

OBSERVATIONS

IN REPLY TO

C. WHARTON JONES'S STRICTURES.

By DR. MARTIN BARRY.

[*From the London Medical Gazette for Aug. 17, 1839.*]

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My claims to originality in my "First Series" of Researches in Embryology* are before the scientific public, and by their deliberate opinion will be ultimately decided. It is not out of any anxiety to vindicate these claims that I offer the present communication; it is to repel certain charges made by T. Wharton Jones, in the MEDICAL GAZETTE, July 20, 1839, having a personal bearing, and calculated to induce an impression of unfairness and want of candour on my part, in alluding to the works of other physiologists in general, and his own in particular. My "pretensions to novelty, originality, and correctness," are before a tribunal on whose intelligence and impartiality I can rely, and from whose decision I will not shrink.

The first accusatory remark of T. Wharton Jones is an allusion to the following passage, which he quotes from my "First Series":—

"It may not be improper, in the first place, to furnish an idea of what has been already published on some branches of the subject; *for it is one to which the attention of physiologists in this country has scarcely begun to be directed.*"

To this quotation T. Wharton Jones appends the following note:—

"Had there been no other, the labours of Dr. Allen Thomson in this field ought to have been sufficient to have obviated the reproach so sweepingly implied, in Dr. Barry's allegation, against the physiologists of this country."—*Loc. cit.* § 1.

I think it is obvious from the context of the passage cited, when taken in conjunction with what had preceded it—and which T. Wharton Jones has not quoted—that I meant to make no invidious allusion whatever to the labours of English physiologists on the subject of embryology in general, but that the remark objected to in that passage has a reference only to that portion of the science of embryology which regards the existence of the microscopic ovum in the ovary of mammals, as discovered by Baer, and upon which so little had

been observed or written in this country as compared with German research, that I thought—and still think—myself perfectly justified in stating, that the attention of physiologists in England had scarcely begun to be directed to it*.

One of the objects of T. Wharton Jones, in his communication above referred to, was to apprise me that a paper of his—read before the Royal Society in 1835, but not printed in the Philosophical Transactions—was published in the MEDICAL GAZETTE for Session 1837-38, pp. 680-88; London, 1838†. I was not previously aware that the paper in question, by T. Wharton Jones, had been published, or that it was any where accessible, otherwise I should have deemed it right to include some statements made by that author along with similar ones previously made by others, which I noticed in my "First Series," for the purpose of showing that they differed from my own observations. I have now seen, in the MEDICAL GAZETTE, No. 18, 1838, the paper to which T. Wharton Jones has directed my attention; and I refer him to my "First Series" for the points of difference just alluded to.

I will here observe, that there is the less necessity for going into the scientific part of the question at any length with T. Wharton Jones, inasmuch as I find in his paper no new fact whatever, with the single exception that he has stated the Graafian vesicle of mammals to be analogous to the capsule of birds‡. With that one exception there is, indeed, nothing that appears to me

* It affords me particular pleasure to quote the following from an eminent German author:—"The best and most condensed survey of the whole doctrine of generation—based on his own observations—has been given by Allen Thomson, in the article 'Generation,' in Todd's Cyclopædia of Anatomy and Physiology, vol. ii. p. 424."—(Rudolph Wagner, Lehrbuch der Physiologie, Erste Abtheilung. Physiologie der Zeugung und Entwicklung, p. 6. Leipzig, 1839.) I may add my belief, that no one is more competent to offer an opinion on this subject than Professor Rudolph Wagner.

† A considerable portion of it was republished by him in the MEDICAL GAZETTE, July 20, 1839.

‡ I was not aware that T. Wharton Jones had asserted the analogy above-mentioned at the time when my "First Series" was written, for the reason before given. But it will presently be seen how much he knew of the real nature of either of the structures in question. As to the

* Philosophical Transactions, 1838, Part II., page 301.

to have been new at the time when he presented his paper to the Royal Society, in 1835; and to tread over beaten ground for the mere sake of personal controversy, is neither pleasant nor profitable. But there is one point upon which T. Wharton Jones seems to set up "pretensions to novelty, originality, and correctness," on which I must be permitted to say a few words.

