Cambridge city center, early 1950s. Detail of a map published by W. Heffer & Sons.

JAMES D. WATSON

THE ANNOTATED AND ILLUSTRATED

DOUBLE HELIX

Edited by

Alexander Gann & Jan Witkowski

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For Naomi Mitchison

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Preface to Annotated and Illustrated Edition

In Cold Spring Harbor's Blackford Bar, one evening in June 2010, Sydney Brenner suggested looking through the papers he had recently donated to the Cold Spring Harbor Laboratory Archives. Among his own papers were, he knew, some of Francis Crick's correspondence that had become muddled in with his during the 20 years they had shared an office in Cambridge. A few days later we discovered that the trove included letters to and from Crick written during the period when he and Jim Watson at Cambridge, and Maurice Wilkins and Rosalind Franklin in London, were searching for the structure of DNA.

Mislaid some 50 years earlier ("thrown out by an over efficient secretary," Crick believed), these letters had escaped the attention of the historians of molecular biology who first started looking into this new field in the mid-1960s. The letters provided some new insights into the proceedings, and in particular the personal relationships of the protagonists in the DNA story.

The most celebrated account of that story is *The Double Helix*, Watson's novelistic version of the events as they appeared to a 23-year-old American in Cambridge in the early 1950s. Written not in the tone of a formal autobiography nor in the measured language of the historian, his racy and thriller-like telling was reviled by some and praised by many upon publication in 1968.

In writing our article on the lost Crick correspondence, we naturally reread *The Double Helix*. We were struck by how Watson's account in the book accurately represented the vivid, contemporary descriptions of people and events found in the letters, and not just those of Crick and Wilkins, but Watson's own. The social whirl of parties, tennis, French lessons, holidays, and other events that featured prominently in the book—the "gossip," as Crick characterized it—were recorded in the weekly letters Watson wrote to his

sister Elizabeth during his time in Cambridge. And the science covered in the book was also discussed in contemporary letters to Max Delbrück and other friends, and not just the DNA work, but Watson's research on bacterial genetics and tobacco mosaic virus, projects that figure prominently in the story. In all of this contemporary correspondence, the character of Watson himself—the brash, self-confident yet at times also self-deprecating, young man portrayed in his book—was transparent. We became intrigued to see all the contemporary accounts we could find—not just those revealed in the letters of Watson, Crick, and Wilkins, but of Franklin, Linus Pauling, and others as well.

We also noticed just how many other characters appear in *The Double Helix*—many unrelated to the central scientific story. Watson, eager to keep the narrative moving, often provides only the briefest of information, sometimes not even identifying the most intriguing of minor characters. We don't get to learn the interesting story of the "local doctor" who had rowing oars mounted on the wall of his surgery, or the identity of the "antiquarian architect" who kept his house free of gas and electricity—or anything much about Bertrand Fourcade except that he was the "most beautiful male" in Cambridge. And what was the novel of ill-judged sexual indiscretions of Cambridge dons that Watson reads at one point in the story? We wanted to know.

And so the idea of an annotated edition of *The Double Helix* took shape, a version in which an array of viewpoints and voices would be added as commentary, together with background information and illustrations to enrich the text. The current volume is the result. In addition to the numerous photographs (a number being published for the first time), we have reproduced many letters and other documents in full or in part as facsimiles. One of the pleasures of visiting archives is to see and handle original documents and while we cannot match that experience, we hope that readers will enjoy seeing letters and manuscripts as their original recipients saw them.

The sources of material used in our annotations are many, both published and unpublished. Of the former, we used many books—including the histories and biographies of the field. These are listed in the bibliography at the

end of the book. Among unpublished sources, Watson's letters to his sister and also his parents are a major source of information about his Cambridge life and have not been used before, except by Watson, while his letters to Delbrück, Luria, and others provide scientific content. In addition to Watson's papers, we have drawn on those of Crick, Wilkins, Pauling, and Franklin among others. We have also included reminiscences written by Ray Gosling specially for this edition. Gosling worked with both Wilkins and Franklin in those years and actually took the most famous and influential diffraction pictures of DNA. The source for each annotation is included in a reference list at the back of the book.

In addition to the annotations and illustrations, we have added a number of other pieces. We have included Watson's account of winning the Nobel Prize, previously published in his later book, *Avoid Boring People*. This seems, on the occasion of the 50th anniversary of that award, a fitting conclusion to the tale. We have also added five appendices. These include one in which we reproduce facsimiles of the first letters Watson and Crick each wrote in 1953 describing the discovery, and another in which we publish for the first time a chapter from the draft manuscript of *The Double Helix* which was left out of the published book. While not describing anything new about the work on DNA, the missing chapter fills in the story of Watson's summer spent in the Alps in 1952.

We have corrected some errors of fact by adding annotations where necessary, but Watson's original text is unchanged.

It will be clear that this edition is not an exhaustive academic treatise. Rather, we chose items that appealed to us and hope that this somewhat quirky selection will prove useful and enjoyable to both new readers and those familiar with the original text.

Alexander Gann
Jan Witkowski
Cold Spring Harbor 2012

Sir Lawrence Bragg's Foreword to the Original Edition

This account of the events which led to the solution of the structure of DNA, the fundamental genetical material, is unique in several ways. I was much pleased when Watson asked me to write the foreword.

There is in the first place its scientific interest. The discovery of the structure by Crick and Watson, with all its biological implications, has been one of the major scientific events of this century. The number of researches which it has inspired is amazing; it has caused an explosion in biochemistry which has transformed the science. I have been amongst those who have pressed the author to write his recollections while they are still fresh in his mind, knowing how important they would be as a contribution to the history of science. The result has exceeded expectation. The latter chapters, in which the birth of the new idea is described so vividly, are drama of the highest order; the tension mounts and mounts towards the final climax. I do not know of any other instance where one is able to share so intimately in the researcher's struggles and doubts and final triumph.

Then again, the story is a poignant example of a dilemma which may confront an investigator. He knows that a colleague has been working for years on a problem and has accumulated a mass of hard-won evidence, which has not yet been published because it is anticipated that success is just around the corner. He has seen this evidence and has good reason to believe that a method of attack which he can envisage, perhaps merely a new point of view, will lead straight to the solution. An offer of collaboration at such a stage might well be regarded as a trespass. Should he go ahead on his own? It is not easy to be sure whether the crucial new idea is really one's own or has been unconsciously assimilated in talks with others. The realization of this difficulty has led to the establishment of a somewhat vague

code amongst scientists which recognizes a claim in a line of research staked out by a colleague—up to a certain point. When competition comes from more than one quarter, there is no need to hold back. This dilemma comes out clearly in the DNA story. It is a source of deep satisfaction to all intimately concerned that, in the award of the Nobel Prize in 1962, due recognition was given to the long, patient investigation by Wilkins at King's College (London) as well as to the brilliant and rapid final solution by Crick and Watson at Cambridge.

Finally, there is the human interest of the story—the impression made by Europe and England in particular upon a young man from the States. He writes with a Pepys-like frankness. Those who figure in the book must read it in a very forgiving spirit. One must remember that his book is not a history, but an autobiographical contribution to the history which will some day be written. As the author himself says, the book is a record of impressions rather than historical facts. The issues were often more complex, and the motives of those who had to deal with them were less tortuous, than he realized at the time. On the other hand, one must admit that his intuitive understanding of human frailty often strikes home.

