

Letter from James Watson to Max Delbrück. March 12, 1953.

Watson writes to provide Delbrück with a detailed overview of his and Francis Crick's unpublished structure for DNA. In so doing, Watson stresses the need for Delbrück to avoid sharing this information with Pauling. [Courtesy of The James D. Watson Collection, Cold Springs Harbor Laboratory Archives]

[Transcript of Watson-Delbrück letter, March 12, 1953, page 1]

March 12, 1953

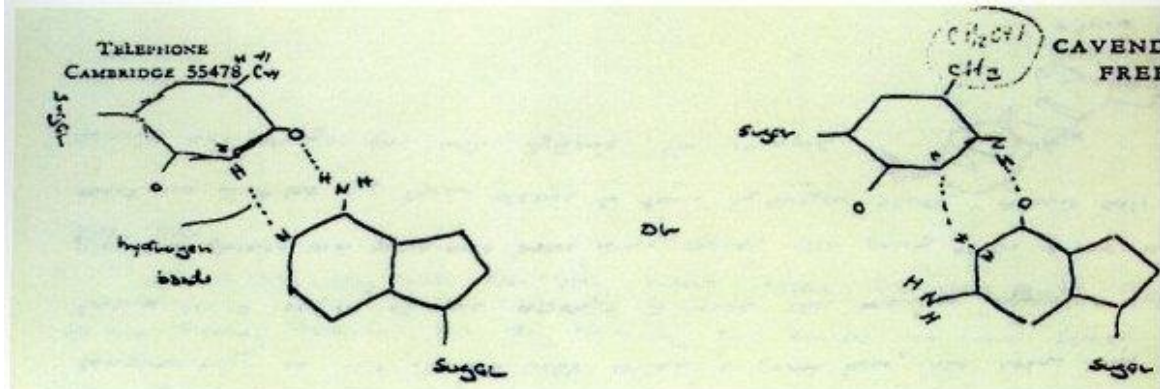
Dear Max

Thank you very much for your recent letters. We were quite interested in your account of the Pauling Seminar. The day following the arrival of your letter, I received a note from Pauling, mentioning that their model had been revised, and indicating interest in our model. We shall thus have to write him in the near future as to what we are doing. Until now we preferred not to write him since we did not want to commit ourselves until we were completely sure that all of the Van der Waals contacts were correct and that all aspects of our structure were stereochemically feasible. I believe now that we have made sure that our structure can be built and today we are laboriously calculating out exact atomic coordinates.

Our model (a joint project of Francis Crick and myself) bears no relationship to either the original or the revised Pauling-Corey-Schomaker models. It is a strange model and embodies several unusual features. However since DNA is an unusual substance, we are not hesitant in being bold. The main features of the model are (1) The basic structure is helical - it consists of two intertwining helices - the core of the helix is occupied by the purine and pyrimidine bases - the phosphates groups are on the outside. (2) The helices are not identical but complementary so that if one helix contains a purine base, the other helix contains a pyrimidine - this feature is a result of our attempt to make the residues equivalent and at the same time put the purines and pyrimidine bases in the center. The pairing of the purine with pyrimidines is very exact and dictated by their desire to form hydrogen bonds - Adenine will pair with Thymine while Guanine will always pair with Cytosine. For example



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Thymine with Adenine

Cytosine with Guanine

While my diagram is crude, in fact these pairs form 2 very nice hydrogen bonds in which all of the angles are exactly right. This pairing is based on the effective existence of only one out of the two possible tautomeric forms - in all cases we prefer the keto form over the enol[,] the amino over the imino. This is definitely an assumption but Jerry Donohue and Bill Cochran tell us that for all organic molecules so far examined, the keto and amino forms are present in preference to the enol and imino possibilities.

The model has been derived almost entirely from stereochemical considerations with the only x-ray consideration being the spacing between the pair of bases 3.4Å which was originally found by Astbury. It tends to build itself with approximately 10 residues per turn in 34Å. The screw is right handed.

The x-ray pattern approximately agreed with the model, but since the photographs available to us are poor and meagre (we have no prototypes of our own and like Pauling must use Astbury's photographs) this agreement in no way constitutes a proof of our model. We are certainly a long way from proving its correctness. To do this we must obtain collaboration from the group at Kings College London who possess very excellent photographs of a crystalline phase in addition to rather good photographs of a paracrystalline phase. Our model has been made in reference to the paracrystalline form, and as yet we have no clear idea as to how these helices

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pack together to form the crystalline phase.

