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13th April 1967.

Dr J.D. Watson,
Harvard University Biological
Laboratories,
16 Divinity Avenue,
Cambridge, Mass. 02139,
U.S.A.

Dear Jim,

The new version of Honest Jim is naturally a little better, but my basic objections to it remain the same as before. They are:

I. The book is not a history of the discovery of DNA, as you claim in the preface. Instead it is a fragment of your autobiography which covers the period when you worked on DNA.

I do not see how anybody can seriously dispute this, for the following reasons:-

a) Important scientific considerations, which concerned you at the time, are omitted. For example the work of Furberg, which established the relative configuration of the sugar and the base. There are many other examples.
b) Such scientific details that are mentioned are referred to rather than described. For example, you do not explain exactly why you got the water content of DNA wrong, nor make it clear that if there had been so little water electrostatic forces were bound to predominate. You do not mention that Pauling worked from an old X-ray picture of Astbury's which had both the A and B pictures on the same photograph. There are many other examples.
c) The thread of the argument is often lost beneath the mass of personal details. For example I asked both Bragg and Doty the following question. "Since we had realized that 1:1

base ratios mean that the bases went together in pairs why did we not immediately use this idea when we started model building the second time?" Neither could give the correct answer.

d) No attempt is made to ask or answer questions which would interest the historian (such as the one above). For example, the advantages or disadvantages of collaboration, or when the structure would have been solved if we had not solved it. Nothing is said about the importance of the MRC, nor why they decided to finance "biophysics" after the war.

e) Gossip is preferred to scientific considerations. For example, you explain how Bragg and I had a misunderstanding but you omit to say what the scientific issue was.

f) Much of the gossip and even some of the science is irrelevant to a history of DNA. For example, your work on TMV and bacterial genetics is only of marginal importance to the main theme. Whole chapters, such as Chapter 15 on your visit to Carradale, are irrelevant as far as DNA is concerned. Even when personal matters should be mentioned they are described in quite unnecessary detail.

g) Absolutely no attempt is made to document your assertions, many of which are not completely accurate because of your faulty memory. You have not troubled to consult documents which you could easily lay your hands on, nor have you made available to others the documents you yourself have, such as the letters you wrote at the time to your mother, which are in fact not even mentioned in the book. Dates are given in the book only very casually.

It is thus absolutely clear that your book is not history as normally understood. However once it is realized that it is not history but a part of your autobiography many of the points made above become irrelevant. Unfortunately you yourself claim it as history, and the misguided but worthy people who are supporting you in publishing it also use this as their major excuse for publication.

Should you persist in regarding your book as history I should add that it shows such a naive and egotistical view of the subject as to be scarcely credible. Anything which concerns you and your reactions, apparently, is historically relevant, and anything else is thought not to matter. In particular the history

of scientific discovery is displayed as gossip. Anything with any intellectual content, including matters which were of central importance to us at the time, is skipped over or omitted. Your view of history is that found in the lower class of women's magazines.

The objections to your book considered as an autobiography are not so clear-cut as the objections to it considered as history. The criticisms are naturally more a matter of personal opinion. However it seems to me that some of these are beyond dispute. My second major objection is therefore:

II. Considered as autobiography your book is misleading and in bad taste.

a) Your book is misleading because it does not in fact accurately convey the atmosphere in which the work was done. Most of the time we were engaged in complicated intellectual discussions concerning points in crystallography and biochemistry. The major motivation was to understand. Science is not done merely by gossiping with other scientists, let alone by quarrelling with them. The most important requirements in theoretical work are a combination of accurate thinking and imaginative ideas. To understand these they must be described at the intellectual level involved. I conceed that the idea that scientific work is impersonal is ridiculous, but you have gone much too far in the other direction in trying to correct this misconception.

b) Your book is in poor taste because of the style. I select a few examples:

"waited for the day when he could fall flat on his face by botching something important."

"The scuttlebuck about Peter centred on girls and was confused. But now Ava Helen gave me the dope that Peter was an exceptionally fine boy whom everybody would enjoy having around as much as she did".

"..... Linus' chemistry was screwy".

"..... he knew that the presence of popies does not inevitably lead to a scientific future".