In 1835 this author presented to the Royal Society an account of the germinal vesicle discovered by him in the mammiferous ovum; adding, in a postscript in reference to this discovery, "it would appear that M. Coste has anticipated me." The Memoir of T. Wharton Jones not having been printed in the Philosophical Transactions, the author published it at full length in the MEDICAL GAZETTE, in 1838 (No. 18, p. 680), with the same postscript in which he had yielded the priority of discovery to Coste. It is remarkable, however, that T. Wharton Jones had not in the meantime heard that in 1834 a thesis was published at Breslau, in which ten quarto pages were devoted to a description of the germinal vesicle as existing in the ovum of all orders of the class mammalia, and remarks connected with it. This thesis was by Bernhardt*, but the drawings are by Valentin, who seems to have been the discoverer of the vesicle in Germany. This thesis was particularly referred to in a translation I gave from the German of Valentin, in the Edinburgh Medical and Surgical Journal, No. 127, 1836. But farther, in the year 1834 R. Wagner sent to Müller's "Archiv" a particular description of the germinal spot, discovered by him within the germinal vesicle, a structure which he considered constant in the class mammalia—so extended had been his observations. This discovery by Wagner was mentioned in the Edinburgh Medical and Surgical Journal, No. 126, 1836, above alluded to, and it was at the same time stated in that journal, on the authority of Professor Johann Müller, that the account had been sent by Wagner to the "Archiv" in 1834. Upon these points T. Wharton Jones is silent.

T. Wharton Jones quotes two pas-

analogy between the ovum of the bird and the Baerian vesicle of the mammal, this was pointed out by several authors in 1834.

* Symbolæ ad Ovi Mammalium historiam ante prægnationem.

sages from pages 312 and 314 of my "First Series," in which I showed that a vesicle, called by me the "ovisac," in mammals, corresponded to one which had been denominated the "chorion" in other vertebrata; and also, that it was by those vesicles acquiring a covering susceptible of becoming highly vascular, that the structure is produced, usually termed a "Graafian vesicle" in mammalia, and a "capsule" in other vertebrata. T. Wharton Jones adds, "All this is true; but I dissent from the allegation that it was not clearly stated before. I can answer for myself, that I am not one of the authors who have mistaken the ovisac of birds for the analogue of the chorion of mammals." (*Loc. cit.* § 6.) I did not say that T. Wharton Jones was one of the authors who had mistaken the ovisac of birds for the analogue of the chorion of mammals. On the contrary, it does not appear from his papers that he ever saw the vesicle which I denominated the "ovisac" of birds, in its originally independent state; and as to the corresponding vesicle, called by me the "ovisac" in mammals, I am not aware that any observer had described it before myself. With what degree of justice, therefore, T. Wharton Jones made the above assertion, will now be obvious.

T. Wharton Jones says (MED. GAZ. 1838, No. 18, p. 682), "I shall mention the points most worthy of notice in the structure of the hen's egg, in the *first stage* of its formation," and he then describes the capsules, as "varying in magnitude, from that of a *millet-seed* to a full-sized yelk." In my "First Series," (plate v. fig. 22), the ovisac—*i. e.* the subsequent inner layer of the capsule—of the bird was figured (having the incipient ovum within it) of a size so minute that some thousands of such ovisacs might be contained within a "*millet-seed*." Yet, according to T. Wharton Jones, he described the hen's egg in the "*first stage*" of its formation.

T. Wharton Jones says (MED. GAZ. 1838, No. 18, p. 686), "I might also add, that the granular membrane, proligerous *disc*, and granular fluid of the Graafian vesicle, are probably superadded parts, of which there is no trace within the capsule of the bird's ovary." In my "First Series," it was shown that a fluid containing "granules

essentially the same [as the very remarkable ones in the ovisac of mammalia] exist at an early period in the ovisac of birds."—(*L. c.* p. 329, pl. v. fig. 23; pl. viii. fig. 76). It was also shown (pp. 329, 330), that the situation of the ovum in the Graafian vesicle had not been understood by those who stated it to be contained in a "disc." T. Wharton Jones took some pains to point out the resemblance between the Graafian vesicle of the mammal and the capsule of the bird. Yet now, when made aware how much he knew of the real nature of either of these objects, he considers them "structures of secondary importance in the economy of the ovum—structures which, on that account, had been less carefully studied" (*MED. GAZ.* July 20, 1839, § 9).

To vindicate my own motives and conduct against charges of misrepresenting or undervaluing the labours of others, or claiming undue credit for my own, being my chief object, I have no wish to recriminate on T. Wharton Jones; but when he chooses to quote a passage from the abstract of a paper by me, as an illustration of my opinion upon an important subject of physiological investigation, I cannot help observing that it was not quite fair to have garbled it in the manner in which the following passage has been garbled:—"He (Dr. B.) has traced the chorion from stage to stage, up to the period when it becomes villous, and shows that it is not, as he formerly supposed, the thick transparent membrane itself of the ovarian ovum, but a thin envelope closely investing that membrane, and not appreciable as a distinct structure until the ovum has been crushed. * * * The chorion subsequently thickens, and imbibes a quantity of fluid, presenting a gelatinous appearance."—(*Loc. cit.* § 7.) The asterisks certainly indicate that something has been omitted, but the reader would understand thereby that what was omitted was not necessary to the proper understanding of the question in dispute between us. By the omission, however, which T. Wharton Jones thought proper to make, he has got rid of the difficulty of noticing certain anatomical facts, the "pretensions" of which "to novelty, originality, and correctness," it was, perhaps, easier to omit than answer*.

