

COMMENT

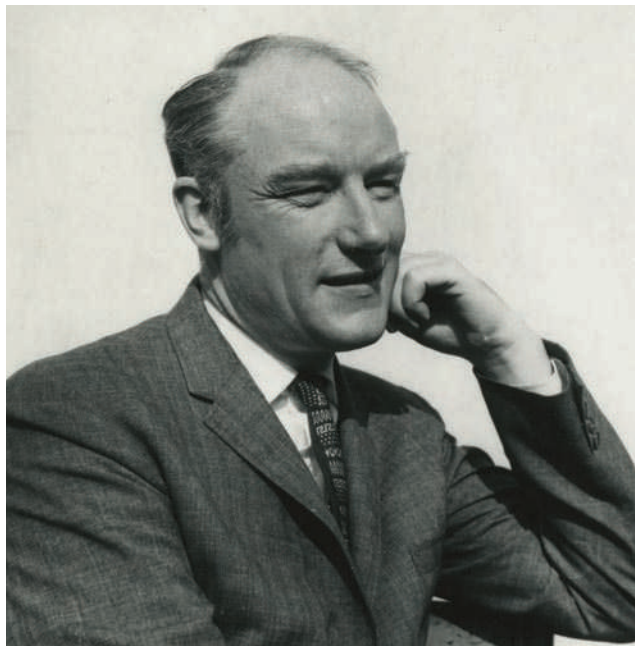
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The lost correspondence of Francis Crick

Alexander Gann and **Jan Witkowski** unveil newly found letters between key players in the DNA story. Strained relationships and vivid personalities leap off the pages.

In the summer of 1975, James Watson wrote to Francis Crick proposing that something be published on the story of the RNA Tie Club, an informal group of 24 members who exchanged ideas about RNA and the genetic code. Crick responded, on 16 July: “Almost all my own early correspondence was unfortunately thrown away without my knowledge by an over-efficient secretary.” The Wellcome Library in London, which acquired the majority of Crick’s professional papers from him in 2001, also quote this passage and warn that: “Researchers should note that there has been some loss of early correspondence.”

It turns out that this lost correspondence was never thrown out, but became mixed in with Sydney Brenner’s papers. Brenner and Crick shared an office in Cambridge from 1956 to 1977. They moved offices and buildings several times — from the Cavendish Laboratory to the ‘Hut’ to the new Medical Research Council (MRC) Laboratory of Molecular Biology (LMB), and between offices within the LMB. It is not surprising that some of Crick’s correspondence became intermingled with Brenner’s papers. A line in a 1961 letter from Crick to the

eminent phage geneticist Waclaw Szybalski supports this conjecture: “Do forgive me for not replying earlier to your letter of 15th December, but it arrived at Christmas time and got mislaid among Sydney Brenner’s papers.”

Earlier this year, we found the missing correspondence in the papers that Brenner donated to the Cold Spring Harbor Laboratory Library archives. The extensive Crick material, nine archive boxes of correspondence, photographs, postcards, preprints, reprints, meeting programmes, notes and newspaper cuttings, dates from 1950 to 1976, the bulk from the mid-1950s to the mid-1960s. (The catalogue of the complete Brenner Collection is at go.nature.com/6mYBhP.)

The letters of greatest interest, unveiled here for the first time, are those between Crick and Maurice Wilkins when they were both searching for the structure of DNA. They reveal telling details of the relations between the rival parties, and give vivid insights into the personalities involved. There is also previously unknown correspondence to and from other key players in the development of molecular biology.

COLD SPRING HARBOR LABORATORY ARCHIVES

Most relevant to Watson's 1975 enquiry about the RNA Tie Club are 30 letters between Crick and George Gamow, the club's founder. Other significant contributors include Alexander Rich, Leó Szilárd, Gunther Stent, Sol Spiegelman, Seymour Benzer, Charles Yanofsky, Paul Berg, Marianne Grunberg-Manago and Mahlon Hoagland.

