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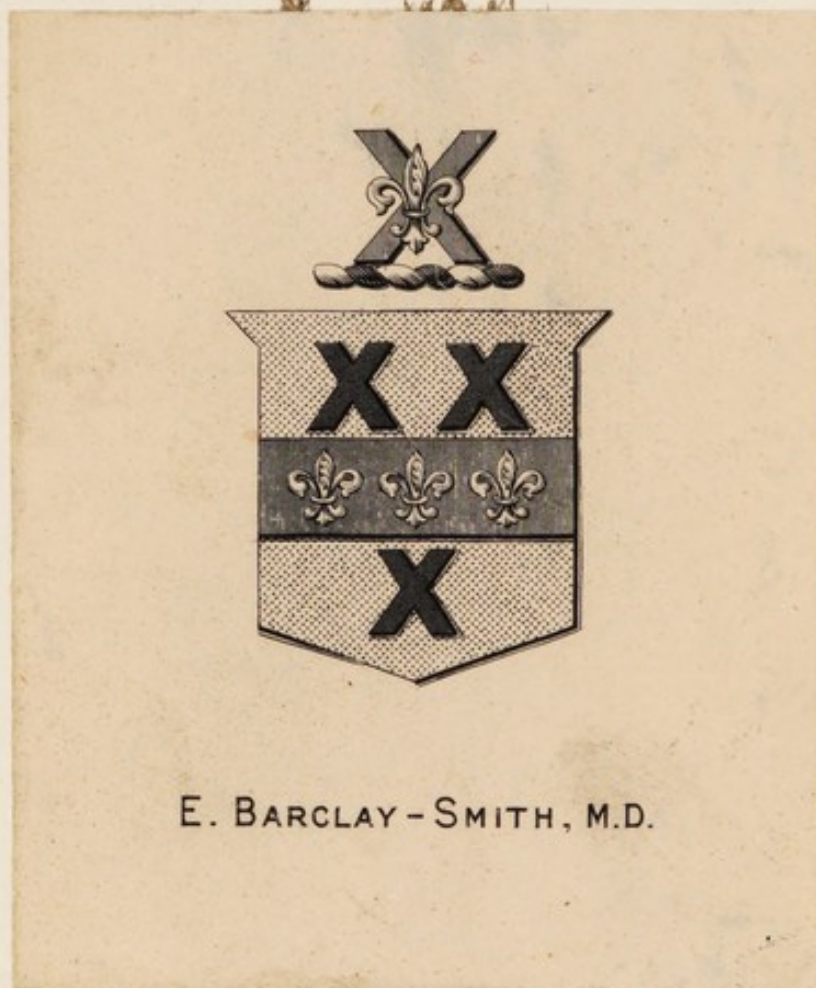


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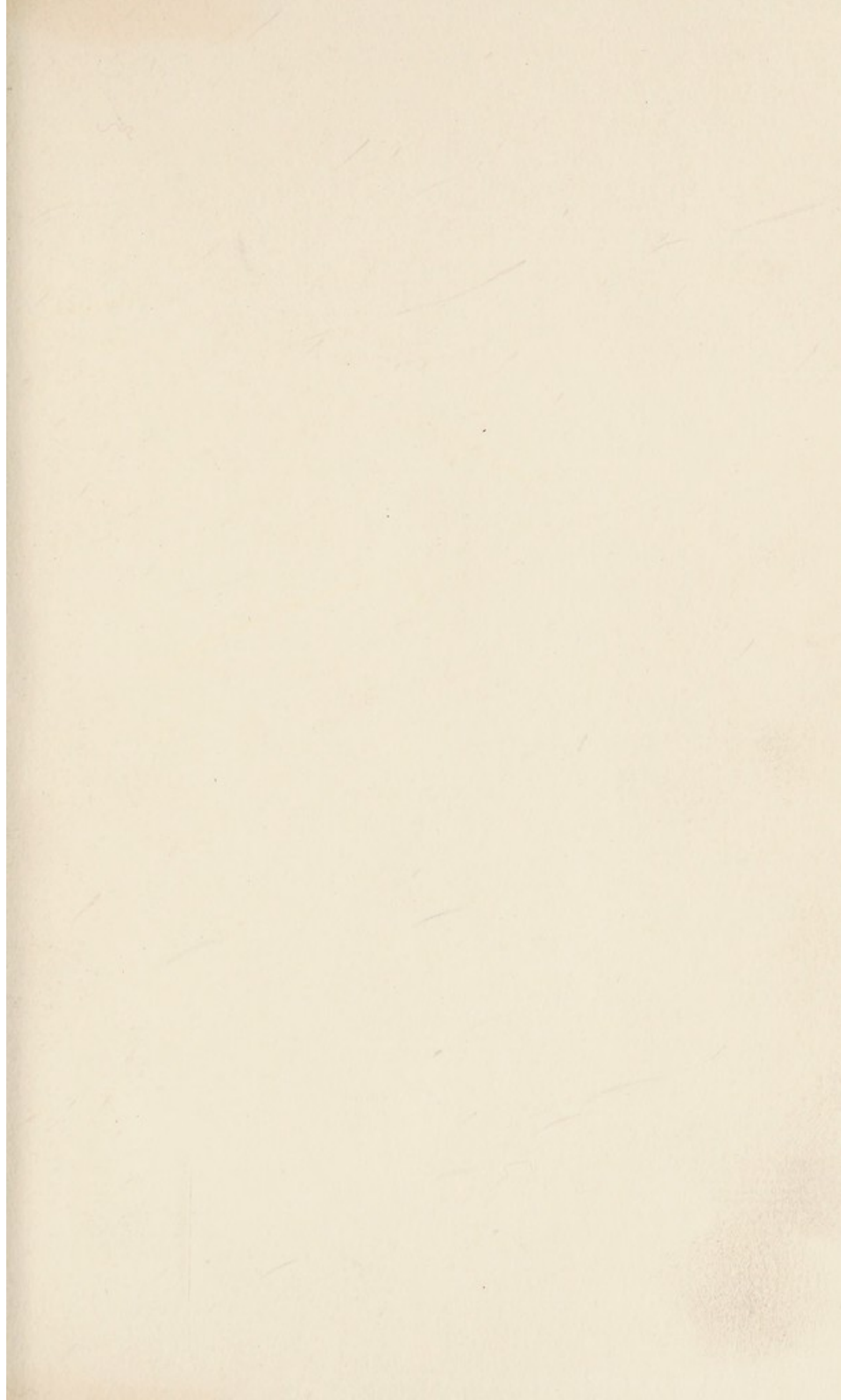


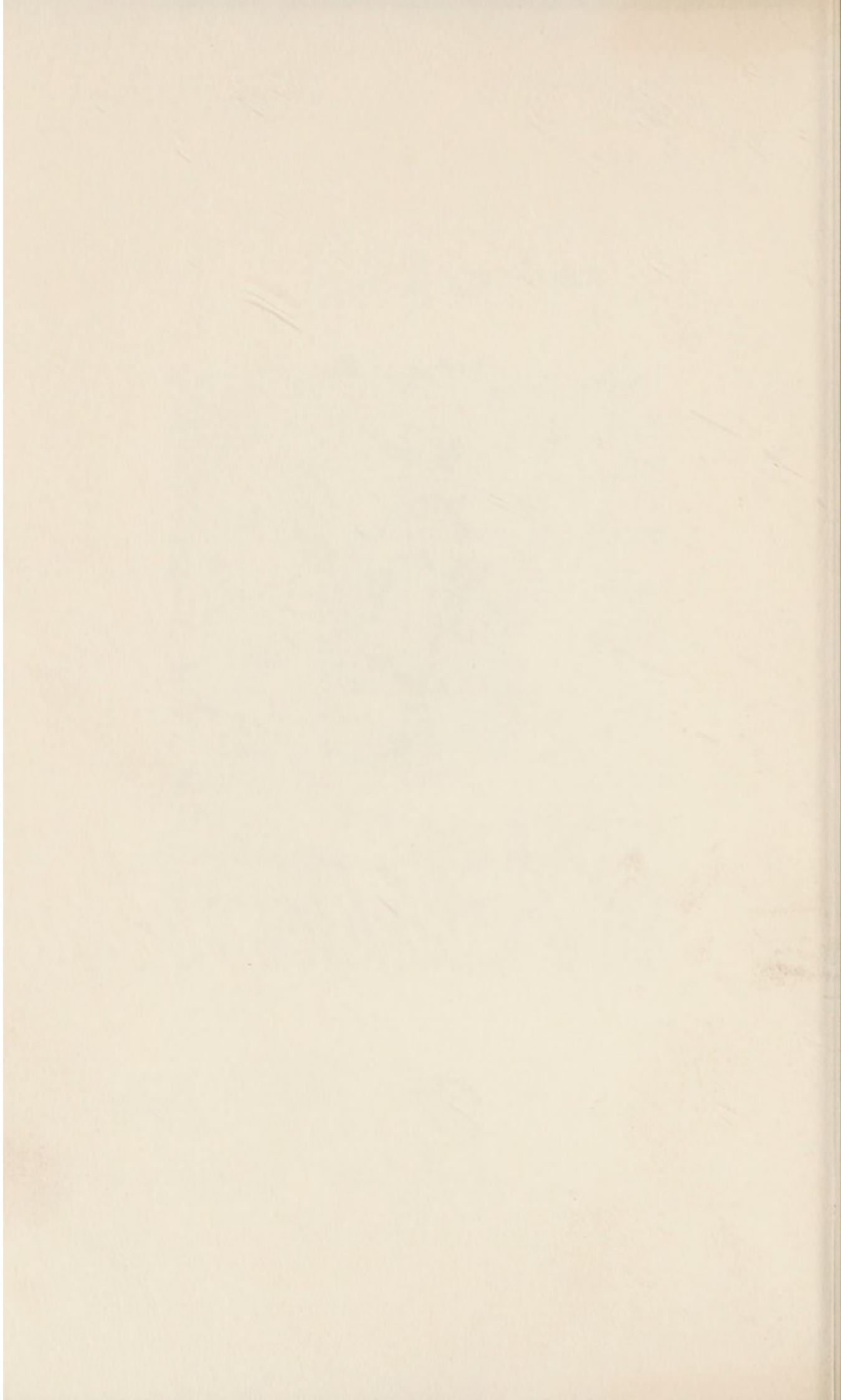
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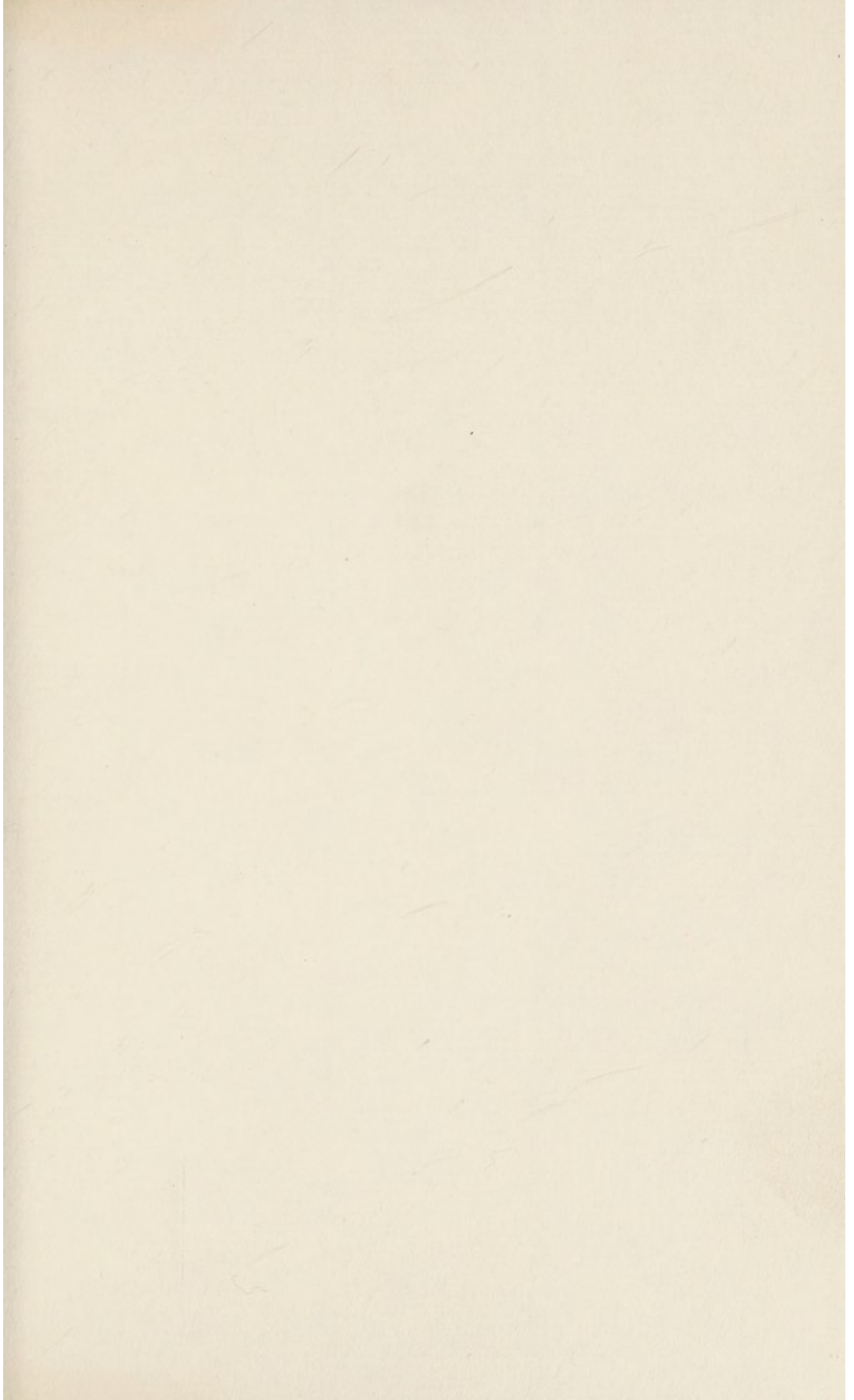
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OBSERVATIONS,

ANATOMICAL AND PHYSIOLOGICAL,

WHEREIN

Dr. HUNTER'S Claim to some DISCOVERIES
is examined.

WITH FIGURES.

BY

ALEXANDER MONRO junior, M. D. and Professor
of *Medicine* and of *Anatomy* in the University of
Edinburgh.

EDINBURGH:

Printed by HAMILTON, BALFOUR & NEILL,

AUGUST M,DCC,LVIII.

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I N T R O D U C T I O N.

IT gave me some concern, at my return from abroad, to find that I lay under the imputation of having prevented Dr. HUNTER from reaping the fruits of his industry and invention, by my publishing his Discoveries as my own.

If there appeared to the Doctor due grounds for suspecting that he was treated in that disingenuous manner, he had undoubtedly good reason to complain; and he acted a fair and open part in making his complaint public, since, by this means, he gave me, if I was innocent, the opportunity of justifying myself.

But it will likewise be acknowledged, that a person ought to be extremely cautious in making an accusation, by which the character, as well as the property, of another is attacked: For, should this accusation be proved not only unjust and ill founded, but evidently absurd, he draws upon himself that censure, which the discerning and impartial part of mankind seldom fail to inflict, where weakness and precipitancy disclose to them injurious detraction, under the disguise of truth and humanity.

These are general obvious maxims, of which perhaps I make an improper application. But, as I am as far from expecting that the strongest assertions, without proof, will influence the judicious Reader, as from wishing to bias him otherwise than by fact and argument, I hasten to these.

My being abroad for a considerable time after Dr. HUNTER's charge against me first appeared in the *Critical Review*, and my indispensable occupations since my return, have hitherto made it impossible for me to vindicate myself in a proper manner: Circumstances which have been particularly favourable to the Doctor, since he has thereby had the opportunity of painting his own cause in the best dress, and mine in the

worst ; whilst ignorance of many of the most material facts made it impossible for my friends, during my absence, to point out fully and clearly, the specious and partial colouring of the former, or misrepresentation of the latter. For, as they (*a*) proposed chiefly to prevent the first, and therefore generally the most lasting prejudices, which such reflections, if acquiesced in, might occasion ; they imagined, that, had they wrote to me for information, this design would have been frustrated by the long delay ; and they therefore printed, without my knowledge, what occurred to them with regard to this affair.

This last fact I intended only to have hinted, as an apology for the present publication ; but Dr. HUNTER, very unexpectedly indeed, puts me under the necessity of proving its truth. For altho', by his silence, he seems to allow, that the first of the two papers published in my defence, is, as it has been called, " A State of facts related by a friend (*b*) ;" yet he confidently alleges, that I have had part in the second, notwithstanding that my Brother names himself the Author of it (*c*). Dr. HUNTER's own words, which he makes conspicuous, by giving them the title of a *Postscript*, will best show his reason for this suspicion. " *P. S.* (*d*) Dr. ALEXANDER MONRO junior, who was abroad, has been lately in town, and we are therefore bound to believe, he has approved of the steps which his Brother has taken in his defence."

The plain narration of a few facts, will evidently prove how intirely groundless this insinuation is ; and, I am ashamed to say, will make it highly probable, that the Doctor's conscience must have rejected what his pen here affirmed.

I arrived not at *London* from *Holland* till *December* 10. late in the evening. But my Brother's letter is dated *December* 7. and was delivered that very day

(*a*) See Art. 8. of *Crit. Review* for *November* 1757. (*b*) See ditto. This Paper was wrote by my Father. (*c*) Art. 8. for *December* 1757. (*d*) Art. 9. for ditto.

day to the Authors of the *Critical Review*; for he has their answer, acknowledging the receipt of it, dated the morning of the 8th; that is, several days before I came to *Town*, and whilst I was yet in *Holland*.

Now, these are circumstances Dr. HUNTER cannot well be supposed ignorant of, since my Brother desired “ a copy of his letter to be sent immediately to “ Dr. HUNTER, to be examined and answered by “ him (e).” So that, without supposing any particular correspondence between the Reviewers and Dr. HUNTER, it is to be presumed, that letter was no sooner received by them, than sent to him.—The Doctor likewise knew the exact time of my being in *Town*: For I called, in company with my Brother, at his house, *December* 13. and, not finding him at home, left my name with his servant; and, at the same time, I desired him, as well as my Brother, to tell the Doctor, that I would have called more than once, but that I was only passing through *London* in my way to *Scotland*, and was to set out next morning. Besides, Dr. HUNTER does not date his reply to my Brother’s letter till the 22d of *December*: And, before throwing out in print such an insinuation, he had time enough to inform himself of the truth, which he could have done without difficulty, as I had the honour of talking to several Gentlemen with whom the Doctor is intimately acquainted; particularly to Dr. CLEPHAN Physician, Mr. WATSON Surgeon and Mr. HALLEY a Gentleman then attending Dr. HUNTER’s Lectures; who, as is common on such occasions, asked me, when I came to *Town*, and what stay I proposed to make. And, what to some may appear not the least to be admired, Dr. HUNTER here calls my Brother’s veracity in question, without assigning any reason for so doing; for my Brother, just before he dates his letter, says, “ He hopes Dr. HUNTER will excuse this, nor think
“ I

(e) *Cr. Review* for 1757, p. 523.