* The words omitted to be quoted by T. Wharton Jones were these:—"When the chorion first

Another paragraph of T. Wharton Jones (*l. c.* § 10) contains the following passages, quoted from the abstract of my "Second Series" of "Researches on Embryology"*:—

"The knowledge at present supposed to be possessed of the early stages in the development of that ovum (the mammiferous ovum) consists chiefly of inferences from observations on the ovum of the bird.

"But there exists a period in the history of the ovum of the mammal regarding which we have hitherto scarcely any direct or positive knowledge."

T. Wharton Jones then remarks—"For a perfect and unqualified contradiction of this very bold assertion, I would refer to my memoir, contained in the *Philosophical Transactions*, Part II. for 1837, page 339, already alluded to."

Now what is the "perfect and unqualified contradiction" of this "very bold assertion" for which T. Wharton Jones refers to his memoir? The contradiction thus alluded to appears to be certain inferences from observations on the ova of the frog and newt; consequently, more conjectural than demonstrative.

As to the "mode of origin of the chorion," another point on which T. Wharton Jones dwells at some length, my "Second Series," to appear in the forthcoming volume of the *Philosophical Transactions*, will show what are really the differences between the results of his observations and my own, on the relative value of which I leave physiologists to determine. I shall only farther remark, that T. Wharton Jones's opportunities of observation, as compared with my own, may be stated as *one to twenty-five*—that gentleman, on his own admission, having examined *four* rabbits, whereas the number devoted to anatomical inspection by myself exceeds *a hundred*; and also, that the paper in question will show that T. Wharton Jones has only renounced one erroneous "notion"—as to "the mode of origin of the chorion"—to fall into another.

admits of demonstration as a distinct structure, the ovum consists of *three* membranes; a state which the author *has seen* in an ovum no farther advanced than about an inch into the Fallopian tube."—(*Proceedings of the Royal Society*, No. 38, 1839.)

* *Proceedings of the Royal Society*, No. 38, 1839.

ANSWER TO THE
NOTE OF T. WHARTON JONES.

BY DR. MARTIN BARRY.

[From the London Medical Gazette.]

of Aug. 31, 1839

THE observations of T. Wharton Jones, contained in last week's number of the MEDICAL GAZETTE, reduce the whole of the matter in dispute to a mere question of priority as to the discovery in the mammiferous ovum of the germinal vesicle and the germinal spot. On that question the Editor of the MEDICAL GAZETTE says, p. 805, "a comparison of dates will determine the mere matter of priority." I adopt his criterion, and, giving the dates, as proved by publication, I leave his readers to judge to whom the priority rightfully belongs. By a glance at those dates, every candid critic will be able to determine whether any one in this country can participate with the professors on the continent in the honour of those discoveries.

Valentin and Bernhardt, having pub-

lished an account of the germinal vesicle in October 1834, and R. Wagner having sent to Müller's "Archiv" an account of the germinal spot in the same year, the discovery of both the vesicle, and the spot on the *inside* of it, were made known to the world previously to T. Wharton Jones's publication of his discovery of the germinal vesicle, and (the object he now supposes to have been) the germinal spot, in June 1835.

Yet T. Wharton Jones claims the priority of discovery of both vesicle and spot—a claim which the dates above stated satisfactorily refute.

One word more, with regard to the object which T. Wharton Jones supposes to have been the germinal spot. The description he has given seems to belong to a body, not *within*, but *external* to the germinal vesicle, and, if so, one that cannot in reality have been the germinal spot. His description is this:—"On one side of the vesicle there is a small *elevation*, which, *projecting among* the grains composing the walls of the granular sac, *fixes the vesicle in its place**."

* MED. GAZ. Jan. 27, 1838, p. 684.

REPLY TO T. WHARTON JONES'S
REJOINDER,

By DR. MARTIN BARRY.

[From the Medical Gazette of Sept. 14th 1839.]

T. WHARTON JONES, in a spirit more controversial than courteous, accuses me of "a pure invention" in alleging* that he had claimed priority of discovery as to the germinal vesicle and germinal spot in the mammiferous ovum. Let us see whether the facts of the case justify him in the use of such an imputation.

* MEDICAL GAZETTE, Aug. 31, 1839, p. 848.

