NOTES AND RECORDS
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TWO NOBEL LAUREATES IN CONVERSATION: ROBERT ROBINSON LISTENS TO DOROTHY HODGKIN'S ACCOUNT OF HER LIFE SCIENTIFIC

by

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In 1974 the Nobel laureate Sir Robert Robinson OM PRS (1886–1975) was gathering information for the memoirs he was writing. As part of his research, he recorded a conversation with his former student, fellow Nobel laureate Professor Dorothy Hodgkin OM FRS (1910–1994), during which she outlined the key stages of her career. She explained the principles underlying crystallography and described her work on the structure of biological molecules including penicillin and vitamin B_{12}—for which she received the Nobel Prize—and on insulin. This paper includes a verbatim transcript of the conversation, which reveals the key figures in Hodgkin's career and the technical breakthroughs which underlay the elucidation of the structure of very large complex molecules. The paper includes a commentary on the value of oral accounts and concludes on the issues raised and not raised during the conversation. Sir Robert was President of the Royal Society between 1945 and 1950 when women were first elected Fellows. Hodgkin was elected in 1947. However, no mention is made of the challenges facing women developing a scientific career in the first half of the twentieth century.

Keywords: crystallography, Dorothy Hodgkin, Robert Robinson, biological chemistry, women scientists

INTRODUCTION

When Robert Robinson (1886–1975) became Waynflete Professor of Chemistry at the University of Oxford in 1930 he was already a titan in the world of science, having been Professor of Chemistry at the universities of Sydney, Liverpool, St Andrews, Manchester and University College London as well as having briefly led the British Dyestuffs Corporation laboratories.[1] And he was still only in his mid forties. At Oxford, under Robinson's leadership, the Dyson–Perrins Laboratory became an international centre for natural product chemistry, attracting both students and established researchers. In 1947 he received the ultimate accolade: the Nobel Prize for his work on 'plant products of

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1 T. I. Williams, *Robert Robinson: chemist extraordinary* (Oxford University Press, Oxford, 1990).

biological importance, especially the alkaloids'.[2] Although a slightly distant head of department at Oxford, Robinson nevertheless inspired a generation of students including the future Nobel laureate, Dorothy Crowfoot (1910–1994), later known as Dorothy Hodgkin, who by 1930 was in the third year of her studies at Somerville College.[3] Despite shining as an Oxford undergraduate and completing a Cambridge PhD, Hodgkin's early career presents a stark contrast to that of Robinson. She was in her mid thirties before she was appointed to her first permanent university-funded appointment at Oxford as demonstrator. She was elected Fellow of the Royal Society in 1947 but not promoted by the university to reader until a decade later. It was not until 1960 (when she was 50) that she became the Royal Society's first Wolfson Professor, a personal appointment. In 1964 she, like Robinson, became a Nobel laureate for 'her determinations by X-ray techniques of the structures of important biochemical substances'.[4]

Robinson retired from Oxford in 1955. By 1974, when this conversation was recorded, he was frail and unwell, almost blind. Nevertheless he was gathering information for his memoirs[5] and wanted to understand more about the work and career of his former Oxford colleague. The spoken word is perhaps the ideal medium of information for individuals who can no longer read the written or printed word. Happily, by the 1970s, sound recording on to magnetic tape had become commonplace, with cassettes a convenient and relatively safe form of storage. Robinson had become a director of Shell Chemicals on his retirement from University of Oxford and from 1967 he was also a director of Shell Research Ltd. It was at one of Shell's London offices that this conversation was recorded.

Oral history has developed into a recognized and respected research technique since World War II.[6] Different styles emerged as techniques developed. Historians in the USA tended to use the genre to record the thoughts and memories of the rich and famous, whereas in the UK the focus tended towards so-called ordinary working people whose stories were often missing from archival sources and whose testimonies provided important evidence for broader social trends. Many historians, trained in the importance of documentary sources, were sceptical of the evidential value of interviews. Nevertheless, oral accounts have become important primary sources for many studies exploring a wide variety of themes. The 'Voices of Science' project at the British Library is ensuring that the stories of individuals who have contributed at all levels in science and technology in the recent past are preserved.[7] Historians of modern medicine have gained much from the seminar series Wellcome Witnesses to Twentieth Century Medicine, which took place in the late 1990s.[8] These seminars allowed clinicians and scientists involved in significant developments, such as autoimmunity, monoclonal antibodies and research in general practice, to record memories and views. Memories, of course, can be unreliable and oral accounts must be checked and confirmed by other sources, as is usual with documentary sources. Wellcome seminar participants were encouraged to debate and share opinions as well as give simple accounts of events. Much of the historical value lay in this

2 https://www.nobelprize.org/prizes/chemistry/1947/summary/.

3 Georgina Ferry, *Dorothy Hodgkin: a life* (Granta, London, 1998); republished as *Dorothy Crowfoot Hodgkin: Patterns, proteins and peace: a life in science* (Bloomsbury, London, 2019).

4 www.nobelprize.org/prizes/chemistry/1964/summary/.

5 Robert Robinson, *Memoirs of a minor prophet: 70 years of organic chemistry* (Elsevier, Amsterdam, 1976).

6 Alistair Thomson, 'Four paradigm transformations in oral history', *Oral Hist. Rev.* **34**, 49–70 (2007).

7 https://www.bl.uk/projects/national-life-stories-oral-history-of-british-science.

8 J. N. Blau, 'Book of the month: Wellcome witnesses to twentieth century medicine vols 1 and 2', *J. R. Soc. Med.* **92**, 206–207 (1999).

material, which is often very subjective. Historians have argued that this very subjectivity is the essence of oral evidence[9]—an opportunity to hear interpretations of past events, allowing interviewees to put virtual highlighter pens over events and experiences. Thus, oral accounts provide information and insight inevitably absent in archival or published sources, especially in the scientific literature where 'objectivity' is prized and where the personal aspect may be lost. Hodgkin presents much that is personal and subjective in this conversation, whose structure, though set by Robinson, was very much controlled by Hodgkin. Robinson was not a historian but a memoirist. He had invited Hodgkin to the conversation because she was for a brief time his student. He wanted to understand her scientific biography, to fill in gaps in his memory and to humanize the published record. Although the account of her career is therefore partial, the conversation nevertheless has provided historians with a unique and personal account of one of the twentieth century's most influential and talented scientists.

Transcription made from digitised version of a recording made on cassette tape of a conversation between Professor Sir Robert Robinson OM FRS (1886–1975) and Professor Dorothy Hodgkin OM FRS (1910–1994) in 1974, held at the Royal Society in the Robert Robinson Collection, ROR/5/2/6

The cassette was deposited by the family of Sir Robert Robinson along with his personal papers. The conversation was transcribed in December 2021 from a digital copy supplied by the Library of the Royal Society.