The collection also includes letters on broader topics. One exchange, from 1963, is with C. P. Snow on his idea that the DNA story should be written up for a general audience (this was five years before Watson published *The Double Helix*). Another is with J. Robert Oppenheimer about having molecular biologists join the Institute for Advanced Study in Princeton, New Jersey. *Nature* writes for advice on who should replace editor John Maddox the first time, in 1973. R. W. Burchfield, editor of the *Oxford English Dictionary*, writes in 1964 for information about the word 'codon'. There are jokey postcards for friends, letters Crick marked to be filed under "lunatic" and two from Peter Wright, of *Spycatcher* fame, requesting a meeting in October 1962 to discuss an unidentified "confidential matter", to which Crick consents. The archive contains carbon copies of some of Crick's outgoing correspondence.

Crick's witty responses to requests are exemplified by this letter to D. C. Martin, executive secretary of the Royal Society, on 22 September 1967:

Dear Martin,

If we had to produce a caricature of the sort of visitor we do not like to have it would be someone working in a different field from ours, offering to give a lecture on a subject in which we are not interested, and being unable to speak English. Unfortunately Professor P----- manages to fulfil all these requirements. I am sure you will understand, therefore, that we feel there is little point in his paying us a visit.

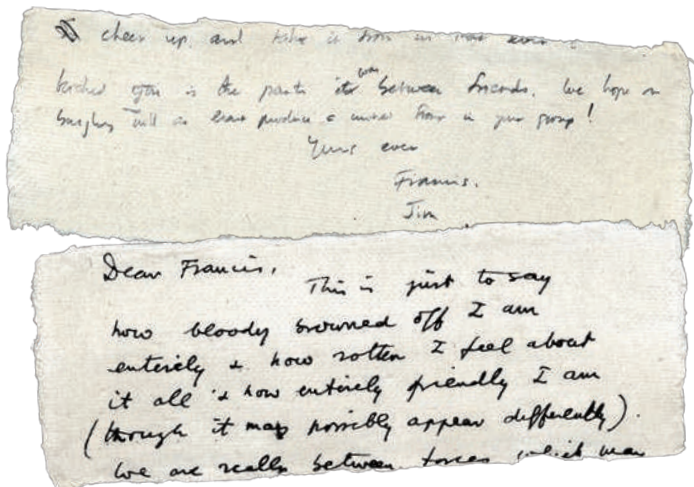
Yours sincerely

F. H. C. Crick

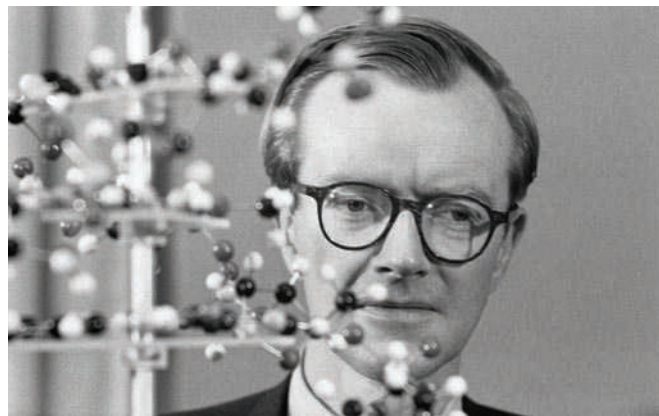
THE CRICK-WILKINS CORRESPONDENCE

Thirty-four of the new-found letters (and three postcards) are between Crick and Wilkins from 1951 to 1964; eleven were written between 1951 and 1953, as the structure of DNA was being pursued by Wilkins and Rosalind Franklin at King's College London and by Watson and Crick at the Cavendish Laboratory in Cambridge. Only one of these letters has previously been quoted or referred to in print — and that only as a short extract in Wilkins's book *The Third Man of the Double Helix*². The letters from this period are predominantly handwritten; presumably no other copies exist. We have selected quotations most relevant to the DNA story, including the authors' insertions, but we have not attempted to reproduce deleted text. However, words underlined in the original texts are underlined here and we have retained the original spellings;

COLD SPRING HARBOR LABORATORY ARCHIVES



Letters between Francis Crick and Maurice Wilkins reveal their contrasting characters.



CORBIS

Maurice Wilkins: the 'third man of the double helix'.

for example, Franklin is referred to as both Rosy and Rosie.