“ I have done him any injury, by doing justice to a
 “ Brother, who is *not present* to answer for himself.”
 D. M. *December 7. 1757. (f).*

To explain the motives influencing other people's actions, is a difficult task; and, I am far indeed from pretending, with certainty, to penetrate into the secret purposes of the Doctor. I shall, however, humbly offer to the Reader, my conjecture of the design aimed at by this remarkable *Postscript*; and leave it to the sequel to show, that this conjecture is not without foundation. The Doctor was, perhaps, conscious, that his accusation of me could be fully refuted, and his aspersions turned upon himself, by one acquainted with all the circumstances; and might therefore catch at any occasion to propagate the belief, that all that could be said to the purpose for my cause was said already; thereby to prejudice people against what I should publish: At the same time, he might wish to include me with my Brother, in a performance of his (g), for which no body will suspect or accuse him of Plagiarism. The Public, however, will, I flatter myself, be more just, and allow it is but fair, that both parties be heard before judgment is passed.

With regard to two subjects, upon which I had published my thoughts; the one concerning the *seminal ducts* injected with quicksilver, the other relating to the *Origin of the valvular lymphatic vessels*; the Doctor has repeatedly insinuated that I am guilty of Plagiarism from him; though he is obliged to own he has no certain knowledge in the case (b); and declares too he is far from wishing to convict me of it (i).

Whether the Doctor wishes to do what by *every means* he has endeavoured, is a question which he himself only can determine. For my part, I confess, I can't help thinking, that such wishes are inconsistent with the injurious suppositions accompanying them: And, at the same

(f) *Cr. R.* for ditto, p. 527. (g) Art. 9. for *Dec. 1757*;
 (b) *Cr. R.* p. 531. (i) *Cr. R.* p. 528.

same time, that they betray a weak attempt to procure the character of good nature, in order to gain the favour of the Reader, and more effectually undermine his antagonist; they prove how unmerited such a character is. Presuming too, that injurious reflections, if ill founded, return with double force upon him that uses them, I shall be as sparing of them as fact and argument will allow, and shall altogether shun wrangling about words and expressions; as these arts appear to me presumptions of a bad cause, which cannot bear a fair hearing.

But, before I enter upon the dispute, the Doctor will allow me to apply a rule of his own to himself. By my being lately in Town he thought himself *bound* to believe that “I approved of what my Brother had said in “my defence^(k).” By Dr. HUNTER’s having been all along in Town, he himself of course *binds* us to believe, that he approves of all that has been said on the other side. I have, however, other reasons which are more *binding*: Such are the Doctor’s referring to the papers published by the Reviewers, without disapproving any thing there advanced; and his *publishing*, in his Lectures, the principal part of what I shall venture to criticise.

To be more clearly understood, in treating both subjects, I shall first endeavour to vindicate my own title to the discovery, and then examine the grounds of the Doctor’s pretensions. And, following the order of time, shall begin with what relates to the *seminal ducts*.

OF THE SEMINAL DUCTS.

ABOUT twenty years ago, my Father fully described the manner of injecting quicksilver from the *Vas deferens* backwards into the other seminal ducts; but ingenuously owns he could not make it pass above
half

(k) See his *P. S.*

half way through the *Epidydimis* (l)——The illustrious Dr. HALLER, pursuing this experiment nearly in the same manner, had the good fortune, not only to force the quicksilver through the whole *Epidydimis*, but likewise to make it enter within the coats of the *testis*; where it filled what he calls *rete testis*, from which straight vessels go off; and from these he could, in some of his preparations, observe that the quicksilver had entered into the beginning of a few of the serpentine ducts: At this place it stopped, and was extravasated. It filled, however, and he has accurately explained, considerably more than can possibly be seen or known before the *testis* is opened; first, in a Dissertation at *Gottingen* (m); and afterwards, in the *Philosophical Transactions*, for the months of *January - - - April 1750* (n), where he has illustrated his description, with a figure.— On the 9th of *January 1753*, in attempting to make such a preparation, I was so lucky as to impel the quicksilver still farther into the seminal ducts than Dr. HALLER had done; making it fill, for a considerable length, a very great number of the serpentine ducts of the *testis*, into the beginning of which only it had penetrated in Dr. HALLER's experiments. This preparation was publicly demonstrated, the very next day, to the Gentlemen attending the College of Anatomy at *Edinburgh*; as has been already proved, and allowed to be so by Dr. HUNTER (o). Above a year and a half thereafter, I printed an account of this preparation, with figures (p), in which I have done Dr. HALLER all justice, and have assumed to myself nothing which he had described; but,

(l) *Medical Essays of Edinburgh*. vol. v. art. 20. § 29.

(m) *Observ. de vasis seminalibus*. *Gottingæ*.

(n) *Philosoph. Transf.* p. 494. § 12.

(o) *Gr. R.* p. 525. 530.

(p) *Physical Essays of Edinburgh*, published 1754. vol. i. art. 16. My delaying this publication so long, and being so far from concealing my intention of publishing from Dr. HUNTER, that my Figures were shewn to him at *London* before they were engraved, *C. R.* p. 526. are strong presumptions that I was ignorant of the Doctor's having employed himself on this subject.

but, in his name, have given his description and figure, with its explication. What I pretend to, is, to carry on the description of the feminal ducts farther than Dr. HALLER had done. I have besides painted a very remarkable vessel, sent off from the *Epidydimis*, which Dr. HALLER had given the first hint of, but had described in words only: And I have proposed some experiments, by which any person, though not expert in dissection, may be convinced, that, by far the greater part of the *Epidydimis* is composed of a single tube convoluted in a most wonderful manner.

So far is uncontroverted: But then Dr. HUNTER alleges, that I had first learn'd the description of the internal *tubuli* of the *testis* from him at second hand, for reasons which I shall explain in his own words, (q).

“ About the beginning of *November* 1752, in presence
 “ of Mr. GALHIE and some others, I injected the *vas*
 “ *deferens* in the human body with Mercury, and by
 “ that means filled the whole *Epidydimis*, and the tubes
 “ that come out from the body of the *testis* to form
 “ it; and observed, in this operation, that the Mercury
 “ continued to run, and the body of the *testis* to be-
 “ come gradually more turgid and heavy for some time,
 “ after the external parts were completely filled.

“ I shewed this preparation next night at my pu-
 “ blic Lecture, said that I believed we should find the
 “ internal *tubuli* likewise filled, but that I would not
 “ venture to open it till I had got another, lest I should
 “ spoil what was already a valuable preparation; and
 “ desired my Brother to lose no opportunity of making
 “ the trial.

“ This was communicated as a piece of Anatomical
 “ news to Dr. DONALD MONRO then at *Edin-*
 “ *burgh*, by a letter from Dr. GARROW Physician at
 “ *Barnet*, some time in the same month.”

Dr. HUNTER, in what follows, gives the history of a second preparation, said to have been made a week or

a fortnight after this, in which the internal tubes of the *testis* were filled with Mercury and demonstrated. And he concludes, by saying, that “ considering that
 “ letter of Dr. GARROW, and the constant intercourse
 “ between the Schools of Anatomy at *London* and *E-*
 “ *dinburgh*, the presumption must always be, that I
 “ had learn’d from him, at second hand, the structure
 “ of the feminal tubes.”

He does not however venture to affirm, nor does he show other grounds than bare possibility for presuming, that any information was sent to *Edinburgh*, except what was contained in this letter of Dr. GARROW : Nor had he the pretence of supposing that more was afterwards conveyed by the same channel ; for he desired Dr. GARROW not to write further upon the subject ; and Dr. GARROW says he complied with this request (r).

This letter therefore of Dr. GARROW is the only part of the charge against me, which I am, in justice, bound to pay attention to. For surely it is very unreasonable to expect or insist, that I should vindicate myself from that part of the accusation which is neither founded on certainty nor on probability. I think I have some where or other heard of a maxim to this purpose, that says, *Quæ verbo objecta, verbo negare satis sit.*

Were it therefore to appear, that I could have learned by Dr. GARROW’s letter nothing which I printed as my discovery, the just and the good natured part of the Readers would, I flatter myself, allow that I was sufficiently cleared. Yet, not to leave the shadow of suspicion or handle of calumny, I shall not stop here ; but, after examining Dr. GARROW’s letter, and drawing a conclusion from thence, pursue the Doctor in his presumptions, so far at least as to show, that they are highly improbable, if not impossible.

My

(r) *Cr. R.* p. 527, 528.

My Brother, not imagining that there would be any dispute about this affair, did not preserve Dr. GARROW'S letter (s); but Dr. GARROW, being desired to recollect and specify its contents, declares (t), That, to the best of his remembrance, his words were; " That Mr. HUNTER had injected the *vas deferens*, " that the quicksilver was seen in the *Epidydimis*, that " he believed it had penetrated further, but did not " care to cut the preparation till he had made another " such." My Brother, in his answer to this letter, which Dr. GARROW preserves, says, " Mr. HUNTER " will get the quicksilver to go no further than the " *Epidydimis* (u);" which, at least, shows that he had learn'd no more from it.

It was natural for Dr. HUNTER to believe that the contents of this letter were imparted to my Father and to me. My Brother has solemnly declared that he never mentioned it to either, because he did not think he learn'd any thing new from it (x). But, although this declaration be most certainly true, yet I neither desire nor wish that any regard should be paid to it; but that, on the contrary, it be supposed that all possible use was made of this letter.

All that can be known from it, is, that the *Epidydimis* may be filled with Mercury, and that there probably is, to make it stronger for Dr. HUNTER I shall say certainly is, a communication between it and the *testis*.—Now Dr. HALLER, near two years before it is alleged by Dr. HUNTER that his preparation was attempted to be made, had not only described and painted so much in the *Philosophical Transactions*, but had moreover explained accurately and fully the manner in which the *Epidydimis* communicates with the *testis*, and had proved that the threads of the *testis* were hollow tubes; in short, had carried on his description further than can possibly be done without cutting open the coats of the *testis*, which Dr. GAR-

B

ROW'S

ROW'S letter and Dr. HUNTER'S own words expressly say was not done by Dr. HUNTER.—So that it is evident, that from Dr. GARROW'S letter, or even from Dr. HUNTER'S own representation of the facts, nothing could possibly be learn'd which was not long before more fully described and delineated by Dr. HALLER.

Dr. HUNTER, as would seem, apprehending that it might come out that there was no foundation in what could be certainly observed in this preparation to justify his attack upon me, next avails himself greatly of the passage in Dr. GARROW'S letter, “ That Dr. HUNTER believed his quicksilver had penetrated further, “ *viz.* than the *Epidydimis* ;” for he calls it, in his reply to my Brother, “ the principal part of the information.”

To show to what frivolous resources the rage of detraction may drive a person, I shall endeavour to demonstrate, that no conjecture from this preparation can possibly comprehend one circumstance which I have published as my discovery.

For, had I even been told the grounds of this *belief* as the Doctor now relates them, *viz.* that he observed, in injecting, the testicle gradually became more heavy and turgid ; still it was more probable that the quicksilver was extravasated in this single experiment of Dr. HUNTER, as Dr. HALLER had before found it the event of many experiments : For there is no certain criterion by which you can know whether the quicksilver goes on in the internal tubes, or is extravasated ; because, before reaching the *testis*, it passes through a single tube, not above one eightieth of an inch in diameter, several yards in length, and many thousand times convoluted, of which the *Epidydimis* is composed ; so that the appearances in both cases are nearly the same.

Further, I shall not only suppose, that the Doctor's *belief* had been certainly founded, but that I had known that it was so ; yet, without opening the *testis*, the very utmost that could have been con-

conjectured was, that the threads of the *testis* were hollow tubes, and could be filled with quicksilver. But Dr. HALLER had proved this two years before, by demonstrating and describing the beginnings of some few of them so filled (y): Consequently Dr. HUNTER's *belief* could not possibly extend farther than Dr. HALLER had before made certainly known; which I have no where claimed as a discovery of mine. All that I pretend to here, is, to carry on the description of these *tubes* in their distribution through the *testis*, which no *man in his senses* can say could be known till it was seen.

Hence, Dr. HUNTER did not observe, nor could he conjecture any circumstance from this preparation, which was not long before published by Dr. HALLER, or which I have ever claimed as my discovery: And therefore, his accusing me at all of Plagiarism from him on this subject, and still more his persisting in it and making such a pother about this letter of Dr. GARROW, must, to every man of common sense, appear not only highly unjust and malevolent, but equally weak and ridiculous.

I come now to consider the last effort, which the Doctor's *imagination* has been able to suggest to him in this affair, for throwing a reproach upon me, *viz.* that the presumption must be, that, by the intercourse between the Anatomical Schools of *London* and *Edinburgh*, I was informed of the second preparation which he alleges he made. And he endeavours to fix this suspicion, in a manner that shows I was not much to blame in doubting the sincerity of his good wishes; for he assures us the negative can never be proved, tho' I may be conscious of it.

The Doctor seems here to lay down as a maxim, what can by no means be allowed, *viz.* that whatever he ventures at random to suppose, which I cannot dis-

prove,

(y) Haller in *Ph. Tr.* l. c. *Aliquoties contigit, ut fluidum metallum etiam hæc tenerrima vascula (scilicet testis serpentina) subiverit, ut omnino cavos canales esse minime dubitari possit.*

prove, is probable: For he does not so much as tell us, what sort of intercourse he means. Nay, it is certain, that it cannot be understood, in the manner he explains it in the same page, by Gentlemen going to the one place, after having studied at the other: Since, as the Colleges of *Edinburgh* begin in the end of *October* and are continued till *May*, Gentlemen do not leave *London* in the middle of Winter after Dr. HUNTER's first Course, to go to *Edinburgh* where the Colleges are near half over. Hence an intercourse by letter can only be imagined, and that not from Dr. GARROW, whose hands were tied up, but from some other person, an enemy to truth and justice, as well as to Dr. HUNTER.

Now, this being in itself an unlikely supposition, which Dr. HUNTER has not been able to produce the smallest probable grounds for making; and as I can most solemnly declare, That I did not then, by any means whatever, receive the least information relating to the subject, and defy Dr. HUNTER, or any person, to show the contrary: I appeal to the Reader, if the suspicion is not as unjust, as it is injurious.

But if, in these circumstances, it be allowed to be so, what shall we say? Should it appear that Dr. HUNTER has affirmed of this second preparation what by no means corresponds with fact; and that it is even highly probable, if not more than probable, that he himself was incapable of supplying that information, which he has, with such confidence, endeavoured to persuade the world I had received and used unfairly.

In the *first* place, it is manifest, that no demonstration of the internal *tubuli* of the testicle, had been given by Dr. HUNTER long after the time he positively and repeatedly affirms it was done.

In the *second* place, it appears highly probable, that no such preparation was shown, if made, by Dr. HUNTER, till after the time he allows mine to have been publicly demonstrated.

Thus

Thus he tells us, “ that, about the beginning of
 “ *November 1752*, he made his first preparation. In
 “ some such time as a week or fortnight after this,
 “ *his Brother shewed him* a preparation of the internal
 “ tubes of the testicle very generally filled with Mer-
 “ cury, and *he shewed it* that very evening at his pu-
 “ blic Lecture (z).” Now, for *the beginning of No-*
vember, I shall allow him to the middle of it; and,
 even by this, his preparation should have been demon-
 strated some time in that month. Yet Dr. GARROW
 not only is ignorant of any such demonstration when he
 wrote to my Brother, which was in the beginning of
December (a); but, after receiving his answer from *Edin-*
burgh dated *December 14.* which, from the course
 of the post, could not be sooner than the 20th,
 he still knows as little about it; since he then asks Dr.
 HUNTER, “ If the quicksilver had penetrated farther
 “ than the *Epidydimis*, for that he intended soon to write
 “ to Dr. D. MONRO, and would acquaint him if it
 “ had (b).” Dr. HUNTER did not even at that time
 answer Dr. GARROW’s question: And, when it is
 inquired why he did not; he evades, by telling us, he
 treated that question as it deserved. It is therefore
 plain, that he could not answer it. For, supposing
 the worst construction Dr. HUNTER could put on
 this question to be true, that my Brother was endea-
 vouring to fish out his discoveries to rob him of them;
 I only ask, if there could be a more effectual way of dis-
 appointing this design, than by showing to Dr. GAR-
 ROW such a preparation, especially if it had been pu-
 blicly demonstrated, and desiring him to inform my
 Brother that he had seen it.—There are other circum-
 stances too which confirm me in the belief, that Dr.
 HUNTER could not then demonstrate any such prepa-
 ration, *viz.* that he did not publicly lay claim to this dis-
 covery before or when I printed it, whilst the evidence
 was recent; but let it lie over for several years, when
 dates might not be so easily ascertained: And I observe
 he

(z) See *Gr. R.* 437. (a) *Gr. R.* p. 527. (b) *Gr. R.* p. 523.

he runs on to mention his second course that Winter for *February, &c.* (c) which being intirely out of the question, can only be introduced with the intention of confounding the Gentlemen, who, about that time, attended his Lectures.

Supposing, therefore, Dr. GARROW had put this question the very instant he received my Brother's letter, it appears that Dr. HUNTER had given no such demonstration about a month after the time he has alleged; not 20 days, in place of two or three months, as he gives out (d), before my preparation was demonstrated; and not above 10 days before finishing his first Course (e). But as it is by no means likely, that, in an affair so little interesting, Dr. GARROW would immediately run with my Brother's letter to Dr. HUNTER, there remain but a very few, if any days of that Course, in which it can be supposed such a demonstration was made. Hence it is far more probable, that it was not made that Course; consequently, from the vacation between his Courses, which is about a fortnight, my preparation was the first publicly demonstrated. And as Dr. HUNTER does not treat of the male organs till about the middle of his Course, my demonstration, probably, was given upwards of six weeks before his; and therefore, by Dr. HUNTER's own rules, the presumption is, that he had received information of what I had done; and that my preparation was the original, of which his was only the imitation.

Without having produced such glaring instances of misrepresentation of matter of fact (f), I needed no apology for not giving implicit faith to Dr. HUNTER, since

(c) *Cr. R.* p. 437. (d) *Cr. R.* p. 437. (e) *Cr. R.* p. 531.
He tells us, "My Course begins in *October*, and ends in *December*."

(f) Had it been necessary for my argument, I could have exposed many more examples of the like dealing in Dr. HUNTER's answer

since this is a compliment, which, without assigning any reason, he has neither paid to my Brother nor to me: For he is not satisfied with the general declaration, of my preparation's having been publicly shown at such a time;

answer to my Brother's letter*. To show that I don't exaggerate in saying so, I shall point out some of the most obvious.

Where my Brother is giving the history of what had been proposed and attempted by my Father, and executed by Dr. HALLER, relating to the seminal tubes; Dr. HUNTER breaks in upon the middle of his narration, with a letter of reference (D), before my Brother has got so far as to mention what Dr. HALLER had done. By this contrivance he diverts the Reader's attention: At the same time, he tries to persuade him, that my Brother meant to draw a conclusion at this place, and is so officious as to help him to one; though my Brother is not half done with his premises, and shows no such intention. So that Dr. HUNTER is here making merry with his own joke, or rather without a joke.

My Brother, proceeding in his narration, says, "In the year 1751, I studied under Dr. HALLER at *Gottingen*, where I found that he had been attempting to make this same preparation, *viz.* of the *seminal tubes* filled with Mercury; and that he had succeeded better than my Father, or I, and had got the quicksilver to pass quite through the *Epidydimis*, into the beginning of the seminal vessels of the testicle, but could not get it to go further." As he is only relating what was done in 1751, he could not have mentioned Dr. HUNTER's preparations, which were not then in being; but it is evident, that more is explained in this passage than in Dr. GARROW's letter. Therefore, if a conclusion is to be drawn from it, it must be, that Dr. HALLER had succeeded better than my Father, or than Dr. D. MONRO, or than Dr. HUNTER. But this latter circumstance regarding himself, Dr. HUNTER thinks it prudent to suppress. See his remark (E).