Robert Robinson (RR): What I should like to hear from Professor Hodgkin, Dorothy to me, is a brief account of the stages that are used in the investigations of structure by X-ray crystallographic methods. And by that I don't mean the apparatus, nothing to do with the apparatus at all, but the use for example of heavy atoms and so on and illustrations of the typical compounds that were used in the progress of this research and what happened in the cases. You started didn't you, Dorothy, with Bernal?[10]

Dorothy Hodgkin (DCH): No I started before that and so I should place myself back in the context of Oxford University. You see I came up to Oxford with the idea of working on crystals and even of working on X-ray analysis through reading the books that the Braggs had written for children: *Concerning the nature of things*,[11] and *Old trades and new knowledge*,[12] the lectures that were given at the Royal Institution during the middle 1920s

9 Paula Hamilton, 'The oral historian as memorist', *Oral Hist. Rev.* **32**, 11–18 (2005).

10 J. Desmond Bernal (1901–1971) was lecturer in crystallography at the University of Cambridge from 1927 until 1938. He then moved to Birkbeck College, London, where he pioneered the exploration of biological molecules by crystallography. Dorothy Crowfoot worked with Bernal at Cambridge from 1932 until 1934. The relationship was close and lasting. Bernal was a central intellectual figure in left-wing scientific circles between the two twentieth-century world wars. Maurice Goldsmith, *Sage: a life of J. D. Bernal* (Hutchinson, London, 1980). Gary Wersky explores Bernal's relationship with Joseph Needham as well as his socialist views in *The visible college, a collective biography of British scientists and socialists of the 1930s* (Free Association Books, London, 1988).

11 William Henry Bragg (1862–1942) became Director of the Royal Institution in 1923. In 1915 he had received the Nobel Prize jointly with his son Lawrence for their work demonstrating the elucidation of crystal structures using X-ray analysis. He became a towering public figure in the 1920s and 30s, serving as President of the Royal Society between 1935 and 1940. John Jenkin, *William and Lawrence Bragg, father and son: the most extraordinary collaboration in science* (Oxford University Press, London, 2008). W. H. Bragg, *Concerning the nature of things: six lectures delivered at the Royal Institution* (Bell & Sons, London, 1929).

12 W. H. Bragg, *Old trades and new knowledge: six lectures delivered to a 'juvenile auditory', at the Royal Institution, Christmas 1925* (Bell & Sons, London, 1926).

for Christmas. And then, of course, I began to study chemistry properly and I went to the crystallographic course but I also worked mainly on chemistry. And I remember your coming very well in the middle of my undergraduate period in the 1930s and the excitement that we had and the excitement of going to your lectures. And we saw you of course as a great international figure bringing to Oxford people from all over the world so that the Dyson–Perrins Laboratory became a league of nations. And we also had a myth of you working always on Sundays with your wife and child in the lab.[13] I don't know whether this was right. But it was a myth for us who were listening to you at the time. But better still, of course, I remember your lectures and pieces out of them that have affected my life since. And particularly the lectures on porphyrins and the lecture on strychnine. And somewhere after one of these lectures, I remember a somewhat fantastic dream, in which I imagined myself walking about among the trees and picking the atoms off the trees like great birds. And this was thinking of the possibility of working on some of these more complicated molecules of which the structures were still unknown by X-rays. And so I began X-ray work in fact in Oxford with Tiny Powell[14] for my Part II year on a quite simple problem thallium dimethyl halides before I went to work with Bernal. But it was the fact that Bernal was just beginning to work on more complicated problems and particularly on the steroids that attracted me to go to him to work with him. And at that time our work was very much in the nature of exploration. The first crystal structures of organic compounds had just been solved. As you know, one of the first amongst aromatic compounds was hexamethyl benzene of which the structure was done by Kathleen Lonsdale[15] and I actually found her paper written in 1929 when I was reading in the Radcliffe Library and used it of course in one of my undergraduate essays. Now the method that she used of course was to pass X-rays through the crystals and find how many atoms there were in the unit cell which must themselves constitute molecules. But she started from a very theoretical position in which she didn't allow herself to know that these atoms constituted molecules at all but in the course of her finding positions for them by trial methods in the crystal structure they constituted a flat benzene ring with six methyl groups around them. And she followed this by calculating an electron density projection for hexachlorbenzene which was in fact the first electron density projection calculated for an organic compound. And again the method was to measure the intensities of the diffracted beams as accurately as possible to find an approximate solution to the structure by trial methods which made it possible to calculate the relative phases of the X-ray beams and then to constitute the Fourier synthesis to show the electron density in projection. So this was the position at which I was beginning X-ray work. But I must say that with Bernal we did very little of this, practically nothing. We explored. We put X-rays through all sorts of different compounds, from the protein, pepsin to quite simple metallo-organic

13 Robinson married Gertrude Maud Walsh (1886–1954) in 1912. They met while both worked and studied in the laboratory of W. H. Perkin Jr, at the University of Manchester. Their son, Michael, was born in 1926. Gertrude Robinson published over 30 papers, mainly on the structure of fatty acids and flower pigments. See article by Trevor I. Williams in the *Oxford dictionary of national biography*, https://doi.org/10.1093/ref:odnb/31620.

14 Professor Herbert Marcus Powell (1906–1991) was known universally as 'Tiny'. In 1944 he became head of Oxford's Crystallography Laboratory. He was awarded a personal Chair in 1963.

15 Dame Kathleen Lonsdale FRS (1903–1971) established the structure of benzene while working at the University of Leeds between 1928 and 1930. Lonsdale had worked at the Royal Institution with W. H. Bragg in the 1920s. In 1945, she was the first woman physicist to be elected Fellow of the Royal Society. Jennifer Wilson, 'Dame Kathleen Lonsdale FRS (1903–1971): her contribution to crystallography', *Chem. Text* **7**(4) (2021).

compounds and measured the unit cell sizes, molecular weights and made guesses about the kind of molecule that was present. And partly we were held up from going further by the complexities of the problems we were looking at. Because we couldn't begin trial methods with molecules as complicated as strychnine and you must know he had a whole set of photographs of strychnine which were taken by somebody else but which I indexed when I was first in his laboratory. So we were quite held up in our approach. Although the method of possibly using heavy atoms had already been suggested through work of Cork on the alums in 1927,[16] J. M. Robertson was the first seriously to apply it to an organic compound.[17]

RR: Oh was he?

DCH: In the, the middle 1930s to the phthalocyanines which were a gift because you could take the central heavy atom in and out and it was on a centre of symmetry in the crystal which made the calculations very easy. I thought it would be a good thing to try the same kind of calculations on the sterols but of course they were asymmetric molecules and much more complicated. The heavy atoms were not in special positions easily found.

RR: You s…? didn't you?

DCH: I started by using cholesterol chloride and bromide and they were not isomorphous with one another or not enough isomorphous with one another. And then I followed with cholesterol iodide.

RR: Iodide.

DCH: Yes. But of course the change that occurred that made it possible to try those sorts of compounds was the change introduced by Patterson[18] who showed that it was possible to calculate directly from the X-ray intensities without any knowledge of what you had in the crystal at all or the phases at all, a map which would show a distribution of the vectors between the atoms in the crystal. Now mostly of course since there were very very many vectors, the maps would be very confused, but if you have an outstandingly heavy atom such as iodine the map simplifies very largely to just the vectors between the iodine atoms and this at least allows you to find the iodine atom positions and begin a new kind of trial calculation using the iodine phases to start the analysis and this was what I did for cholesterol iodide.