The author has shown the manuscript to some of us who were involved in the story, and we have suggested corrections of historical fact here and there, but personally I have felt reluctant to alter too much because the freshness and directness with which impressions have been recorded is an essential part of the interest of this book.

W. L. B.

Sir Lawrence Bragg (1890–1971) was the director of the Cavendish Laboratory of Cambridge University at the time of the discovery of the double helix. He and his father, William Henry, the originators of X-ray crystallography, received the Nobel Prize in 1915.

Preface to the Original Edition

Here I relate my version of how the structure of DNA was discovered. In doing so I have tried to catch the atmosphere of the early postwar years in England, where most of the important events occurred. As I hope this book will show, science seldom proceeds in the straightforward logical manner imagined by outsiders. Instead, its steps forward (and sometimes backward) are often very human events in which personalities and cultural traditions play major roles. To this end I have attempted to re-create my first impressions of the relevant events and personalities rather than present an assessment which takes into account the many facts I have learned since the structure was found. Although the latter approach might be more objective, it would fail to convey the spirit of an adventure characterized both by youthful arrogance and by the belief that the truth, once found, would be simple as well as pretty. Thus many of the comments may seem one-sided and unfair, but this is often the case in the incomplete and hurried way in which human beings frequently decide to like or dislike a new idea or acquaintance. In any event, this account represents the way I saw things then, in 1951–1953: the ideas, the people, and myself.

I am aware that the other participants in this story would tell parts of it in other ways, sometimes because their memory of what happened differs from mine and, perhaps in even more cases, because no two people ever see the same events in exactly the same light. In this sense, no one will ever be able to write a definitive history of how the structure was established. Nonetheless, I feel the story should be told, partly because many of my scientific friends have expressed curiosity about how the double helix was found, and to them an incomplete version is better than none. But even more important, I believe, there remains general ignorance about how science is "done." That is not to say that all science is done in the manner described here. This is far from the case, for styles of scientific research vary almost as much as human personalities. On the

other hand, I do not believe that the way DNA came out constitutes an odd exception to a scientific world complicated by the contradictory pulls of ambition and the sense of fair play.

The thought that I should write this book has been with me almost from the moment the double helix was found. Thus my memory of many of the significant events is much more complete than that of most other episodes in my life. I also have made extensive use of letters written at virtually weekly intervals to my parents. These were especially helpful in exactly dating a number of the incidents. Equally important have been the valuable comments by various friends who kindly read earlier versions and gave in some instances quite detailed accounts of incidents that I had referred to in less complete form. To be sure, there are cases where my recollections differ from theirs, and so this book must be regarded as my view of the matter.

Some of the earlier chapters were written in the homes of Albert Szent-Györgyi, John A. Wheeler, and John Cairns, and I wish to thank them for quiet rooms with tables overlooking the ocean. The later chapters were written with the help of a Guggenheim Fellowship, which allowed me to return briefly to the other Cambridge and the kind hospitality of the Provost and Fellows of King's College.

As far as possible I have included photographs taken at the time the story occurred, and in particular I want to thank Herbert Gutfreund, Peter Pauling, Hugh Huxley, and Gunther Stent for sending me some of their snapshots. For editorial assistance I'm much indebted to Libby Aldrich for the quick, perceptive remarks expected from our best Radcliffe students and to Joyce Lebowitz both for keeping me from completely misusing the English language and for innumerable comments about what a good book must do. Finally, I wish to express thanks for the immense help Thomas J. Wilson has given me from the time he saw the first draft. Without his wise, warm, and sensible advice, the appearance of this book, in what I hope is the right form, might never have occurred.

J. D. W.
Harvard University
Cambridge, Massachusetts
November 1967

THE ANNOTATED AND ILLUSTRATED

DOUBLE HELIX

Prologue from the Original Edition

In the summer of 1955, I arranged to join some friends who were going into the Alps. Alfred Tissieres, then a Fellow at King's, had said he would get me to the top of the Rothorn, and even though I panic at voids this did not seem to be the time to be a coward. So after getting in shape by letting a guide lead me up the Allinin, I took the two-hour postal-bus trip to Zinal, hoping that the driver was not carsick as he lurched the bus around the narrow road twisting above the falling rock slopes. Then I saw Alfred standing in front of the hotel, talking with a long-mustached Trinity don who had been in India during the war.

Since Alfred was still out of training, we decided to spend the afternoon walking up to a small restaurant which lay at the base of the huge glacier falling down off the Obergabelhorn and over which we were to walk the next day. We were only a few minutes out of sight of the hotel when we saw a party coming down upon us, and I quickly recognized one of the climbers. He was Willy Seeds, a scientist who several years before had worked at King's College, London, with Maurice Wilkins on the optical properties of DNA fibers. Willy soon spotted me, slowed down, and momentarily gave the impression that he might remove his rucksack and chat for a while. But all he said was, "How's Honest Jim?" and quickly increasing his pace was soon below me on the path.[1]

Later as I trudged upward, I thought again about our earlier meetings in London. Then DNA was still a mystery, up for grabs, and no one was sure who would get it and whether he would deserve it if it proved as exciting as we semisecretly believed. But now the race was over and, as one of the winners, I knew the tale was not simple and certainly not as the newspapers reported. Chiefly it was a matter of five people: Maurice Wilkins, Rosalind Franklin, Linus Pauling, Francis Crick, and me. And as Francis was the dominant force in shaping my part, I will start the story with him.

[1] **Willy Seeds' remark provided Watson with the title he originally wanted to use for what became *The Double Helix*. See the handwritten title page from an early draft (shown overleaf) and Appendix 4.**

Chapter 12

I gave John and Elizabeth Kendrew the scoop about DNA when I joined them for breakfast on Monday morning.[1] Elizabeth appeared delighted that success was almost within our grasp, while John took the news more calmly. When it came out that Francis was again in an inspired mood and I had nothing more solid to report than enthusiasm, he became lost to the sections of *The Times* which spoke about the first days of the new Tory government. Soon afterward, John went off to his rooms in Peterhouse, leaving Elizabeth and me to digest the implications of my unanticipated luck. I did not remain long, since the sooner I could get back to the lab, the quicker we could find out which of the several possible answers would be favored by a hard look at the molecular models themselves.

[1] **Kendrew had married Elizabeth, the widow of a close friend who was killed in World War II, in 1948. Elizabeth was a physician who qualified in 1951. The Kendrews divorced in 1956.**

4 THE TIMES TUESDAY OCTOBER 30

MR. CHURCHILL'S PROGRESS IN FORMING GOVERNMENT

DAY OF INTERVIEWS WITH POLITICAL COLLEAGUES

Mr. Churchill continued the task of forming his Government yesterday, and interviewed several of his political colleagues at his home in Kent.

A new Speaker of the House of Commons will be elected when Parliament meets to-morrow.

ELECTION OF NEW SPEAKER

LIKELY CANDIDATES

From Our Parliamentary Correspondent

Mr. Churchill spent a busy day yesterday at his country home at Chartwell, Westerham, where he continued his task of forming a Government and had consultations with many of his political colleagues. There are still seven or eight members of the Cabinet to be appointed, including a Lord Chancellor, together with nearly a score of Ministers who will not be in the Cabinet, the Law Officers, and some 30 Under-Secretaries. It is expected that a further list of appointments will be made known to-day.