"In *May* 1752, continues my Brother, when I came to *London*, I saw Number 494 of the *Philosophical Transactions*, which had been published in 1751, and found that Dr. HALLER had given both an account and figures of the *Epidydimis* and seminal vessels of the testicle prosecuted by dissection, much farther than those can be seen before the testicle is cut." Dr. HUNTER tells us (F), Dr. D. MONRO's argument here is, "Dr. HALLER had traced these vessels within the coats of the *testis* by dissection. Ergo, Dr. HUNTER's preparation was common, and he could not fill them with Mercury." Let the Reader only observe, that my Brother has related in the immediately preceding sentence, that Dr. HALLER had got the Mercury to pass quite through the *Epidydimis* into the beginning of the seminal vessels of the testicle, and

* Cr. R. for December 1757. art. 9.

a time; but he questions Mr. DONNE about it (g). The Doctor, therefore, will pardon my using the same freedom with him; and desiring him to produce the testimony of some few of the number, who saw the preparation in question in his *Autumn Course* for the year 1752. As for what they saw in the following Course, viz. for *January, February, &c.* 1753, that is intirely out of the question; and, if any testimony is attempted to be produced, it is hoped this will be attended to.

Having,

and he may readily determine what return the candour of this remark deserves: For sure it is not absurd to have said, that Dr HALLER first filled these vessels with Mercury, and then traced them by dissection. But, supposing the two paragraphs I have quoted from my Brother's letter had been utterly unintelligible, still, as Dr. HUNTER knew that Dr. HALLER had filled those vessels he described with Mercury, his remark was disingenuous.

After my Brother has endeavoured to prove, that, before I made my preparation, no intelligence had been sent to *Edinburgh* of Dr. HUNTER's having filled the ducts within the *testis* with Mercury; he adds, "For the filling the *Epididymis* I count as nothing, my Father " and Dr. HALLER had done it before him." In answer to this, exclaims Dr. HUNTER, (I) "I shall refer the Reader to his Father's " own words, quoted by him as above, from the *Medical Essays*, " which expressly say he never could do it." Here it is plain, that, as my Brother had fully quoted my Father's words, and had in express terms said, that Dr. HALLER had succeeded better than him, he could have had no intention to deceive the Reader; and, for the same reasons, Dr. HUNTER had not the least pretence of alleging he was ignorant of the truth. Neither could this expression, which I allow to be inaccurate, for it should have been *my Father had attempted and Dr. HALLER had done it before him*, occasion a mistake of any consequence, since Dr. HALLER's having done and printed it, was the same thing to the argument as if my Father too had done the like: So that Dr. HUNTER has here laid hold of a trifling inaccuracy of expression, the meaning of which he thoroughly understood, as a means of imposing upon the Reader.

It would be no difficult task to detect several passages more of this performance, dictated with the same spirit, as particularly the last paragraphs of the Remarks (G), (K).—Surely Dr. HUNTER has not reflected, that, in the present case, his every unfair representation of matter of fact is doubly culpable, being not only a mean subterfuge, but serving unjustly to impair the character of another.

Having therefore, I flatter myself, shown, that Dr. HUNTER's attack upon me, relating to the structure of the *testis*, is altogether undeserved; it evidently follows from thence, that, whatever I have published as my discovery upon this subject, is equally so, as if Dr. HUNTER had not, to this present hour, made any experiment concerning it.

The Doctor has thought proper to mention the respect and civilities he showed me at *London*, with the appearance indeed of compliment, which he has, however, so misplaced, that some think it rather implies a reproach of my ingratitude (*b*). Now, as I am not conscious of deserving this reproach, I shall explain to the Reader the real sense of these civilities, by an example relating to the present subject; and assure him, that I could produce several others such like, if not to myself, to those at least in whom I must think myself interested.

On coming to *London*, I presented my *Inaugural Dissertation, de Testibus in variis animalibus*, to the Doctor. A few days thereafter he demonstrated the male organs; and, among other things, observed, that some had described remarkable vessels coming off from the *Epidydimis*, and affirmed that they were seen frequently (*i*): But that, for his part, he had made a considerable number of experiments, and never had seen any such vessels; and that he, therefore, very much questioned if such discoveries, or rather pretences to discoveries, were much to be trusted. I don't say these very words were used by him, but he spoke to that purpose, and in a manner which cannot well be described; but which, with his never citing Dr. HALLER, plainly showed at whom he levelled. Most unluckily, however, for the Doctor, when he handed about his preparation, I evidently saw in it one
C of

(*b*) *Cr. R.* p. 438.

(*i*) I before mentioned my having first painted such a vessel in the *Phys. Ess. of Edinburgh*, V. 1. I had afterwards given three or four figures of it from different subjects, in my *Inaugural Dissertation*.

of these very vessels, as conspicuous as I had ever observed before; which I remarked to Dr. FARR, now Physician at *Lymington*, who chanced to sit next to me, and afterwards particularly to Mr. J. HUNTER, Brother to the Doctor.

Whilst this serves as a sample of the Doctor's civilities and respect; it may at the same time give an idea of his accuracy in making observations, and circumspection in drawing conclusions.

A proof too of the Doctor's candour is, that, since that time, he demonstrates such vessels, and passes over in silence by whom they were first remarked and described; or, in what way, or by whom, they were first pointed out in his own preparations to his Brother, and so to himself.

Altho' the manner in which the *semen* is conveyed from the *testis* to the *Epidydimis*, by a dozen or more vessels, called from thence *vasa efferentia*, had been explained by Dr. HALLER; and altho' it had likewise been proved, by experiments and dissection, that the greater part of the *Epidydimis* was composed of a single convoluted tube (*); yet, to compleat the history of the progress of the *semen* through the *Epidydimis*, one problem remained to be determined, *viz. at what place, and in what manner these numerous tubes joined to form this single one (k)*. As I have traced this in some of my experiments, I imagined it would be best explained by a figure.—See Tab. 1. where

A. A. Represents the sides of the *testis*, from which the coats are dissected off.

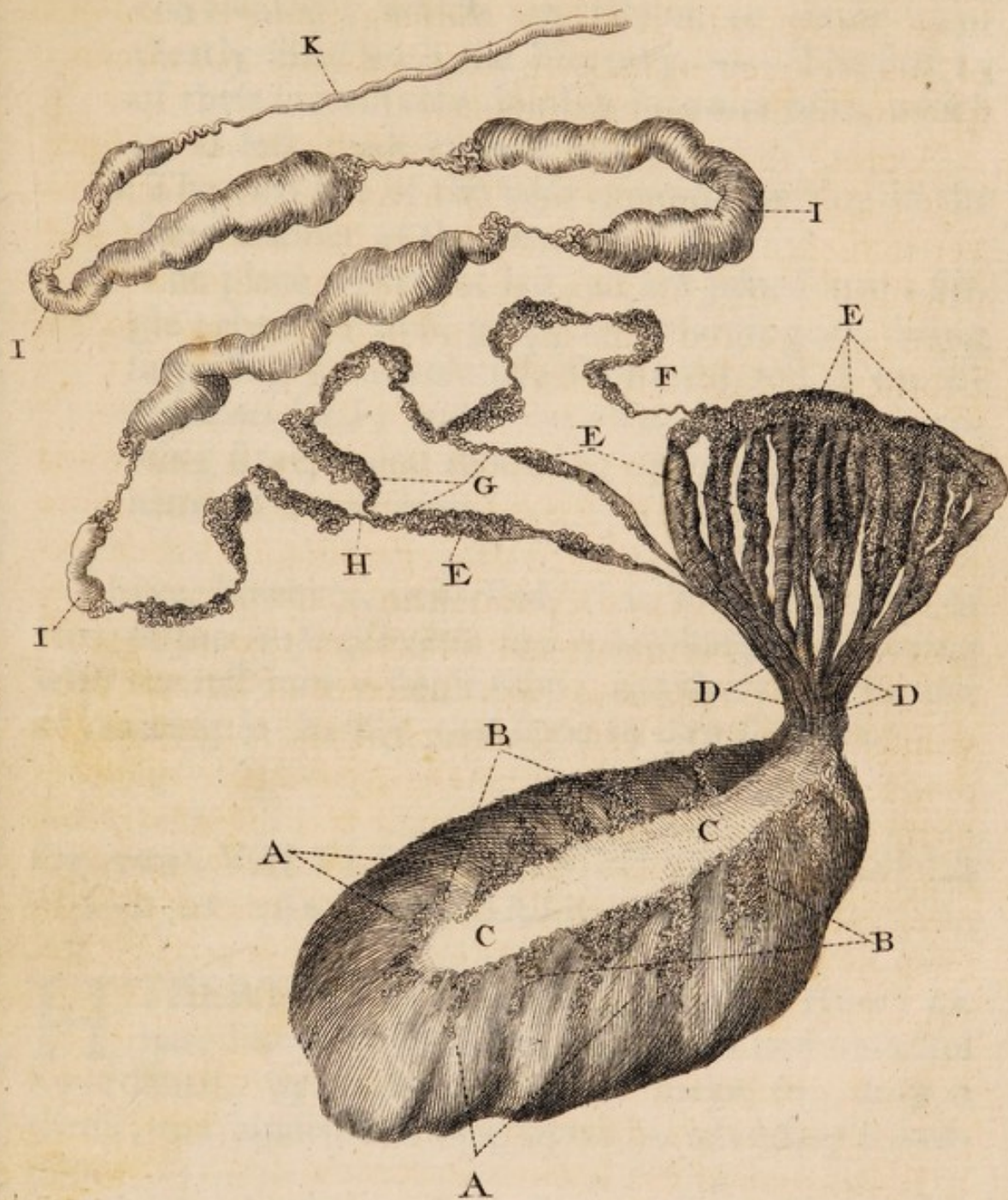
B. B. Some of the seminal serpentine ducts of the *testis*, filled with Mercury injected by the *vas deferens*.

C. C. That

(*) See *Edinburgh Physical Ess.* vol. 1. art. 16. or my *Inaugural Diss. de Testibus, &c.* p. 30, 31, 32, 33.

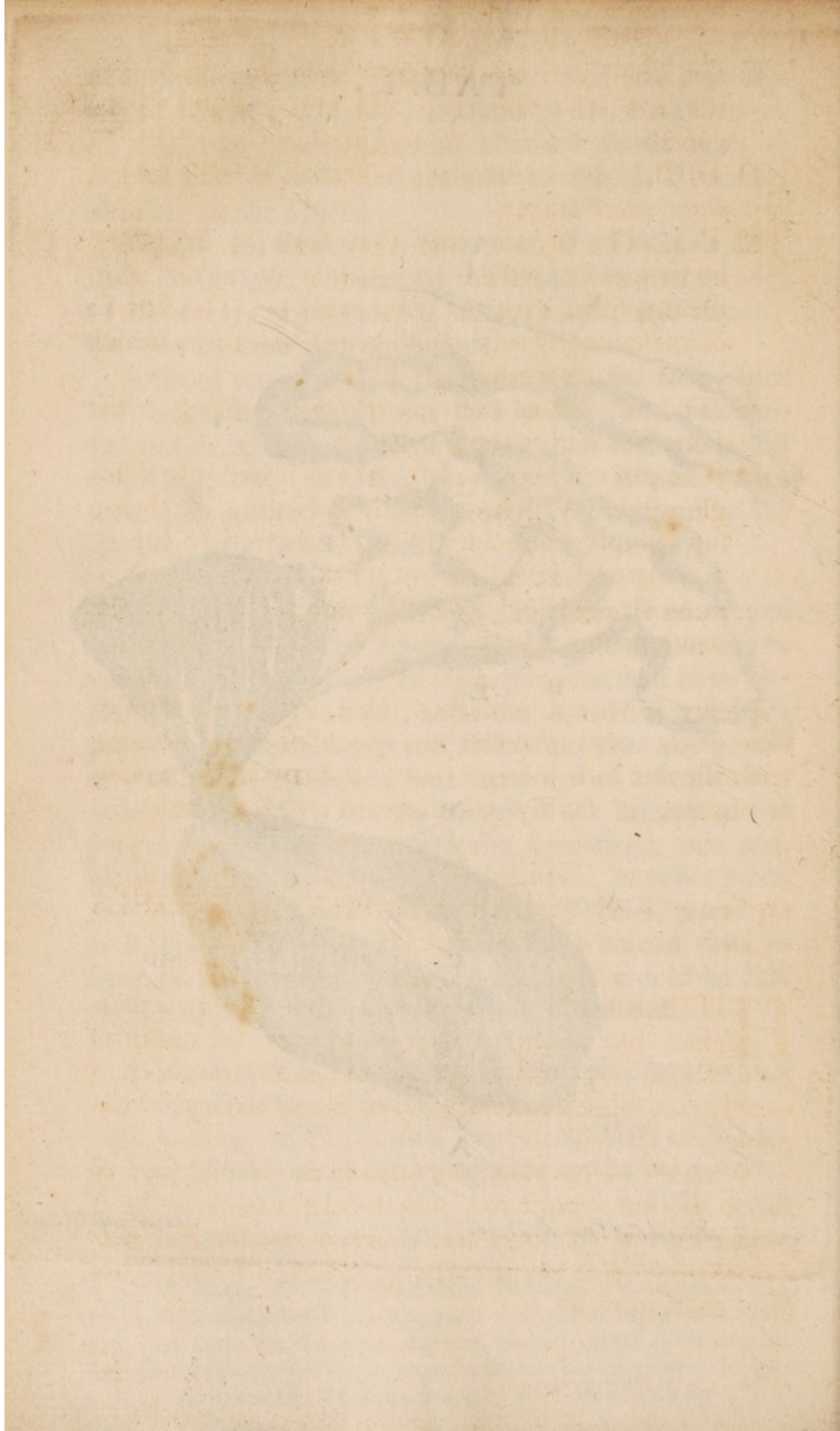
(k) *Ill. Haller. De vasis seminalibus* Gotting. § 3. Quando vero intumescens Epidydimis summae parti albugineae adhaeret firmiter, tunc vero locus est, sed quem definire nequeo. quod nimis angusta sint vasa capitis Epidydimidis, neque ulla arte evolvi queant, in quo finditur hoc vasculum, haecenus simplex, et in multa vasa abit.

TAB. I.



Hopfer ad. nat. delint. Berolini. 1756.

A. Bell. sculp. Edin.



- C. C. That part of the *tunica albuginea* to which the *rete testis* adheres; and under which it lies concealed.
- D. D. The *vasa efferentia*; whereof in this subject there were fifteen.
- E. E. E. The *vascular cones* they form by becoming convoluted; which are seldom or never completely filled with the Mercury.—The first 13 of these successively joining into one tube, which is, at last, made evident at F.
- G. The two last of the *vasa efferentia* uniting in the same manner as the former.
- H. The place where, at last, all are joined into a single tube: Which, gradually enlarging and being surprisngly convoluted, forms the rest of the *Epididimis* I. I. I; and is cut off at K, where, becoming straight and mounting upwards, it has the name of *vas deferens*.

I have, however, remarked, that sometimes one or two of the *vasa efferentia* are much longer of joining with the rest into a single tube; and that their manner of uniting is hardly the same in two subjects.

OF THE VALVULAR LYMPHATIC VESSELS AND OF THE LYMPHATIC GLANDS.

HITHERTO I have proved, that Dr. HUNTER has, like a spiteful, but impetuous and unskilful Swordsman, by endeavouring to make too deep a thrust, run himself headlong upon his adversary's weapon.

Whether he has done the same in the second part of his accusation against me, which relates to what he is pleased to call the *important discovery that the Valvular Lymphatic Vessels are a System of Absorbent Veins*; I am now to examine.

As this subject is generally little understood, and might appear obscure to many Readers, I shall premise such a general account of the lymphatic vessels, as may be necessary for understanding clearly the present dispute.

They were discovered upwards of an hundred years ago; and above twenty years after the circulation of the red blood had been made publicly known by the immortal HARVEY. AS HARVEY had proved, that the red veins received their blood from the arteries; so, it being observed, that the lymph could only flow, upon account of numerous valves, from the smaller branches of the lymphatic vessels to their trunks, and that they were evidently veins, Anatomists made no question, but that, in their origin, they were similar to the red veins. When therefore it came to be undeniably shown by the Microscope, that a red artery and its corresponding vein made one continued reflected tube; the same thing, by analogy and from other reasons too, was supposed of the valvular lymphatic vessels: For it was not expected it could be thus demonstrated, because of their smallness and the pellucidity of the lymph. When at last it was found, that air, watery liquors, and quicksilver, injected into the arteries, passed readily into the valvular lymphatic vessels; and it was also remarked that the lymph was sometimes tinged with the red blood; and these experiments were repeated and confirmed by a succession of the greatest Anatomists, *viz.* NUCK, C. BARTHOLIN junior, BORRICHIVS, COWPER, LISTER, BERGERUS, MORGAGNI, WALTHERUS, &c: it was generally allowed to be unquestionably proved, that the valvular lymphatic veins had their corresponding lymphatic arteries, and that the circulation of the lymph was similar to that of the red blood. And, from the time of these experiments, there seems only to have been an emulation among Anatomists, who should best illustrate

this

this opinion by additional arguments, or assign the most plausible uses for this system of vessels (l).

From this account, it evidently follows, that no person can be said to have good reason to doubt of this common opinion, unless he is able to explain, in some other way, these positive experiments in proof of it; for there is not the least ground to suspect, that the event of them was not such as is related, since it is attested by so many of the best and most faithful Writers.

Without therefore accounting in some other way for these experiments, and refuting the arguments drawn from them, to propose a contrary opinion as a remarkable discovery, is certainly betraying a very weak and precipitate manner of hurrying to conclusions, contradicted by premises.

Soon after I began to apply myself to the study of Anatomy, I more than once satisfied my curiosity in viewing the lacteal and lymphatic vessels in a living animal, but without making at that time further reflections concerning them. Towards the end of Autumn 1752, my Brother, who had been abroad, returned to *Edinburgh* with the design of taking his degree in Medicine; and as he proposed to write his Inaugural Dissertation on the *Dropsy*, his subject happened to lead him to examine the nature of the chyle. The experiments he made (m), in which I assisted him, induced me to try others upon the lymphatic vessels; for we had observed, that, by tying the thoracic duct, not only the lacteals, but likewise the lymphatics continued longer filled, and that their branches, being more distended, could be farther traced than in the ordinary way of only laying open the *abdomen*.

In

(l) See my Dissertation, *De venis lymphaticis valvulosis*, p. 6, 7, 8, 9.

(m) See his *Dissert. Inaugural. de Hydrope*. M. Jun. 1753. p. 8. in notis. Or his Essay on the Dropsy, and its different species, Second Edit. p. 22. Not. (b).

In Summer 1753, I first attempted to fill the lymphatic vessels with quicksilver, introduced by pipes put into openings made into some of their smaller branches. But not succeeding well in this manner, I then tried to inject them in the reverse way from the thoracic duct, in hopes that the quicksilver would pass their valves, as it frequently did those of the heart and large arteries: Or, if it was stopped by the valves; that, as these seemed to be weaker than the coats of the vessels, I might, by increasing the pressure, at last force them. But, after several experiments, I was convinced that this was impracticable; the coats of the vessels always giving way sooner than the valves.

As I greatly wished to have some preparations of these vessels, I next endeavoured, in imitation of the experiments of NUCK, COWPER, &c. to fill them from the arteries. I had never observed them filled in the common way of making injections into the arteries, with oily materials coloured with powders; which I imagined was owing to these being too gross to enter the subtile and colourless arteries supposed to give rise to them. I therefore more frequently injected air and quicksilver, which latter at that time I conceived to be a very penetrating fluid; but with no better success: For I found that the quicksilver very readily bursted the vessels, and then I used to desist. At last I thought of employing what the Painters call Size, from which I flattered myself with great expectations, as it is very subtile, and seemed somewhat to resemble the lymph in its properties; but was likewise disappointed in several trials which I made with it.