This recovered correspondence gives us a more nuanced sense of the interactions between the principal players in this most famous of scientific stories (see 'Cast list'). Throughout, we find letters that call to mind Brenda Maddox's line: "History can be grateful, for Wilkins penned another of his vivid letters."³

Each of the new letters can be linked to one of four important stages on the path to discovering and publishing the structure of DNA; we take these stages in turn.

DECEMBER 1951: THE FIASCO OF THE FIRST MODEL

On 21 November 1951, Franklin described her latest results in a colloquium at King's. Watson attended but left mistaken over the amount of water in the DNA structure — a misapprehension he passed on to Crick when they met at Paddington railway station en route to Oxford, where Crick wanted to discuss helical diffraction theory with Dorothy Hodgkin.

Watson's order-of-magnitude underestimate of the water content led Crick to believe that there were very few possible structures for DNA and the right one might be found through model building alone. In a week they had a model that satisfied the apparent restrictions and invited the people at King's to come and see the "clever thing" they had done. As soon as Franklin saw the model — a triple helix with the bases on the outside, the chains held together through electrostatic bridges between sodium ions and the phosphate groups — she knew it was wrong.

This debacle precipitated a moratorium on further DNA work for Watson and Crick, who were doing no experimental work of their own. By most accounts, John Randall, the head of the MRC unit at King's, and William Lawrence Bragg, his equivalent at the Cavendish, called this halt after a quiet chat³⁻⁵. But the recovered papers reveal correspondence between Wilkins and Crick in parallel to — perhaps even in place of — direct communication between Randall and Bragg. Thus, on 11 December 1951 we find a typed letter from Wilkins to Crick, which, despite a friendly opening — "My dear Francis" — soon adopts a rather formal tone:

I am afraid the average vote of opinion here, most reluctantly and with many regrets, is against your proposal to continue the work on n.a. [nucleic acids] in Cambridge. An argument here is put forward to show that your ideas are derived directly from statements made in the colloquium and this seems to me as convincing as your own argument that your approach is quite out of the blue...

... I think it most important that an understanding be reached such that all members of our laboratory can feel in future, as in the past, free to discuss their work and interchange ideas with you and your laboratory. We are two M.R.C. Units and two Physics Departments with many connections. I personally feel that I have much to gain by discussing my own work with you and after your attitude on Saturday begin to have very slight uneasy feelings in this respect.

Wilkins admits that if Watson and Crick were working in a laboratory remote from King's it would be a different matter, and that if the Cavendish people feel this stance unfair they should let the King's unit know. Wilkins suggests that Crick show the letter to Max Perutz (and by that route, perhaps it would go to Bragg as well) and that he is at Randall's request letting him have a copy. The strong sense that Wilkins is mediating between Randall and the Cavendish is reinforced by a distinctly less formal, handwritten letter, sent the same day, clearly free from Randall's oversight:

Dear Francis, This is just to say how bloody browned off I am entirely & how rotten I feel about it all & how entirely friendly I am (though it may possibly appear differently). We are really between forces which may grind all of us into little pieces... I had to restrain Randall from writing to Bragg complaining about your behaviour. Needless to say I did restrain him, but so far as your security with Bragg is concerned it is probably much more important to pipe down & build up the idea of a quiet steady worker who never creates 'situations' than to collect all the credit for your excellent ideas at the expense of good will.

And you see it does make me a bit confused about our discussions if you get too interested in everything which is important; where I say confused I mean confused, I am now largely incapable of any logical thinking in relation to polynucleotide chains or anything.

And poor Jim — may I shed a crocodile & very confused tear?

Wilkins ends with regards to his friend John Kendrew and as an afterthought at the top of the letter he adds: "(possibly you might like to show this to John)". Wilkins saw Kendrew, a senior figure at the Cavendish, as a sympathetic and skilled mediator². Wilkins may have believed that Kendrew could help things behind the scenes; or perhaps he merely wanted to be sure that his friend knew his true feelings and role in the affair.