RR: At what stage did you work on penicillin salts?

DCH: Well this was the next stage on. You see, cholesterol iodide we were working on just at the beginning of the war and it was the first compound in which we faced the fact that the molecule was asymmetric and that we had to work in three dimensions but our method was very simple it was to calculate an electron density projection and then lines through each atomic position in three dimensions so it was a very primitive form of three-dimensional

16 J. M. Cork, 'The crystal structure of some of the alums', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* **4**, 688–698 (1927).

17 Robertson worked with W. H. Bragg at the Royal Institution in the 1920s, including on the structure of the phthalocyanides using the position of heavy metal ions in isomorphous compounds. In 1960 his research group at the University of Glasgow finally determined the structure of limonin, the bitter principle of citrus fruits. This was an exceptional achievement, second only to the determination of the structure of B_{12}. Struther Arnott, 'John Monteath Robertson, FRS (1900–1989)', *Biogr. Mems Fell. R. Soc.* **39**, 351–362 (1994).

18 (Arthur) Lindo Patterson (1902–1966), suggested the Patterson function while working at the Massachusetts Institute of Technology between 1933 and 1936. He had been introduced to crystallography while working with W. H. Bragg at the Royal Institution between 1925 and 1927. Joan R. Clark, 'Memorial of Arthur Lindo Patterson (July 23 1902–Nov 6 1966)', *Am. Mineral.* **53**, 576–586 (1968).

electron density calculation and it was largely carried out by Harry Carlisle[19] with sort of mathematical tables in his hand in the train going back and forwards from Princes Risborough where he was doing some war work at that time. But because by that time we really knew what the sterols were we thought it would be more interesting to work on something that we didn't know the chemical structure of at all and this was why I was very much interested when I talked to Chain[20] of the possibility of working on penicillin because when it was first isolated of course nothing at all was known about it. But in a curious way we held up our own investigation because we were very keen on having a heavy atom derivative of penicillin and this encouraged them to try crystallizing the barium salt which as you know never crystallized at all.

RR: You used the potassium salt, didn't you?

DCH: Eventually, you see but this was in a sense a sad story. Because we couldn't get crystals, the organic chemistry of the compound, the investigation ran ahead.

RR: What a shame!!

DCH: And you discovered of course, or it was discovered what the degradation products were and therefore what were possible formulae, long before penicillin itself was crystallized. And then it was the sodium salt that was crystallized.

RR: Then I remember that besides you only had at first the F hadn't you the penicillin F.

DCH: Yes, at first we only had the F but in fact it really crystallized as easily as G though it was G that was first crystallized. And in fact what happened you know was they brought the news across that the sodium salt had been crystallized by Squibb so of course I said to Edward Abrahams[21] oh make us some. And he made some and he said that we have it all ready. And so they brought it over. And they had it in a desiccator, you know dried so we took it out on a slide and left it lying about while we were talking and then I looked at it under a microscope and it was crystalline. And it had picked up water from the atmosphere while we were talking and crystallized. So again it was just their bad luck that they had always dried the penicillin.

RR: Well that's a little detail of history which is apt to be lost, isn't it?

DCH: Yes, yes.

RR: Then when the question this loss of water from the penicilloic acid became the important point. You did at first get results which agreed with the oxazolone thiazolidine formula didn't you?

DCH: Not really. Our trouble was the sort of gradual approximation.

RR: Yes, Yes, but how gradual was it? How far off was it ever?

DCH: Well it started you see in the way we had started with Bernal originally by our taking just X-ray photographs and then you get a unit cell in which there must be so many atoms. So we started by assuming that the structure would be the oxazolone structure and trying by trial and error to find a position of the atoms in the lattice that would fit the intensities.

19 Harry Carlisle had been Bernal's research assistant at Birkbeck. During the war, Hodgkin 'took over' much of Bernal's apparatus and his research assistant! He and Hodgkin published the structure of cholesterol iodide in the *Proceedings of the Royal Society* in 1945. He returned to Birkbeck after the war.

20 Ernst Chain (1906–1979) emigrated to Britain from Germany in 1933 as Nazi control made life difficult for those of Jewish faith. On the recommendation of Gowland Hopkins, in whose Cambridge biochemistry laboratory he completed a PhD, he joined the team Howard Florey assembled at the University of Oxford to develop penicillin into an effective therapeutic agent.

21 Sir Edward Abraham (1913–1999) worked in Ernst Chain's Oxford group. He played a crucial role in purifying penicillin using chromatography, and from that purified sample he obtained the sodium salt. In autumn 1943 Abraham proposed the β-lactam ring structure for penicillin: the ring contained three carbon atoms and one nitrogen. This cyclical structure was unknown in biological molecules at that time and Robinson initially favoured the oxazolone structure.

RR: And how near to that could you get?

DCH: Well now we actually, … Here the investigation splits slightly. We had three salts: the potassium, the rubidium and the sodium. And the potassium and rubidium were isomorphous and allowed us to determine a certain number of phase constants directly without reference to any chemical structure but unfortunately not all. So that the map they gave was a muddled confused map in which we could imagine all sorts of things … Charles Bunn took the oxazolone structure as most probable and found a position of the atoms in the unit cell which fitted quite a lot of the intensities but not quite all.[22] And that was the stage you were thinking of. And then when we put them together we found that in fact the region in which he had placed the oxazolone ring was a region which in our map must be the ions and the carboxyl group. So it was clear that something was quite wrong with the whole idea of how the atoms were arranged even though part of it part of the structure solution was quite correct. In fact the only part that was quite correct was where the benzene ring was and as this was the part that was most difficult to see in our map it helped us immediately together to reach a direct solution in which we could just apply the automatic refinement to …

RR: The whole thing. I am interested in the extent of the variation from the correct structure that was shown. I gather it was not very very great.[23]

DCH: No no it wasn't and at one moment we thought … I mean we did try the atoms in the oxazolone position in the central part of the molecule while placing the rest of the structure as we thought correctly and then all of the atoms gradually moved over and it isn't very far in space to the alternative positions.[24]

RR: The biosynthesis possibly does go through the oxazolone and then it suffers internal β-lactam formation. There is no question whatever that β-lactam was the right formula for the stuff itself but the fact that the oxazolone is not very far removed from it is obvious from models and I am interested to hear that the X-ray results were well not consistent with it but not very very far removed from it.

DCH: No no not they are very far removed from it. In fact in the early stages it was quite easy to think that it was correct.

RR: Well that's the point that's interested to have had made. Well then the next then you went on to much more difficult problems, didn't you?

22 Charles Bunn (1905–1990) was a chemist at ICI Northwich laboratories, who, by the beginning of the war, had significant experience of analysing long-chain hydrocarbons. He developed the 'fly's eye' method of crystal analysis using monochromatic light and a camera with multiple pin holes to produce a diffraction pattern that could be compared to the X-ray images. Bunn's experimental techniques and his repeated analyses, shared multiple times with the Oxford team, finally produced a proposed structure which fitted with all the experimental evidence. See obituary by U. W. Arndt, *Biogr. Mems Fell. R. Soc.* **37**, 70–83 (1991).