Discouraged by these fruitless attempts, I probably would not have pursued the subject farther, ascribing my want of success to my being ignorant of some circumstances, which I imagined had been suppressed by the Anatomists above cited in relating their experiments; but, at last, in making a preparation of the *testis* of a Boar, in which I was trying to inject the seminal tubes with quicksilver from the artery, I observed. with

no small pleasure, several lymphatic vessels filled with this fluid. Imagining from this that I had made a very fine injection, I was not a little surpris'd, upon examination, to find that none of it had got into the spermatic vein: And, upon opening the *testis*, it was so far from having penetrated into the seminal tubes, which I had hop'd, that it did not seem to have gone a great way into the arteries. In injecting the arteries indeed, for example the mesenteric, I had often observed, that the injected matter pass'd more readily by the lateral branches into the cavity of the guts, than into the corresponding veins; which I imagined might be owing to the greater length of the latter, and to their containing a liquor coagulable by cold and rest. But then I could not apply this to the lymphatic vessels, whose first sources, from their refusing admittance to the red blood, I conceived as greatly smaller than those of the red veins; and whose course seem'd to be nearly the same; and which, I knew from experiment, contained a like coagulable fluid. On a second perusal of NUCK and COWPER, I found, however, that their experiments, the circumstances of which I had not before so particularly remarked, had succeeded much in the same manner as this of mine.

Several times afterwards I filled a few lymphatic vessels much in the same way, without knowing how to account for it. As I us'd however to push on the quicksilver till it was extravasated, there had by consequence been an extravasation in all my experiments, which I neglected as an incident nowise material.

These experiments, however, incited me to proceed, and, at the same time, to attend more to circumstances.

At last, in injecting the spermatic artery, without observing any lymphatics appear, I happened too suddenly to increase the height of the column of Mercury, upon which it immediately burst the vessels, and escap'd into the cellular membranes; and, to my no small surprize, at the same instant, filled the lymphatic vessels.

vessels.—Now, my former experiments seemed to me fully explained; for it immediately struck me, that the lymphatics came from the cellular membranes; and indeed, at that time, I made no doubt, but that the quicksilver had insinuated itself into their very first origin.—This observation, therefore, was what gave me the first hint, that the lymphatics were not continued from the arteries; but that they came from the cellular membranes, and consequently were absorbents.

After that, I again and again repeated the like experiment, and never could certainly observe any lymphatics filled without an extravasation: And that they were filled by the extravasation only, was plain from this, that the success was the same when the quicksilver was injected into the veins or excretory ducts; nay, when it was directly poured into the cellular membranes.

Being, from these experiments, led first to doubt of, and then to be able otherwise to explain, at last to refute the principal experiments in direct proof of lymphatic arteries; I began to examine with more freedom the other arguments in support of them. And, by degrees, collecting and considering attentively all the different experiments and reasonings upon the subject, and several appearances in diseases; these, in place of seeming to prove the common opinion, on the contrary, all appeared to me to show, that the valvular lymphatic vessels were a system of absorbent veins.

It remains now to be proved, that, at that time, such experiments were made; and such conclusions, supported by the same arguments, drawn from them, as I have published at length in my Dissertation, *De venis lymphaticis valvulosis*, printed at Berlin 1757.

It would, perhaps, be in vain for me to urge with Dr. HUNTER, that my Father has affirmed*, he, above

* *Cr. R.* p. 432.

above four years ago saw lymphatic vessels filled with quicksilver by extravasation; as the Doctor appears to call his testimony greatly in question*. This much, however, does not admit of doubt, That such preparations were made before the Winter 1754-5; for the lymphatic vessels of the *testis* filled by extravasation, and painted in my *Inaugural Dissertation* published *October* 1755, with some others, were then publicly demonstrated in the Anatomical Theatre at *Edinburgh*.

During that Winter, I employed myself in collecting from Authors what had been said upon the subject: For, although the notion I had conceived of the lymphatics being absorbents, seemed, so far as I knew, very probable; yet I thought I might possibly find some other experiments to the contrary, or, perhaps, some to confirm it. At any rate, I judged it highly imprudent to declare an opinion in print, especially so different from the common one, without carefully weighing what had been, or might be argued on both sides.

I had advanced so far in my inquiries into this subject by the end of Winter, that I would have published them the Summer following, had I not been prevented from pursuing it so closely, by my Father's desiring that I should take my Degree in Medicine that Summer, before I went abroad. As I had made a considerable number of experiments upon the *testis*, not only of man but likewise of different animals, and had explained these with figures of the parts, I chose this for the subject of my Dissertation: And proposed to add the Treatise on the lymphatic vessels as an Appendix to it; since the experiments upon this organ had led me to those on the lymphatics. But upon writing out both at length, which I did first in *English*, I found they were larger than I had expected, and that I had not time to translate them into *Latin* and print them. I therefore omitted that part which particularly related to the lymphatic vessels, and contented myself with mention-

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* Thus, in *Cr. R.* p. 530, Dr. HUNTER treats my Father as accessory to his Sons, in committing the fraud he alleged they were guilty of, relating to the seminal ducts.

ing the success of my experiments upon these vessels, in a general way ; and that, from these and other arguments, I was persuaded the lymphatics were a system of absorbents, and were not immediately derived from the arteries, as was commonly believed, promising to explain myself at length upon this subject, when my time would better permit (*m*).

It is, therefore, evident, I had then wrote a Treatise on this subject. That this Treatise contained the very same experiments, arguments, and doctrine I afterwards published in *May* 1757, at *Berlin*, can, by good fortune, be fully and satisfactorily proved.

My Father has declared, that he read a Dissertation of mine on this subject in 1755 ; which, so far as he can remember, contained the same arguments and experiments, as are since published in the one at *Berlin* (*n*). As I was desirous of carrying a copy of this Dissertation with me when I went abroad, and of leaving another at home, my Cousin Mr. J. MONRO was so obliging as to transcribe it for me, as I was then busied with other things. The copy wrote by myself is still in my possession ; that wrote by my Cousin was read by my Brother Dr. D. when I went first to *London*.

It is natural for us to show any work we are engaged in, to such of our friends as we imagine will be the most free in giving their opinion of it : And, supposing such a work was shown to none other, we presume, it will be allowed that their concurring evidence, provided they are people of a general good character, is sufficient proof of any fact, especially where they are not very highly interested.

But, to put the matter beyond the most distant suspicion, I shall add the testimony of two other Gentlemen, *viz.* Dr. BLACK Physician and Professor of Medicine at *Glasgow*, and Dr. REIMARUS Physician at *Hamburgb*, who, in the Summer 1755, perused my
Treatise :

(*m*) *Dissert. Inaugural. de Testibus in variis animalibus.* cap. 12. p. 55, 56.

(*n*) Art. 8 of the *Gr. R.* for *November* 1757.

Treatise: And whose veracity and knowledge, as it is presumed Dr. HUNTER will not adventure to call in question, so neither can they be supposed biassed by interest or connection.—I wrote to both these Gentlemen, desiring them to declare fully, in the manner they would allow to be published in their names, what they knew of this matter. Their answers, which follow in their own words, are such, I imagine, as don't need much comment to explain them.

COPY of a Letter from Dr. BLACK, Physician and Professor of Medicine at Glasgow, to Dr. ALEXANDER MONRO, junior.

SIR, Glasgow, 24th March, 1758.

“ In answer to your Letter, in which you require
 “ of me to declare what I remember of an Essay of
 “ yours, which you allowed me to read before you
 “ left *Scotland*. I can freely declare, that I read that
 “ Essay in *September 1755*; that the whole substance
 “ and design of it were, in every material point, the
 “ same with those of the Dissertation you have since
 “ published at *Berlin*.

“ The design of it was, to propose and support a new
 “ opinion, with respect to the origin and use of the
 “ lymphatic veins, which was, that they are a distinct
 “ system of vessels, having no immediate connection
 “ with the arteries and veins, but arising, in small
 “ branches, from all the cavities and cells in the body,
 “ into which fluids are thrown; and that their use is
 “ to absorb the whole, or the thinner parts, of these
 “ fluids, and restore them to the mass of circulating
 “ humours.

“ Your Essay contained an ample review of the o-
 “ pinions of different Authors upon the same subject,
 “ and an examination of the experiments and observa-
 “ tions from which such opinions had been deduced;
 “ in which you endeavoured to show, that these ex-
 “ periments and observations were, in fact, unfavour-
 “ able

“ able to the common opinion, and tended to support
 “ that which you offered. You likewise observed,
 “ that the singular structure of these vessels, and their
 “ disposition in different parts of the body, were such
 “ as particularly adapted them to perform the office of
 “ absorbents. And added some experiments of your
 “ own, which pointed out their use to be of this kind,
 “ and were very unfavourable to the common opinion.

“ I remember, likewise, that you had, for some time,
 “ been busily employed in Anatomical experiments
 “ upon this subject, and shewed several preparations
 “ which furnished arguments in support of your opi-
 “ nion. You had thrown quicksilver into the arteries,
 “ with such force as to produce extravasations, and
 “ from these the lymphatics were filled, either by the
 “ weight of the quicksilver in the arteries, or by a
 “ gentle agitation and alternate compression of the
 “ parts in which the extravasations were formed. You
 “ told me at the same time, that an easy method of
 “ exhibiting the lymphatics, is, to fill the cells of the
 “ conglobate glands with air, which passes freely into
 “ such lymphatics as rise from them, to take their
 “ course towards the lacteal sac.

“ But the proofs which gave me the greatest pleasure
 “ and conviction, were those deduced from some *phæ-*
 “ *nomena* occurring in the practice of medicine, in
 “ which it is often observed, that where acrid matter
 “ is applied to the pores of the skin, or has access to
 “ the cells of the cellular membrane, as in the case of
 “ an ulcer, the neighbouring conglobate glands, which
 “ are between the parts affected and the center of the
 “ body, are disordered with swelling and pain; mani-
 “ festly from the matter's being absorbed singly, or
 “ mostly by the lymphatics, and its being collected
 “ and allowed to stagnate, for some time, in the cells
 “ of these glands, into which the lymphatics empty
 “ themselves, and, by its *stimulus*, produces the disor-
 “ der which follows. The instances which you gave
 “ of this kind, were, I think, nearly or altogether the
 “ same

“ same with those adduced in your late Dissertation.
 “ The case of a blister applied to the head, and the in-
 “ stance of buboes, in the Venereal disease, are still
 “ fresh in my memory. I think I likewise remember
 “ the observations in inoculated patients; though I
 “ cannot be positive, with respect to this or the other
 “ examples you have made use of. Upon the whole,
 “ your *Latin* Dissertation seems to me to contain the
 “ same matter, and that treated in the same manner,
 “ as the Essay I read in the year 1755. And, though
 “ you may have, no doubt, improved upon that Essay
 “ in your Dissertation; I must confess, I received as
 “ clear an idea of your doctrine, and as full a convi-
 “ ction of its truth, from that Essay, as I have recei-
 “ ved since.

“ I am, Sir, your obedient, humble Servant,

“ JOSEPH BLACK.”

Dr. REIMARUS had studied under the illustrious
 Dr. HALLER at *Göttingen*, and afterwards under the
 very accurate ALBINUS at *Leyden*, before he came to
Edinburgh, at which place he passed the Winter 1754-5:
 And went from *Edinburgh* to *London*, where he attend-
 ed Dr. HUNTER'S Autumn Course 1755. And there-
 fore was present in both places, at the very time which,
 as in his letter he justly expresses himself, may seem *critical*.

When I had no correspondence with him, and when
 he did not so much as know of my intention of soon
 publishing my treatise on the lymphatic vessels, he
 printed the following passage in his Inaugural Disserta-
 tion at *Leyden*. (a) “ Vasa enim lymphatica absorben-
 “ tia esse, neque arteriis, ut creditum fuerat, continuari,
 “ pluribus argumentis verisimillime comprobatum vidi
 “ ab amico ingenioso cl. A. MONRO jun. in Tractatu
 “ necdum edito, quem *Edinburgi* legere mihi licuit.
 “ Idem quoque videri cel. G. HUNTERO *Londini*, ex
 “ Prælectionibus ejus Anatomicis intellexi.” The first
 sentence

(a) *Dissert. Inaugural. de Fungo articular. p. 8. in not. ad lit. g.*

sentence of this passage certainly carries along with it a much higher degree of conviction than the latter. He is persuaded, that the lymphatics are absorbents, from a Treatise of mine which he read at *Edinburgh*, by a variety of arguments which render it extremely probable : They only seem to be absorbents to Dr. HUNTER. Whether Dr. REIMARUS intended any such contrast, the following Letter will best show.

COPY of a Letter from Dr. REIMARUS, to Dr. D. MONRO Physician, Crown and Scepter Court, London (*).

SIR, *Hamburg*, 17. Feb. 1758.

“ I am informed, and to my great concern, that
 “ Dr. HUNTER raises a controversy with your Bro-
 “ ther about the theory of the lymphatic vessels be-
 “ ing absorbents, and asserts the honour of having
 “ uttered first this sentiment in his Lectures. I am
 “ of opinion, that (according to the terms we use in
 “ *German*) most disputes are originally but *misun-*
 “ *derstandings*, and that they may be composed by a
 “ declaration. Now, as I profess myself very much
 “ obliged to your Father and Brother, as well as to
 “ Dr. HUNTER; and, as I had the honour to attend
 “ the Lectures in question, at the *very time* that may
 “ seem *critical*; I shall plainly and honestly declare
 “ what I know of the matter, as far as I can remem-
 “ ber; and, observing that respect for merit, and
 “ that impartial integrity for truth I ever make my
 “ duty, I hope not to give any offence, which at
 “ least I seek always to avoid.

“ *First*, Dr. MONRO, your Brother, did me the
 “ honour at *Edinburgh*, before he printed his Thesis,
 “ to show me an *English* Manuscript Treatise on the
 “ lymphatics, lacteals, and conglobated glands, which
 “ was fuller in many points than the *Latin* one,
 “ he

(*) As Dr. REIMARUS was formerly acquainted with my Brother, and the Post from *Hamburg* to *Edinburgh* passes by the way of *London*, he has thought proper to address this Letter to him.

“ he has published afterwards at *Berlin*; but, as to the
 “ lymphatic vessels, contained nearly the same things,
 “ except very few additional remarks, such as con-
 “ cern experiments he has made at *Berlin* or the like.
 “ He also shewed me several preparations of lym-
 “ phatics, that made his opinion probable, being filled
 “ by extravasation from the cellular membranes, par-
 “ ticularly those about the testicle, described in his
 “ *Inaugural Dissertation*, which, I think, being obser-
 “ ved by him, gave him the first hint of the thought
 “ in question. I saw him likewise busy with turning
 “ over every Author he could think of having
 “ spoke of the lymphatic vessels. He had quoted
 “ many of them in his Manuscript, (perhaps more
 “ than in the printed Treatise); and there, as well as
 “ by word of mouth, he accounted for their mistakes,
 “ in believing the lymphatics were filled from the ar-
 “ teries or veins, and gave his reasons for thinking
 “ otherwise. These accurate examinations of Authors,
 “ such as he quotes in his Treatise, which he had not
 “ the time nor opportunity indeed to look at du-
 “ ring his travels in *London* or *Berlin*, will declare it
 “ also to the Public to be a well premeditated piece;
 “ whereas an extemporaneous Pamphlet, published
 “ only to lay claim to the honour of an invention,
 “ would but quote two or three Authors cursorily.
 “ He then asked me whether I had heard any thing
 “ concerning the very origin of the lymphatics, par-
 “ ticularly of those illustrious Gentlemen ALBINUS
 “ and HALLER: To which I answered, that Dr. HAL-
 “ LER did not seem to have made particular experi-
 “ ments on the lymphatics; and Dr. ALBINUS, tho’
 “ he spoke of the fabric of the conglobated glands,
 “ and the passage of the lymphatics thro’ them, and
 “ shewed preparations of lacteal vessels filled with Mer-
 “ cury on the surface of the guts; yet I did not hear
 “ him declare the origin of the lymphatics: Nor did
 “ I hear any thing of this new theory by those Gentle-
 “ men who had attended Dr. MECKEL at *Berlin*, or
 “ of

“ of any body else ; and was glad to see this part of the
 “ physiology explained with so much probability, of
 “ which the opinion before proposed was always
 “ doubtful to me. I afterwards came up to *London*
 “ (before your Brother) and there had the pleasure to
 “ hear Dr. HUNTER (who had not yet seen your Bro-
 “ ther’s Thesis) utter the same sentiment in his Le-
 “ ctures, *viz* that he was persuaded the lymphatic
 “ vessels were absorbents, and not coherent with some
 “ small arteries. As much as I have taken down and
 “ remember, he made use of these arguments, 1.
 “ Because their structure is particular, and different
 “ from other veins, with respect to their copious
 “ valves. 2. Because they are quite similar to the la-
 “ ctal vessels which we know are absorbents. 3. Be-
 “ cause if the venereal poison is received on some part
 “ of the body, it makes the next conglobated glands,
 “ thro’ which the lymphatics coming from such a
 “ place must pass, swell up ; as in Nurses, who re-
 “ ceived it by the breast, the axillary glands ; in chil-
 “ dren who get it by the mouth, the glands about the
 “ neck. The two first of which arguments Dr. MON-
 “ RO had much insisted on ; but the last observation,
 “ I think, I heard first of Dr. HUNTER (a) ; and after-
 “ wards

(a) The Reader will here remark, that although Dr. REIMARUS
 says, he does not think I then made use of the argument from the
 route of the venereal poison, Dr. BLACK positively affirms that my
 having insisted on it is still fresh in his memory. Neither of these
 Gentlemen took notes in writing from my Dissertation ; and, as I
 did not then mention above one or two examples of this, these
 may have escaped Dr. REIMARUS’s remembrance ; but what fixed
 them in Dr. BLACK’s was, that, upon my having said to the
 Doctor, It was somewhat surprizing that the celebrated ASTRUC
 had accounted for the *bubo* from a sort of absorption by the lym-
 phatic vessels, and yet had, in other places, derived the lymphatics,
 without exception, from the arteries : Dr. BLACK, who had been
 consulting ASTRUC’s Book some time before, would not believe that
 there was any such obscurity and contradiction in it, till I turned up
 to him several passages which prove it : And which, in so large a
 work, might well have escaped the observation of a person who
 was not reading with a view to this particular subject. — But of
 this more fully afterwards.

“ wards the fact was confirmed by other experienced
 “ Gentlemen in *London*. Yet your Brother had quoted
 “ other similar instances, as that of the absorption
 “ of Spanish flies, p. 50. of his Treatise, &c. I don't
 “ know Dr. HUNTER was more particular on this subject,
 “ nor his having refuted the experiments of
 “ NUCK, COWPER, LISTER and others, who pretended
 “ to have injected the lymphatic vessels directly from
 “ the arteries or veins, which I had seen explained by
 “ Dr. MONRO in his manuscript; nor his having
 “ shewed preparations of lymphatics filled from the
 “ cellular membranes, or by extravasated wax that
 “ was injected into the arteries, or mentioned experiments
 “ of having done this, such as I had seen with your Brother.
 “ I think, therefore, that the discovery made by one of these
 “ Gentlemen, is no objection to the same found by the other.
 “ I did not yet see the *Critical Review*, in which Dr. HUN-
 “ TER is said to vindicate to himself this invention,
 “ and therefore don't know what observations he might
 “ suspect to be borrowed from his Lectures. But as I
 “ ingenuously did justice to them, mentioning them by the
 “ by, § XVI. (g), in my *Inaugural Dissertation* published at
 “ *Leyden*, when I knew nothing of Dr. MONRO's going
 “ about publishing his Treatise, not having any correspondence
 “ with him at *Berlin*; So I profess now I am convinced,
 “ Dr. MONRO as well as Dr. HUNTER drew their theory
 “ from observation and reason; and I can't deny I saw
 “ myself your Brother's discovery, and his applying himself
 “ eagerly on this subject before he or I had seen *London*.