Mr. R. A. Butler, the new Chancellor of the Exchequer, was in conference with Mr. Churchill for a long time yesterday afternoon. He was accompanied to Chartwell by Sir Edward Bridges, Permanent Secretary to the Treasury. Before this Mr. [illegible] some constituency parties to record only four votes, instead of the seven to which they were entitled, and to limit them to the four previous Bevan supporters on the executive—Mr. Bevan, Mrs. Castle, Mr. Driberg, and Mr. Mikardo.

Some unionists think that the Bevanites should be warned that if that sort of procedure is repeated, the unions will press for a reversion to the old constitution under which all delegates to the conference voted for the constituency representatives on the executive. There are still some leaders who would like to do that in any case, but the question will not arise until the time comes for putting down resolutions for next year's conference, and much may happen before then. Many Labour leaders think that now they are in opposition it will prove less difficult to reconcile the differences between the Bevan group and the rest.

The main business before the executive will be the appointment of the various sub-committees. Arrangements will also have to be made for the appointment of a national agent in place of the late Mr. E. F. Windle and of a chief research officer in place of Mr. W. Fienburgh, who has been elected to Parliament.

Labour's policy in opposition will be discussed at a joint meeting between the executive and the Parliamentary party at the House of Commons to-morrow morning. This joint meeting is normal procedure at the beginning of a new Parliament. [illegible]

MR. EDEN REVIEWS OIL DISPUTE

AMBASSADOR ON WAY HOME

LONDON CONSULTATIONS

From Our Diplomatic Correspondent

Mr. Eden, the Foreign Secretary, has asked Sir Francis Shepherd, the British Ambassador to Persia, to come home for consultations. Sir Francis Shepherd has cut short a holiday in Beirut and is understood to be already on the way to London.

The Ambassador's recall does not necessarily presage a new move by the British Government towards settlement of the oil dispute. Mr. Eden, before going to Paris next week for the opening meetings of the United Nations General Assembly, wished to have the views of the Ambassador on the possibilities of a settlement, as seen by an observer in Teheran. In Paris he will doubtless review the situation with Mr. Acheson, the United States Secretary of State, in the light both of the Ambassador's views and information and of the conversations which continue in Washington between the United States Government and the Persian Prime Minister. The United States Government has been trying to discover whether Dr. Moussadek is receptive to a basis for negotiation which the United Kingdom might also consider reasonable.

PREMATURE HOPE

It is not known that the researches have yielded any positive results; and Washington may well have found Dr. Moussadek still stubbornly insisting that British interests in the Persian oil industry are limited to opportunities for purchasing its products and to commerce[illegible]

ROYAL VISIT TO MONTREAL

GREETING BY CROWD OF 250,000

From Our Special Correspondent

MONTREAL, Oct. 29

The crowds that welcomed Princess Elizabeth and the Duke of Edinburgh to-day in Montreal were the largest of the tour. Montreal had declared that it was going to give the Princess the biggest welcome in the whole of Canada. There was a huge assembly in Dominion Square, and the people roared their greeting as the royal car came into view and the Princess and the Duke went into the Windsor Hotel.

In a few minutes they came out on to the magnificently draped balcony, which had been arranged like a small stage, with pastel blue and white hangings on the inside, and red, white, and blue curtains, draped from above, falling round the edge of the balcony, which was floodlit. A tremendous cheer went up as the Princess and the Duke appeared. The Royal couple stayed for several minutes, but when they had gone in the crowd set up a great chant asking them to come back, and in spite of the presence of 500 police in front of the hotel the crowd broke through the lines and pushed their way under the balcony, still shouting for the Princess to come out again.

The police tried desperately hard to keep the crowd back, but people forced their way right up to the hotel steps chanting, "We want the Princess." Mounted police backed their horses into the crowd without much effect. The people did not disperse until the Princess had made a long second appearance.

All the way from the airport, over 18 miles of route, there was hardly a space on either side of the road that did not have cars parked or crowds of people lined up, waving flags. The arrival in Montreal was exactly three weeks to the day after the Princess and the Duke had landed on the same airfield at the[illegible]

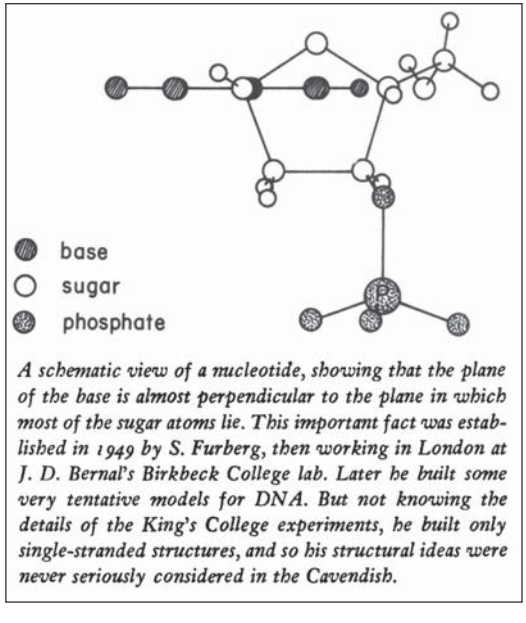
This is the headline in The Times *of October 30, reporting the early days of the new Tory government. In the General Election of October 23, 1951, the Labour government of Clement Attlee had been ousted and Sir Winston Churchill became Prime Minister for the second time.*

Both Francis and I, however, knew that the models in the Cavendish would not be completely satisfactory. They had been constructed by John some eighteen months before, for the work on the three-dimensional shape of the polypeptide chain. There existed no accurate representations of the groups of atoms unique to DNA. Neither phosphorus atoms nor the purine and pyrimidine bases were on hand. Rapid improvisation would be necessary since there was no time for Max to give a rush order for their construction. Making brand-new models might take all of a week, whereas an answer was possible within a day or so. Thus as soon as I got to the lab I began adding bits of copper wire to some of our carbon-atom models, thereby changing them into the larger-sized phosphorus atoms.[2]

Much more difficulty came from the necessity to fabricate representations of the inorganic ions. Unlike the other constituents, they obeyed no simple-minded rules telling us the angles at which they would form their respective

Sven Furberg, 1950.

[2] **Watson refers to Sven Furberg's determination of the structure of cytidine in the caption of this figure (*right*). Published in *Nature* in 1949, it is recognized as a tour-de-force. Furberg was a Norwegian physical chemist who spent two years with Bernal at Birkbeck College.**



A schematic view of a nucleotide, showing that the plane of the base is almost perpendicular to the plane in which most of the sugar atoms lie. This important fact was established in 1949 by S. Furberg, then working in London at J. D. Bernal's Birkbeck College lab. Later he built some very tentative models for DNA. But not knowing the details of the King's College experiments, he built only single-stranded structures, and so his structural ideas were never seriously considered in the Cavendish.

Wolfenstein. The rates obtained by Lewis and by Williams at the higher altitude for a chamber separation of 10 metres and for showers of the same density are also much greater than the predicted rate, by factors of 280 and 12 respectively. Part of the discrepancy may well be attributed to Wolfenstein's inclusion of the zenith angle effect, whereas recent experiments[2] indicate that extensive showers penetrating to the lower altitudes are incident nearly vertically.

Further details of the present work are to appear in the *Australian Journal of Scientific Research*, Series A, **2** (1949).

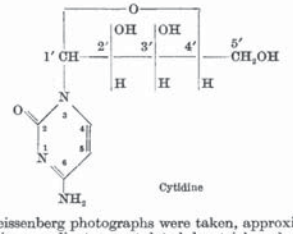
J. R. Prescott
C. B. O. Mohr

Physics Department,
University of Melbourne.
March 17.