A handwritten, heavily corrected, draft reply from Crick and Watson to Wilkins dated two days later on 13 December 1951 begins:

Dear Maurice,

Just a brief note to thank you for the letters and to try to cheer you up. We think the best thing to get things straight is for us to send you a letter setting out in a mild manner our point of view. This will take a day or so to do, so we hope you'll excuse the delay. Please don't

CAST LIST

The search for the structure of DNA

Cavendish Laboratory, Cambridge



William Lawrence Bragg: at 25 years old, Bragg shared the 1915 Nobel Prize for Physics with his father for the development of X-ray diffraction. He remains the youngest person to win a Nobel prize. When Watson arrived in Cambridge in 1951, Bragg was Cavendish professor of physics, directing Perutz, Kendrew and Crick. He moved to the Royal Institution in 1954. In 1968 Bragg wrote the foreword to Watson's *The Double Helix*, without which the book may not have been published.



Max Perutz: joined the Cavendish Laboratory in 1936 and spent 25 years determining the structure of haemoglobin, for which he shared the 1962 Nobel Prize in Chemistry with Kendrew. As Crick's PhD supervisor, he often interceded with Bragg on Crick's behalf. Perutz was director of the MRC's Laboratory of Molecular Biology from its inception in 1962 until his retirement in 1979.



John Kendrew: joined in 1945 and determined the structure of myoglobin, sharing the 1962 Nobel Prize in Chemistry with Perutz. Later Kendrew was an important advocate for molecular biology. He was one of the founders of the European Molecular Biology Organization and the first director of the European Molecular Biology Laboratory.



Francis Crick: was 33 and still without a PhD when he arrived at the Cavendish in 1949. He quickly established himself as

a theoretician and, following the discovery of the double helix, he played a central part in unravelling the genetic code. In 1977, he moved to the Salk Institute in San Diego, California, where he studied consciousness.



James D. Watson: arrived at the Cavendish in 1951 having become convinced by Wilkins's diffraction patterns of DNA that crystallography was the way to the gene. He quickly formed a close partnership with Crick. In 1968, Watson published *The Double Helix*, his best-seller about this period. After building a department at Harvard University, he became director of Cold Spring Harbor Laboratory and the first director of the Human Genome Project.

King's College London



John Randall: professor of physics 1941–70. He established and directed the MRC Biophysics Research Unit that included Wilkins, Franklin and Gosling. Randall wrote to Franklin while she was in Paris telling her that she would have sole control of the DNA research at King's. This set up the disastrous misunderstanding with Wilkins who believed from Randall that Franklin and he would be working together on DNA.



Maurice Wilkins: accompanied Randall to King's College in 1941. He began work on DNA in 1950 and produced, with graduate student Gosling, the best X-ray diffraction images of DNA taken up to that time. Having met Crick in 1946, they later became good

friends, and shared, with Watson, the 1962 Nobel Prize in Physiology or Medicine.



Rosalind Franklin: came to King's College in 1951. She had been recruited to work on proteins but was reassigned by Randall to work on DNA. Wilkins believed she was joining his group and this misunderstanding poisoned their relationship. Franklin and Gosling discovered the transition between the A and B forms of DNA, and took the famous Photograph 51. At Birkbeck College in London from 1953, Franklin did outstanding work on the structure of tobacco mosaic virus. She died of cancer in 1958, and thus was not eligible for consideration for the 1962 Nobel Prize.



Raymond Gosling: was originally Wilkins's graduate student in 1949 and was reassigned to Franklin in 1951.

California Institute of Technology



Linus Pauling: was the winner of two Nobel prizes, one for chemistry (1954), the other for peace (1962) and author of the classic book, *The Nature of the Chemical Bond*, which Crick gave Watson for Christmas 1951. The arch-proponent of model building as a way to solve structures, in 1951 he used this approach to dazzling effect in deriving the α -helix and β -sheet, fundamental structural features of proteins. This was a bitter blow to Bragg: the Cavendish group had published an incorrect structure only six months earlier. Pauling's son Peter did his PhD at the Cavendish during the hunt for the DNA structure.

worry about it, because we've all agreed that we must come to an amicable arrangement.

They point out that Wilkins is in a “fortunate position” — that in a short time “it is extremely probable” that his unit will have solved one of the “key problems in biomolecular structure”. Thereafter, a few crossed-out lines include the following: “By doing so you will have opened the door to many of the really crucial biological problems”.