“ I wish I could contribute any thing to the honour
 “ of all men of merit, as well as to their mutual harmony,
 “ for the general benefit of Truth, Arts and Sciences: And
 “ as I am always very ready to give due praises to any one
 “ of my Professors, without

E

“ doing

“ doing injury to another, so I take the liberty to
 “ declare these impartial sentiments to you. I am,

“ *Sir, your very obedient, humble servant,*

“ J. A. H. REIMARUS.”

The only circumstance, therefore, in the power of detraction to insinuate, is, that I had learned Dr. HUNTER'S arguments on this subject, before that time, from some of the Gentlemen who had attended his Lectures. In answer to which, I can solemnly declare, that, before writing that Treatise, I never had any correspondence by letter with any person who had attended Dr. HUNTER'S Lectures; nor was I ever informed of any argument of his, or used by him, upon the subject. And if any of the Readers can witness the contrary of what I here affirm to be true, unless they proclaim it to the world, they must be conscious, that they are abettors of falshood, injustice and ingratitude.

Dr. HUNTER may, perhaps, reply, that a declaration of this kind from me, however probable, is in vain, if there be bare possibility against me. Should he even do so, I will not quarrel with him about it, because the sequel proves indeed that such a declaration is quite superfluous.

Having thus ascertained my own title to the discovery of the true origin of the valvular lymphatic vessels; I come next to inquire into the grounds upon which Dr. HUNTER founds his claim to it.

In the account given, in the *Critical Review* (a), of my Treatise about the origin of these vessels, observations

(a) For *September* 1757. Art. 8.

tions are introduced, of which Dr. HUNTER gives his public approbation (*b*), which, as is said, will invalidate my claim to the discovery that the lymphatics are a system of absorbents; since these observations are alleged to have been made for eleven years by past in every Course of Lectures by Dr. HUNTER, and to have been particularly delivered, in a full manner, in my presence.

It was in *January 1756* that I heard the Doctor upon this subject: For I only came to *London* the 11th of *November 1755*; and therefore attended the end of his first Course, and beginning of his second, that winter; and he introduces this subject about the beginning of the Course. The observations which the Doctor at that time made, tho' not so distinct in several particulars, were, except perhaps the addition of one or two circumstances, nearly as full as those inserted in the *Critical Review*; and which, to do him all justice, I shall copy in his own words. But I shall evidently prove, that, before that time, he never made the most material remarks, and the only ones which lay the ground-work for a just and allowable conclusion; but that he gleaned them from my *Inaugural Dissertation*, which I presented to him on my coming to *London*.—Such are the two first experiments with which he sets out; by means of which only, what had passed for positive and direct proofs of lymphatic arteries, can be refuted.

“ That the lymphatic veins perform this office (of absorption, *viz.*) seems probable, says Dr. HUNTER, from the following remarks (*c*).

“ I cannot inject them as other veins, by filling the arterial system; so that, in all probability, they are not continuations of the arteries (*).

“ I have

(*b*) *Cr. R.* for *December*, Art. 9.

(*c*) *C. R.* 1757. p. 438.

(*) That Dr. HUNTER, at that time, made this remark, I do recollect; but that he made the following observation, I certainly cannot remember. Though, as I have nothing but my memory in

“ I have sometimes observed in injecting, that they
 “ were immediately filled with wax when the arteries
 “ burst, and the wax was effused into the cellular mem-
 “ brane. This looks as if they took their rise from
 “ these cells, like the veins in the spongy part of the
 “ penis.”

Now both these experiments are insisted on, and explained fully and distinctly in my *Inaugural Dissertation*, chap. 12 (*d*); where I have, in several continued pages, shown, that the lymphatic vessels of the testicle can be filled by extravasation into the cellular membrane: And I have represented them so filled with quicksilver in six different figures; as I can to this day demonstrate them. And, at the end of the chapter, I sum up the whole in the following manner (*e*). “ I
 “ have explained these experiments at greater length,
 “ as they first incited me to try others on the lym-
 “ phatic vessels in general: And as I have found, that
 “ these could not only be filled from the excretory
 “ ducts of the glands, but likewise in a manner not
 “ hitherto remarked by Authors, *viz.* by an effusion
 “ of fluids into the cellular membranes and cavities of
 “ the body, of which I have already given several ex-
 “ amples; and that, without an effusion into the cel-
 “ lular membranes, they never, in my experiments,
 “ did admit liquors injected into the blood-vessels to
 “ enter them: These, among other things, furnished
 “ me with arguments of no small weight to prove
 “ *That the valvular lymphatic vessels, through the whole*
 “ *body, were a system of absorbent veins; and that they*
 “ *did not proceed from the branches of the arteries, as is*
 “ *the common opinion.*” And I add: “ But at present
 “ to propose all that might be disputed upon this sub-
 “ ject, would far exceed the bounds of such a Differ-
 “ tation; and it will be much fitter to treat of them
 “ apart

proof of it, not being acquainted with any Gentleman who then took notes, I shall rather allow that he then made it, than enter into a fresh dispute with him.

(*d*) *Dissert. Inaug. de Testibus in variis animalibus. cap. 12. p. 53.*

(*e*) *Ditto. p. 55, 56.*

“ apart, *viz.* of their origin, fabric, manner of acting,
“ and use, when my time shall better permit.”

I should have been very far from concluding that, because I presented my Dissertation to Dr. HUNTER, he had therefore perused it; had he not put this beyond a question, by naming me in his Lecture, at which I was present, as having hinted the opinion of the lymphatics being absorbents in a general way; which he could only have known from the passage just now quoted (*f*).

These experiments therefore being fully explained and insisted on in my *Inaugural Dissertation*, which the Doctor had perused, it is possible he might have first learned them from it.

That he did collect them in this way only, and never had made or imagined any such experiments before, the sequel does not allow us to doubt.

In the first place, the Doctor's dry manner of relating them; his not specifying the time or particular method in which they were made, or in whose presence; his evading these questions when asked (*g*); are strong presumptions that such experiments were none of his. In confirmation of which, the Reader may remark, that, in the page immediately following, mention of preparations of the lymphatics, said to have been made in 1756 by Mr. J. HUNTER, is introduced; and though these can no ways affect or enter into the present question, yet the Doctor does not neglect to inform us that they were shown to many pupils, &c, I hope there is no reason to doubt it; but experiments done before that time are not ventured to be produced.

The only experiment said to have been done before that time, which Dr. HUNTER has any where specified,
is

(*f*) And the Doctor mentions his having done so, where he says “ When I saw by his Thesis he was opening another field,
“ &c. I took the first opportunity, &c. by delivering this doctrine
“ fully, at my Lecture in his presence.” *Cr. R.* p. 438.

(*g*) *Cr. R.* p: 434.

is in the *Critical Review*, p. 226. in these words ;
 “ Among other things, he (Dr. HUNTER) observed,
 “ that the lymphatics were raised by blowing or pour-
 “ ing Mercury into the conglobate glands.”

DOCTOR HUNTER’S assuming this experiment as his own, is certainly either the most undeniable proof of ignorance of what had been done upon the subject ; or the most palpable invasion of the property of our Forefathers, if the expression can be allowed, that has ever perhaps appeared in print.

The celebrated NUCK has bestowed upwards of twenty pages in explaining the structure of the conglobate glands : In which he has endeavoured to prove, that these glands are composed of a cellular substance inclosed in a membrane ; and that the lymphatics or lacteals of the first order, entering the gland upon one side, pour the lymph or chyle into these cells ; and that the lymphatics or lacteals of the second order re-assume the chyle or lymph from these cells, and going out upon the opposite side, carry these liquors towards the heart.—He thence denominates the lymphatics or lacteals of the first order *vasa ingredientia* or *inferentia* ; he compares the glands to moss, calling them *muscosæ* ; and names the lymphatics or lacteals of the second order *egredientia* or *efferentia*. He has represented the glands filled with air and quicksilver from the *vasa inferentia* ; the *vasa efferentia* filled from the glands ; or all injected together, in a great number of different figures (*b*).—COWPER has painted the same thing in his tables (*i*) : And from them it has been transcribed by numberless writers.—Neither was any such preparation of these glands exhibited by Dr. HUNTER : From which it was reasonable to conclude that he had not made any such experiment ; and that he was but the echo of NUCK or COWPER.

But

(*b*) NUCK *Adenographia*, from p. 27, to 50. And in Figures, from Fig. 10. to 29. (*i*) Appendix to his Explication of BIDLOO’S Tables.

But supposing such an experiment had been first made (*k*) by the Doctor, in my opinion he had better omitted to mention it in proof of the general doctrine of the lymphatics being absorbents; for, by doing so, he brings himself under the necessity, either of demonstrating that the lymphatics derive their first origin from these glands, or of owning that his conclusion has little connection with the premises.

The common opinion is, that the lymphatic vessels don't begin from these glands, but that they only pass through them. For my part, the more experiments I tried, and the oftener I reflected on the subject, I was more fully persuaded of this. For I found that lymphatic vessels could be proved to be at a greater distance from the heart than any conglobate glands hitherto described; and that these lymphatic vessels, after running a considerable way, only then reached these glands. Thus conglobate glands are found in the lower part of the face, under the tongue, on the fore and back parts of the neck, in the arm-pit, and about the bending of the arm, in the groin, in the cavities of the breast and belly, in the mesentery, &c. But I don't know for certain that they have been seen in the skin at the top of the head, on the surface of the tongue, at the nipple in women's breasts, in the hand or foot, upon the villous or internal coat of the intestines; from all which places it is proved (*l*) that lymphatic vessels arise, and, only in their progress towards the heart, pass through the conglobate glands situated as above described.

Since, therefore, the lymphatic vessels begin at a greater distance from the heart than the conglobate glands, and only pass through these in their way to that organ, entering them upon the one side and going out

(*k*) Tearing the outer membrane of the conglobate glands and breaking their substance, and pouring in Mercury, had been my common way of showing the lacteals of the second order, or lymphatics going forwards from them, as Dr. BLACK observes in his Letter.

(*l*) See my Dissertation *de venis lymphat.*

out upon the other; it appeared to me almost as absurd to conclude, that because quicksilver poured into the conglobate glands, passed into the lymphatic vessels of the side nearest to the heart, that, for this reason, the lymphatics entering the opposite side of these glands, were absorbents; as if I had concluded, that because liquors injected into the cells of the *penis* passed into the open mouths of its veins, that, therefore, the arteries which bring the fluids to these cells, were likewise absorbents. Which comparison is the more just, that Dr. HUNTER adopts the common opinion, that the conglobate glands are composed of a like cellular substance.

But what silences all wrangling and evasion here, is, that Dr. HUNTER, in adopting this notion that the conglobate glands are cellular, and affirming it after he says he had injected them, has adopted and endeavoured to confirm a mistake. *For I can, most undoubtedly, demonstrate that these glands are not cellular, but that they are a plexus, formed by the lymphatics and lacteals dividing, as they enter one side of the glands, into a great number of small branches; which, after being bended and convoluted, are again collected, without opening into cells, into larger branches on the opposite side, from which they go onward to the heart.*—And, of all the parts of an animal body, their structure most resembles that of the *rete mirabile Galeni* in quadrupedes; which, by injection, I find to be such a division of the carotid artery into very small branches, that, after joining into large trunks, and dividing a second time, are distributed to the brain (*m*).

Further,

(*m*) This structure of these glands I first with certainty discovered, by injections of wax and quicksilver, in a subject at *Berlin*, in the beginning of *November 1756*, in which the lacteal vessels were enormously enlarged, caused by a *Hernia*; in the same manner that the red veins become varicous, when the return of the blood to the heart is not free.—These preparations, made in presence of Professor BUTNER of *Berlin* and of Dr. M^r FARLANE junior of *Edinburgh*, were demonstrated,

Further, Dr. REIMARUS, who immediately before Dr. HUNTER read my Inaugural Dissertation, attended his Lectures, and who noted in writing all that he said upon the subject, expressly affirms, (b) “ I don’t know Dr. HUNTER was more particular on this subject, nor his having refuted the experiments of NUCK, COWPER, LISTER and others, who pretended to have injected the lymphatic vessels directly from the arteries or veins, which I had seen explained by Dr. MONRO in his manuscript; nor his having shewed preparations of lymphatics filled from the cellular membranes, or by extravasated

F

“ wax

demonstrated, with figures I caused to be drawn of them, to several Physicians and others at *Berlin*: Particularly to Dr. Meckel Professor of Anatomy, in whose house I lodged that Winter; to Professor SPROEGEL; and to Doctors LOESEKE, ROLOF, PALLAS, JAE-NISCH; and since, to many eminent Physicians at *Hamburg* and in *Holland*; and publicly last Winter in the Anatomical Theatre at *Edinburgh*.

I cannot, however, but, in justice to the very accurate Dr. ALBINUS, observe, that I had been informed by Dr. REIMARUS, that this very ingenious Anatomist proposed, in his Lectures, nearly what I have described, as his idea of the structure of a lymphatic gland; and used to compare the distribution of a lymphatic through a conglobate gland, to that of the *vena portarum* through the liver.— On my return through *Holland*, when Dr. ALBINUS was so obliging as to show me his elegant cabinet of Anatomical preparations, I took the occasion of making mention to him of what I had heard from Dr. REIMARUS, and likewise of my own preparations. The Doctor, who is as remarkable for his candour as his accuracy, told me, that this was more a conjecture of his than a fact he could certainly prove; and that the preparations which had formerly suggested it to him, had now, some how or other, fallen by.

But I have not, by the strictest inquiry, been able to discover that any person, except Dr. ALBINUS, had ever, before *November 1756*, pretended to propose, far less to demonstrate any such structure of these organs.

This may serve as a caution to Dr. HUNTER for the present, if his intention really be to print on this subject; and teach him, for the future, to be more careful in distinguishing in his preparations, what is nature, and what is the effect of art, or rather the effect of the want of art: Which latter only could have led him into the belief that the conglobate glands were cellular.—But of these organs I shall treat more fully when I publish the figures of them.

(b) See Dr. REIMARUS’s Letter.

“ wax that was injected into the arteries, or mentioned experiments of having done this, such as I had seen with Dr. MONRO.”

Dr. FARR Physician at *Lymington*, who attended the same Course with Dr. REIMARUS, and whom I lately wrote to, declares, that, so far as his memory serves him, no such experiments were then mentioned, nor were such preparations demonstrated by Dr. HUNTER.

But what is presumed to be an indisputable proof of what I am advancing is, that, even in the Course following, when I heard the Doctor on this subject, that is two months after receiving my Dissertation, he did not show a single preparation of lymphatics filled in the manner he maintains he had done. The only preparation of them he then pretended to demonstrate, was of some branches on the spleen of a Calf; which he filled, as he himself explained, by opening these branches with a knife, putting in pipes, and pouring quicksilver through them. His exhibiting, therefore, a preparation made in this way, from which nothing could be concluded as to the origin of these vessels, and this not of the human body neither, shows evidently that he had no others: For if, as he tells us, he intended by that Lecture to put me on my guard, and to assert his own title, he could not have done it so effectually, as by proving that he had made experiments himself upon this subject, and by demonstrating his preparations before me, in the presence of so many witnesses. The Doctor, therefore, was not, nor indeed did he pretend to be, at that time possessed of any such preparations. But, suppose he had formerly had such and had by misfortune lost them, which however has not been hinted, he knew how to supply his loss; and, as this was one, among other things, out of the common way of thinking, upon which he valued himself*, he certainly would not have neglected to have made such preparations to illustrate it by, as he was often employed in making others less curious and useful.

It

* *Cr. R.* for 1757. p. 438.

It is therefore most evident, that Dr. HUNTER never had made any such experiments or preparations, nor even imagined the thing possible: And consequently he first learned from my Inaugural Dissertation, and from the one I published at *Berlin*, that the common experiments offered as direct proofs of lymphatic arteries could be refuted by experiments.

Hence he is, in this respect, not only guilty of a self-convicting Plagiarism from me; but, by attempting to turn my own experiments and words against myself as *stolen* from him (*a*), has added an abuse to injury.

The arguments therefore that follow are the only ones by which the Doctor formerly endeavoured to prove the lymphatics to be a system of absorbents. And these, I shall plainly show, are copied without any acknowledgment from a few common books, not only as to fact, but as to every conclusion which it was allowable to draw from them *alone*.

Thus, says he, “ If they were continuations of arteries, why should they be so plentifully provided with valves, which are not found in the other veins of the *viscera* ? ”

This has been remarked by almost every Author who has mentioned the subject; and it was imagined they had these valves, because their first sources from the arteries were so small, that the impulse of the heart was not sufficient for carrying forwards the lymph (*b*).
—Which way of accounting for these valves was abundantly plausible, whilst there appeared to be other such convincing arguments in proof of lymphatic arteries.

“ But

(*a*) I'm sorry I cannot find another word to convey my idea of Plagiarism by; for, I'm afraid I expose myself to a still more severe rebuke from Dr. HUNTER for persisting in the use of this unpolite word *stolen*, than my Brother had from him for employing it before. See *Crit. Review*, p. 528.

(*b*) See *Lister de Humor*, c. 23.—*Bergerus in Phys. Med.* &c.

“ But the most striking argument,” continues the Doctor, “ is the analogy between the lymphatics and lacteals. These two systems are, to all appearance, the same in their coats, in their valves, in their manner of ramifying, in their passage thro’ the lymphatic or conglobate glands, and in their termination, viz. in the route of the chyle. As they are perfectly similar, in every other respect, we must suppose them to be so in their origin and use. The lacteals are known to begin from the surface of the intestines, and to be the absorbents of those parts. There is no difference but the name. The same vessels are called *lacteals* in the intestines, and *lymphatics* in the other parts of the body.”

This analogy is so obvious, that it has struck many a writer long before Dr. HUNTER. To mention but a few of the most common school-books, BOHN draws this comparison at length, in a section to which he gives the title of “ *Vasa lactea sunt etiam lymphatica* (a),” — PALFYN has the following passage (b), “ *Quelques uns croyent que les veines lactees ne sont autre chose que des vaisseaux lymphatiques qui passent par le mesentere; avec cette difference, que ceux qui sont destinez a charier le chyle commencent par des petites branches qui portent de la surface interieure des intestins.*” — Heister is still more explicit; for he subjoins the description of the lymphatic vessels to that of the lacteals, upon account of the analogy of their structure, “ *Vasa lymphatica ob similem structuram chyliferis mox subjungimus. Descriptio. Sunt vasa subtilia, tenera, pellucida, liquidum vehentia aquosum, lymphæ dictum, in intestinis vero, præsertim tenuibus, digestionis tempore chylum quoque vehunt, et tunc, ut supra dictum, vasa lactea vocantur, quæ in intestinis eadem*

(a) *Bohn Circ. Anat.* 1686.

(b) *Anatomie, par Palfyn.* 1726, chap. 10.

“dem sunt vasa (c).” And again, in describing the lacteals, he says, “Vasa lactea extra tempus digestio-
 nis tantum lympham vehunt, et vasa lymphatica
 sunt (d).”——See other Authors, who deliver themselves to the same purpose, quoted by Dr. HALLER (e).