[1] Lewis, L. G., *Phys. Rev.*, **67**, 228 (1945).
[2] Williams, R., *Phys. Rev.*, **74**, 1689 (1948).
[3] Euler, H., *Z. Phys.*, **116**, 73 (1940).
[4] Wolfenstein, L., *Phys. Rev.*, **67**, 238 (1945).
[5] Carmichael, H., *Phys. Rev.*, **74**, 1667 (1948).
[6] Bridge, H., Hazen, W., Rossi, B., and Williams, R., *Phys. Rev.*, **74**, 1083 (1948).
[7] Bernadini, G., Cortini, C., and Manfredini, A., *Phys. Rev.*, **74**, 845 (1948).
[8] Montgomery, C. G., and Montgomery, D. D., *Phys. Rev.*, **72**, 131 (1947).
[9] Alichanian, A., and Asatiani, T., *J. Phys. U.S.S.R.*, **9**, 175 (1945).

Crystal Structure of Cytidine

A study of the crystal structure of cytidine is being carried out by X-ray analysis. The crystal specimens were kindly supplied by Dr. D. O. Jordan, University of Nottingham, and were found to be orthorhombic with {110} dominating. An optical investigation shows that the sign is positive, with $\alpha \| c$, $\beta \| b$ and $\gamma \| a$. Cell dimensions are: $a = 13{\cdot}93$ A., $b = 14{\cdot}75$ A., $c = 5{\cdot}10$ A.; density, 1·53; four molecules per unit cell; space-group, $P\,2_12_12_1$.



Cytidine

Weissenberg photographs were taken, approximate atomic co-ordinates postulated by trial and error, and the Fourier map of the 001-projection shown in Fig. 1 eventually obtained. This map is now being refined.

Fig. 2 gives the interpretation of the peaks. The chemical formula is fully confirmed, thus showing cytidine to be cytosine-3-*d*-ribofuranoside. The glycosidic linkage is of the β-type, in accordance with the findings of Davoll, Lythgoe and Todd[1]. The bond-angles to the atom C_1' of the five-membered ring are not far from the tetrahedral angle, and the planes of the two ring systems are nearly perpendicular to each other. Details of the structure cannot be given at this stage; but the pyrimidine-ring appears

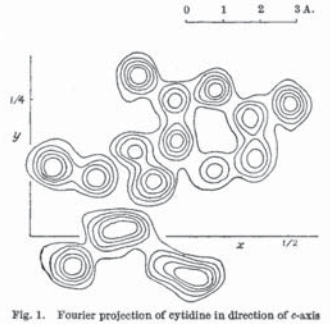


Fig. 1. Fourier projection of cytidine in direction of *c*-axis

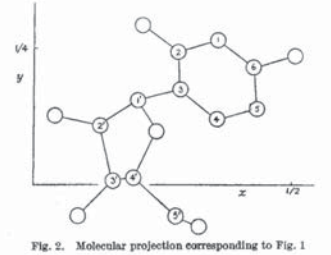


Fig. 2. Molecular projection corresponding to Fig. 1

to be flat, and there is some indication that the ribose-ring may not be planar.

Neighbouring molecules in the crystal are held together by hydrogen bonds.

It is hoped to publish later a more detailed account of the structure.

X-ray work on cytidylic acid is also in progress. The cell-dimensions are $a = 8{\cdot}74$ A., $b = 21{\cdot}4$ A., $c = 6{\cdot}82$ A., and the space-group $P\,2_12_12$.

S. Furberg

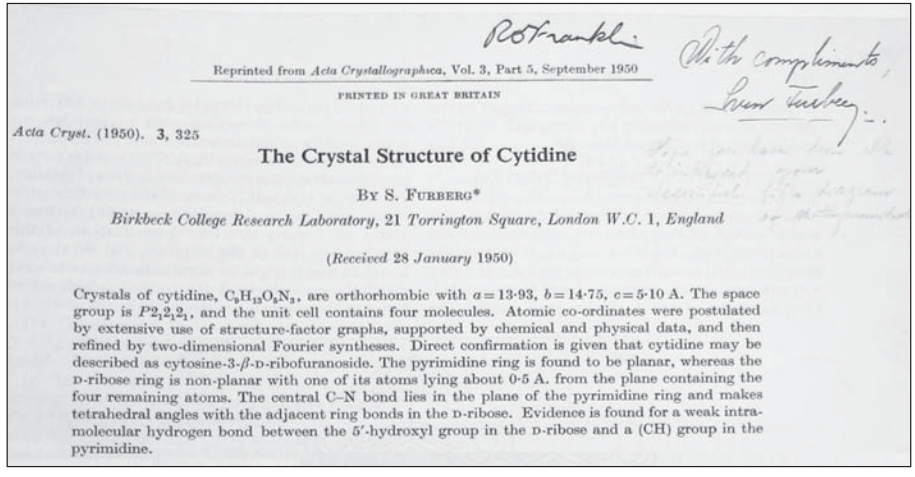
Birkbeck College Research Laboratory,
21 Torrington Square,
London, W.C.1.

[1] Davoll, Lythgoe and Todd, *J. Chem. Soc.*, 833 (1946).

Runge Bands of O_3 in Flame Spectra

In a recent note, Hornbeck[1] has reported observing the Runge emission bands of O_3, $^2\Sigma_u^- \rightarrow {}^2\Sigma_g^-$, in the spectra of explosion flames of carbon monoxide and oxygen, and has shown that this banded structure is favoured relative to the continuous background by excess of oxygen in the mixture. In a diffusion flame of carbon monoxide burning in oxygen at atmospheric pressure, we have confirmed the presence of the Runge bands, the (0,13), (0,14) and (0,15) bands, with heads at 3233, 3370 and 3516 A., being conspicuous. These bands, however, are emitted by a different part of the flame from the main carbon monoxide flame spectrum, and it is clear that the

*Furberg's paper on the structure of cytidine (*Nature, *July 2, 1949).*



R.E. Franklin

With compliments,
Yours Furberg

Reprinted from *Acta Crystallographica*, Vol. 3, Part 5, September 1950

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Acta Cryst. (1950). **3**, 325

The Crystal Structure of Cytidine

BY S. FURBERG*

Birkbeck College Research Laboratory, 21 Torrington Square, London W.C. 1, England

(*Received* 28 *January* 1950)

Crystals of cytidine, $C_9H_{13}O_4N_3$, are orthorhombic with $a = 13{\cdot}93$, $b = 14{\cdot}75$, $c = 5{\cdot}10$ A. The space group is $P2_12_12_1$, and the unit cell contains four molecules. Atomic co-ordinates were postulated by extensive use of structure-factor graphs, supported by chemical and physical data, and then refined by two-dimensional Fourier syntheses. Direct confirmation is given that cytidine may be described as cytosine-3-β-D-ribofuranoside. The pyrimidine ring is found to be planar, whereas the D-ribose ring is non-planar with one of its atoms lying about 0·5 A. from the plane containing the four remaining atoms. The central C–N bond lies in the plane of the pyrimidine ring and makes tetrahedral angles with the adjacent ring bonds in the D-ribose. Evidence is found for a weak intramolecular hydrogen bond between the 5′-hydroxyl group in the D-ribose and a (CH) group in the pyrimidine.