But in place of such high sentiments, the letter instead ends with:

...so cheer up and take it from us that even if we kicked you in the pants it was between friends. We hope our burglary will at least produce a united front in your group!

*Yours ever
Francis
Jim*

These three dispatches highlight the different moods and styles of the two camps — the agonised tone of Wilkins’s letters and the cavalier ring of Watson and Crick’s reply, even at this low point for the Cambridge team, with its reminder that the group at King’s was already divided and dysfunctional.

JANUARY 1953: RETURN TO MODEL BUILDING

On 28 January 1953, a manuscript from Linus Pauling (see ‘Cast list’), describing his incorrect triple helix model for DNA, arrived at the Cavendish, and two days after that Watson visited King’s with Pauling’s manuscript in hand to show Wilkins and Franklin. Watson had his infamous altercation with “Rosy”¹, and was shown the crucial Photograph 51 by Wilkins. Although taken by Franklin months earlier in May 1952, this X-ray diffraction photograph of B-form DNA, with its unambiguous evidence that DNA was helical, had only recently been given to Wilkins by graduate student Raymond Gosling, as Franklin was leaving for Birkbeck College in London.

Faced with the possibility that Pauling might solve one of the “really big problems in biology”, Bragg authorized Watson and Crick to start model building again. Bragg was still smarting from Pauling’s success just two years earlier in discovering the protein structural motifs, the α -helix and β -sheet.

These events are foreshadowed in a handwritten letter from Wilkins to Crick, dated “Fri”, most likely to be 23 January 1953. It discusses Franklin’s upcoming colloquium on 28 January — her last at King’s. She was to summarize her data before handing over her materials to Wilkins and moving to J. D. Bernal’s group at Birkbeck College. Unsurprisingly, Watson and Crick wanted to attend; Wilkins struggles to explain why he has put them off:

There is also a silly muddle over Franklin’s talk here. I got a big notice saying it was internal only — just a discussion between colleagues who worked in the same lab. Then a lot of notices went round about the Colloquium & I took it for granted all had had the other note... I think that as the intention was to have it a private fight it would be best to keep it entirely so, as I said to Jim. It should be either public or private. Let’s have some talks afterwards when the air is a little clearer. I hope the smoke of witchcraft will soon be getting out of our eyes.

This “witchcraft” line, referring to Franklin’s imminent departure from King’s, is likely to find its place in the canon of well-known allusions to her. With the quest for the structure of DNA poised for its final act, the postscript of this letter again notes the dismal morale and crippled state of communications at King’s:

PS. Tell Jim the answer to his question ‘When did you last speak to her’ is this morning. The entire conversation consisted of one word from me.



SCIENCE SOURCE/SPL

Rosalind ‘Rosy’ Franklin’s X-ray diffraction photographs proved crucial to determining the structure of DNA.

A letter from chemist John Griffith to Crick ends this chapter of our story. Handwritten and dated 2 March 1953, it describes a second set of calculations, performed at Crick’s request (Crick asked him for the first set in 1952) on stacking interactions between bases. Griffith writes that “depressingly”, the base adenine repulses uracil — the base found only in RNA. Is it possible Crick was already thinking about how RNA might be made on a DNA template?

MARCH 1953: WRITING UP THE PAPERS

Watson and Crick announced their double-helical model in one of a group of three papers in *Nature* on 25 April. The other two — from Wilkins and from Franklin — presented supporting X-ray diffraction data from the King’s group. Four notable letters are concerned with how this publishing solution was arrived at.

Watson and Crick quickly wrote their own paper and sent a copy to Wilkins on 17 March. Two new-found letters in Crick’s handwriting, on a single sheet of paper, shed light on this first move. On one side is the draft of a letter to Wilkins to accompany Watson and Crick’s manuscript:

Dear Maurice,

I enclose a draft of our letter. As it has not yet been seen by Bragg I would be grateful if you did not show it to anyone else. The object of sending it to you at this stage is to obtain your approval of two points:

- a) the reference number 8 to your unpublished work.*
- b) the acknowledgement.*

If you would like either of these rewritten, please let us know. If we don’t hear from you within a day or so we shall assume that you have no objection to their present form.

