

A brief account of facts with regard to certain researches on the heart, in reply to statements in a memoir of Dr. Hope.

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A

BRIEF ACCOUNT OF FACTS

WITH REGARD TO

CERTAIN RESEARCHES ON THE HEART,

IN REPLY TO STATEMENTS IN

A Memoir of Dr. Hope.

BY

CHARLES J. B. WILLIAMS, M.D. F.R.S. &c.

“ Let not, then, your good be evil spoken of.”—ROM. xiv. 16.

London :

JOHN CHURCHILL, PRINCES STREET, SOHO.

1842.

BRIEF ACCOUNT OF FACTS

AND

CERTAIN DISSEMINATIONS ON THE HEART

IN RELATION TO STATEMENTS IN

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A BRIEF ACCOUNT,

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IT is not without much reluctance that I bring the following pages before the public. I am induced to do so, not by any desire to uphold my own claims to scientific discovery, still less by any wish to disparage those of one who is no more, but simply by a conviction that it is my duty to defend myself from unjust imputations against my probity and charity. I shall endeavour most carefully to abstain from all expression of any other feeling than that of regret, that misunderstandings should have occurred, and that their evil effects should be now renewed by what I cannot but consider the indiscreet publication of *ex parte* statements, calculated to provoke reply. I shall therefore confine myself as strictly as possible to an explanation of my own conduct, by a brief account of facts, authenticated by references to published works, and to the testimony of others.

In the first edition of my *Rational Exposition, &c.*, published in 1828, is the following passage :—" The signs that are produced by the action of the heart I have found neither so certain in their indications, nor so intelligible in their causes, as those which I have hitherto described ; and I have therefore deemed it proper to postpone any attempt to include them within the plan of this work, until more extensive observation and study shall have supplied the desired intelligence." —page 46.

In 1829, my attention was further directed to the phenomena of the heart, in consequence of the observations of Mr. Turner of Edinburgh, which called in question the accuracy of some of Laennec's views. Dr. Forbes, alluding to Mr. Turner's remarks, concludes :—" I recommend this subject to the especial

notice of Dr. Williams."—Transl. of Laennec, third edition, 1829, p. 559.

Soon afterwards, I found that Dr. Hope was pursuing the subject of diseases of the heart ; and therefore, except in clinical study, I did not then prosecute it further : only at the request of Dr. Hope, I assisted at some of his experiments (Aug. 10, 1831), and revised the manuscript of his work before it was printed in November 1831.

By his express desire, I wrote a review of Dr. Hope's work, which appeared in the *London Medical and Physical Journal* for December 1831 and January 1832. In this review, I pointed out certain defective points in the experiments and views with regard to the sounds of the heart. "We do not think that Dr. Hope has completely established these points, as far as relates to the physical causes of the sound." (Then follows a discussion as to the cause of the first sound.) And, in conclusion :—"This matter is, therefore, still open to discussion," &c.—*Lond. Med. and Phys. Journal*, Dec. 1831, p. 518.

Nearly two years elapsed without anything further being attempted by Dr. Hope in the way of experimental investigation, although extended observation had more convinced me that it was needed. In September 1833, I appended to a second edition of the *Rational Exposition*, a summary of the chief views which had been broached on the subject of the motions and sounds of the heart, with a discussion as to their several merits and defects, a selection of the most tenable of these views, and suggestions for bringing them to the test of further experiment. To show how this appendix paved the way for the experiments, I will quote a few passages.

"I have added an appendix, in which are discussed various unsettled points connected with the auscultation of the heart, more with a view to guide future inquiries, than to settle the matter by argument."—Preface to Second Edition of my *Rational Exposition*, Sept. 1833.

“ Although it seems fairly established, that the first or dull sound is produced by the systole of the ventricles, and the second or quick one by their diastole, it is by no means clearly explained in what way these actions generate these sounds. The following causes have been severally assigned as physically capable of generating the first sound during the systole of the ventricles.

“ 1. The collision of the particles of fluid in the ventricles.—(Dr. Hope.)

“ 2. The rush of blood into the great arteries.—(Mr. Carlisle.)

“ 3. The closing of the mitral and tricuspid valves.—(M. Rouanet.)

“ 4. The muscular contraction itself.