Rosalind Franklin's copy of Furberg's definitive paper on the structure of cytidine. Furberg's barely legible note beneath his signature reads "Hope you have been able to interpret your beautiful fibre diagram of Na-thymonucleate." Unfortunately we do not know when Franklin received this reprint.

chemical bonds. Most likely we had to know the correct DNA structure before the right models could be made. I maintained the hope, however, that Francis might already be on to the vital trick and would immediately blurt it out when he got to the lab. Over eighteen hours had passed since our last conversation, and there was little chance that the Sunday papers would have distracted him upon his return to the Green Door.

His tenish entrance, however, did not bring the answer. After Sunday supper he had again run through the dilemma but saw no quick answer. The problem was then put aside for a rapid scanning of a novel on the sexual misjudgments of Cambridge dons. The book had its brief good moments, and even in its most ill-conceived pages there was the

A Perch in Paradise.

[3] **The novel was *A Perch in Paradise*, by Margaret Bullard (Hamish Hamilton, 1952). Bertrand Russell enjoyed it, too. In a letter to Bullard on April 10, 1952, he wrote:**

"If Cambridge is as you represent it, it must have become more amusing since I was an undergraduate, which was in the early '90s. In those days we were all strictly celibate, which cannot be said of your characters. I am finding your novel amusing and pleasant reading and am hoping that it gives a true picture of Cambridge life."

Herbert (Freddie) Gutfreund flanked by Crick and Watson, outside Clare College, 1952.

Hugh Huxley, Kendrew's Ph.D. student and Ann Cullis, Max Perutz' assistant, 1950s.

question of whether any of their friends' lives had been seriously drawn on in the construction of the plot.[3]

Over morning coffee Francis nonetheless exuded confidence that enough experimental data might already be on hand to determine the outcome. We might be able to start the game with several completely different sets of facts and yet always hit the same final answers. Perhaps the whole problem would fall out just by our concentrating on the prettiest way for a polynucleotide chain to fold up. So while Francis continued thinking about the meaning of the X-ray diagram, I began to assemble the various atomic models into several chains, each several nucleotides in length. Though in nature DNA chains are very long, there was no reason to put together anything massive. As long as we could be sure it was a helix, the assignment of the positions for only a couple of nucleotides automatically generated the arrangement of all the other components.

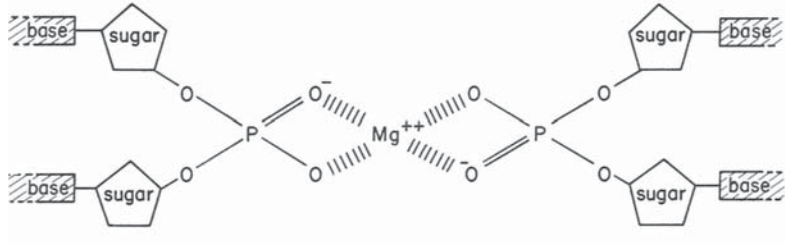
The routine assembly task was over by one, when Francis and I walked over to the Eagle for our habitual lunch with the chemist Herbert Gutfreund. These days John usually went to Peterhouse, while Max always cycled home. Occasionally John's student Hugh Huxley would join us, but of late he was finding it difficult to enjoy Francis' inquisitive lunchtime attacks. For just prior to my arrival in Cambridge, Hugh's decision to take up the problem of how

muscles contract had focused Francis' attention on the unforeseen opportunity that, for twenty years or so, muscle physiologists had been accumulating data without tying them into a self-consistent picture. Francis found it a perfect situation for action. There was no need for him to ferret out the relevant experiments since Hugh had already waded through the undigested mass. Lunch after lunch, the facts were put together to form theories which held for a day or so, until Hugh could convince Francis that a result he would like ascribed to experimental error was as solid as the Rock of Gibraltar.[4] Now the construction of Hugh's X-ray camera was completed, and soon he hoped to get experimental evidence to settle the debatable points. The fun would be all lost if somehow Francis could correctly predict what he was going to find.

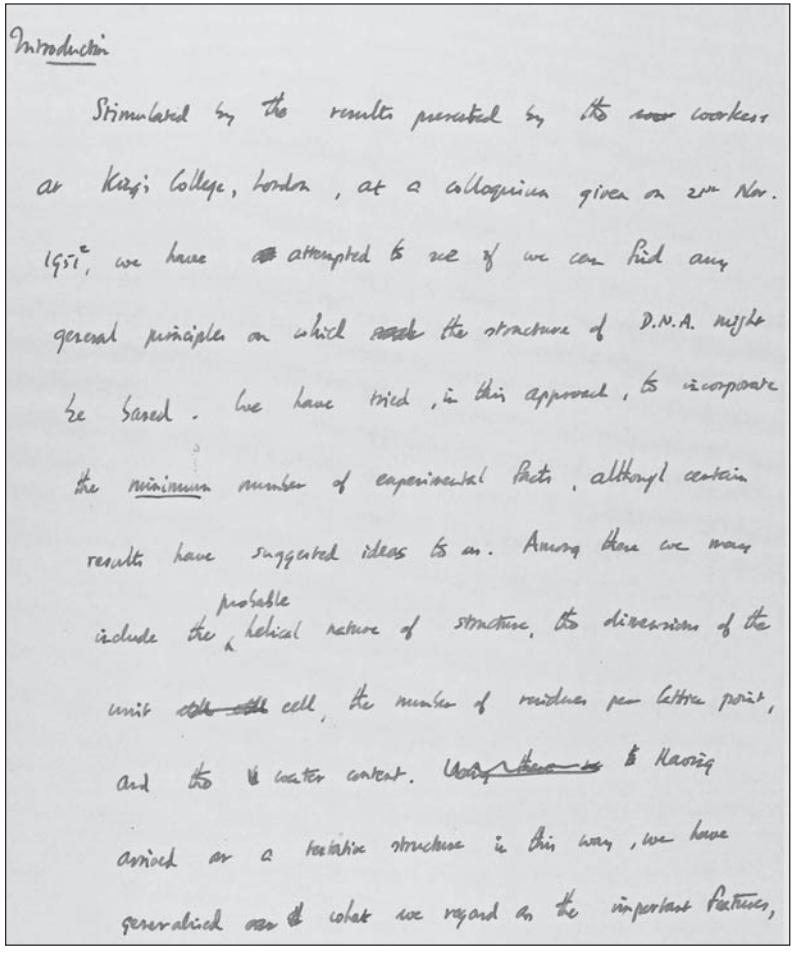
But there was no need that day for Hugh to fear a new intellectual invasion. When we walked into the Eagle, Francis did not exchange his usual raucous greetings with the Persian economist Ephraim Eshag, but gave the undistilled impression that something serious was up.[5] The actual model building would start right after lunch, and more concrete plans must be formulated to make the process efficient. So over our gooseberry pie we looked at the pros and cons of one, two, three, and four chains, quickly dismissing one-chain helices as incompatible with the evidence in our hands. As to the forces that held the chains together, the best guess seemed to be salt bridges in which divalent cations like Mg^{++} held together two or more phosphate groups. Admittedly there was no evidence that Rosy's samples contained any divalent ions, and so

[4] The Rock of Gibraltar is the promontory, 1400 feet high, bordering Spain and guarding the entrance to the Mediterranean. It has been in British hands since the Treaty of Utrecht in 1713. Despite numerous sieges, the Rock has never been captured, hence the expression used by Watson.