In the answer which T. Wharton Jones, attempted to my former article, MED. GAZ. Aug. 24, 1839, he quotes the words of the Editor of that periodical, and adopts them as part of his own statement, not disclaiming or qualifying any thing therein alleged. Now the Editor, arguing on behalf of T. Wharton Jones, and speaking of the discovery of the germinal vesicle and spot, says as follows:—

"Hence, it appears that, like many other discoveries in science, the germinal vesicle of the mammiferous ovum was discovered about the same time in this country and on the continent. But it will be seen from Mr. Jones's memoir,

that he had investigated the subject so fully and successfully as to be able to give such a description of it as is only to be drawn from the united labours of Coste, Valentin, and Wagner*.”

He goes on to say, “*A comparison of dates will determine the mere matter of priority.*” Priority for whom? Not for Valentin—not for Wagner; but for T. Wharton Jones. How is this shown? The writer immediately tells us, that “Mr. Wharton Jones’s observations were made in the beginning of September 1834, and his memoir read before the Royal Society, 18th June, 1835.” Why are these dates given at all? To prove that which the writer said a comparison of dates would determine—the *matter of priority*: to prove, in fact, that T. Wharton Jones was not the last, but the first in the field,—and thus was the priority of discovery set up for T. Wharton Jones. This gentleman adopted, as I have said, the very words in which that claim was preferred, by republishing them as part of his own statement: and yet, in the face of all this, he now says, “This is a *pure invention* on the part of Dr. Barry”—and then adds, “I am not aware that any thing I ever wrote or said can be construed into such a meaning; and I do not perceive that you, Mr. Editor, in the remarks reprinted in the last number but one of the *MEDICAL GAZETTE*, advanced any claim of priority in my behalf.”—So much for T. Wharton Jones’s denial of having advanced a claim of prior discovery, and so much for my “*pure invention.*”

What I stated in answer to this claim of priority set up for T. Wharton Jones, was as follows: “Valentin and Bernhardt having published an account of the germinal vesicle in October 1834, and R. Wagner having sent to Müller’s ‘*Archiv*’ an account of the germinal spot in the same year, the discovery of both the vesicle, and the spot on the *inside* of it, was made known to the world previously to T. Wharton Jones’s publication of his discovery of the germinal vesicle, and (the object he now supposes to have been) the germinal spot, in June 1835†.”

As to observations made *previously* to publication, they must, of course, have been made previously in every case. T. Wharton Jones says he made his in September 1834: at what distance of time the observations of Valentin and Wagner preceded their respective publications, I know not; but I take it that the date of publication, in each case, is an authentic and correct criterion as to the question of priority: it is the public evidence of the fact; but as to the private transactions to which T. Wharton Jones alludes connected with his observations, I have now heard of them for the first time. Since, however

T. Wharton Jones now disclaims priority of discovery, I shall not dwell upon that part of the subject any longer.

But T. Wharton Jones states that this matter of the germinal vesicle and spot is only an episode to the original question, and then proceeds to say, “the main point in dispute, I beg it to be remembered, is contained in my communication of the 20th of July, the object of which was to disprove the assertion, that the study of embryology is ‘one to which the attention of physiologists in this country has scarcely begun to be directed,’ and to expose the unfounded nature of the pretensions to novelty, originality, and correctness, which Dr. Barry has introduced into both series* of his ‘*Researches.*’” Here T. Wharton Jones has only repeated his first unwarranted construction of a passage of mine, which I explained in a manner that showed it did not bear the meaning he had chosen to put upon it. When he thought fit to repeat his attack, it would have been only candid in him to have accompanied it with my explanation. As he has not thought proper to do so, I here subjoin it:—

“I think it is obvious from the context of the passage cited, when taken in conjunction with what had preceded it—and which T. Wharton Jones has not quoted—that I meant to make no invidious allusion whatever to the labours of English physiologists on the subject of embryology in general, but that the remark objected to in that passage has a reference only to that portion of the science of embryology which regards the existence of the microscopic ovum in the ovary of mammals, as discovered by Baer, and upon which so little had been observed or written in this country, as compared with German research, that I thought—and still think—myself perfectly justified in stating, that the attention of physiologists in England had scarcely begun to be directed to it†.”

Had T. Wharton Jones extracted more than a sentence isolated from the context, it would have shown that I had no intention of making a sweeping attack on British physiologists in reference to embryological science in general. I certainly never suspected, until I saw how my words had been misconstrued, that the most perverted ingenuity could have attributed to them a meaning so different from the true one.

As to the invidious allusion to what he is pleased to designate my “pretensions to novelty, originality, and correctness,” I shall only repeat what I before said‡, that they “are before a tribunal on whose intelligence and impartiality I can rely, and from whose decision I will not shrink.”

* *MED. GAZ.* for Aug. 24, 1839.

† *Ibid.* for Aug. 31, 1839, p. 847.

* *Phil. Trans.* for 1838 and 1839.

† *MED. GAZ.* for Aug 17, 1839.

‡ *Ibid.*