But then Dr. HUNTER may perhaps reply, that although they did remark this analogy of structure; yet they did not conclude from thence, that therefore they *must* be similar too in their origin and use. Why truly not, because they saw numerous experiments repeated by men of the greatest knowledge, and most reputed accuracy, which were thought to prove the direct contrary, *viz.* that they were sent off from the arteries. And till these experiments were refuted by other experiments, or explained some other way, which I have proved Dr. HUNTER never did, they might possibly think it inconsistent to make any such conclusion. Whether in this they shewed themselves less acute reasoners than Dr. HUNTER, I submit to the Reader.

Besides, in place of being absurd, it might seem no ways improbable, that vessels of the like structure might have a different origin in different parts of the body; as for this there was the analogy of the sanguineous veins. For tho', in most places, these are continuations or reflections of the arteries, yet in some few they evidently take their rise from cavities, as in the *penis* and *clitoris*, which even Dr. HUNTER is pleased to allow: And their small colourless branches are in many places absorbents, as I shall afterwards prove.

That the Doctor may have no pretence of alleging that I do his arguments injustice, or misrepresent them,
 I shall

(c) Heister in *Compend. Anat.* § 215. 1732.

(d) Ditto, § 213.

(e) Haller in *Boerb. inst.* § 129.

I shall quote them fully, even where he seems to repeat unnecessarily ; as he does by again introducing the mention of the valves of the lymphatics. “ This doctrine explains the use of valves, in the lymphatics. “ In other veins, whether large or small, the fluid is “ supposed to move onwards by an *impetus* received in “ the arterial system : but, the case is not the same “ in vessels that suck up a fluid from a surface. These “ require valves, that every lateral pressure upon them “ may have the effect of an impulse at the beginning “ of the canal, in driving the fluid on towards their “ termination.”

Authors persuaded, from the experiments so often cited, of the existence of lymphatic arteries, which they conceived to be excessively small, and that therefore the effect of the impulse of the heart was much weakened, imagined the uses of the valves to be to assist this impulse (*a*). Dr. HUNTER, upon the supposition of their being absorbents, could not use the words *assist this impulse*; and therefore was obliged to substitute for them, *to have the effect of an impulse*, in other respects repeating what was commonly said.

The Doctor's finishing argument, which is the third he made use of, before perusing my *Inaugural Dissertation*, appears to me as much a more striking argument than what the Doctor has been pleased to call the *most striking*, as reasoning from the analogy of one branch of the lymphatic vessels to another is more convincing, than reasoning from the similarity of the lacteal to the lymphatic vessels. The argument is, that “ This doctrine of the lymphatics is farther confirmed by the absorption and “ progress of the venereal poison. The lacteals were “ discovered, traced, and their use ascertained from “ the circumstance of a manifest and particular colour in their contents, upon some occasions at least. “ We

(*a*) See *Lister* and *Bergerus* above cited.

“ We have not the same advantage, with respect to
 “ the lymphatics: but, in them, what we cannot
 “ trace with the eye, we find out by the effects of this
 “ poison. We know from observation, that this *vi-*
 “ *rus* may be taken in at any particular part of the
 “ body, and thence diffuse itself over the whole con-
 “ stitution. We must suppose it absorbed by the
 “ same vessels which absorb its antidote Mercury, or
 “ any thing else that is carried into the mass of blood
 “ by absorption. These things being of a more in-
 “ offensive nature, pass unobserved; but, this poison,
 “ from its irritating and destructive quality, is apt to
 “ raise disturbance in its passage, before it reaches far
 “ enough to mix with the blood. Hence the lym-
 “ phatic glands; through which every absorbed liquor
 “ must pass, are so often the parts first affected by the
 “ venereal taint when it is spreading its contagion
 “ through the constitution. This is the theory of
 “ the venereal *bubo*. If the infection be received in
 “ the most common way, the *bubo* happens in the
 “ groin, because the lymphatics of the genitals pass
 “ through the inguinal glands: but, if the infection
 “ be received at the hand, (a case that sometimes oc-
 “ curs) the *bubo*, for the like reason, is formed in the
 “ arm-pit: When the disease is communicated by the
 “ lips, the glands of the neck inflame and tumify.”

Here, I shall first lay before the Reader a few re-
 markable passages of well known books, from which
 the Doctor *might have* culled the foregoing observa-
 tions: And then I shall show in what circumstances
 these observations can be applied, to make this opi-
 nion in general probable. COWPER has the follow-
 ing, among other remarks to the same purpose (a).
 “ If any parts of the legs or thighs are diseased, as in
 “ an *anasarca* with an *erysipelas*, abscess, exulceration,
 “ especially with a *caries* of the bone and the like;

“ you

(a) Cowper in Append. to his Explic. of Bidloo's Tables, Explic. of T. 1.

“ you will commonly find the inguinal glands tumid
 “ and hard: The like may be observed of the axillary
 “ glands, when the *mammæ*, arms, cubits or hands,
 “ are in like manner affected. The intumescence of
 “ these lymphatic glands, in the cases above mention-
 “ ed, is caused by the vitiated *lymphæ*, arising from
 “ the diseased parts, not passing the *vesiculæ glandulosæ*,
 “ whence a tumour is begun, and is still increased by
 “ the accession of the succeeding *lymphæ*, and the whole
 “ gland becomes distended to a vast magnitude, &c.
 “ The like intumescence of these glands also happens
 “ in venereal cases, especially when the external parts
 “ of the *penis* are ulcerated (b).”

This general way of reasoning, used by COWPER,
 and adopted by BOERHAAVE, who, in enumerating
 the symptoms of the *Lues Venerea*, says (c), “ And al-
 “ so swellings of the inguinal glands, in both sexes,
 “ or venereal buboes; the contagion being commu-
 “ nicated by the *resorbent lymphatics*,” has been in
 the mouth of every practitioner since that time: But
 no person has explained himself so fully, and with
 such perspicuity as Dr. ASTRUC, in different places of
 his learned work, *De Lue Venerea* (d), of which the
 following is a translation.

B. 3. Ch. 5. “ The causes of buboes are to be de-
 “ duced from the contagion inspissating the lymph,
 “ which contagion is conveyed into the inguinal
 “ glands two ways; *viz.* either by the circulation of
 “ the blood, or by another shorter and more expedi-
 “ tious way, by means, *viz.* of the lymphatic vessels,
 “ which go to the inguinal glands.”

B. 2. Ch. 3. “ The venereal poison, in fine, pe-
 “ netrates by passing thro’ the lymphatic vessels, when
 “ the virulent drops, insinuated by the pores of the
 “ parts into the lymphatic vessels which bedew the
 “ skin,

(b) Cowper on the *Penis* in Append. to his *Myotom. reform.*

(c) Boerb. *Aphor.* 1448.

(d) Astruc. *de Lue Venerea.* Edit. Secund. 1740.

“ skin, are carried with the circulating lymph into
 “ the nearest conglobate glands to which the lymph
 “ flows, where they exert their malignity. Thus
 “ swellings of the inguinal glands are the conse-
 “ quences of impure coition, of shankers of the geni-
 “ tals, of a gonorrhœa which runs too sparingly; thus
 “ too, swellings of the maxillary and parotid glands
 “ come upon venereal *aphthæ* of the gums, tongue,
 “ palate or throat, or after venereal ulcers in suck-
 “ ing children, or in those who have contracted the
 “ infection by the lips; in like manner, swellings of
 “ the axillary glands use to be attendants of vene-
 “ real sores, chops or ulcers of the nipples of nurses:
 “ Part of the poison in such cases being carried from
 “ the genitals to the inguinal glands, or from the in-
 “ ternal parts of the mouth to the parotid or maxillary
 “ glands, according to the laws of the circulation,
 “ which the lymph obeys.”

B. 3. Ch. 5. “ Buboes proceed either from an old
 “ venereal contagion, or *2dly*, from a contagion recently
 “ contracted, which, being received upon certain parts,
 “ is conveyed by the circulation into those glands along
 “ with the returning lymph. Thus nurses, who receive
 “ the infection from children, have often buboes in
 “ the conglobate glands situated at the bottom of the
 “ breast, to which the lymph returning from the
 “ nipples, into which the first seeds of the disorder are
 “ insinuated by suction, is first conveyed; or in the
 “ axillary glands to which the lymph afterward goes
 “ forward. So in like manner children who are in-
 “ fected by the nurse, or those who receive it by the
 “ lips, are liable to buboes in the maxillary or jugular
 “ glands, to which the lymph is carried that returns
 “ from the cheeks, tongue, gums, or internal parts of
 “ the mouth, into which the first seeds of the conta-
 “ gion, mixed with the milk or spittle, penetrate.”

See more to the like purpose in *B. 3. Ch. 9. 10.*

These last observations, therefore, introduced in so pompous a manner by Dr. HUNTER, that one would

be apt to imagine they owed their birth to him, prove to be no more than a copy of part only of what Dr. ASTRUC, to say nothing of COWPER and BOERHAAVE, had clearly and fully explained. Nay, the reasoning too, is so nearly the same as to these particular cases, that a person who does not weigh each word, will not observe the difference.

Hence, all the facts, from which Dr. HUNTER used to endeavour to prove the lymphatics to be a system of absorbents, are to be met with in common books. His principal argument too, and substance of what he advanced, appears to be copied from COWPER or from Dr. ASTRUC. And to strengthen this argument, all he did, was, To apply the resemblance between the lacteals and lymphatics, as a ground for imagining the latter to be absorbents; which inference, obvious reasons, already explained (*k*), must have prevented others from drawing: and, To presume that because the lymphatics have valves they probably absorb, laying it down as a principle that vessels which suck up fluids from a surface require valves; in which principle, as I shall afterwards prove, the Doctor is mistaken.

Consequently, Dr. HUNTER can claim nothing as his, but the general conclusion, or *declaration* as it has more properly been called (*l*), that the lymphatics are a system of absorbents. And, were it worth the while, I could deprive him of that too; by showing that others, particularly GLISSON *, had started it before him. But as an opinion without arguments to support it, especially if contradictory to one that seems well founded, does not deserve any attention, I shall wave this; and proceed to show, that it was far from being allowable to make any such general conclusion from all the observations and arguments that were made use of by Dr. HUNTER.

In

(*k*) p. 45.

(*l*). in C. R. p. 434. note †.

* *Gliffon de Hepate, cap. 45.*

In the first place, it might have been a presumption that there was some strong objection to this conclusion, that COWPER, BOERHAAVE, and ASTRUC, who knew fully the chief argument from which Dr. HUNTER deduced it, and whom we cannot suppose unacquainted with the resemblance of the lacteal to the lymphatic vessels, did not draw it. Which, as it is besides a piece of history that has been too much overlooked, I shall be the more particular in making clear.

COWPER, when treating of the rise of the lymphatics, delivers himself in the following manner (*m*):

“ The first origination and extremities of the lymph-
 “ ducts are too subtile and fine to be discerned by
 “ the eye, even assisted by the microscope, and must
 “ give room for suspicion and conjecture. The arte-
 “ ries and veins, we have above demonstrated, are
 “ but one continuous reflected tube: For the truth of
 “ this assertion, in the transparent parts of animals we
 “ have the evidence of our senses; and that the same
 “ continuity is kept up through the whole system of
 “ the body, no rational man, who will please to reflect
 “ on the uniformity of nature, can with any pretence
 “ of reason doubt. Now as these vessels communicate
 “ with each other, and admit a prompt passage of air,
 “ tintured liquors, mercury, &c. from each to other,
 “ &c.———This rise of the lymph is still more clear,
 “ if we consider, in some states or habits of body,
 “ when the crasis of the blood is depraved, some parts
 “ of it pass this way, and the lymph is tinged by
 “ it.———From these demonstrative and convincing
 “ experiments, we may conceive the true origin of
 “ the lymph-ducts is from the extremities of the
 “ blood-vessels, and their use, &c.” And, in the ex-
 “ plication of one of his Tables (*n*), his words are,
 “ Fig. 6 represents (according to our conception) the
 “ origination

(*m*) Introduction to BIDLOO's Tables.

(*n*) Appendix to BIDLOO, T. iii.

“ origination of the lymph-ducts from the extremities
 “ of the blood-vessels.”

BOERHAAVE, in various places, derives the lymphatics, without exception, from the arteries (o).

And, in the passages above cited from Dr. ASTRUC, we not only meet with the expressions, “ The virulent
 “ drops, insinuated by the *pores of the parts* into the
 “ lymphatic vessels which *bedew* the skin, are carried
 “ with the *circulating* lymph——carried according to
 “ the laws of the *circulation*, which the lymph obeys—
 “ contagion conveyed by the *circulation* into these
 “ glands, &c.” But in *B. iii.* he speaks of the “ *lymph*
 “ *flowing about*,” in *B. iv.* he says, “ The *lymph* is
 “ *carried* from the heart into the parts *by the arteries*
 “ *themselves.*” And *B. iv. Ch. 2.* he explains his opinion fully and without ambiguity, in the following manner, “ There are *two humours only*, which, by a
 “ perpetual *flux and reflux*, *bedew* all parts of the body
 “ and are poured into all, *viz. the blood and the lymph.*
 “ The venereal contagion therefore must be mixed
 “ with one of them, or, which comes to the same at
 “ last, with both. For *both*, though they *separate*
 “ *from each other in the extreme capillary arteries*, to be
 “ *carried back* by peculiar vessels towards the heart,
 “ the blood by the veins, the lymph by the lymphatic vessels or ducts, are at last *again* confounded in
 “ the left subclavian vein; and, being afterwards
 “ mixed and intimately blended by the contractions
 “ of the heart and arteries, they reciprocally communicate any malignity they have contracted.”

A person intirely unacquainted with the numerous experiments on the lymphatic vessels, on comparing these latter with the foregoing paragraphs, would be apt to accuse these celebrated Writers of contradiction; and would certainly be greatly surprized, that they should derive the lymphatic vessels from the arteries, when they had before shown that they introduced poison

son into the body by absorption. But should this person be informed, that direct and positive proofs of arteries corresponding to the valvular lymphatic vessels had been offered by a succession of the most accurate Anatomists; and that this opinion had been universally received, and seemed to be established on nearly the same grounds as the circulation of the red blood; he would then be more apt, perhaps, with them, to conclude, that these *phænomena* of the bubo were rather to be accounted for from the passage of the acrid matter through the pores of the parts into the lymphatic vessels: For we have a very evident example of the ready passage of fluids thro' the pores of the parts, from the yellow colour with which the coats of the *colon* and the neighbouring bowels are tinged in a living animal, where they are contiguous to the gall bladder.

But, supposing that such a solution of these *phænomena* was not accounted satisfactory, still the utmost that could have been concluded from them, was, that in some few places the lymphatics seem to absorb: For surely no person, till he had made experiments, by which he was able to explain in some other way and refute what were held as direct proofs of arteries corresponding to the valvular lymphatic veins, could venture to deny their existence.

I have however clearly proved, that Dr. HUNTER never had shown any preparations, nor made experiments upon the lymphatics, from which any conclusion relating to their origin could possibly be drawn: And that, so far from pretending to explain or refute the experiments of NUCK, COWPER, LISTER, &c. in proof of lymphatic arteries, he never so much as mentioned them. Consequently, Dr. HUNTER'S pretensions to even the smallest share of what he is pleased to call *the important discovery, that the valvular lymphatic vessels are a system of absorbent veins*, are evidently founded on a *declaration* or conclusion contradicted by premises.—And, this conclusion, so far from meriting praise, can only be said not to deserve censure, on the
supposition

supposition that he was ignorant of what had been done on the subject.

To put this matter in a clearer light, if possible, let the Reader place before him the facts as they stood when Dr. HUNTER proposed his opinion of the lymphatics being absorbents. On the one hand, it was plain that the lacteal vessels were absorbents, because a liquor, milk, for example, poured into the cavity of the intestines soon appeared in these vessels; and, as the venereal matter in several cases, fell upon such of the lymphatic glands only as were placed betwixt the part primarily affected and the heart, it seemed probable that this was owing to some particles of it insinuating themselves into some branches of the lymphatic vessels, and being carried by them to these glands.—— On the other hand, Authors had repeatedly observed, that fluids, quicksilver, for instance, poured into the arteries in different parts of the body, returned by the lymphatics, and that the lymph was frequently tinged with the red blood; and therefore many of the lymphatics seemed to be derived from and continued with the arteries.

This being the state of the case; and both these propositions being supported by good arguments no ways inconsistent with or contradicting each other, and there appearing no reason to doubt of either: We of course must believe what is said of both to be true, *viz.* That the lacteals, and perhaps too some branches of the lymphatics, absorb; But that the latter are chiefly derived from the arteries.

And we then only can be said to have just reason to apply one of these propositions universally, *viz.* either That the lymphatics come from the arteries, or That they are absorbents, when we are enabled by experiments to disprove the other.

Having shown, therefore, that Dr. HUNTER's opinion concerning the lymphatics was destitute of pro-
per

per foundation; and it being a strong presumption too that it was never so much as esteemed a probable conjecture by those who understood the subject, that so lately as the 1756, many years after the Doctor had, as he gives out, *publicly* proved his opinion, two learned Gentlemen of *London*, who made Anatomy their particular study, *viz.* Dr. LAURENCE and Mr. J. DOUGLAS should have delivered themselves in print on this subject, without the least deference to the Doctor's opinion (*a*); I say, considering all this, I cannot but think, that the Gentlemen, to whom Dr. HUNTER has appealed as evidences of what he has asserted, have good reason to complain of his being so free with their names.——Appealing to witnesses in a general way, without desiring themselves to say what they will answer for, is more frequently calculated to lead us from, than to the Truth. So that, if any Gentleman shall be prevailed on to appear as a witness in this affair, it is to be hoped, he will be so good himself as to specify particular facts, and not subscribe to vague assertions only.

OF ABSORPTION BY THE BRANCHES OF THE RED VEINS.

DR. HUNTER has not only built thus far without a proper foundation, but upon this he has erected a new superstructure, *viz.* That the lymphatic vessels alone absorb, denying this office altogether to

(*a*) Dr. LAURENCE falls in, without the least diffidence, with the common opinion, which he insists on at great length. (See his *Treatise de Hydrope* from p. 90 to 100.) Among other things, to the same purpose, he has the following sentence; “*venæ lymphaticæ tanta exilitate ab extremis suis arteriis prodeunt, ut conspectum nostrum orientes omnino fugiant, &c.*” And in Mr. DOUGLAS's learned *Treatise on the Hydrocele*, we meet with the following passage: “Although numerous lymphatic vessels can be traced on the spermatic cord, liver, &c. yet we know very little about them; their origin, course, and many other particulars remain still to be ascertained, before we can, with any show of truth, draw corollaries from them relating to diseases.”

to the branches of the red veins. Which notion of his, though repugnant to an opinion, that, from its seeming to be established upon numerous experiments and from its being universally received, might have claimed respect, he appears to think sufficiently confirmed by this plain argument, That to offer to believe otherwise, is *inconsistent*.— With what? With the following aphorisms and laws of nature, which the Doctor has been pleased to dictate (*b*). “ He advanced
 “ the doctrine of the lymphatics being *the system* of
 “ absorbents.—*He believed the lymphatics to be the system*
 “ of absorbing vessels.—Every absorbed liquor *must pass*
 “ through the lymphatic glands.—That the inhalant
 “ branches of the sanguiferous veins take also a share
 “ of the absorbed liquors, is the *old doctrine* which
 “ seems to be *inconsistent* with the discovery made, as
 “ to the use of the lymphatics. That the lymphatic
 “ veins are a system of absorbents, has been proved :
 “ That the sanguiferous veins are furnished with in-
 “ halant branches for the same purpose, has been *sup-*
 “ *posed* : But Nature would hardly form two systems
 “ for the same operation. Such a *supposition is incon-*
 “ *sistent* with the simplicity, uniformity, and perfe-
 “ ction of her works.”