[5] Eprime Eshag was an Iranian economist, an ardent follower of John Maynard Keynes, who came to Cambridge to work for a Ph.D. thesis on the history of monetary theory. He worked for the United Nations before joining Wadham College, Oxford. According to his obituary he was also "an unrepentant man of many girlfriends" who married late, in 1992, dying 6 years later at the age of 80.



How Mg^{++} might be used to bind negatively charged phosphate groups in the center of a compound helix.



Introduction

Stimulated by the results presented by the new workers at King's College, London, at a colloquium given on 2nd Nov. 1951, we have attempted to see if we can find any general principles on which the structure of D.N.A. might be based. We have tried, in this approach, to incorporate the minimum number of experimental facts, although certain results have suggested ideas to us. Among these we may include the probable helical nature of structure, the dimensions of the unit cell, the number of residues per lattice point, and the water content. Working & Having arrived at a tentative structure in this way, we have generalized what we regard as the important features,

First page of Crick's memo on their triple helix model.

[6] **This is the first page of a memorandum Crick wrote soon after Franklin's colloquium, setting out the principles which would guide Watson and him in devising a structure for DNA. In contrast to Franklin's view that experimental data were paramount, they would try "…to incorporate the *minimum* number of experimental facts" while acknowledging "…that certain results have suggested ideas to us." Crick emphasized that care had to be taken not to reject a model just "…because of some difficulty which will sort itself out at a later stage."**

we might be sticking our necks out. On the other hand, there was absolutely no evidence against our hunch.[6] If only the King's groups had thought about models, they would have asked which salt was present and we would not be placed in this tiresome position. But, with luck, the addition of magnesium or

possibly calcium ions to the sugar-phosphate backbone would quickly generate an elegant structure, the correctness of which would not be debatable.

Our first minutes with the models, though, were not joyous. Even though only about fifteen atoms were involved, they kept falling out of the awkward pincers set up to hold them the correct distance from one another. Even worse, the uncomfortable impression arose that there were no obvious restrictions on the bond angles between several of the most important atoms. This was not at all nice. Pauling had cracked the α-helix by ruthlessly following up his knowledge that the peptide bond was flat. To our annoyance, there seemed every reason to believe that the phosphodiester bonds which bound together the successive nucleotides in DNA might exist in a variety of shapes. At least with our level of chemical intuition, there was unlikely to be any single conformation much prettier than the rest.

After tea, however, a shape began to emerge which brought back our spirits. Three chains twisted about each other in a way that gave rise to a crystallographic repeat every 28 Å along the helical axis. This was a feature demanded by Maurice's and Rosy's pictures, so Francis was visibly reassured as he stepped back from the lab bench and surveyed the afternoon's effort. Admittedly a few of the atomic contacts were still too close for comfort, but, after all, the fiddling had just begun. With a few hours' more work, a presentable model should be on display.

Ebullient spirits prevailed during the evening meal at the Green Door. Though Odile could not follow what we were saying, she was obviously cheered by the fact that Francis was about to bring off his second triumph within the month. If this course of events went on, they would soon be rich and could own a car. At no moment did Francis see any point in trying to simplify the matter for Odile's benefit. Ever since she had told him that gravity went only three miles into the sky, this aspect of their relationship was set. Not only did she not know any science, but any attempt to put some in her head would be a losing fight against the years of her convent upbringing. The most to hope for was an appreciation of the linear way in which money was measured.

Our conversation instead centered upon a young art student then about to marry Odile's friend Harmut Weil. This capture was mildly displeasing to

Members of the M.R.C. Biophysics Unit at the annual Cricket match (1950s). From left to right: Maurice Wilkins, William (Willy) Seeds, Bruce Fraser, Mary Fraser, Ray Gosling (standing), Geoffrey Brown.

Francis. It was about to remove the prettiest girl from their party circle. Moreover, there was more than one thing cloudy about Harmut. He had come out of a German university tradition that believed in dueling.[7] There was also his undeniable skill in persuading numerous Cambridge women to pose for his camera.

All thought of women, however, was banished by the time Francis breezed into the lab just before morning coffee. Soon, when several atoms had been pushed in or out, the three-chain model began to look quite reasonable. The next obvious step would be to check it with Rosy's quantitative measurements. The model would certainly fit with the general locations of the X-ray reflections, for its essential helical parameters had been chosen to fit the seminar facts I had conveyed to Francis. If it were right, however, the model would also accurately predict the relative intensities of the various X-ray reflections.

A quick phone call was made to Maurice. Francis explained how the helical diffraction theory allowed a rapid survey of possible DNA models, and that he and I had just come up with a creature which might be the answer

[7] **Alexander Todd (pictured on page 46) went to Frankfurt in 1929 to do research for his Ph.D. He recounts in his autobiography how he attended a duel, at 5 a.m., the object of which was for each opponent to inflict a wound on the face of the other. Afterwards, the combatants and observers "despite the early hour consumed vast quantities of beer" at a nearby inn.**

we were all awaiting. The best thing would be for Maurice immediately to come and look it over. But Maurice gave no definite date, saying he thought he might make it sometime within the week. Soon after the phone was put down, John came in to see how Maurice had taken the news of the breakthrough. Francis found it hard to sum up his reply. It was almost as if Maurice were indifferent to what we were doing.

In the midst of further fiddling that afternoon, a call came through from King's. Maurice would come up on the 10:10 train from London the following morning. Moreover, he would not be alone. His collaborator Willy Seeds would also come. Even more to the point was that Rosy, together with her student R. G. Gosling, would be on the same train. Apparently they were still interested in the answer.

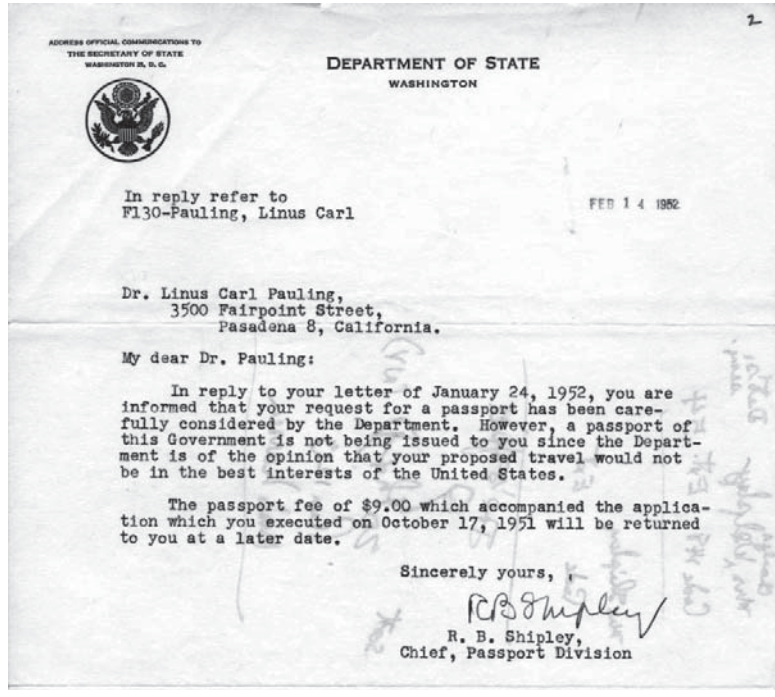
Chapter 17

Linus, however, was blocked from descending on London. His trip abruptly terminated at Idlewild through the removal of his passport.[1] The State Department did not want troublemakers like Pauling wandering about the globe saying nasty things about the politics of its onetime investment bankers who held back the hordes of godless Reds. Failure to contain Pauling might result in a London press conference with Linus expounding peaceful coexistence. Acheson's position was harassed enough without giving McCarthy the opportunity to announce that our government let radicals protected by U.S. passports set back the American way of life.