23 During and immediately after World War II, as the strategic importance of penicillin was recognized, a massive scientific collaborative effort got underway. In all, 130 British scientists in 11 research groups and 299 American scientists in 21 groups produced 750 communications, most of which were confidential. The overall aims were to determine the chemical structure of penicillin and work out a means of synthesizing the chemical commercially. Robinson's team worked with the biochemists Ernst Chain and Edward Abraham in Howard Florey's group in the nearby Sir William Dunn School of Pathology. His initial structure was proved incorrect by the crystallographic analysis of Hodgkin and others.

24 Isolation of certain of the degradation products of penicillin taken from different sources indicated that 'penicillin' could take various forms. Further chemical analysis suggested the acidic group of the molecule to be in the form of thiazolidineoxazolone with two carbon rings, one with sulphur and the other with nitrogen double-bonded to carbon. X-ray analysis and infra-red spectrometry, however, indicated a β-lactam structure, with only one carbon ring, the nitrogen being in a 'straight line' formation. Penicillin proved a very difficult molecule both to analyse and to synthesize, and despite their collaborative efforts the wartime teams failed to work out how to synthesize penicillin commercially. Peter Mitchell's review of the *Report on a collaborative investigation by American and British chemists* on the chemistry of penicillin (edited by Hans Clarke, John Johnson and Robert Robinson and published in 1949 by Princeton University Press and Oxford University Press)—'A description of penicillin', *Nature* **164**, 851–852 (1949)—provides a useful summary of the chemistry and the inter-relationship between the two structures.

DCH: Yes. Now I would like to insert one or two episodes in here and perhaps hear from the point of view of history two points. The structure of penicillin was one in which really almost all of our calculations were still calculations of the electron densities in projection but of course it would have been much more simple if we had been able to work in three dimensions.

RR: May I interrupt one moment? You attributed that a little earlier to Kathleen Lonsdale. But didn't the Braggs do that too?

DCH: Working out structures in general yes. But Kathleen Lonsdale who started of course by working with W. H. Bragg in fact carried out the first X-ray analysis of an aromatic compound. Not the first …

RR: Not the first at all but the first of an aromatic compound.

DCH: No no. But of an aromatic compound.

RR: That's what I thought. I thought that the Braggs had already established that method of determining the electron concentration and the pattern.

DCH: Yes yes. It was just the first applications in organic chemistry.

RR: I'm sorry I interrupted you.

DCH: No, no, that's alright. The point I was making about the three-dimensional X-ray analysis was quite interesting in relation to our work. You see in the middle of the war we were still doing calculations very largely by hand and adding machines and then we had a consultation with Comrie as to how we could speed up our calculations and he had the idea that they should be put on punched card machines.[25] And he said when he saw what we were involved in doing, 'Oh dear, you will have to have special machines after all. Now if you only wanted the whole electron density calculated then it would be easy'. I said of course it would be much better for us to have the whole electron density calculated. So after that he put the calculations on to punched card machines. And that was one step forward.

RR: A big step forward. And of course the one involving considerable expenditure on apparatus.[26]

DCH: Well on computing yes of course but it was to become much more serious still later. Now the next major step forward actually came from the work of Professor Bijvoet and others in Holland just after the war.[27] And this was a step involving the use of the isomorphous replacement method not just as we had tended to use it in central–symmetrical projections to calculate sines but by a series of measurements to calculate the full phase angles appropriate to the scattered X-ray beams. And the first compound that this was tried on was strychnine.

RR: Was it really?

DCH: Yes really. And if you remember I had this little correspondence with Professor Bijvoet in which he sent me his first projections and said it isn't the Robinson structure and then you came across and said no it was the Robinson structure because he was

25 After leaving the Nautical Almanac Office, Leslie Comrie (1893–1950) had set up the Scientific Computing Service Ltd in 1936. Punched card machines had been in use since the late nineteenth century to store and process large amounts of information. See his obituary by H. S. W. Hattie, *Biogr. Mems Fell. R. Soc.* **8**, 96–107 (1952).

26 Comrie's organization provided a commercial service. Hodgkin initially thought the processing of crystallographic data would not involve any payment. Her misunderstanding resulted in some friction.

27 Johannes Martin Bijvoet (1892–1980) was a professor at the University of Utrecht. He published his work on the absolute configuration of molecules in 1946. He became a Foreign Member of the Royal Society. See his obituary by M. P. Groenewede and A. F. Peerdeman, *Biogr. Mems Fell. R. Soc.* **29**, 26–41 (1983).

looking at an old paper of yours which had the structure in which you no longer believed and so you had these slides that Bijvoet sent over for your Chemical Society lecture.[28]

RR: Did he realize that that was so?

DCH: Yes yes. By that time he realized … you came over to see me and then I wrote to him and said it's the same structure. You both believe in the same structure.

RR: Well the days when we shall determine structures by these laborious methods of the organic chemist are probably over. Recent work seems to suggest that the whole thing can be done by X-ray crystallography.

DCH: Well of course very often if you are just pursuing one line of compounds you can very easily find out how one molecule differs from another without a full X-ray analysis. I think it's the more complicated involved problems that require X-ray work all the time. But I've often thought it would be very good to work out very closely how one should use the different techniques together. More than even is done nowadays though it's done of course more than it used to be.

RR: I don't want to go into an argument about the falling off of the use of organic chemical methods because I have strong views about that and which I briefly expressed to Cindy once. And that was that you must do the organic chemistry in order to understand the relations of the molecules to other molecules and the transformations and where they fit in and in fact the whole of the chemistry.

DCH: Oh yes. Yes, I don't think there is any real conflict between us or the methods.

RR: No, no. You have made an enormous contribution. An enormous contribution.

DCH: You see one of the very interesting facts of history is that the methods that Bijvoet introduced first in strychnine are essentially the methods that have carried us over to proteins and very much larger molecules. As you might expect.

RR: I mentioned to you in a letter that I may be wrong in this but I think the molecular biologists so-called and I strongly object to that phrase because we are all molecular biologists really aren't we?[29]

DCH: Yes yes.

RR: They seemed to think of big molecules in a cell as isolated entities. But if you get your information for example about insulin there are an enormous number of molecules of insulin, don't you?

DCH: Yes of course yes.

RR: So it isn't an isolated entity that you are talking about at all but a collection of molecules just like any other collection of molecules.

[Voice 'off'—presumably technician: 'Can we stop here?']

RR: Is everything operating now?

[voice 'off': 'Yes']

RR: Even strychnine in spite the complexity of molecules, rings is a comparatively simple compound. But you have been able to probe the structure of much more complicated compounds later on. And I think one of the first that was investigated was B_{12}. Didn't

28 Robinson first worked on strychnine with W. H. Perkin Jr, in Manchester, publishing a structure, which was later disproved, in 1910. By the 1930s, working with associates he had determined several anomalies with this structure and proposed an alternative arrangement of carbon and nitrogen rings.