“ The first of these explanations is ingeniously proposed by Dr. Hope, but he advances no facts in direct proof of the hypothesis. In a number of experiments which I have made on the generation of sound, I have found liquids, of all bodies, the most difficult to excite to sonorous vibration.”—p. 198. * * * “ On making an experiment with a gum-elastic bottle, by filling it with water, and then forcibly compressing it by the end of the stethoscope, I have failed in procuring any sound at all approaching to that of the heart’s construction,” &c. * * *

“ If this (the second explanation) were true, the large arteries, rather than the heart, would be the principal seat of the sound. * * * I shall not pursue the illustration of this (the fourth) explanation further, for I introduce it here only interrogatively, as deserving a place among other views, on the claims of which future observation and experiment must decide.”—p. 200.

“ We now come to the subject of the second sound, which, although certainly occurring at the moment of the diastole of the ventricles, has received several different explanations as to its physical cause. The only two which appear tenable in the present state of our knowledge are :—1. The reaction of the arterial columns of blood against the semilunar valves ; 2. The

impulse of the blood from the auricles refilling the ventricle at its diastole.

“ 1. The first of these bears a very inviting aspect, for the second sound is just of that abrupt flapping character, that might be supposed to result from the action of a thin valve. But it may be objected to this view, that the arteries, more than the heart, should be the seat of the sound. * * * Still I do not consider this view entirely disproved, and it should claim attention in future investigations.”

“ 2. This is Dr. Hope’s explanation of the second sound, &c. * * * It needs, however, as Dr. Copeland observes, further confirmation. I would add, that the whole subject of the sounds of the heart requires further research; and I shall have accomplished a good object if these remarks should induce Dr. Hope, who has already thrown so much light upon it, to follow up the investigation, until he shall have cleared away the difficulties and doubts that at present beset it.”—p. 201.

Another year passed away without any attempt being made by Dr. Hope to pursue the inquiry. I had mentioned the subject to the late Dr. Mackintosh of Edinburgh, and expressed my regret that it should be left in the present unsettled state, when a few experiments might decide it. Dr. M. then proposed that if I would come to Edinburgh, at the next meeting of the British Association, he would find means to enable me to make these experiments. This meeting took place in September 1834, and having some communications to make at it, I repaired to Edinburgh on the 4th of September, with the full intention of performing the experiments. I found that Dr. Macintosh was absent on the continent, on account of his health; but the following letter will show that I did not at once abandon my intention:—

68, *Torrington-square*, 5th Aug. 1839.

My dear Sir,—I perfectly well remember, that when I saw you in Edinburgh at the meeting of the British Association, in September 1834, you stated that a great object of your visit was to perform experiments to decide

certain points respecting the physical causes of the sounds of the heart ; and I perfectly remember further, that the reason that such experiments were not performed was entirely owing to our not being able to procure animals for your purpose.—I am, my dear Sir, very truly
yours, W. SHARPEY.

Dr. C. J. B. Williams.

On my return from Edinburgh (Sept. 18), I told Dr. Hope that I had intended to bring to the test of experiment the doubtful points which I had often mentioned to him. I again recommended him to undertake the matter, and offered him my assistance.

It was not until after this, as it is stated in the Memoir of Dr. Hope, on the first of October, that certain modes of performing the experiment occurred to him ; and in the course of that month he made a preliminary trial, assisted by Mr. Henry James Johnson. This trial led to no results.

On the third of November Dr. Hope made another attempt to perform the experiments, at Mr. Field's, in Oxford-street, Dr. Thomas Davies, Mr. H. J. Johnson, Mr. Field, and myself, being present by invitation. No satisfactory results were obtained from this attempt.

I was anxious that this inquiry should be prosecuted, as a third edition of my work was in the press, and was unwilling to leave the matter undecided merely for want of a few experiments. After fruitlessly urging Dr. Hope to proceed with the experiments, I resolved again to undertake them myself. Accordingly, in February, 1835, I applied to Sir B. Brodie for some woorara, requested the use of the dissecting-room in Kinnerton-street, and engaged the assistance of the lecturers in that school, and of others, including Dr. Hope.

The following letters will show that I acted under the full impression that I was the only directing and responsible party throughout the experiments. The statement in the memoir, "On this occasion Dr. Hope consented to give Dr. Williams an equal share in the experiments" (p. 154), is wholly without foundation.

(From the late Mr. Field, Veterinary Surgeon.)

224, *Oxford-street*, Aug. 16, 1839.

Dear Sir,—In reply to your note I have much pleasure in stating, that I remember perfectly well your calling upon me, and saying, that as Dr. Hope would not proceed with the experimental investigation of the sounds of the heart, you intended to do so yourself; at the same time you asked me where you could procure animals, and invited me to be present at the experiments. This was, I believe, in the early part of 1835, certainly prior to the performance of your experiments at the Kinnerton-street school.—I am, dear Sir, your's faithfully,

JOHN FIELD.