*Jim has gone to Paris, lucky dog
Yours*

Clearly Watson and Crick were eager to submit their letter to *Nature* as soon as possible, and did not anticipate that either Wilkins or Franklin would be publishing anything at this stage. Wilkins had already declined Watson and Crick’s offer of co-authorship when he had visited Cambridge to view the new model on 13 March. (This date is consistent with the accounts given by Wilkins², Watson¹ and Robert Olby^{4,6}, but at variance with Horace Judson’s description⁵).

On the reverse of the same sheet of paper is a draft letter to A. J. V. Gale, one of the two editors of *Nature*. Presumably the Cavendish Laboratory had a closer relationship with Gale, the editor who

handled manuscripts in the physical sciences, than with L. J. F. ('Jack') Brimble who handled the biological submissions.

Dear Gale,

Not long ago you published a short letter from Pauling & Corey on nucleic acid. We have also been working on this problem & we would be grateful if you could do the same for us. The letter, entitled "A structure for D.N.A." is enclosed.

We have shown the letter in draft to Wilkins. It was agreed that rather than collaborate in testing our structure on their data, we would publish the structure by ourselves, and they would check it later against their data.

Both Prof Bragg & Perutz have read the letter and have approved our sending it to you. We would be grateful if you could give us a rough idea if & when you are likely to be able to publish it.

Yours sincerely

J. D. W

Francis

This draft reaffirms how Crick then saw the situation. He expected that the double-helix model would be published right away, to be followed later, and independently, by data from King's testing of the model. It is unlikely that Watson and Crick ever sent such a letter to Gale. In the end it was apparently Bragg who submitted the paper to *Nature* two weeks later.

Despite Crick's expectations, the day after Wilkins received the draft manuscript from Watson and Crick, he wrote to say that the two King's groups would be sending papers to *Nature* as well, in a famous letter beginning: "I think you're a couple of old rogues"⁴. The subsequent discussions over the wording and content of the three papers, and the possibility of a fourth paper (to appear elsewhere) by Bruce Fraser, a research student at King's, are familiar from known correspondence. Now two new handwritten letters from Wilkins to Crick flesh out the story.

One, brief and undated, clearly accompanied a draft of Wilkins's own manuscript and was perhaps handed over rather than mailed. It seems to have been written under the assumption that Crick would receive it before that very evening:

Dear Francis,

Herewith almost uncorrected draft. How should we refer to your note? Welcome suggestions & [illegible] acknowledgements.

It looks very much as though I will be too late tonight so maybe we had better cancel the supper idea which is a pity but anyway I got the bloody thing finished.

Have you a structure for collagen yet?

This last remark is probably sarcasm. Crick attended a meeting on the structure of collagen in London on 27 March organized by Randall. Only two weeks after beating King's to the DNA structure, Crick weighed in with uninvited interpretations of the King's collagen data, much to Randall's displeasure⁶. Although 27 March is close to the publishing date, perhaps Wilkins' note was accompanying a final draft of his DNA manuscript, and was sent or handed to Crick in London after this meeting.

The second new letter from Wilkins to Crick is dated "Mon", most likely 23 March, and was written in response to a long letter from Crick⁶. First, Crick had voiced concern that Franklin was hoping to see Pauling on his forthcoming visit to England. "It is not impossible that she might consider turning over the experimental data to Pauling. This would inevitably mean that Pauling would prove the structure and not you."

Second, Crick had wanted everyone to see everyone else's manuscripts — "We are not happy about the position of Rosy and Gosling... It is not reasonable for letters to be sent in jointly to *Nature* without having been read by all concerned. We want to see hers, and I've no doubt she wishes to see ours." Third, Crick had asked how he and Watson should refer to Fraser's unpublished three-chain model, with bases on the inside, and whether indeed it should be published at all, as Wilkins wanted.

Crick had concluded that "because the present situation is embarrassing to us, we have written a short note to Randall to suggest a meeting on Wednesday (we could come on Tuesday if the letters are ready by then)".