We would recommend to Dr. HUNTER, to endeavour to show in his works, somewhat of that uniformity and perfection of which he here professes admiration : For, unluckily, the origins of the lymphatic absorbent vessels are compared by himself to those of some of the branches of the red veins, where he concludes, that, as the lymphatic vessels are filled when a fluid is effused into the cellular membrane, “ This
 “ looks as if they took their rise from these cells, like
 “ the veins in the spongy part of the *penis* (*c*).—
 Here too it being granted, that, whilst most of the branches of the red veins are continued from the arteries,

(*b*) *Gr. R.* p. 438.—225.—226.—434.

(*c*) *Ditto* p. 225.

teries, some begin from intermediate cells ; let the Doctor decide how this is consistent with his notion of the uniformity of Nature.

Lacteal vessels have not as yet been certainly observed in birds, or in the more common fishes, nor, in general, in the animals called Oviparous. And, from a considerable number of experiments I have made, I am convinced, they want the lymphatic as well as the lacteal vessels.

These animals, therefore, have not only the liquors that are thrown out by their arteries into the different cavities of their body absorbed by the inhalant branches of the red veins ; but they are intirely nourished by the absorption of these, both in the embryo state, and ever after. But, as we cannot observe, that, in them, the veins differ in structure from the sanguineous veins in man ; hence, it is not necessary that absorbent vessels should have the valvular structure of the lymphatics : And it is also to be presumed, that the structure of the branches of the red veins in man is such as renders them capable of absorbing. This presumption becomes still more probable, by reflecting on the way in which the tears are taken up at the *puncta lacrymalia*, without the help of valves.

If likewise we consider the manner of the nutrition of the *fœtus* of Viviparous animals, we discover what appears to be a most striking confirmation, that the branches of the sanguineous veins absorb : Which it is as amazing should not have occurred to Dr. HUNTER, as that, knowing it, he should have thought it merited no consideration.

The *fœtus* in Quadrupeds is, without doubt, nourished intirely by the absorption of the veins of the *placenta* : and, by analogy, it is highly probable, that the human *fœtus* is nourished altogether in the same manner. Hitherto, however, there have been no valvular lymphatic veins, or others, but the branches of

the red, or of the umbilical vein, discovered in the *placenta*. Yet, were there any lymphatic veins, we have a better chance of observing them in this, than in any other organ of the body; since, to reach the *fœtus*, they must run along the umbilical cord, where they could not escape the view in so long a course. And we can not only investigate the structure of this organ recently extracted from the living body; but we have likewise the opportunity of examining the umbilical cord, whilst the motion and circulation of the fluids is maintained between the Mother and the *fœtus*. Yet, whatever diligence I have employed here in search of lymphatic vessels, has proved as fruitless as the labour of others had done. It remains, then, to Dr. HUNTER, to prove the existence of valvular lymphatic vessels in the *placenta*; or, by allowing that the branches of the umbilical, that is, of the red veins, do absorb, to retract such *crude notions*; which betray a want of due reflection even on a subject about which the Doctor is daily occupied.

But these arguments, though they seem to render the common opinion more than probable, are by no means the only ones which favour it: There are many other more direct proofs of it. Thus, fluids injected from the trunks into the branches of the veins sweat out upon the surface of the skin and into the different cavities of the body (*d*); which evidently shows that many of their branches begin from these; and hence must be inhalant.

I shall, however, content myself with mentioning a few experiments upon the veins of the stomach and intestines, as these to the generality will appear the most convincing; since there is here a system of
vessels,

(*d*) See the elegant Treatise of KAAU BOERHAAVE *de Perspiratione*. Or, *Element. Physiolog.* of the illustrious Dr. HALLER, L. 2. S. 2. § 22, 23, 24.

vessels, *viz.* the lacteals, universally allowed to be particularly intended for the office of absorption.

Not only air, watery, or glutinous liquors, but even oily substances, loaded with a colouring powder, pass from the mesenteric veins into the cavity of the guts, without meeting with such resistance as to raise any suspicion of a rupture of the vessels (*e*). And some Anatomists, who are esteemed very cautious in their experiments and conclusions, have even pretended to demonstrate, by the microscope, the orifices of these veins upon the villous coat of the intestines (*f*).

In such trials as I have made, fluids get more readily into the cavity of the guts from the veins, than from the arteries: And this ready outlet of the injection, into the cavity of the guts, takes off the force of it so much, that it is remarkably difficult to make the injection pass from the veins into the arteries; though it frequently goes from the latter into the former.

To confirm this by an authority, which to the Doctor and the Reader may seem less exceptionable than my own, I shall borrow one experiment by injection from the very worthy and ingenious Dr. HALEs (*g*). The Doctor's injecting materials are, rosin and tallow of each two ounces, to which three ounces of vermilion, mixed with eight ounces of turpentine varnish, are added.

“ When this injection melted was poured in with a
 “ force no greater than that of the arterial blood, to
 “ wit, with a column of four and a half feet height,
 “ some of the vermilion came always into the ca-
 “ vity of the bowels. And it was the same whether
 “ the injection was made by the *aorta* or *vena porta*;
 “ for

(*e*) RUYSCH *adv. Anat.* D. 2, & 3.—HALLER *on Boerb. Inst.* § 106.—Ditto *Pr. Lin. Phys.* § 712.—ALBINUS *Anat. intest. ten.*—LIEBERKUHN *de villis intest.*—KAAU BOERHAAVE *de persp.* § 467.—
 (*f*) HALEs ALBINUS, LIEBERKUHN.
 (*g*) HALEs *Stat. Eff.* V. 2. *Exp.* 21. § 12, 13.

“ for in both cases the red streaks of vermilion might
 “ be seen with a microscope in the *papillæ* of the mu-
 “ cose coat of the bowels. — As none of this vermi-
 “ lion passed into the lymphatics, fat vesicles or ex-
 “ travasated parts, as the water previously injected
 “ did; this is a proof that the water, which was im-
 “ pelled with no greater force than the vermilion,
 “ did not burst any vessels when it became extrava-
 “ sated, but that it passed through the finest discerning
 “ tubes.” So far Dr. HALES. But this likewise is
 a proof, that the vessels were not burst by the in-
 jection; and therefore, that the veins naturally do
 open into the cavity of the intestines.

As I cannot but suppose that Dr. HUNTER has, in
 making injections, observed the like, I must think this
 of a piece with the rest: And that he has never reflected
 what consequence must follow directly from it, *viz.*
 that we either have been hitherto under a very great
 mistake as to the manner in which the blood circulates
 or moves in the veins; or that branches of the mesen-
 teric veins are absorbents. — Let the Doctor either
 prove the former, or own the latter; as he pleases.

But, to leave no room for wrangling, such as alleg-
 ing, that there may be lymphatic vessels capable of
 absorbing an immense quantity of fluid, and though
 there is not apparently any thing to conceal them, as
 in the umbilical cord, yet that they may some how or
 other be invisible; or, that, in making injections,
 though ever so cautiously, we cannot be absolutely and
 demonstratively certain that we don't burst the vessels,
 &c, I shall refer to the Doctor's consideration the few
 following experiments. BILSIUS, after tying the me-
 senteric arteries in a living animal, in a short time
 found a cineritious liquor, like chyle, in the mesen-
 teric veins (*a*); J. VAN HORNE, having made a
 ligature upon the mesenteric veins in a living animal,
 observed

(a) As related by BOHN and GLISSON.

observed a white liquor, mixed with the blood in these veins, between the intestine and the ligature; MERY, by experiment found that spirit of wine passed from the intestines into the mesenteric veins *; and the accurate and ingenious KAAU BOERHAAVE, after pouring water into the cavities of the stomach and intestines of an animal recently killed, observed it first enter the small, and afterwards going forwards in the larger branches of the gastric and mesenteric veins (b).——If Dr. HUNTER can account for these experiments in any other way than from the absorption by the branches of these veins, I shall then think his opinion may deserve some farther regard.

Although it can be no disadvantage to any one with whom Dr. HUNTER may have a controversy, that he *rashly ushers into the world productions and opinions that are imperfect and erroneous*; yet I own I am surprized he was not before now more upon his guard; as the woful example of a *very near friend* of his own might have stared him in the face, some of the first of the very few of whose productions (c), in which too the doctrine of others is attacked, have in fact proved so lame and unjust, that he has been forced to give them up (d).

C O N.

* See both these Experiments related by J. FANTONUS in *Anatom. Dissert.* 5.

(b) KAAU BOERHAAVE *de perspir.* § 469, 470, 487.

(c) Inserted into the *Philos. Transactions*, vol. 42.

(d) I would indeed deservedly incur the accusation of Plagiarism, did I not own how very much I am indebted to the Commentator on art. 8. of the *Crit. Review* for November 1757, p. 436. for the manner and *expressive words* of the above paragraph: And, to speak the truth, I have so servilely imitated that original, that the only merit I can here pretend to is, the endeavouring to illustrate by a *memorable example* a precept of singular importance to the quiet of the Public, which that *experienced Critic* seems *feelingly* to enforce, *viz.* that a person ought not to indulge the over eager desire of seeing himself in print, lest it should conduce as little to his own honour or profit, as to the good of others.

CONCLUSION FROM THE TWO IMMEDIATELY
PRECEDING SECTIONS.

AFTER what I have made appear, any person, surely, who will take the trouble to peruse my *Essay* upon the valvular lymphatic vessels, from which occasion was taken for raising the present dispute, must be astonished at the following sentence : “ That two
“ persons engaged in the same studies should light on
“ the same discovery, is no ways improbable ; but that
“ that they should support it by a number of argu-
“ ments and experiments intirely the same, tho’ it
“ be possible, is surely so improbable, that I could
“ wish Dr. MONRO had, for his own sake, mention-
“ ed me (Dr. HUNTER) in a marginal note (e).”

The arguments used by Dr. HUNTER have been already numbered and his title to them explained ; his experiments indeed have been shown to be *without number* ; and the agreement of his conclusion with the premises has not been passed over in silence.

The general plan I have followed in my *Essay* is, First, to examine what conclusions one would draw, from considering only the structure and appearances of the lymphatic vessels, with regard to their origin. After this, I have examined the experiments brought in support of the common opinion ; beginning with those which were universally believed to be direct proofs of lymphatic arteries. And after showing, that, by not attending to all the circumstances of these experiments, but by drawing a conclusion from the event of them in general, an opinion had been adopted without due grounds ; which fair reasoning, upon them and other experiments used in support of it, seemed to refute : I have then, and then only, offered arguments which prove that the lymphatics in many places of the body are

(e) Dr. HUNTER in *Cr. R.* p. 439.

are absorbents. And I conclude, that, as hitherto there is no experiment which proves them to be continued from the arteries; and, it is certain, they absorb from many parts, both external and internal in the body; these, together with the analogy of the lacteals, their singular fabric, course, and other *phænomena*, all conspire to render it highly probable, that they are universally a system of absorbents. And from the whole I have drawn practical inferences.— This I found myself under the necessity of extending to about threescore pages, and of dividing into upwards of thirty sections or sets of arguments: At the same time I have so much endeavoured to shun being prolix, and to calculate it for such as were already tolerably well acquainted with the Physiology, that I doubt not but many Readers may think I have treated the subject with too much brevity.

Upon the whole, I must conclude, that, altho' in my Dissertation on the lymphatics I have referred to almost every Author on the subject, the Public will allow I have been guilty of no omission, in not taking any notice of Dr. HUNTER; as I have shown that he did not mention any fact which was not to be met with in common books; that his conclusion from these facts was altogether improper; and that he further denied the office of absorption to the branches of the red veins, contrary to reason and experiment.

I am hopeful too, that the Doctor himself, upon considering this, will not only excuse my not having mentioned him, where it could have been so little to his praise; but that he will also think himself obliged to me, that, so far from having industriously sought the occasion of fixing dishonour upon him, I even shunned it when it offered.

But even upon the supposition that these observations used by Dr. HUNTER had not been borrowed, and that his conclusion from them had been well founded; still there was no reason whatever for me to make the
least

least mention of the Doctor, as I learned nothing from him ; for these and many other observations, with various experiments, were remarked and fully explained, and the conclusion, that the lymphatics were a system of absorbents, was drawn from them in my Treatise (a), previous to my acquaintance with him, or knowledge of his arguments.

Were I, before quitting the subject, in return for the profusion of the Doctor's good wishes for my sake, to offer him my best wish, it would be, that he had not attacked me at all ; for, by that means, he has forced me, contrary at least to my intention, if not to my inclination, to bring to light many circumstances necessary for my own defence, from which Truth would not allow me to draw conclusions greatly to his honour : And for which therefore he has himself only to reproach.

If, however, the Doctor shall still persevere to allege, that his cause is not so desperate as I have represented it, it is to be expected he will endeavour to make this appear in a plain way, by facts well vouched and conclusions fairly deduced from them.

For if, instead of these, he shall answer truth by exclaiming against me for telling it, because it happens to gall him ; shall wrangle about words and expressions ; shall affect not to comprehend, what the rest of the world may think but too plain ; shall again insult the patience of the Public by making presumptions upon presumptions, confessing, at the same time, that there is no certain knowledge in the case (b) ; in short, shall answer facts by suppositions ; arguments and plain conclusions by evasion, and perplexity, and an attempt at a sort of wit, which, especially in an affair of this nature, must ever recoil upon him that uses it : The discerning part of the Readers, I presume, will allow, that I do the Doctor no injustice in concluding, that he
gives

(a) See the Letters of Drs. *Black* and *Reimarus*, &c.

(b) As in *Cr. R.* p. 531.

gives up his cause and silently avows his conviction ; and that he is labouring to raise dust, in order to screen himself and to get off, like what story tells of some of the combatants of old, who, when worsted, escaped in a cloud.

AN ATTEMPT TO EXPLAIN THE USE IN GENERAL
OF TWO SYSTEMS OF ABSORBENT
VEINS, IN THE VIVIPAROUS ANI-
MALS.

IF the branches of the red veins, as well as the lymphatic vessels, absorb, which I have endeavoured to prove, Dr. HUNTER owns himself quite at a loss to conceive, what purpose one or other of these two systems for absorption can serve ; nay, he *declares* it is *inconsistent* with the uniformity and simplicity of the works of nature, to form both (c).

If by *uniformity* the Doctor means, as the word strictly signifies, one way only of doing a thing, I must agree with him : Otherwise, it appears to me perfectly consistent with nature, and may, as I apprehend, be explained in the following manner.

As the valvular lymphatic veins suck up liquors, in the viviparous animals, from many different parts of the body ; there is no question but that they might have been so adapted, perhaps by the addition only of more branches, as to have performed the whole absorption.

Since, too, the branches of the red veins in the oviparous animals are the only absorbents ; and that the structure of the branches of the red veins in the viviparous animals, is not only, so far as we can discover, entirely the same as in the oviparous, but many of them certainly do absorb ; so, neither can it well be doubted, but that the branches of the red veins in the vivi-

I

parous

(c) *Gr. R.* p. 434.

parous animals might have been made capable of performing also the whole absorption.

Hence we may infer, that it was not necessary, solely for performing absorption, to have created a peculiar system of valvular vessels, such as the lymphatic, since this office could have been performed in the different classes of animals, perhaps as well, by the branches of the red veins: and, that as the red veins assist in the absorption in the viviparous animals, Nature seems, in creating the lymphatics in them, to have had some other more essential or primary purpose than absorption in view.—Let us therefore try to disclose this purpose, by comparing the Oviparous with the Viviparous animals.

The Oviparous animals which want the lacteal vessels, are also destitute of the valvular lymphatics: whereas the Viviparous animals, which have lacteal vessels, are likewise furnished with lymphatics; and the chyle and lymph are blended together before they are mixed with the blood. *Hence the lymphatics appear to be primarily created as subservient to the lacteals.*

It may, however, be argued, that, although the oviparous animals want the lacteals, yet there was the same necessity for the lymph, whatever its effect is, to be mixed with the chyle.

To this it may be answered, that, in the oviparous animals, the chyle, being taken up by the small and very numerous branches of the mesenteric veins, is insensibly mixed with the blood in their larger branches, and is soon intimately blended in the *vena portarum* with the blood returning from most of the bowels of the *abdomen*; so that, before it reaches the heart, it is obliged to undergo a compleat circulation in the liver: By which means the bad effects it might have had in disturbing the functions of the heart, as shall be mentioned soon, are sufficiently guarded against, without a mixture of lymph.

But

But, granting that the lymph was intirely subservient to the chyle, it may still be asked, why could not the lymphatics alone have served the purposes of absorption?

There appear to be two reasons for this. The first, That there seems to be a necessity that the red branches of the veins should receive the addition of a fluid to serve particular purposes in the different organs. Thus, for example, that the *vena portarum* should receive particles by its inhalant branches, by which the blood in it may be better fitted for the separation of bile: that the pulmonary veins should take in from the air a something, a vivifying spirit or what else you please to call it, which is essential to life, &c.—The second reason is, That the whole of the absorbed liquors are by no means proper to be mixed with the chyle, but only the more consistent, mild, animalized, and saponaceous part; by which the chyle may be more perfectly dissolved, and its particles in less danger of concreting and obstructing the small vessels: by which it may be fitted to incorporate more readily with the red blood, which contains so large a proportion of oily matter: and, that its acrimony and stimulating quality may be blunted and subdued, which might otherwise be in danger, by irritating and vellicating the heart, of raising the most dangerous commotions in the whole machine. For this organ must be so delicately framed, as to be set in motion by the *stimulus* of the blood in its mildest state: and we may remark, that it is sensible of every new supply of chyle, notwithstanding all the preparation it undergoes; for we find the pulse fuller and quicker after a meal, and this not owing to the quantity only, but likewise to the quality of the food, since different kinds of food produce different effects*.—This hypothesis is finely illustrated, by observing, that half a pound of recent warm milk, a liquor

* See the very ingenious Essay of Dr. WHYTT on the Vital Motions.

quor far more similar to the blood than the crude chyle, injected at once into the crural vein of a mastiff dog, brought on dreadful symptoms, and killed him soon (a).

Two systems of absorbent vessels were therefore necessary, The one, the branches of the red veins; which seem to carry off the thinner and more watery parts of the fluids: to serve immediately for particular purposes in the organ to which they belong, and in a distant or secondary way for the uses of the chyle.—The other, the valvular lymphatic veins; which appear to absorb the more consistent, animalized, and saponaceous part: and to be primarily created for the preparation of the chyle, and to perform the offices of absorption in a secondary way only.

This doctrine is strengthened too by remarking, that many of the humours, which are secreted into cavities, from which there is no outlet but by absorbing vessels, differ greatly from the lymph in their appearance and properties. For some are of a thin watery nature, at least do not coagulate by moderate cold or by rest, properties which the larger share of the lymph possesses; and which is a greater change than, it can well be thought, the passage of such humours thro' the absorbing vessels could operate. Hence the thinner parts are by some other means carried off; which can only be by the branches of the red veins. Add, that the experiments of several very accurate Anatomists prove the extremities of the mesenteric veins rising from the cavity of the intestines in birds to be considerably larger than in the human subject (b). Now, as it is natural to imagine, that this is owing to the chyle's being absorbed by them, is it not likely, that in man, in whom it is taken up by the lacteal vessels, these are larger too than the inhalant branches of their mesenteric veins? to which not only the colour of their contents

(a) See this experiment in *Lower de corde*.