To Dr. C. J. B. Williams.

(From Mr. Tatum, Lecturer on Anatomy, and Proprietor of the Anatomical Theatre, Kinnerton-street.)

My dear Williams,—I have no hesitation in stating, in answer to your note, that you, and you only, applied to me for the use of the dissecting-rooms, in Kinnerton-street, in February 1835, for the purpose of performing experiments to explain some doubtful points relative to the sounds of the heart, alluded to in your appendix, published in 1833. I assisted at some of those experiments; and I considered them to be solely under your direction.—Ever most truly yours,

THOS. TATUM.

7, Berkeley-street, Aug. 16, 1839.

(From the late Mr. Good, Assistant-Surgeon to the Lock Hospital, &c.)

15, *New Burlington-street*, Aug. 17, 1839.

My dear Williams,—I am sorry I was prevented returning an answer to your note by its bearer. I very well remember your giving me your appendix on the sounds of the heart, &c. and you afterwards asked me to be present to witness certain experiments on the motions and sounds of the heart, which you purposed performing on asses, at the Anatomical Theatre, in Kinnerton-street. I was consequently present during the performance of the majority of these experiments;

and on several occasions you consulted me as to the best mode of performing them, and requested me to assist you, which I did. I certainly considered all the experiments I witnessed as performed at your suggestion; and that they were under your entire direction.

Believe me to be yours very faithfully,
W. H. Good.

(From Mr. Henry Charles Johnson, Demonstrator of Anatomy, &c.)

6, *Saville-row*, Aug. 22, 1839.

My dear Williams,—Many of the circumstances connected with the experiments made on asses, in 1835, have, I fear, escaped my memory. You, I remember, asked my permission to use the dissecting-rooms in Kinnerton-street, and requested my assistance at the experiments you were about to perform. You explained also to me the nature of the inquiry, and desired me to peruse the appendix to which you have alluded. I certainly considered that the experiments were made under your direction, not having had communication with any other person on the subject, and it was not until after their completion that I was made acquainted with Dr. Hope's claim to participation in their arrangement.—Believe me, dear Williams, yours very faithfully,

HENRY CHARLES JOHNSON.

(Statement of the Porter to the Anatomical Theatre.)

I recollect well that in February 1835, Mr. Tatum and Mr. Henry Charles Johnson ordered me to get the room ready for Dr. Williams, that he might perform some experiments. After these experiments were performed, Dr. Williams paid me for the animals, and I received no order or payment from any other person.

(Signed.)

HENRY HEMSON,

Porter to the Anatomical Theatre,
Kinnerton-street.

Aug. 17th, 1839.

It is therefore plain, that I had the entire arrangement and direction of the experiments; but I did not refuse to avail myself of such suggestions of those who

assisted me, as seemed to me likely to be useful. I also defrayed all the expense.

It was not until my account of these experiments* was in the press, in the beginning of March, that Dr. Hope claimed "at least an equal share in them," and disputed my right to publish them as my own. (See his Letter, "Memoir," &c. p. 155.) He wished the matter to be settled by arbitration of two friends, naming Sir Benjamin Brodie as willing to act for him. I was so convinced of the clearness of the case, that it seemed an unnecessary formality to name another arbiter on my part, and I was quite willing to abide by Sir Benjamin Brodie's decision, which was as follows:—

14, *Saville-row*, March 19, 1835.

My dear Sir,—I understand from yours and Dr. Hope's statements,—

1st. That you and Dr. Hope have both been engaged for a considerable time in researches respecting the pathology of the heart.

2ndly. That Dr. Hope formerly instituted experiments, with a view to illustrate this subject, at which he invited you to be present.†

3rdly. That since then, you and Dr. Hope have been in the habit of discussing questions arising out of these experiments, and that you contemplated making others conjointly.

4thly. That you frequently urged Dr. Hope to proceed with the projected experiments, but that his various engagements prevented his doing so.

5thly. That at last you applied to Mr. Tatum for the use of the new dissecting-room; procured animals for the purpose of the experiments; asked several gentlemen to assist you in making them; and invited Dr. Hope to them also.

* It must be stated that the experiments were successful in deciding some very important points against the views of Dr. Hope, as set forth in all his publications, and in my Appendix of 1833.

† That this refers to Dr. Hope's experiments in 1831, is plain from the next propositions.

In addition to all this, I am informed by some of the gentlemen who assisted you, that the experiments were made almost entirely under your direction.