This is Wilkins's exasperated response:

Dear Francis,

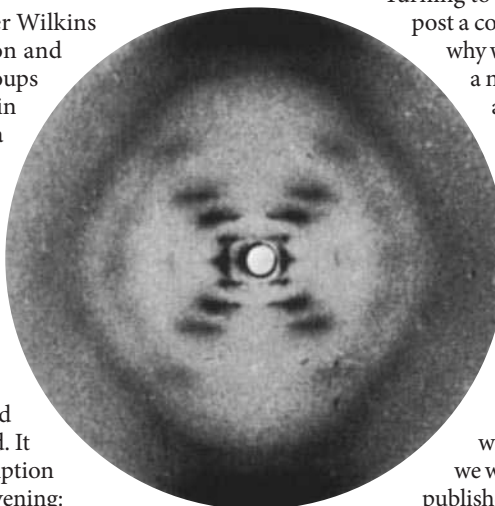
It looks as though the only thing is to send Rosy's & my letters as they are & hope the Editor doesn't spot the duplication. I am so browned off with the whole madhouse I don't really care much what happens.

If Rosy wants to see Pauling, what the hell can we do about it? If we suggested it would be nicer if she didn't that would only encourage her to do so. Why is every body so terribly interested in seeing Pauling... Now Raymond wants to see Pauling too! To hell with it all.

Turning to the manuscripts, Wilkins continues: "We will post a copy of Rosy's thing to you tomorrow. I don't see why we have to have a meeting." It is not clear whether a meeting ever happened. Olby states: "Evidently, a meeting did take place"⁶, but Wilkins is clearly not enthusiastic; and today Watson has no recollection of any such event.

The letter bears a postscript "Raymond & Rosie have your thing so everybody will have seen everybody else's."

On Fraser's model, Wilkins writes: "I feel your remarks about Bruce's model, in your note, not in very good style. Why be bitter about it?" In the event, Watson and Crick included a rather dismissive paragraph in their paper, remarking that Fraser's structure was "...rather ill-defined and that for this reason we will not comment on it." The structure was never published.



Photograph 51 showed that DNA was helical.

JUNE 1953: AFTER THE PAPERS APPEAR

From 20 April, a few days before the papers appeared in *Nature*, we now have an exuberant postcard Wilkins sent to Crick: "The bumper issue of *Nature* will soon be out!" Another new-found handwritten letter from Wilkins to Crick is dated 3 June 1952, but the text — here in full — seems to place it in 1953:

My dear Francis,

I gather you have got the coordinates of your model or some worked out. Do you think we could have a copy of what you have?

The crystalline data is clearing up nicely. To think that Rosie had all the 3D data for 9 months & wouldn't fit a helix to it and there was I taking her word for it that the data was anti-helical. Christ.

We have redone a lot of the 3D more accurately on mouse & will need all the extra accuracy for dealing with some of the finer points.

Regards & to Odile too.

Yours

M

P.S. I think I have a flat.

The tone smacks of this having been written after the double-helix model had been published. Details also fix it to the later date: Wilkins's

asking for coordinates of the model; commenting that the “crystalline data” was clearing up; and his using mouse DNA — Wilkins’ initial paper refining the DNA structure, published in *Nature* in October 1953, includes, for the first time, data from mouse DNA. And in *The Double Helix*, Watson wrote of Wilkins looking for a new flat at the end of January 1953: “Our bottle of Chablis, however, diminished my desire for hard facts, and as we walked out of Soho and across Oxford Street, Maurice spoke only of his plans to get a less gloomy apartment in a quieter area.”

In early June 1953, Watson gave his first public presentation of the model at the annual Cold Spring Harbor Laboratory symposium. Watson and Crick’s paper in that year’s symposium volume included a photograph provided by Wilkins of an X-ray diffraction pattern of the A structure (crystalline) DNA. We now know that Crick sent Wilkins a revealing observation about this picture on the day the symposium began, 5 June 1953:

This is the first time I have had an opportunity for a detailed study of the picture of Structure A, and I must say I am glad I didn’t see it earlier, as it would have worried me considerably.

It was the A-structure diffraction pattern that had led Franklin away from believing that DNA, in that form at least, was helical, despite her already having produced the most persuasive helical pictures of the B structure — including photograph 51. The crystalline DNA gave better quality diffraction data, more suited to her painstaking, quantitative approach, and so she focused on the A form during 1952. It was at this time that she and Gosling made a handwritten, black edged funeral card announcing the death of “DNA Helix (crystalline)”.