The examples show some of the more glaring features, but longer quotations and comments would be needed to bring out the attitude behind much of the writing. For example, the tone used to describe Rosalind's work in the Epilogue is perfectly reasonable, but contrasts ludicrously with the descriptions of her in the text itself.

c) It is not customary to write intimate books about your friends without their permission, at least until they are dead. I would remind you that Betrand Russell delayed the publication of his autobiography till he was over 90, and that Lord Moran's much criticized account of Churchill's health was not published till after the latter's death.

The fact that a man is well-known does not by itself excuse his friends from respecting his privacy while he is alive. Only if a person himself either gives permission or discusses his own personal affairs in public should his friends feel free to write about them. The only exception is when private matters are of prime and direct public concern, as in the case of Mrs Simpson and King Edward, and even then the British press wrote nothing for many months. It cannot really be said that the way the structure of DNA was discovered is of major public concern.

Next I must deal with miscellaneous points made by various people. I do not concede that pure scientific research lies in the public realm in the same way that politics or military affairs do. People who engage in the latter activities naturally expect to have their behaviour written about. But the point of science is what is discovered, not how it was discovered or by whom. It is the results which need to be brought home to the public. It is quite inexcusable to invade someone's privacy to describe how the structure of DNA was discovered to people who don't even know what it is, nor why it is important. I have no objection to a genuine historical description. It is vulgar popularization which is indefensible.

I am also unimpressed by the number of people who favour publication. The fact is that those most intimately concerned, Maurice, Pauling and myself have all protested strongly. Bragg was furious when he first read the book, and was only subsequently talked round. He still has considerable doubts. I understand that Beadle has advised you not to publish. No doubt a number of people who know nothing about the

subject have enjoyed the book, but what is that to me? I very much doubt if any historian of science has thought it worth much. You should realize that the approval of people like Jacques Barzun is not worth having.

Nor can I see any logic in the argument that since 50 people have read it already, it will make no difference if 50,000 now read it. I can assure you that it makes all the difference to me. Nor do I believe that the book is so fascinating that everybody will want to have an underground Xerox copy.

I agree your book is "unprecedented" but that by itself does not make it acceptable. It certainly "provides something which cannot be brought out in any other way", namely a lot of tasteless gossip about the past. It does not illuminate the process of scientific discovery. It grossly distorts it.

Again there is no force in the argument that if the book were only to be published in 40 to 50 years time no-one would be alive to set straight the errors. The people actually involved can correct the errors now, using the present manuscript. Wide publication would not make this process any easier.

I must also point out to you, once again, the risks you will run if you publish such a book. The picture which emerges of yourself is not only unfavourable but misleadingly so. Moreover I do not think you realize what others will see in it. One psychiatrist who saw your collection of pictures said it could only have been made by a man who hated women. In a similar way another psychiatrist, who read Honest Jim, said that what emerged most strongly was your love for your sister. This was much discussed by your friends while you were working in Cambridge, but so far they have refrained from writing about it. I doubt if others will show this restraint.

Finally we come to the question, what should you do about the book. I can see only two courses which you can honourably take:

1. Scrap the present book, and write a proper history of the subject. I can understand that you may not wish to do this, especially as Olby is planning to write such a book.

2. Put the book on one side, with instructions that it may be published either when all the major participants agree to it, or after those who object are dead.

Dr J.D. Watson. 13th April 1967.

There is no reason why your book, as it stands, should not be made available to selected scholars, provided any documents you may have (such as your letters to your mother) which bear on the subject are also made available at the same time.

My objection, in short, is to the widespread dissemination of a book which grossly invades my privacy, and I have yet to hear an argument which adequately excuses such a violation of friendship. If you publish your book now, in the teeth of my opposition, history will condemn you, for the reasons set out in this letter.

I have written separately to Wilson pointing out several cases of factual errors in your latest draft. I enclose a copy of my letter to him.

Yours sincerely,

Francis

F.H.C. Crick.

Copies to: President Pusey.
Sir Lawrence Bragg.
M.H.F. Wilkins.
L. Pauling.
T.J. Wilson.
J.T. Edsall.
P. Doty.
J.C. Kendrew.
M.F. Perutz.
A. Klug.