Now, if these statements be correct, I own that I do not see that Dr. Hope can well complain of your making use of the experiments in the new edition of your work, now in the press; at the same time, I am of opinion, that in doing so, you should be careful to explain what share Dr. Hope had in projecting and planning the experiments in the first instance, and that you should acknowledge whatever assistance you derived from his suggestions at the time of the experiments being made.—I am, dear Sir, always yours truly,

B. C. BRODIE.

Dr. Williams.

Every one to whom I have shown the foregoing letter* has understood it, as I did, that it recognizes my full claim to the experiments, whilst it requires me to acknowledge whatever assistance I may have derived from Dr. Hope, both in planning and in performing them. This acknowledgment I fully and scrupulously made in the following passages.

“I concluded the appendix before quoted, by recommending Dr. Hope to investigate experimentally these points, which were there shown to be doubtful; but his engagements and other circumstances having prevented him from doing so, I lately undertook the task. I made a point of ensuring Dr. Hope’s presence and testimony at the experiments, and I conferred with him and several other gentleman as to the best mode of performing them.”—Third Ed. p. 170.

“I was present at an experiment attempted by Dr. Hope in November last, at Mr. Field’s; and he then

* It was stated by Dr. Hope, and is repeated in the “Memoir,” that Sir B. Brodie made a mistake in this arbitration; but no proof has been given of any such mistake having been made. I was printing the experiments for my work, when Dr. H. claimed “at least an equal share in them,” requiring me to stop the press until the claim of each had been decided.—(See his letter, “Memoir,” p. 155). Sir B. Brodie’s letter established my claim only.

planned modes of suspending the action of the valves, similar to some of those afterwards adopted in my experiments."—Ibid.

"This observation was, I think, suggested by Dr. Hope."—p. 171.

"The results, after Obs. 6, were witnessed by Dr. Hope, Mr. H. Johnson, and myself, the other gentlemen having left. The observations were noted during or immediately after the experiments; generally by Dr. Hope or myself."*—p. 172.

"This observation was suggested by Mr. Keate, and observation 2 by Dr. Hope."—p. 173.

"This experiment was repeated several times by Dr. Hope and myself."—p. 173.

"Obs. 8. Experiment 6 was repeated with the same result, and whilst Dr. Hope listened, I withdrew the awl from the aorta. He immediately said, 'Now I hear the second sound.' I then removed the hook from the pulmonary artery; Dr. H. said, 'Now the second sound is stronger, and the murmur has ceased.'"
—p. 174.

Believing, as I did, that my claim to the experiments was secured by Sir B. Brodie's decision, and my own account being printed, I had no hesitation in sending Dr. Hope the original notes, according to his request expressed through Dr. Macleod in the following letter:

My dear Sir,—I have seen Dr. Hope, who agrees to the proposals I mentioned to you this morning, viz. 1st. That each party should be at liberty to make use of the experiments in the forthcoming edition of his work,—due reference being made to the cooperation of the other in the investigation: and 2d. That you should let him have the original notes, in order to prevent any discrepancy as to details and results. The bearer is therefore directed to wait for them. As under these circumstances, there appears to remain no

* I still possess the original notes of the second experiment, which are all in my own handwriting. Those of the first were written partly by Dr. T. Peregrine, and partly by Dr. Hope, who retained them after I sent them to him.

point of dispute, I presume that you will meet on good terms as heretofore.—Yours faithfully.

March 22, 1835.

R. MACLEOD.

On this letter is written the following memorandum :

“ *Mem.*—The notes were sent with the stipulation expressed to Dr. Macleod, that although I would not object to Dr. Hope’s publishing the experiments with due acknowledgments, I must continue to consider, and to publish the experiments as mine.

“ C. J. B. W.”

On writing to Dr. Macleod, to enquire whether he had any recollection or memorandum on these points, he returned me the following answer :

1, *Lower Seymour-street*, Sept. 7, 1842.

My dear Sir,—I really cannot at this distance of time call to mind all the details with regard to the several points to which you refer in your note of to-day.

I have no hesitation, however, in stating, that you appeared to me to be the principal party in conducting the experiments alluded to ; and that I have ever since taken this view of the question, both in my own mind, and in communicating with others on the subject.—I remain, yours very truly,

R. MACLEOD.

P.S.—I may add, that on reading your published account of the experiments, it did not appear to me that you had in any degree departed from the terms of that agreement of which I had been the medium.

R. M'L.

From the statements in the *Memoir of Dr. Hope*, it appears, that one source of misapprehension on his part was in his supposing that I had agreed “to acknowledge the experiments to be conjoint.”—(p. 158.) It is uncertain where this mistake originated ; for myself, I can truly affirm that I never made such an agreement. From the time of receiving Sir B. Brodie’s letter up to this hour, I have on all occasions conformed to what I considered the terms of the letter,

by describing the experiments as mine, or performed by myself, with the assistance of Dr. Hope and others.