(b) *Du Hamel H. ac. sc. L. 2.*—*Bohn in Circ. Anat.*—*Brunnerus in Diatrib.*—*Peyerus de gland. intestinorum*.

contents, but likewise some observations, particularly of LEWENHOEK and LIEBERKUHN (*a*), give weight. And, transferring the analogy from the intestines to the other parts of the body, it seems probable, that the bibulous orifices of the valvular lymphatic absorbents are universally larger than the inhalant branches of the red veins.

The observation, that some of the lymphatic veins don't join with the lacteals, but open apart into the red veins, seems at first sight an objection to what we have proposed respecting the primary use of the lymph. But if, on the other hand, we consider, that the chyle is not poured into the *cava inferior*, which would be its shortest and readiest road to the heart, but that it is made to climb thro' the length of a thoracic duct, in which course the only advantage almost it seems to reap is from the addition of a greater quantity of lymph: that there are but few lymphatic veins, which do not terminate in this duct: and that these too open near the part at which it discharges itself; we will find this observation an argument of no force against what has been advanced.

(*a*) *Lewenboek oper. v. 1.*—*Lieberkuhn de villis intestinor.*

P O S T S C R I P T.

AFTER the preceding pages were printed, I received the *Philosophical Transactions* for 1757, in which I find that Dr. AKENSIDE, a Gentleman eminently distinguished by his Poetical genius and taste in polite literature, proposes (*a*), as a conjecture which he had likewise for some time entertained, That the lymphatics are a system of absorbents: And I own it gives me

(*b*) *Phil. Transf.* vol. 1. part 1. for 1757, art. xl. Observations on the origin and use of the lymphatic vessels of animals: being an Extract from the *Gulstonian Lectures* read in the Theatre of the College of Physicians of London, in June 1755. By MARK AKENSIDE M. D. Fellow of the College of Physicians, and of the Royal Society. Read to the Royal Society November 10. 1757.

me a sensible pleasure to observe, that, although the Doctor says he hinted this conjecture in the *Gulstonian Lectures June 1755*, yet the paper which is called an Extract from these Lectures, was not read to the *Royal Society* till *November 10. 1757*, that is, several months after my Treatise on the lymphatic vessels was sent to *England*, and an Account given of it in the *Literary Reviews* there; as I cannot help construing the Doctor's having presented this Extract at that time, and not before, into an approbation of what I had published; as if he had then only discovered, that his conjecture was so well founded as to be worthy of the attention of the Public.

What the Doctor offers on the subject may be reduced to the following heads, on which I shall take the liberty of making a few remarks.

He begins by saying "It is proved, by a multitude of experiments, that the lymphatics communicate with the blood-vessels. They may be distended by blowing air, or by injecting water or mercury, into an artery; and the lymph, which they carry, is frequently, in a morbid state, found tinged with a mixture of the red globules or *crassamentum* of the blood. Upon this foundation two different theories have been raised, concerning the connection of the lymphatics with the arteries."

Of these two theories, the first which the Doctor considers is that of *BOERHAAVE*, who supposed that there were subordinate series of arteries: And, according to his idea, the arteries giving rise to the valvular lymphatics were of the third order; they being derived from the red arteries by the intervention of ferous ones.—To this hypothesis of *BOERHAAVE*, the Doctor opposes nearly the same arguments which the illustrious *Dr. HALLER* has done in his *Prim. Lin. Phys.* § 44.

The other theory is, that the arteries giving rise to the valvular lymphatic vessels are immediately derived from the red arteries.—This is the opinion of *Dr.*

HALLER.

HALLER (*l. c.* §. 181 : To which our Author objects,
 1. “ That the lymphatics are traced into many
 “ parts of the body, and lost there ; and therefore
 “ most probably have their origin there, where no
 “ large gland nor blood-vessel is to be found in their
 “ neighbourhood.”

If I rightly understand this argument, the Doctor supposes that the lymphatic vessels have been traced, by dissection, to their origin, where there is no gland nor blood-vessel.—This, however, cannot well be imagined, since even the nascent branches of the red veins, which, according to Dr. HALLER’s scheme, must be greatly larger than those of the lymphatics, are, by their exility, quite invisible to the naked eye : And therefore, even these, although they were not involved in almost inextricable plexuses, would elude the dexterity of the most subtile dissector. Further, though there may be lymphatics where glands cannot be demonstrated, yet I question very much if the Doctor can prove that there is *no blood-vessel in their neighbourhood* ; for if the lymphatics are absorbents, they absorb within the body what the arteries exhale ; hence, an artery must end near to, or in the neighbourhood of, the beginning of a lymphatic vessel : And don’t the experiments which the Doctor mentions in his first paragraph, prove this ? Or, *do the lymphatics communicate with the blood-vessels which are not in their neighbourhood ?*

2. “ That it contradicts the whole analogy of
 “ nature, to suppose the motion of an animal fluid
 “ more discernible in the veins than in the arteries.”

I greatly suspect that I have not fully reached the Doctor’s meaning in these words, as I cannot discover that the quicker motion of the fluid, in a discernible and therefore large branch of a lymphatic vein, than in an invisible and therefore small lymphatic artery, is any ways inconsistent with the *whole analogy of nature*, or with the analogy of every vein in an animal body.—For is not the velocity of the blood
 greater

greater in the *vena cava* or any large branch of a red vein, than in the red capillary arteries from which these veins derive their origin? And does not the *vena cava* or a large red vein bear the same relation to the red capillary arteries, that the Thoracic duct or a discernible lymphatic vein does to the supposed lymphatic arteries? And how is it possible to conceive that the motion of the fluids should be equally discernible in the lymphatic arteries, as in the lymphatic veins, unless Nature had added another heart and system of vessels solely for the circulation of the lymph?

“ Finally,” adds the Doctor, “ it seems rather an instance of want of thought, and of being imposed upon by words, to call the lymphatic vessels veins, because they are furnished with valves; and then, because they are called veins, to take for granted, that of course they must be the continuation of arteries.”

The first part of this proposition is indeed such an instance of want of thought, as never has, so far as I know, imposed upon any person; otherwise the *aorta*, or pulmonary artery, or heart itself, might have been called a vein also; but the valvular lymphatic vessels have, with great propriety, got this name, because the fluid in them moves from the smaller to the larger branches and towards the heart.—To take for granted, that, because they are veins, they therefore have corresponding arteries, is a *petitio principii*, as I have remarked in my Treatise on the lymphatics (A); but surely this is a presumptive argument more in favour of, than against, the opinion which the Doctor is combating.

So far, therefore, Dr. HALLER's opinion seems rather to be confirmed than refuted.

In the beginning of his last paragraph, the Doctor proposes analogy as the *best way* of reasoning here.—But as this way of reasoning can never be accounted more than presumptive, we can only admit it

to be the best, where the subject itself, into the nature of which we are inquiring, cannot be examined by experiment: And, that this is not the present case, the first paragraph of the Doctor's paper evidently shows.

The Doctor then proceeds to make mention of *the Part*, from which he thinks we ought to draw an inference by analogy, with regard to the origin of the lymphatics. By which part, as may from the subsequent page be at last collected, the Doctor would be understood to mean the guts and lacteal vessels.—— These he figures out in the following manner :

“ There is a certain part of the human body very
 “ abundantly provided with lymphatics ; in which
 “ part we can actually force injections through those
 “ vessels into a cavity, where their extremities open.”

——The Doctor seems to have forgot what he hinted before relating to the valves of the lymphatics ; for, upon account of these, this is an experiment in which I never could succeed* ; nor do I know that any accurate Author has alleged he ever did.

“ And from this cavity, continues the Doctor, on
 “ the other hand, we can at pleasure introduce a co-
 “ loured liquor into their extremities, and trace it
 “ from smaller into wider canals ; from capillary tubes,
 “ without valves into large lymphatic trunks, copi-
 “ ously furnished with them.”—Neither is it proved, that this second experiment, of filling the lacteals from the cavity of the intestines, can be done *at pleasure*, or without the assistance of that energy which life and its remains give.—And that the lacteal vessels, where they pass through the coats of the intestines, are not *without valves* is certain ; that they are furnished with them from their very beginning, is highly probable.

Not, however, to insist further upon what may, perhaps, be thought venial slips from the pen of a Gentleman who does not make Anatomy his particular study : The Doctor's conclusion and indeed the substance of

* See p. 22.

his paper, is, That because the lacteals are a system of absorbents, and the lymphatics resemble them in structure, therefore he conjectures that the lymphatics are likewise a system of absorbents*; although he set out with informing us, that “ it is proved, by a multitude of experiments, that the lymphatics communicate with the blood-vessels.”

I must here, in justice to Dr. HUNTER, acknowledge, that I really think Dr. AKENSIDE had better followed his example, and suppressed the mention of the multitude of experiments in proof of lymphatic arteries corresponding to the valvular lymphatic veins, as he could not refute them; for, by doing so, the inconsistency of his doctrine, tho’ in reality the same, would not have been so apparent to every Reader.

* This argument has been fully considered, p. 44. 45.

OF THE LACHRYMAL GLAND AND ITS DUCTS.

IT is generally well known that in the larger quadrupeds, and particularly in the Ox, there are ducts proceeding from a gland, situated within the orbit, above the eye-ball, opening upon the inner side of the upper eye-lid, so large as to allow probes to be introduced into them: and therefore this is believed to be the lachrymal gland, or organ for separating the tears.

In man the *glandula innominata Galeni*, having nearly the like situation, and apparently the like structure, it was commonly believed, notwithstanding ducts could not be demonstrated to be sent off from it, that its use was the same; and it was supposed that the smallness only of these ducts concealed them from observation.

Several, however, of the most celebrated Anatomists (a) having exhausted their patience to no purpose in quest

(a) Particularly MORGAGNI, VATERUS, HALLER, ZINN.——
And

quest of such ducts, and arguments from analogy being no more than presumptions of an opinion, it has by some been much doubted if the real use of this gland was to furnish the tears, as had been imagined: And two of the latest writers on the subject, the illustrious HALLER and the accurate ZINN (*b*), think it more probable that the greater part, if not all of this liquor, comes from the exhalant arteries of the *tunica conjunctiva*; which they observe allow water, injected into them, to sweat out every where upon the inner side of the eye-lids.

Sometime in the Summer 1753, endeavouring to discover ducts from the *glandula innominata*, I observed two or three small orifices upon the inner side of the upper eye-lid, near the external *cantbus*; at which I introduced bristles some way, in the direction towards that gland, and therefore did not doubt but these were ducts

And *Morgagni, Adv. 1. an. 22. Adv. 6. an. 33, 34, 35.* not only owns that he himself could not discover these ducts, but shows that the descriptions given of them by some are by no means to be depended on, but seem to be borrowed from quadrupeds.

(*b*) *Haller in Boerhaav. Inst. § 512. not. c.* “ Verum dubii facti sunt recentiores an omnino in homine hæc glandula, (innominata sciz.) lacrymas generet exhalantibus vasis palpebrarum alii tribuerunt: Cl. Vaterus ductibus Meibomianis. Neque mirum si brutis animalibus ductus fuerint, denegati hominibus.

Haller in Pr. Lin. Phys. cap. 18. § 498. “ Lacrymam partim arteriæ conjunctivæ tunicæ exhalant, argumento imitantis naturam injectionis aquosæ; partim creditur deponere glandula, &c. In homine nondum satis certo, neque mihi unquam, visi sunt ductus (sciz. glandulæ innominatæ.)

J. G. Zinn. Med. & Botan. in Ac. Got. P. de Oculo. C. 13. § 1. p. 153, 4. “ Lacrymas maxima certe ex parte exhalare videntur arteriæ conjunctivæ et membranæ internæ palpebrarum, quæ, argumento injectionis aquosæ naturam imitantis, aqueum semper humorem stillant; partim etiam in homine creditur deponere glandula conglomerata, &c.—” He adds, “ Ex illa glandula in bove aliisque animalibus ductus conspicui descendunt, &c.—” “ In homine autem huc usque accuratissimorum Anatomicorum aciem omnino ductus illi effugerunt: neque mihi hac in re illis feliciorum esse contigit, etsi omni diligentia variaque administratione in illos inquisiverim.”

ducts sent from it. I showed this to several Gentlemen, and particularly to my Father, who always mentioned it in his Lectures since that time; but was not careful in preserving it, imagining the like might easily be done in every subject.—Attempting this however afterwards several times, and not succeeding readily, I began to suspect I had fallen into some mistake. But at last, by being more cautious, I have been able to clear up the truth, in a manner which makes it undoubted.

Upon examining narrowly the inner side of the upper eye lid towards the external *cantbus*, in a subject this last winter, I thought I discerned three or four orifices, which seemed large enough easily to admit bristles: And, dissecting upon the outer side of the *conjunctiva*, threads appeared to go from these towards the *glandula innominata*.—Taking out the eye of the other side, and observing likewise such orifices at the same place, I began to consider, how I should be able to prove these to be ducts, without using force.

The manner I took was no other than macerating the eye for a night in water tinged with blood, fancying it might enter these small ducts in the same way it is attracted into capillary glass tubes. And, according to my expectation, after washing the eye with pure water, I not only saw orifices, but reddish coloured streaks or hollow tubes, continued from these, shining through the *conjunctiva*. And, without meeting with resistance, I introduced bristles into two of them; and observed three or four more, two of which were nearly of the same size with those I had introduced the bristles into, but the others were very small.

Thus much was publicly demonstrated, *February 3. 1758*, to the Gentlemen attending the College of Anatomy. And, in presence of a considerable number of them, I took out the bristles I had already put in, and introduced them again into the same ducts; and likewise put others into two more of the reddish coloured

coloured streaks, which they plainly saw, and which, I told them beforehand, they were attentively to observe if the bristles occupied, and if they entered without resistance. These bristles passed more than half an inch in the direction towards the *glandula innominata*; and through a number of smaller glands, which, for distinction's sake, may be called *congregatæ*, and which adhere more closely to the *tunica conjunctiva*, lying between it and the oval shaped thicker body of this larger gland.

Though this seemed to me a sufficient proof that these tubes were the ducts of the *glandula innominata*; yet not to leave the appearance of doubt, I injected one of them with quick-silver, and could distinctly trace the quick-silver passing in a cylindrical tube through the *glandulæ congregatæ* above mentioned, to which it seemed to give branches, and dividing into three small branches as it entered the oval thicker part of the gland. This was likewise publicly demonstrated along with the drawing of it; and all the several steps were shown to my Father; and I still preserve it in spirits as it is represented (a).

I have since that confirmed this observation in two subjects, in whom I could plainly perceive six or seven ducts, and in each subject having injected one of the largest of them with quicksilver, it passed, as above described, into the oval shaped body of the *glandula innominata* (b). On squeezing the *glandulæ congregatæ* on the upper or outer part of the *conjunctiva*, a liquor was emitted by a number of imperceptible orifices on the inner side of that membrane.—Hence the liquor seems to be poured out from the *glandula innominata* and these lesser glandules much in the same manner as the *saliva* is from its glands, viz. partly by larger ducts and partly by imperceptible orifices.

As the quicksilver injected into one of these ducts did not return by any of the others, so they don't appear

(a) T. 2. Fig. 1.

(b) T. 2. Fig. 2.

pear to have communicating branches, as the lactiferous ducts in the *mamma* have.

It may be still more easily demonstrated in birds, that a gland analogous to the *innominata*, supplies a liquor for lubricating the eye; for in them all the small branches join into one common duct of a considerable size, which discharges itself by a large orifice, on the inner side of the *membrana nictitans*, or third eyelid (c).

The *glandula innominata*, therefore, being provided with ducts, it cannot be doubted but that it is the principal, and it seems to me highly probable, that it is the only organ for separating the saltish liquor we, strictly speaking, call tears. For, if the exhalant arteries of the *conjunctiva* could separate this liquor, we can hardly suppose Nature would have added the more complex structure of a gland. Neither do we observe, that our other fluids are separated, partly by the complicated structure of a gland, and partly by exhalant arteries. Thus it would be no very difficult matter to show, with great probability, that the whole of the bile comes from the liver, and the whole of the urine from the kidneys: and that these liquors are not in part separated from the exhalant vessels of the *vesica fellea* or *vesica urinaria*. There is only a watery dew sent off from these exhalant vessels, which neither possesses the properties of bile nor of urine. So, in like manner, it is probable, that, in the eye, the liquor separated from the exhalant arteries of the *conjunctiva* has not the saltish taste and other properties of the tears.

Were it still doubtful, if the gland, whose ducts I have described, separated the tears, I might confirm it by one argument more, *viz.* that I observe, that the
common

(c) T. 2. Fig. 3, and 4.—Although this gland and its duct have been described in some rare birds, by the French Academicians, (See DU HAMEL, H. R. Ac. Sc. or *Mem. de L'acad. des Sc.* 1735, 6.) Yet I thought it might not be unacceptable to give a Figure of it in a subject, which, from its being common, every one has the opportunity of examining.

common fishes; which want the *palpebræ*, and have no occasion for a liquor to lubricate their eye, are not furnished with a gland analogous to this one.

The numerous ducts in man and quadrupeds seem evidently intended for washing the eye equably; and that the tears may not fall out between the eyelids, and run down over the face: Which they might have done, had they been poured out in large drops, that is, by larger or less numerous ducts.——Nor is it an objection to this, that there is but a single duct in birds: since they have not only a *membrana nictitans*, under which this duct opens; but they likewise move chiefly the under eyelid: so that this larger drop may be squeezed thin and spread, as it were, into a sheet, under this membrane, and may be applied by its motion and that of the under eyelid, in the like equable manner.

EXPLICATION OF THE FIGURES OF TABLE II.

FIG. I. Represents the upper eyelid of the human subject, with the *glandula innominata Galeni* or *glandula lachrymalis* (*b*).

- a.* The inner side of the upper eyelid.
- p.* The two *puncta lachrymalia* at the internal *cantus*, into which a wire is introduced and the ends of it twisted behind.
- b.* Part of the under eyelid.
- c.* The external *cantus*.
- d.* The thicker conglomerated part of the *glandula innominata*.
- e.* A number of smaller glandules lying between *d* and the *conjunctiva*, which, for distinctions sake, I have called *glandulæ congregatæ*.
- f.* Four

(*b*) This preparation had been kept several days in spirits, before the drawing was made.

- f. Four bristles introduced into the ducts of the *glandula innominata*.
- g. One of these ducts into which quicksilver was injected, and which is hid where it passes through the *glandula congregata e*, but appears again entering *d* in three branches.
- b. The part where 2 or 3 smaller orifices could be perceived before it was immersed in the spirits.

Fig. II. Represents the like parts in another subject, seen on the outer or upper side.

- a. The outer side of the *tunica conjunctiva* of the upper eyelid.
- b, c, d d, e e. the same as in Fig. I.
- f. The branch of the ocular artery going to the *glandula lachrymalis* filled with wax.
- g. The end of a bristle appearing, which was put into
- b. One of the lachrymal ducts, previously injected with quicksilver.
- i. i. That duct dividing into two branches.

Fig. III. Represents the eye of the common Hen.

- a. An out-line of the comb and beak.
- b. The eye-ball.
- c. The eye-lids.
- d. The *membrana nictitans*, or, what some call the third eye-lid.
- e. A probe put into the duct of the lachrymal gland.
- f. A probe put into the undermost *punctum lachrymale*.
- g. A probe put into the uppermost *punctum lachrymale*, and passed from the nose into the mouth.

Fig. IV.

- a. The bottom of the eye-ball in the same bird.
- b. The optic nerve.
- c. The *glandula lachrymalis*.
- d. Part of the *membrana nictitans*.
- e. The probe in the lachrymal duct.