Francis and I were already in London when the scandal reached the Royal Society. The reaction was one of almost complete disbelief. It was far more reassuring to go on imagining that Linus had taken ill on the plane to

[1] **Initially named after the golf course it displaced, Idlewild was renamed John F. Kennedy International Airport on December 24, 1963, one month after the assassination of President Kennedy.**

Idlewild Airport observation deck, late 1940s.



2

ADDRESS OFFICIAL COMMUNICATIONS TO THE SECRETARY OF STATE WASHINGTON 25, D. C.

DEPARTMENT OF STATE
WASHINGTON

In reply refer to
F130-Pauling, Linus Carl

FEB 14 1952

Dr. Linus Carl Pauling,
3500 Fairpoint Street,
Pasadena 8, California.

My dear Dr. Pauling:

In reply to your letter of January 24, 1952, you are informed that your request for a passport has been carefully considered by the Department. However, a passport of this Government is not being issued to you since the Department is of the opinion that your proposed travel would not be in the best interests of the United States.

The passport fee of $9.00 which accompanied the application which you executed on October 17, 1951 will be returned to you at a later date.

Sincerely yours,

R. B. Shipley,
Chief, Passport Division

Letter from Shipley to Pauling.

[2] **Letter from Ruth B. Shipley, Chief of the Passport Division of the State Department, to Pauling, February 14, 1952. Addressing him as "My dear Dr. Pauling," she nevertheless denies his application for a passport "since the Department is of the opinion that your proposed travel would not be in the best interests of the United States." Shipley was chief of the division from 1928 until 1955, and wielded almost complete power over who got a passport and who didn't. Franklin Roosevelt described her as a "wonderful ogre," Secretary of State Dean Acheson said the Passport Division was her "Queendom of Passports," and in December 1951 *Time* magazine claimed she was "the most invulnerable, most unfirable, most feared and most admired career woman in Government."**

New York. The failure to let one of the world's leading scientists attend a completely nonpolitical meeting would have been expected from the Russians. A first-rate Russian might easily abscond to the more affluent West. No danger existed, however, that Linus might want to flee. Only complete satisfaction with their Cal Tech existence came from him and his family.[2]

Several members of Cal Tech's governing board, however, would have been delighted with his voluntary departure. Every time they picked up a newspaper and saw Pauling's name among the sponsors of a World Peace Conference they seethed with rage, wishing there were a way to rid Southern California of his pernicious charm. But Linus knew better than to expect more than confused anger from the selfmade California millionaires

PAULING RAPS PASSPORT BAN

Caltech Man Denounces U. S. Refusal of Trip to London

"The damage done to the nation by the refusal to permit me to attend scientific meetings in England must be attributed to the McCarran Act, and is an argument for the repeal of this act."

Dr. Linus Pauling, California Institute of Technology chemistry department head, made this statement yesterday in discussing the State Department's refusal to issue him a passport, as disclosed in yesterday's Examiner.

He had accepted an invitation to speak before the Royal Society of London.

On April 28, despite an earlier appeal by letter to President Truman, officials of the State Department upheld an original refusal to issue him a passport on the ground "that my proposed travel would not be in the best interests of the United States."

Advised on April 21 by a State Department official that the decision was made because he was suspected of being a Communist, Dr. Pauling stated:

"I then submitted to the Department of State my statement, made under oath, that I am not a Communist, never have been a Communist, and never have been involved with the Communist Party, as well as other documents.

SCIENTIST—Dr. Linus Pauling stands beside a mod showing protein structure that is the same as that in the horn holds. He protested U. S. refusal to permit his attendance London scientists' meeting where he was to explain his theorie —Los Angeles Examiner phot

"The action of the State Department in refusing me a passport represents a different way of interfering with the progress of science and restricting the freedom of the individual citizen. In my opinion, it reflects a dangerous trend away from our fundamental democratic principles, upon which our nation is based."

Los Angeles Examiner *article.*

AN AMERICAN SCIENTIST

TO THE EDITOR OF THE TIMES

Sir,—On May 1 (possibly an unfortunate choice of day) the Royal Society held a significant symposium on the progress in our knowledge of proteins. As I had the honour to be President of the Royal Society when Professor Linus Pauling, of Pasadena, was awarded the Davy Medal (1947) and again when he was elected a foreign member (1948), it is perhaps appropriate that I should express the keen disappointment generally felt when it was learned that he had not been granted the necessary permit to make the journey to England in order to participate in the discussion. Pauling had an important contribution to make, and it is deplorable that we were deprived of the opportunity to talk it over with him.

It would be insincere to pretend that we have no inkling of the reason for the drastic action taken by the American authorities in this and several similar cases (*e.g.*, that of Dr. E. B. Chain), but that does not lessen our surprise and consternation. It is an ironical circumstance that Pauling's theoretical views have been criticized in the U.S.S.R. as incorrect, western, and bourgeois; or, alternatively, as partly correct but anticipated by the Russian chemist Butlerow. To avoid any misunderstanding it must be added that I am not writing on behalf of the Royal Society.

Yours faithfully,

ROBERT ROBINSON.

The Dyson Perrins Laboratory, South Parks Road, Oxford, May 2.

Letter to The Times.

whose knowledge of foreign policy was formed largely by the *Los Angeles Times*.[3]

The debacle was no surprise to several of us who had just been in Oxford for a Society of General Microbiology meeting on "The Nature of Viral Multiplication." One of the main speakers was to have been Luria. Two weeks prior to his scheduled flight to London, he was notified that he would

[3] **The withholding of Pauling's passport received comment on both sides of the Atlantic. On the left, a piece printed in the *Los Angeles Examiner* from May 12, and, above, a letter from Sir Robert Robinson, former President of the Royal Society, to *The Times* on May 2. Probably because of such press attention, the State Department reversed its decision soon afterwards, allowing Pauling to travel later that summer (see Chapter 19).**

THE NATURE OF VIRUS MULTIPLICATION

SECOND SYMPOSIUM OF THE
SOCIETY FOR GENERAL MICROBIOLOGY
HELD AT OXFORD UNIVERSITY
APRIL 1952

CAMBRIDGE
Published for the Society for General Microbiology
AT THE UNIVERSITY PRESS
1953

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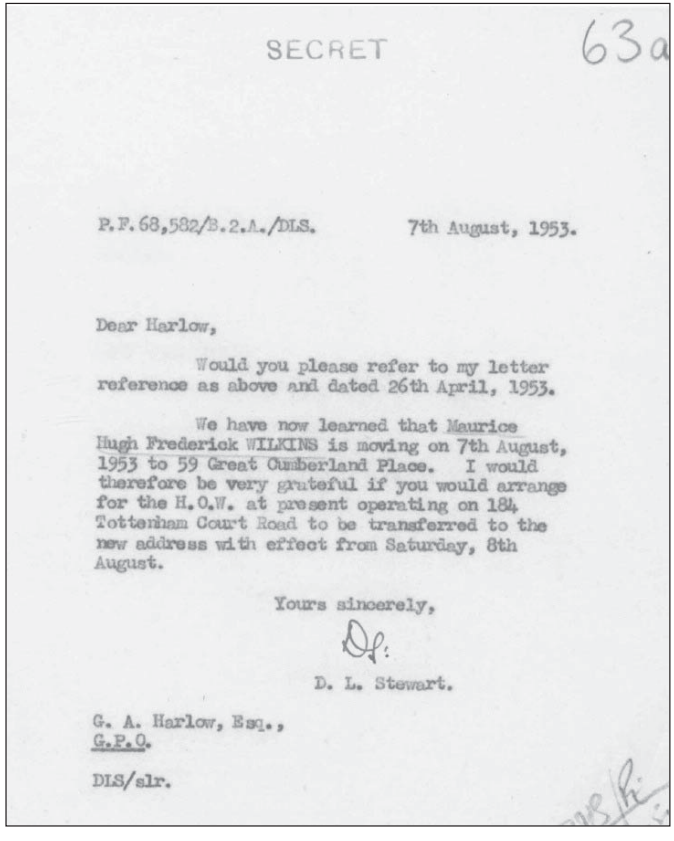
Although Luria's name and article appear in the published volume of the Symposium proceedings, he was refused a visa and did not attend the meeting.