29 Robinson's views on the differentiation of molecular biology echo similar feelings by Nobel laureate Richard Synge (see note 51).

Lord Todd[30] make a good contribution in that by supplying you with a useful crystallizable hexacarboxylic acid?

DCH: Yes he did. Of course we should begin at the beginning with Lester Smith[31] isolating preparing the first crystals in this country of the vitamin itself.

RR: And showing the presence of cobalt.

DCH: And showing the presence of cobalt in it. He crystallized it shortly after it had been crystallized in America by the Merck group. Lester Smith brought the crystals to Oxford and of course the first thing we did was to just to put X-rays through the crystals. I took the first photograph overnight and the second the following morning during a biochemical meeting in Oxford and by the time he went home in the evening we knew the rough molecular weight which was of the order of 14, 15 hundred and about half what he thought it was and so he was quite pleased but still it seemed a very complicated problem to embark on until it was discovered that it contained cobalt a few weeks later. Macrae[32] rang me up from Glaxo, a telephone call came through at lunchtime to say that he had found cobalt in the vitamin: one atom for each molecule of your molecular weight. So that was a very exciting moment.

RR: Macrae of which group?

DCH: Glaxo.

RR: Glaxo. Yes I see.

DCH: And after that I began slowly to begin working on it with the help of a very good postdoctoral fellow and gradually others but still by comparatively primitive techniques although we did work entirely in three dimensions with the help of punched card machines and we saw the whole structure as it were dimly. I remember meeting you in the road and saying that there is something of the nature of a porphyrin in this molecule at a rather early cloudy stage.

RR: Yes yes I remember that quite well.

DCH: But what it didn't somehow make out as a porphyrin and this worried us a great deal until we got the crystals of the hexacarboxylic acid which were obtained by Jack Cannon[33] in Todd's laboratory at Cambridge from the acid degradation of the vitamin. And this was a molecule that constituted the main porphyrin-like nucleus plus side chains but not the attached benzimidazole sugar–phosphate chain.

RR: But it still contained the cobalt.

DCH: It still contained the cobalt. And this was very much easier to work on. It was a smaller molecule. And in fact the structure was solved in just over a year by one of my DPhil students, Jenny Pickworth, and this was the first time at which, no it wasn't strictly it was the second time, that we had the use of an electronic computer. With the assistance of Kenneth Trueblood[34] in Los Angeles at that time. The first one actually was

30 Alexander Todd OM, Baron Todd of Trumpington (1907–1997), became Professor of Organic Chemistry at the University of Cambridge in 1944 after six years in Manchester. There he extended his interest in natural product chemistry including the nucleic acids and proteins. See Daniel Brown and Hans Kornberg, 'Alexander Robertus Todd, OM, Baron Todd of Trumpington', *Biogr. Mems Fell. R. Soc.* **46**, 515–532 (2000).

31 Lester Smith, working at Glaxo, isolated the first sample of vitamin B_{12} late in 1948.

32 T. F. Macrae described the wide range of research undertaken in laboratories around the country in this article: 'The research work of Glaxo Laboratories Ltd', *Proc. R. Soc. B* **146**, 181–193 (1957).

33 Jack Richard Cannon (1927–2014) was an Australian chemist who did his PhD at Cambridge in the laboratory of Lord Todd. He went on to work at the University of Western Australia. https://www.eoas.info/biogs/P005574b.htm.

34 Kenneth Trueblood (1920–1998), Professor of Chemistry at UC Los Angeles, was a pioneer in the use of computers in the analysis of X-ray data to produce three-dimensional electron density maps. The results of successive analyses were exchanged by airmail and telegraph until the complete structure was determined in 1955. Trueblood then secured a Fulbright scholarship, which

calciferol[35] which was a compound that you helped us over the heavy atom derivative making and this we put through as a trial before we let him do the calculations on the hexacarboxylic acid.

RR: Oh yes it was not a very proper sort of period for calciferol but it was used later on as a trial for the other.

DCH: Yes yes.

RR: It was much later than the first examination of the calciferol.

DCH: Very much later. But it was the work that showed the full details of how the central ring was spread out. But of course the B_{12} analysis because we were using rather slow methods of computing as we should now think took quite a long time. The very interesting molecule that followed, the B_{12} coenzyme, was worked on for a matter of a year and a half by an American postdoctoral fellow at Oxford with us and then we had electronic computers and everything went through extremely smoothly to produce a most extraordinary result of course. That the molecule was the same in most regions as the original B_{12} nucleus, but attached to the cobalt by a cobalt–carbon bond. The fragment of deoxyadenosine. And that was the first cobalt–carbon bond found in natural product chemistry. So we then began to recognize even more the power of our methods to show us things we could never have dreamt existed.

RR: And did you go directly from B_{12} to proteins?

DCH: Well really as you know, you had put insulin into my hands in 1935 and so the first work that I did on insulin was back in 1935 and this again was confined to finding the size of the unit in the crystal which in fact was really two molecules of insulin but of size 12 000 and seeming far beyond our reach in X-ray analysis at that time. But all through the years I kept little bits of work going on insulin trying to think of how to carry the structure analysis further but after the B_{12} analysis was finished then I began much more to concentrate on the insulin X-ray analysis.

RR: Is this doubling of the molecule. Is it a question of oxidation of cisteine to cistine?

DCH: No, no it isn't. It's now become a feature of really very many natural protein molecules that they aggregate extremely easily. So in the case of insulin there is an aggregate really of six molecules around two zinc atoms both in solution and possibly even in the pancreas where there is zinc and in these six molecules two combine to form a dimer first of all hydrogen-bonded to one another across a parallel, an anti-parallel pleated sheet. And then the dimer aggregates round the zinc to give you the hexamer and dimerization through the hydrogen bonding across pleated sheets has been observed in lactic dehydrogenase, alcohol dehydrogenase the immunoglobulins, really a large concunabulin, a large number of different protein molecules so that very commonly the molecular weight which was first measured in solution was found to correspond not with the chemical molecule but with a very close aggregate, difficult to break apart.

RR: Well this is er … quite an expected thing to happen, isn't it really?

DCH: Yes it is really. It was guessed at by those who first worked. When I first published the molecular weight of insulin and being very young at the time and not very courageous, I just put down the size of the unit cell molecular weight which was 36 000, very close to the

allowed him to work with Hodgkin in Oxford between 1956 and 1957. https://senate.universityofcalifornia.edu/_files/inmemoriam/html/kennethnyitraytrueblood.html.

35 Calciferol, vitamin D_2, is essential to the growth and maintenance of bones in human bodies. Hodgkin already knew its likely structure when she shared crystallographic data with Trueblood.

weight that Svedberg[36] had measured in solution. I had an agitated letter from Freudenberg[37] saying that the molecular weight insulin molecule measured by Svedberg was surely an aggregate and he had evidence that the molecular weight was less than that, probably 12 000 or even less. And of course eventually it turned out to be only 6000. Mainly through Sanger's work on the actual chemical structure.[38]

RR: Is that K. Freudenburg, the catakin man?

DCH: Yes, yes the old Freudenberg.

RR: I didn't know he was so interested in proteins.