1954–64: AFTER THE DOUBLE HELIX

Another 22 newly found letters between Wilkins and Crick span the decade from 1954 to 1964. Many of these include the exchange of detailed crystallographical information on the ever-improving DNA structure being pursued by Wilkins, and in later letters, advice on academic hires and other administrative matters. There are two topics of more general interest.

The first topic appears in a handwritten note from Wilkins to Crick dated 29 October 1954, which also includes a jab at the fact that neither Crick nor Watson did any experimental work in arriving at the double helix:

Looking at your letter, the list of your forthcoming publications is certainly impressive & I hope you won’t think me malicious if I note with interest your new plan for avoiding experimental work — I mean your book.

What was the book? Another exchange in the new collection offers an explanation. A letter dated 28 June 1954 from Academic Press in New York enthusiastically accepts Crick’s proposal for a book, *The Central Problems in Molecular Biology*. An outline was drawn up, and the title changed to the punchier *Genes and Proteins*. Alas the book was never written. Over the next six years, increasingly desperate pleading from the publisher is matched by evasion and excuse from Crick, all recorded in the recovered correspondence — which even includes the contract Crick was sent but never signed.

What would have happened had Crick written this book? For one thing, it is unlikely Watson would have written *Molecular Biology of The Gene*, first published in 1965, and currently in its 6th edition. Certainly that was his reaction on seeing this correspondence now.

We end with some finds on the subject of ‘Brain drain’, the term coined in the early 1960s as worries mounted about British scientists decamping, mainly to the United States. First a handwritten letter dated 9 May 1959, again from Wilkins to Crick, who was on sabbatical in the Harvard University chemistry department. After thanking Crick for his kind words on his recent marriage, Wilkins moves on to “more important things”:

People keep hinting that you may not come back again to England. I feel very strongly that if you do not return, the Unit will receive a devastating blow that will permanently impare it & may lead to its disintegration. This would wreck the development of Molecular Biology in Britain ... And if Molecular Biology goes down the drain, what about the effect of that on Biology generally?

...if there is anything that can be done over here to help us keep you here please let me know. I know that things are rotten here in many ways, but things are not hopeless & we have much to be proud of. Let me know, Francis, & let other people know. Maybe I could somehow do something ... And if you do go, I hope you will go with a great deal of noise & stink so people over here get a good shaking up!

Concern over the threat of losing Crick seems to have been widespread. Another letter from the new archive is from Nevill Mott, the Cavendish professor of physics at Cambridge following Bragg’s move to the Royal Institution. Dated 6 March 1959, the entire note reads:

Dear Francis,

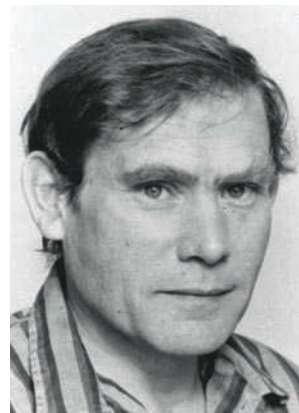
How nice to hear from you.

Plans for your MRC building coming along nicely — so don’t be tempted by.....

Nevill Mott

The threat was real. Crick’s closest colleague at that time was Brenner. Among the many letters between Crick and Brenner in the Brenner Collection at Cold Spring Harbor we find two from this period that clarify matters. A postscript to a letter dated 17 March 1959 reveals: “I am having offers of jobs, but we can discuss these in June.” Apparently it couldn’t wait that long though. Crick writes again to Brenner on 11 April:

As to temptation, I have now decided that if the new lab [LMB] goes through and if you stay at Cambridge, I will stay in Cambridge too, but please keep this to yourself for the moment.



Francis Crick’s papers became mixed with those of Sydney Brenner (pictured) when they shared an office.

Happily for the continued development of molecular biology in the United Kingdom, Crick returned. He and Brenner remained at Cambridge until Crick left permanently for the Salk Institute in San Diego, California, in 1977 — leaving behind him this wealth of personal papers. ■

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We are very grateful to Sydney Brenner for the generous gift of his papers to the CSHL Archives and to Mila Pollock (executive director of library and archives, CSHL) for her enthusiastic work in facilitating the gift.

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