It appears, that Dr. Hope founded his claim to these experiments on his having "invented" a mode of performing them, and having committed to writing certain propositions descriptive of this mode. But even on this ground of claim, he overlooked the prior contributions to this invention in my appendix, published more than a year before : and it did not occur to him that I could not be about to perform the experiments in Edinburgh, without having devised some plan to be pursued, which was nearly a month before he is said to have "invented" his mode of experiment.

Yet to avoid even the chance of not acknowledging every particular of aid derived from him, I inserted the clause quoted above, p. 11,—“I was present at an experiment attempted by Dr. Hope,” &c.

My experiments were conducted without reference to Dr. Hope's propositions, but with the direct object of deciding between the views stated in my appendix. I only made a few memoranda to guide us, which I have preserved, as I wrote them before making the experiment. They are authenticated by two slight verbal alterations in Dr. Hope's handwriting.

Experiment I. — To determine the situation and cause of the second sound.

Lay open the pericardium to the base.

1. Try, by the stethoscope, where the second sound is loudest, at the mouths of the arteries or at the heart.

2. Try if pressure on the arteries at the moment of each systole will stop the second sound.

3. Try if pressure on the auricles just before each systole will stop the second sound.

4. Examine anatomy of the valves preparatory to Experiment II.

Experiment II. — To determine the situation and cause of the second sound.

1. Pass the curved awl into the aorta, and draw back the aortic valves. Observe if this stops the second sound.

Experiment III.—To determine the situation of the first sound.

Pass the awl through the auricle, and pull back the mitral valve: observe how this affects the first sound.*

Put the heart in warm water and galvanise, and observe if sounds continue.

The only remaining point that requires explanation, is the statement that I declined an arbitration proposed by the Rev. Mr. Niven. Now, I can appeal to Mr. Niven in support of my assertion, that up to the summer of 1839, I was most desirous that the difference should be adjusted; for I entertained no feeling of resentment, nor was it on my part that the alienation had begun. It was not until that summer that Dr. Hope expressed to Mr. Niven his wish for another arbitration, with a view to reconciliation. This was in the midst of the contest for St. George's Hospital, which of course prevented me from entertaining the subject at that time. Shortly afterwards, however, I made many attempts to bring the matter to arbitration, having solicited several eminent men, whose position in the profession would, it was hoped, make their decision final, to undertake the office; but I could not prevail on them to do so. At last, at the recommendation of Dr. Latham, I applied to the censors of the College of Physicians, with the proposal, that in their official capacity they should arbitrate between two physicians connected with their body. It was on this occasion that I obtained several of the documents printed above, dated August 1839. The censors' board, after some deliberation, declined to act in this matter.

Thus baffled in all my attempts to effect an arbitration, I was in the meantime obliged to defend myself against imputations conveyed by statements in the third edition of Dr. Hope's work, which, although pub-

* A similar experiment was afterwards made by Dr. Hope and Mr. Mayo, and more recently by the London Committee.

lished in May, I did not see until August of that year. These statements, both by their matter and by their tone, destroyed, in my mind, all hope of reconciliation, and excited me to publish a reply, the warmth of which I now regret, although I cannot deviate from its substance.

But I can sincerely affirm, that my resentment was of short duration ; and for many months before Dr. Hope's death, I declared to several persons who were aware of the dispute, (some of them his own relatives), that although I believed he was under misapprehensions with regard to my conduct, I retained no feeling of animosity towards him.

In conclusion, after reading the "Memoir," I can only lament, (and in saying this I blame myself) that the bond of peace was not more earnestly sought and openly displayed between those who professed to be followers of Him who is the Prince of Peace.

May the history of this controversy be a warning to those who are ready to dispute for points of scientific or literary reputation. Of what value is any credit arising out of these experiments, in comparison with friendship, peace of mind, and Christian consistency which may have been compromised in the dispute?

P.S. I think it is due to the institution with which I am connected, to correct the following statement :—
 "He (Dr. Hope) was told by the late Dr. Birkbeck, a leading member of the council of University College, that on Dr. Elliotson's resignation, the chair of the Practice of Physic would be offered to him."—p. 174.
 Dr. Birkbeck had ceased to be a member of the council several years before Dr. Elliotson's resignation ; and if Dr. Birkbeck made any such intimation to Dr. Hope, it was without the sanction of the Council.