physics, University of Oxford.

But this was another false dawn. The first world war profoundly disrupted academic life, and by the 1920s Townsend's early zeal for the creation of a research school on the lines of the one in which he worked in his Cavendish days was waning. Moreover, he was finding relations with the new Dr Lee's professor, Lindemann, as difficult as they had been with Clifton. Fatally for their relationship, Lindemann saw Townsend as an obstacle to his ambition for a unified department of physics under the single all-powerful professor that he aspired to become. Townsend, for his part, maintained a stubborn independence.

My feeling about Oxford's eclipsing by Cambridge, then, is not that the university opposed the development of physics. It did its best in unfavourable financial circumstances and with men in key positions – Clifton, Townsend, and Lindemann, in particular – who in different ways were defeated by the diffuse Oxonian structures of authority and influence. Yet it was Lindemann who oversaw the dramatic revival of the discipline in the 1930s in a scientific world transformed by National Socialist ideology. By now, Lindemann had withdrawn from any serious engagement with research: he had virtually abandoned his speciality of low-temperature physics in 1924 and had settled into the life of a bachelor don, sharing his time between the administration of the Clarendon and the comfortable setting of Christ Church, where he lived until his death in 1956. But the plight of the Jewish physicists who suddenly found themselves compelled to leave Germany gave him an opportunity that no other professor in a British university could have exploited to such effect. As a fluent German-speaker (a consequence of his Alsatian family connexions) and a man of considerable private means, Lindemann toured German universities in which he knew Jewish colleagues were in difficulties. He aimed high: Einstein and Schrödinger were among those who were courted, and at various times it seemed possible that both men might settle in Oxford. Lindemann's most enduring and, in the end, greatest coup, however, was the agreement that the brilliant low-temperature team from the Technische Hochschule in Breslau would work, at least temporarily, in the Clarendon: in this way, Franz Simon, Kurt Mendelssohn, and Nicholas Kurti all found their way to Oxford (with the aid of funds provided by ICI), and (largely because of the intervention of the war) they stayed. But low-temperature physics was not the only beneficiary: the arrival of Heinrich Kuhn from Göttingen had a similarly invigorating effect on spectroscopy, in which field Kuhn worked with Derek Jackson, a colourful and independently wealthy graduate of the Cambridge Natural Sciences Tripos.

Chance, in the form of the sad events of the 1930s, had again played a decisive role. And this time physics in Oxford did not look back. The replacement of the old Clarendon Laboratory by its present building on the eve of the second world war helped the university's physicists to have a successful war. With the newly available space being allocated to research on radar and the separation of the isotopes of uranium and with Lindemann serving as Churchill's trusted advisor on science (a service that earned him the title of Lord Cherwell), any lingering belief that Oxford was not an appropriate setting for physics, or for science as a whole, became hard to sustain.

By the 1950s, with first Franz Simon and (after Simon's untimely death in 1956) Brebis Bleaney succeeding Lindemann, the Oxford department was well set on the road that made it, in the 1960s, the largest (in terms of its undergraduate numbers) in Britain. To say that, before it achieved this mid-twentieth-century flowering, Oxford physics had lived precariously would be an understatement. But in that very precariousness there lies a lesson, in particular about the crucial roles of chance and the intricacies of local context, that, as historians, we cannot ignore. As the case of Oxford reminds us, the establishment of a big department of physics as a natural research-oriented adornment of the modern university was in reality anything but natural, and it was certainly not inevitable. Things could have turned out very differently.

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