DCH: He did a lot of early work on proteins. Yes.

RR: Did he? He's still alive, I think.

DCH: Yes I met him for the first time, in East Germany in the Leopoldina[39] meeting about four or five years ago, with the structure of insulin in my hand. I was really very pleased of this coincidence.

RR: Was it? I should say so. Quite a triumph. And have you further worlds to conquer, Dorothy?

DCH: I think I should very much like now of course to pursue further the actual reactions of the B_{12} coenzyme. This involves having the coenzyme within the enzyme and then trying also to track the substrates there and see how the atoms are related in the substrate–enzyme complex.

RR: That's the one containing adenosine attached to the cobalt.

DCH: Yes yes, er … the trouble here again is the trouble of crystallization. So far though some of B_{12} enzymes have been reported crystalline we haven't really managed to put our hands on any of them and certainly they are not yet good enough crystals for X-ray work. But I think one of the very fascinating directions in which X-ray work is moving is in the direction of observing some of the pathways of chemical reactions. You may have come across the recent work of Jack Dunitz[40] in this connection and I was very fascinated because his work showing the different distances between carbon and nitrogen in different compounds suggesting a reaction pathway involved the alkaloids, cryptopine and berberine which I remember your putting into my hands very long ago in the 1930s and my doing nothing whatever with them. And I think that there is going to be a very interesting set of relations if we can pursue them further which will let you follow these dotted lines which you write in your study of organic reaction mechanisms a bit more closely.

RR: Very interesting chemistry in those pseudo-bases.

36 Theodor Svedberg (1884–1971), Professor of Physical Chemistry at the University of Uppsala, received the Nobel Prize in 1926 for his work on colloidal chemistry. His special focus was the behaviour of macromolecules. https://www.nobelprize.org/prizes/chemistry/1926/svedberg/biographical/. S. Claesson and K. Pedersen, *Biogr. Mems Fell. R. Soc.* **18**, 594–627 (1972).

37 Karl Freudenberg (1886–1983) became Professor of Chemistry at the University of Heidelberg in 1926. He was known for his work on large carbohydrate molecules such as cellulose. However, as Hodgkin learnt, research in his laboratory was wide-ranging, including the proteins of blood and hormones such as insulin. T. S. Stevens, *Biogr. Mems Fell. R. Soc.* **30**, 168–189 (1984).

38 Fredrick Sanger (1918–2013), working in the biochemistry laboratory at University of Cambridge, determined the amino acid sequence of insulin between 1944 and 1955. He received the Nobel Prize for this work in 1958. https://www.nobelprize.org/prizes/chemistry/1958/sanger/facts/. S. Brenner, *Science* **343**, 262 (2014). G. Brownlee, *Biogr. Mems Fell. R. Soc.* **61**, 437–466 (2015).

39 The scholarly society Leopoldina Nationale Akademie der Wissenschaften was founded in 1662 as a classical scholarly society. In 2008 it was appointed as the German National Academy of Science.

40 Jack Dunitz (1923–2021) trained in crystallography at the University of Glasgow before moving to work with Hodgkin in the late 1940s. In 1953 he left Oxford, first for the USA. In 1957 he moved to the Swiss Federal Institute of Technology in Zurich, where he remained for the rest of his career. Obituary, *The Times*, Wednesday 20 October 2021.

DCH: Yes. And the same within the enzymes if one can get more accurate structure analysis of the systems and with the help of chemists find suitable reactions to follow I think one will be able to follow some of the stages a little bit more closely.

RR: You will have to invent a … you will have to invent a …

DCH: Not me. Not me. But others of course you know in the future.

RR: No no you and others will have to invent something of a moving picture X-ray photograph.

DCH: You know most reactions go too fast but I think some of these ones might be slowed down.

RR: So that you can actually see what is happening and change from one picture to another.

DCH: Yes, this would be fascinating but I …

RR: You don't believe it.

DCH: No no. But I think it needs very much more powerful X-ray beams than we have and some fairly slow reactions but that's where organic chemistry is quite …

RR: But it was done with explosions.

DCH: Yes. Yes.

RR: Explosions were photographed in time. So as to find out what was really happening.

DCH: Yes.

RR: During explosion wave. I believe that will come.

DCH: Yes. I think … this is certainly one of the directions in which …

RR: You have travelled a great deal, haven't you, Dorothy. You have been in many different countries. And you have shown a sort of predilection for Africa.

DCH: Well Africa …

RR: Can could you explain why you are so fond of going to places like Nigeria. Is it political?

DCH: No no, it's all historical. You see actually to start with I was born in Cairo, you know.

RR: Oh born in Cairo. Well that explains quite a lot.

DCH: Explains a lot. My father was then in the Egyptian education service and went on to the Sudan. So I had a rather early acquaintance with Africa but really the later journeys to Africa were all owing to my husband's interests in African university problems particularly.[41]

RR: Of course.

DCH: Ghana, Nigeria and the Sudan.

RR: What do you call that, Workers Educational?

DCH: Workers Educational Association started him off. And this brought him to Oxford Extra Mural Studies and from Oxford Extra Mural Studies …

RR: I knew about that but I thought you had an interest also perhaps in helping these people.

41 Thomas Lionel Hodgkin (1910–1982) was born in Oxford into a prominent academic family. He began his career working on an archaeological dig in Palestine before working in the Colonial Service. He married Dorothy Crowfoot in 1937. During World War II, Hodgkin worked in adult education with the Workers' Educational Association. He later joined the University of Oxford Extra Mural Department. From the 1950s he became increasingly interested in the history of Africa and helped to establish a new and thriving academic discipline. He played an important role in the development of higher education in Ghana and other African countries as they emerged as independent countries. See entry by Michael Wolfers in the *Oxford dictionary of national biography*, https://doi.org/10.1093/ref:odnb/51860.

DCH: I do, I do. Well I do have an interest in, in, I think growing universities. It's always sort of beginnings of work and what you can do when you are at the beginning.

RR: Helping them at the start. Very important.

DCH: But it's also partly one's own experience. Having started in very small ways.

RR: But to take a jump, Dorothy. What about Bristol. You are Chancellor of the University of Bristol, aren't you. Are you enjoying that?[42]

DCH: I do really. I think again it's a very interesting university.

RR: I am sure it is.

DCH: I like it in all sorts of ways. I like it for the kind of …

RR: Most interesting place Bristol really. Very characteristic city. Nowhere else quite like it.

DCH: It's a pity the ships don't come up still to the middle of the town in the way they did.

RR: And what I feel is that it's a pity we haven't got that Severn Barrage scheme working.

DCH: Yes. And there's a great deal of very good scientific work going on. I find myself always enjoying dropping into either chemistry or biochemistry or physics. They all have a number of fascinating problems they are working on.

RR: Well I am sure that's very interesting indeed. I just meant your personal reaction to being Chancellor of a university?

DCH: Well I find that being a Chancellor a little bit difficult quite to believe in, you know.

RR: I don't know whether you remember Sir Maurice Bowra.[43] Somebody once asked him in regards University of Oxford. 'How does it feel, Bowra to be Vice Chancellor—oh he was only Vice Chancellor—of a third rate university?' and Bowra said 'I can't conceive.'