SECRET 63a

P.F.68,582/B.2.A./DLS. 7th August, 1953.

Dear Harlow,

Would you please refer to my letter reference as above and dated 26th April, 1953.

We have now learned that Maurice Hugh Frederick WILKINS is moving on 7th August, 1953 to 59 Great Cumberland Place. I would therefore be very grateful if you would arrange for the H.O.W. at present operating on 184, Tottenham Court Road to be transferred to the new address with effect from Saturday, 8th August.

Yours sincerely,

D. L. Stewart.

G. A. Harlow, Esq.,
G.P.O.

DLS/slr.

One of many documents in Wilkins' MI5 files.

not get a passport. As usual, the State Department would not come clean about what it considered dirt.[4]

Luria's absence thrust upon me the job of describing the recent experiments of the American phage workers. There was no need to put together a speech. Several days before the meeting, Al Hershey had sent me a long letter from Cold Spring Harbor summarizing the recently completed experiments by

[4] **In a letter to his sister on April 3, 1952, Watson writes:**

"I have just learned from mother that Luria will not be coming. I do not know the reason but I would suspect passport difficulties. I am quite sorry that he isn't coming since I had hoped to discuss my future with him. Now I shall have to do some involved letter writing."

Left-leaning attitudes among scientists weren't restricted to Pauling and Luria, nor fear of them exclusive to the U.S. authorities: Maurice Wilkins had been investigated by MI5 and the FBI. They suspected that one of nine scientists from New Zealand or Australia had leaked A-bomb secrets. Wilkins was one of the nine suspects, having worked on the Manhattan project (see Chapter 2). The investigation began in 1945, but was still going on in 1953, as indicated by this request that the H.O.W. (Home Office Warrant, which allowed his mail to be searched) be transferred to his new address. His phone was also tapped. All this despite an informant suggesting around the same time that while Wilkins was "a very queer fish," he was probably a socialist rather than a communist.

[5] François Jacob, then still a student with Lwoff, encountered Watson for the first time at this meeting at Oxford, recording the incident in his autobiography *The Statue Within*.

"At that time, to a French student who had not yet been inside an American university or seen its denizens, Jim Watson was an amazing character. Tall, gawky, scraggly, he had an inimitable style. Inimitable in his dress: shirttails flying, knees in the air, socks down around his ankles. Inimitable in his bewildered manner, his mannerisms: his eyes always bulging, his mouth always open, he uttered short, choppy sentences punctuated by 'Ah! Ah!' Inimitable also in his way of entering a room, cocking his head like a rooster looking for the finest hen, to locate the most important scientist present and charging over to his side. A surprising mixture of awkwardness and shrewdness. Of childishness in the things of life and of maturity in those of science."

Watson in shorts, Cold Spring Harbor, 1953.

Martha Chase and Al Hershey, 1953.

which he and Martha Chase established that a key feature of the infection of a bacterium by a phage was the injection of the viral DNA into the host bacterium. Most important, very little protein entered the bacterium. Their experiment was thus a powerful new proof that DNA is the primary genetic material.

Nonetheless, almost no one in the audience of over four hundred microbiologists seemed interested as I read long sections of Hershey's letter. Obvious exceptions were André Lwoff, Seymour Benzer, and Gunther Stent, all briefly over from Paris. They knew that Hershey's experiments were not trivial and that from then on everyone was going to place more emphasis on DNA. To most of the spectators, however, Hershey's name carried no weight. Moreover, when it came out that I was an American, my uncut hair provided no assurance that my scientific judgment was not equally bizarre.[5]

Max Delbrück (left) with André Lwoff at Cold Spring Harbor, 1953.

Dominating the meeting were the English plant virologists F. C. Bawden and N. W. Pirie. No one could match the smooth erudition of Bawden or the assured nihilism

F. C. Bawden.

N. W. Pirie (second from left).

of Pirie, who strongly disliked the notion that some phages have tails or that TMV is of fixed length. When I tried to corner Pirie about Schramm's experiments he said they should be dismissed, and so I retreated to the politically less controversial point of whether the 3000 Å length of many TMV particles was biologically important. The idea that a simple answer was preferable had no appeal to Pirie, who knew that viruses were too large to have well-defined structures.[6]

If it had not been for the presence of Lwoff, the meeting would have flopped totally. André was very keen about the role of divalent metals in phage multiplication and so was receptive to my belief that ions were decisively important for nucleic-acid structure. Especially intriguing was his hunch that specific ions might be the trick for the exact copying of macromolecules or the attraction between similar chromosomes. There was no way to test our dreams, however, unless Rosy did an about-face from her determination to rely completely on classical X-ray diffraction techniques.

At the Royal Society Meeting there was no hint that anyone at King's had mentioned ions since the confrontation with Francis and me in early December. Upon pressing Maurice, I learned that the jigs for the molecular models had not been touched after arriving at his lab. The time had not yet come to press Rosy and Gosling about building models. If anything, the

[6] **Having met at Cambridge in the late 1920s, Frederick Bawden and Norman Pirie subsequently worked together for many years at Rothamsted Experimental Station at Harpenden. They collaborated first on potato virus X and then, starting in 1936, on TMV. They worked with Bernal and Fankuchen (see Chapter 16) on determining the chemical nature of TMV, and were the first to show the presence of RNA in the viral preparations. François Jacob's autobiography describes them at this meeting as "…old cronies who loved to play the buffoon, trading jokes and metaphysical aphorisms, all in a rapid, choppy English which left me in a cold sweat."**

Pirie's later interests in a broad range of scientific and social issues are reflected in his picture above, which shows him leaving the Soviet Embassy with J. B. Priestly and others after talks on nuclear armament in September 1961.

squabbling between Maurice and Rosy was more bitter than before the visit to Cambridge. Now she was insisting that her data told her DNA was *not* a helix. Rather than build helical models at Maurice's command, she might twist the copper-wire models about his neck.

When Maurice asked whether we needed the molds back in Cambridge, we said yes, half implying that more carbon atoms were needed to make models showing how polypeptide chains turned corners. To my relief, Maurice was very open about what was not happening at King's. The fact that I was doing serious X-ray work with TMV gave him assurance that I should not soon again become preoccupied with the DNA pattern.

Watson in Paris on his way to the Riviera, spring 1952. He wrote to his sister (April 27): "I'm enclosing a photo of myself in Paris. It rather horrifies me since I did not realize how much hair I have. Needless to say I no longer have a crew cut."

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