In the next day or so Crick and I shall send a note to Nature proposing our structure as a possible model, at the same time emphasizing its provisional nature and the lack of proof in its favor. Even if wrong I believe it to be interesting since it promises a concrete example of a structure composed of complementary chains. If by chance, it is right then I suspect we may be making a slight dent into the manner in which DNA can reproduce itself. For these reasons (in addition to many others) I prefer this type of model over Pauling's which if true would tell us next to nothing about [the] manner of DNA reproduction.

I shall write you in a day or so about the recombination paper. Yesterday I received a very interesting note from Bill Hayes. I believe he is sending you a copy.

I have met Alfred Tissieus recently. He seems very nice. He speaks fondly of Pasadena and I suspect has not yet become accustomed to being a Fellow of Kings.

My regards to Mary
Jim

P.S. We would prefer your not mentioning this letter to Pauling. When our letter to Nature is completed we shall send him a copy. We should like to send him coordinates.

Letter from James Watson and Francis Crick to Linus Pauling. March 21, 1953.

Watson and Crick write to forward a copy of their unpublished letter to Nature which describes the DNA structure that they have formulated. They add that their structure is scheduled to be published in tandem with work that has been done by colleagues at Kings College

[Transcript of Watson & Crick letter to Pauling, March 21, 1953]

March 21 1953

Dear Dr. Pauling

We intended to write to you about our DNA structure before this, but one of us (J.W.) has been away in Paris and we have also been delayed because Professor Bragg has been down with flu. We enclose a draft of a letter to Nature which gives the essential features of our structure. We have a model of it and have derived co-ordinates; all of the Van der Waals distances are acceptable.

We felt we could hardly omit any mention of your structure nor did we feel it reasonable to suppress our doubts about it. Without your permission we could not mention that you have modified it. However we can always qualify our remarks in proof.

It is planned that the Kings College workers will publish some of their experimental data at the same time as our letter. Wilkins tells us that he intends to send you a copy of their communication in advance of publication as soon as it is in final draft.

We are looking forward very much to your visit and the opportunity for a full discussion about DNA. Would you mind treating this as confidential for a few days as Professor Bragg has still not been able to hear about it.

Yours Sincerely

Jim Watson
Francis Crick

Letter from Linus Pauling to James Watson and Francis Crick. March 27, 1953.

Pauling writes to thank Watson and Crick for providing him with a pre-publication copy of their Nature letter and to express his excitement at learning which of the two proposed DNA structures is correct. Pauling also briefly details a few corrections that he and Corey have made to their model.

27 March 1953

Dr. J. D. Watson
Mr. F.H.C. Crick
Cavendish Laboratory
Free School Lane
Cambridge, England

Dear Dr. Watson and Mr. Crick:

I am very glad to have your letter of 21 March, and to see the letter that you are sending off to NATURE.

I think that it is fine that there are now two proposed structures for nucleic acid, and I am looking forward to finding out what the decision will be as to which is incorrect. Without doubt the King's-College data will eliminate one or the other.

We have taken care of the trouble of too small van der Waals contacts in our structure by rotating the phosphate groups. I do not think that the matter of repulsion between the charged phosphates is an important one. I am not saying, however, that I feel strongly that our structure is right, rather than yours.

With best regards, I am

Sincerely yours,

Linus Pauling:W

Letter from Francis Crick to Linus Pauling. April 14, 1953.

Crick writes to clarify his understanding of the provenance of the coiled-coils idea as it relates to his and Pauling's respective work on the structure of proteins. Crick also notes that he and Watson "would be most interested to learn what you feel about our D.N.A. structure when you have had time to digest the idea and the experimental data."

UNIVERSITY OF CAMBRIDGE

DEPARTMENT OF PHYSICS

CAVENDISH LABORATORY

Free School Lane,
Cambridge.

14th April 1953

Professor Linus Pauling,
Gates & Crellin Laboratory,
Pasadena 4, California,
U.S.A.

Dear Professor Pauling,

I hope you were not puzzled because we said very little about coiled-coils when you were here, but your letter to Perutz about them only reached us on the day after you left.