DCH: [Laughter] Of course, he was very good at that kind of thing.

RR: Yes repartee. Quick repartee, very good indeed.

DCH: Yes very good.

RR: He was a great loss.

DCH: I last saw him over a lunch party with the Chinese which Mendelsohnn[44] gave in Wadham. And of course he was brought up in China so this was …

RR: You know the story about that. Wait a minute you were going to tell me something?

DCH: Oh no. You go ahead and tell the story.

RR: What was the name of the Principal of St Anne's? Miss …

DCH: Miss Plumer.[45]

RR: Miss Plumer. Yes well Miss Plumer was the daughter of the General Plumer of the Boer War. And during the course of a procession to the Sheldonian on one occasion, Bowra offered her an umbrella and she stepped back a pace or two and said 'Oh no sir, my father was a soldier and he never used an umbrella.' 'Oo' said Bowra he said 'that's interesting. My father was a mandarin—he was never without one.'

DCH: It was very interesting that his father should have become a mandarin.

42 Hodgkin was elected Chancellor of the University of Bristol in 1970. She took an active interest in both the research of the university and the views and activities of its students.

43 Sir Maurice Bowra (1898–1971) was Vice Chancellor of the University of Oxford from 1951 to 1954 and President of the British Academy from 1958 to 1962. See entry by Leslie Mitchell in the *Oxford dictionary of national biography*, https://doi.org/10.1093/ref:odnb/30841.

44 Georg Mendelssohn FRS (1906–1980), German-born medical physicist. See obituary by David Schoenberg, *Biogr. Mems Fell. R. Soc.* **29**, 360–398 (1983).

45 Eleanor Plumer (1885–1967) was Principal of St Anne's College from 1940 until 1953. http://www.st-annes.ox.ac.uk/this-is-st-annes/history/principals/eleanor-plumer/.

RR: Well he was an honorary mandarin. He was in the diplomatic service in China.

DCH: No I hadn't ever heard that story.

RR: What?

DCH: I hadn't ever heard that story. No.

RR: Oh, really. Really.

DCH: No.

RR: Well there are other stories of Bowra which I'm certainly not going to commit to tape. [Laughter] He was a very witty man. President of the, I think for some time, President of the British Academy. Which doesn't mean the pictures.

DCH: No no. He's missed.

RR: Well I think Dorothy you've had a magnificent career and I do congratulate you on what you've been able to do. I don't think any other person that I know of has …

DCH: Now I should have one final remark about this. You see I think it's a very remarkable thing, a fact, that you should care about in your eighty-fourth no I think eighty-sixth year I think ring us up to ask us to do a little work on the structure of this brazilin dimer.

RR: Oh yes.

DCH: And of course we looked back and found that one at least of your first papers was back in nineteen hundred and seven[46] and now the girl who did this was an Australian girl who had really come to help me mind the children at one stage. But she was a good chemist and I thought this was a shame and so I turned her into a crystallographer.

RR: Well I think Rene Jaeger[47] was the person who did this work. Really some of this work.

DCH: The latest work on the chemistry but now what I wanted to say about the crystallography because I haven't said this before is of course that we did this particular or Maureen Mackay[48] did this particular piece of work by really modern X-ray methods involving the direct approach to X-ray analysis which has come in over the years and which does not require you to have a heavy atom present in the molecule at all which depends on your having really very powerful computers and good X-ray measurements. And all of these changes being complete it was nice to find again that our measurements agreed so well with Rene Jaeger's and the work that you have done together.

RR: Well thank you. We were very grateful for that. Was there anything about the bond between the two parts of the molecule which was interesting? Was it long?

DCH: Yes I think it is a shade long. Sorry I've got the actual figures in here.

RR: A shade longer than normal.

DCH: Yes. A shade long. If I looked at it I could tell you the actual figures straight away. But perhaps I will can put it into the text when it comes to the point.

RR: Yes, yes.

DCH: And of course all the hydrogen atoms appear in the electron density map. Everything beautiful just as you would hope for. [End]

46 Robinson became interested in the red dye brazilin while a postgraduate student at the University of Manchester. In 1907/8 he published several papers with W. H. Perkin Jr on brazilin, including 'Some derivatives of y-pyranol allied to certain derivatives of brazilein and haematein. (Preliminary notice)', *Proc. Chem. Soc.* **23**, 149–150 (1907).

47 In the 1960s Robinson worked and published several studies with R. H. Jaeger.

48 Maureen Mackay was the Australian girl who moved in with the Hodgkin family (along with her husband Don, a DPhil student at Balliol) to help with the three children while Thomas was away. She had a degree in chemistry and eventually became a full-time research assistant in Hodgkin's laboratory.

Commentary

The lives and work of both Robinson and Hodgkin have been analysed and their stories told in excellent full-length studies.[49] In Georgina Ferry's biography of Hodgkin, the conversation is referred to in one footnote as a transcript held in Hodgkin's papers at Oxford's Bodleian Library. No mention is made to it specifically in her bibliography of original sources.[50] So what does this conversation add to the historical record?

The longevity of the relationship between Robinson and Hodgkin is very clear, particularly towards the end of the interview when Hodgkin mentions the work her team did on brazilin for Robinson only five years or so previously. Both are respectful of the other. We hear Hodgkin's deference to and respect for Robinson as the professor of whom she was in awe as a student. Even though they were both Nobel laureates, there is a definite sense of generational divide. We hear Robinson address Hodgkin by her Christian name; nowhere is this reciprocated. Robinson's comments in praise of Hodgkin's achievements could be interpreted as somewhat patronizing. However, the conversation is warm throughout.

Hodgkin makes clear the individuals who had played key roles in her career. William Henry Bragg's popular science lectures at the Royal Institution in the 1920s captured Hodgkin's youthful imagination. Kathleen Lonsdale's work on benzene demonstrated that X-ray analysis could be applied to organic molecules. J. D. Bernal encouraged conjecture and exploration. The techniques introduced by J. M. Robertson and subsequently Lindo Patterson facilitated experimentation on increasingly complex molecules. Working out the structure of vitamin B_{12} involved collaboration with colleagues in the pharmaceutical industry as well as Kenneth Trueblood of the University of California.

The conversation also tells us some of the issues faced at the laboratory bench by crystallographers in the mid twentieth century. We hear that the over-riding challenge was to produce good crystals of large organic molecules. Crystallographers often depended on their chemist colleagues to produce samples for analysis. Hodgkin mentions several times that Robinson, over many years, gave her samples of organic compounds. During Hodgkin's career, chemists came to see crystallography as an analytic technique.

We also learn that sometimes serendipity has played a significant role in life at the laboratory bench. When Oxford chemists in Edward Abraham's group prepared the sodium salt of penicillin, they always placed the samples in a desiccator to remove water from the molecule. These water-less samples produced few crystals and were therefore not ideal for crystallography. In the interview Hodgkin recounts that before checking the sample of the sodium penicillin salt brought over to her, it was left on the bench while she chatted to the colleague who had delivered it. In picking up water from the atmosphere, the sodium salt formed beautiful crystals from which Hodgkin's team could then get clear X-ray images, allowing them to confirm the β-lactam structure of penicillin.

49 *Op. cit.* (notes 1 and 3).

50 Ferry, *op. cit.* (note 3), p. 214. Ferry recounts that Hodgkin told a friend that Robinson was 'clinging' to the idea that the oxazolone structure was involved in some way. The conversation here suggests more speculation on Robinson's part. Scepticism was understandable given the novelty of the β-lactam structure in biological molecules. In the conversation, Robinson emphatically accepts the β-lactam structure but speculates about a dynamical relationship with the oxazolone structure. He sounds as though he is throwing ideas around with a colleague.

Robinson established an international reputation and was regarded as one of the most distinguished organic chemists of his generation, deducing chemical structures through the lengthy processes of synthesis and degradation. In their conversation Robinson shared his fear, perhaps unfounded, for the demise of this 'classical' organic chemistry in the face of X-ray analysis and potentially other techniques and tools. He also criticized sharply the differentiation from chemistry of molecular biology. His views echoed similar views expressed by Nobel laureate Richard Synge, who once complained to the biochemist Joseph Fruton: 'If only that tautologous expression [molecular biology] had never been adopted.'[51] Synge and Robinson may not have liked the emergence of molecular biology as a distinct discipline, but, by the 1960s, emerged it certainly had. The first issue of the *Journal of Molecular Biology* was published in 1959 under the editorship of John Kendrew, who, with Max Perutz, was to be awarded the Nobel Prize three years later for the determination of the structure of myoglobin.[52] In 1967, when reviewing a collection of essays on the origins of molecular biology,[53] Kendrew discussed the differing traditions that had already emerged within the youthful discipline: in the USA, molecular biology principally focused on understanding the transmission of biological information; in the UK, by contrast, molecular biology had its roots in the relationship between structure and function and a belief that uncovering the former would lead to a fuller understanding of the latter. Hodgkin's work was from the very beginning rooted in the analysis of structure. Her comments towards the end of the conversation about the direction she felt chemistry might be heading indicate that her interest had moved towards understanding the dynamic role of biological molecules *in vivo* rather than *in vitro*. The techniques that she and others had pioneered over almost four decades had provided only static images of biological molecules, useful in structure determination but providing only partial answers to the questions around function. Hodgkin was clear that, in the early 1970s, this was the challenge for the next generation. She does not mention imaging, which, as computing power increased in the 1980s, allowed the determination of biological molecules of ever-increasing complexity. Although she did not challenge Robinson's comments about molecular biology, her comments indicate perhaps that over the course of her career she had shifted disciplinary allegiances, sharing the emphasis on the relationship between structure and function within the emerging discipline of molecular biology.

Conversations such as these are also interesting for what is *not* discussed. Hodgkin was one of very few women to take up a scientific career in the inter-war years. The peers she mentions are all men with the exception of Kathleen Lonsdale whose work on benzene had inspired Hodgkin while a student at Somerville. The more junior women scientists prominent in her story are those Hodgkin herself had either recruited or trained. This is, of course, to be expected at this time when access to higher education was restricted for all young people but especially for women. However, it was under Robinson's watch as President of the Royal Society that women were first admitted as Fellows to this most elite of scientific institutions.[54] Kathleen

51 R. Synge to J. Fruton, 22 May 1992. J106. The papers and correspondence of Richard Laurence Millington Synge. Trinity College Library, Cambridge. GBR/0016/SYNG. I am grateful to Dr Kersten Hall for sharing this reference.

52 K. C. Holmes, 'Sir John Cowdery Kendrew 1917–1997', *Biogr. Mems Fell. R. Soc.* **47**, 311–332 (2001).

53 J. C. Kendrew, 'Review of *Phage and the origins of molecular biology* by John Cairns, Gunter Stent and James D. Watson', *Scient. Am.* **216**, 141–144 (1967).

54 Research for this interview is part of a wider study of the first women to be elected to the Royal Society. Between 1945 and 1954 only 11 women achieved FRS status: Kathleen Lonsdale (1945), Marjory Stephenson (1945), Agnes Arber (1946), Mary

Lonsdale along with the Cambridge biochemist and bacteriologist, Marjory Stephenson, were the first women to be elected in 1945. Two years later President Robinson welcomed Hodgkin along with the mathematician Mary Cartwright and the zoologist Muriel Robertson to the Society. Yet nowhere in this conversation does Hodgkin raise with the then President this milestone for both women scientists and for the Royal Society. In 1948 the zoologist Sidnie Manton and the biochemist Dorothy Needham were also elected Fellows. However, no further women were elected until 1952, a gap of four years, at a time when there were several women who had made significant contributions to scientific knowledge and who could have expected election. Access to higher education for women in the middle years of the twentieth century was, of course, a serious issue. However, this is not raised even when they chat about Hodgkin's role as Chancellor of Bristol University. This is perhaps all the more surprising as Hodgkin herself actively encouraged women within her own laboratory and supported initiatives to support women in pursuing careers in science. We can only speculate that encouraging women to achieve eminence in scientific careers was not a topic of importance for Robinson and therefore not a topic Hodgkin felt comfortable to raise.

Throughout her career, Hodgkin struggled to secure adequate funding for her own salary, for her team and for equipment. Like many others during the early years, she relied on 'soft' money—grants awarded for specific projects or fellowships from colleges or charitable trusts. Indeed Robinson himself had help secure funds for her to equip her laboratory when she returned to Oxford after completing her Cambridge PhD.[55] Yet this lack of financial support for science in general and women specifically is not addressed anywhere. The conversation structure was flexible and open. She could have raised this issue. Perhaps she accepted the position of women standing behind men in the queue for both jobs and money; perhaps it was just too delicate a topic for Robinson, who was close to the end of his life. The complete silence is surprising given that Robinson had supported Hodgkin as she was repeatedly passed over by University of Oxford for senior posts.

The conversation demonstrates overwhelmingly the collegiality of science. Robinson is clearly proud of his association with Hodgkin, his former student who has had such a 'magnificent career'. It is a privilege for us to 'eavesdrop' on so personal a conversation.

Data accessibility

Cassette tape of a conversation between Professor Sir Robert Robinson, OM, FRS (1886–1975) and Professor Dorothy Hodgkin, OM, FRS (1910–1994) recorded in 1974. Tape held in the Robert Robinson Collection, Royal Society Library, ROR/5/2/6. Digital version available.

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Cartwright (1947), Dorothy Hodgkin (1947), Muriel Robertson (1947), Sidnie Manton (1948), Dorothy Needham (1948), Honor Fell (1952), Marthe Vogt (1952), Rosalind Pitt-Rivers (1954).

55 Ferry, *op. cit.* (note 3), pp. 131–132.

chemistry and encouraged me to develop it further. I am also grateful to Rupert Baker, Virginia Mills and Ellen Embleton of the Library of the Royal Society for providing a digital copy of the cassette tape and information about its acquisition. Finally I would like to thank the two anonymous reviewers whose comments have strengthened the paper.