My recollection of our conversation in the summer is very similar to yours, except that, as I recall it, it was slightly longer than you have remembered it. In particular I touched on knobs-into-holes packing and the possibility of a 7-strand cable. You pointed out to me that this might explain the 27 Å equatorial reflection.

It was natural, therefore, that when Peter told us you were working on coiled-coils that the idea should get round that I had suggested the idea to you. When your Nature article was eventually published it was clear to me that there were very little grounds, if any, for such a belief. In particular you had suggested a definite model, whereas I had not, and, more important, you had put forward a different reason for the coiling.

In my view the idea that the α -helix might be inclined is an obvious one, and it is only a small step from that to the idea of coiling. I attach much more importance to the reasons underlying the coiling, and the proof that a coiled-coil gives both 5.1 and 1.5 Å reflections on the meridian. On these points we have followed independent paths.

On reflection I think it might have made things easier if you had let me know you were writing a paper on the idea, so that I would have had the opportunity of putting forward my ideas simultaneously. However, as things turned out, thanks to the many channels of communication between Caltech. and the Cavendish, this is effectively what happened.

We very much enjoyed your visit here. Watson and I would be most interested to learn what you feel about our D.N.A. structure when you have had time to digest the idea and the experimental data.

Yours sincerely, Francis Crick

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B CAB107 PD AR=WUX CAMBRIDGE MASS 19 512PME=
-DR LINUS PAULING, DEPT OF CHEMISTRY=
CALIF INST OF TECHNOLOGY PASADENA CALIF=

POSSIBILITY HAS ARISEN THAT THECHEMISTRY DEPARTMENT
MAY HAVE A BIO-CHEMICAL PROFESSORSYIP FOR WHICH
FRANCIS CRICK WOULD BE A CANDIDATE. COULD YOU PLEASE
WRITE US WITHIN A FEW DAYS YOUR EVLUATION OF HIM,
COMPARING HIM WITH OTHER POSSIBLE CANDIDATES. YOUR
VIEWS WILL BE MOST VALUABLE FOR US AND OUR COLLEAGUES=
: PROFESSORS BLOCK DOTY AND WESTHEIMER HARVARD
UNIV CHEMISTRY DEPT= 12-518

THE COMPANY WILL APPRECIATE SUGGESTIONS FROM ITS CUSTOMERS CONCERNING ITS SERVICE

19 February 1959

Professor Konrad Bloch
Professor Paul Doty
Professor Frank Westheimer
Chemistry Department
Harvard University
Cambridge, Massachusetts

Dear Professors Bloch, Doty, and Westheimer:

I am pleased to reply to your telegram, asking my opinion of Francis Crick.

I think that Crick is a very clever and intelligent man—the sort of man who should be a professor.

He has a good knowledge of the field of x-ray crystallography. I don't know how much he knows about biochemistry.

Some of his work has been brilliant. Much of it has been done with collaborators, and it might be hard to decide how great the contribution is that Crick made in this collaborative work. However, I have little doubt that he has provided a good bit of the brilliance in the collaborative work.

Crick has an interesting personality. I judge that he did not get along very well with Professor Sir Lawrence Bragg. I think that, on the other hand, he does get along well with most people.

It is not easy to compare Crick with other people in the same general field. I think that he knows much more about x-ray crystallography than Alex Rich does, and that he probably is a more original man. He is not so sound and thorough as Professor Robert B. Corey, my collaborator, but on the other hand he is more imaginative and, of course, much younger. He probably has greater originality than David Harker, although I think that David Harker knows much more about structural chemistry than Crick does. Harker has done some fine jobs in the field of x-ray crystallography, such as his determination, with two students, of the structure of decaborane. Harker's work on the structure of proteins has, however, been disappointing.

I may say that if I were looking for another man to carry on work on the determination of the structure of crystalline globular proteins (that is, if Professor Corey were not doing this work in our laboratory) I probably would have a strong inclination to appoint Dr. Murray Vernon King, who is one of Harker's collaborators, and who has been, I think, in large part responsible for the progress that has been made on that project. King, who received his training with Lipscomb, impresses me as being an able and original man who gets things done.

Crick probably has broader interests, so far as biochemistry goes, than Kendry, who is, of course, making good progress in his attack on the structure of myoglobin.

Professors Bloch, Doty, and Westheimer
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I would expect Crick to be an interesting and effective lecturer.

Sincerely yours,

Linus Pauling:jh