Plates wanting

MEDICAL COMMENTARIES.

PART I.

CONTAINING

A PLAIN AND DIRECT ANSWER TO PROFESSOR MONRO jun.

INTERSPERSED WITH

REMARKS on the STRUCTURE, FUNCTIONS, and DISEASES OF SEVERAL PARTS of the HUMAN BODY.

By WILLIAM HUNTER M. D.

The SECOND EDITION.

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MDCCLXXVII.

ADVERTISEMENT,

Prefixed to the first Edition, *viz. An.* 1762.

THE Author of these Sheets has formed a design of offering to the Public, from time to time, his Observations in Anatomy, Surgery, and Midwifery; and has therefore given such a Title to this First Part, as may in some measure comprehend the whole of his scheme.

The Second Part will contain an account of the Gravid Uterus; *the publication of which has been so long retarded only by the author's desire of sending it into the world with fewer imperfections. In the long interval of time, since proposals for executing this work were first given out, some favourable opportunities of making observations on this subject have occurred, and, it is hoped, have furnished such matter of improvement, as may make some compensation for the delay.—The plates, illustrating the description of the* Gravid Uterus, *most of which have been engraved many years, will be published separately in a large folio volume.*

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INTRODUCTION.

A Controverſy relating to any improvement or diſcovery in the arts, when there are complaints of unfair dealing, is an aukward ſubject for a party principally concerned. The writer muſt plead his own cauſe; and where he thinks himſelf in the right, and ill-treated, he will be led to ſpeak in his own favour, and to the prejudice of another perſon; both which a man of an ingenuous turn of mind would willingly avoid. Another diſadvantage, under which a writer in ſuch a diſpute muſt appear, is, that he will often ſeem to be magnifying trifles, claiming what is not worth his poſſeſſion, and proving or denying things of no conſequence; for the ſubject in diſpute is often of trivial moment in itſelf, but becomes important to the parties concerned, when their moral characters are affected by it. Hence the reader is naturally firſt offended with an importunate eagerneſs in the writer, then tired of ſo much about nothing, and, perhaps, condemns both, without attending to what either has to ſay.

Another circumſtance, as unpleaſant as either of thoſe already mentioned, is, that in order to do juſtice to the cauſe, it may be neceſſary to ſay many things, which an author would wiſh to avoid; he may be obliged either to ſuppreſs his evidence, or to call upon his friends, and publiſh the matter of private converſation; in ſhort, to mention many things, which though, for ſome reaſon or other, it may be diſagreeable to relate them, are yet neceſſary in the defence of truth.

As theſe are the genuine ſentiments of the author of the following pages, it is natural to believe that he would willingly have declined a paper-war with Dr. Alexander Monro jun. of Edinburgh. But the treatment, which he has received from that gentleman, has been ſo very ſingular, that it was inſiſted on by his friends, that he ſhould publiſh a full ſtate of the caſe, ſupported by unqueſtionable teſtimony. He urged, that his original complaint againſt Dr. Monro was not worth the attention of the public,

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in any other way than as a paragraph in a literary journal; and that to anſwer the illiberal cenſures, and expoſe the ſubterfuges, in the Doctor's pamphlet, he ſhould be obliged to deſcend to things of too trivial a nature for the notice of the public. They obſerved, on the other hand, that this ſtep was *become neceſſary*; that the neceſſity of it would prove its excuſe, &c. They prevailed.

If any part be found to be ſevere, it will be the facts rather than the language. Juſtice calls for facts: the intelligent reader will make his own reflections, and will give his cenſure or his approbation, according to his opinion of the merits. The diſpute lies within a ſmall compaſs; and the little, that has been already advanced on this ſide of the queſtion *, is conciſe and to the point. Dr. Monro's pamphlet † is prolix, vague, and without preciſion. Theſe are faults which may happen in controverſial writing, where the cauſe is good and the heart upright; but we more commonly find them where it it is neceſſary to ſtifle truth, to diſguiſe falſhood, or to throw obſcurity over the whole, that the fair inquirer may be tired, and forced to declare, that the queſtion is too difficult and perplexed for him to decide.

The acrimony ſo remarkable in Dr. Monro's pamphlet may be excuſed by thoſe who entertain a favourable opinion of its author. They will tell us, that anger is a very natural paſſion, and is often an attendant upon an honeſt heart. We grant it; but ſtill it is a fault, inaſmuch as it gives a ſuſpicion of a weak or of a bad cauſe, and of a mind prone to do an injury; and experience often verifies the old obſervation, that the aggreſſor is the moſt irreconcileable enemy ‡,

But

* Critical Review for Nov. 1757, Art. IX. and for Dec. 1757, Art. IX. See Appendix, No. III. & V.

† Obſervations Anatomical and Phyſiological, &c. Edinburgh, 1758, 8vo.

‡ If the reader ſhould have forgotten the acrimony of Dr. Monro's performance, and think theſe hints too ſtrong, we would beg the favour of him to ſay, whether *gentlemen* treat any living character, above the rank of the pillory, in the following manner. In his ſecond page, he thinks it *highly probable* that Dr. Hunter's *conſcience muſt have rejected what his pen affirmed*. In page 4, he *conjectures* that Dr. H.'s intention was *to catch at any occaſion to propagate* what was falſe and injurious, being *conſcious* that it was ſo. P. 10, he repreſents Dr. H. as *drove by the rage of detraction* to the *moſt frivolous reſources*. P. 11, he ſays, that Dr. H.'s proceedings *muſt, to every man of common ſenſe, appear not only highly unjuſt and malevolent, but equally weak and ridiculous:* and he mentions ſomething as *the laſt effort which Dr. H.'s imagination has been able to ſuggeſt for throwing a reproach upon him.*

But what I thought the great fault of the performance, was the open violation of truth and candour. How far I had reafon for taking fuch offence, the reader will judge. The original difpute between us is about *facts* and *dates*, and does not allow of quibble or evafion. One of us muft be in the right, the other muft be in the wrong. Therefore, what relates to the injection of the *tubuli teftis*, and to the ufe of the lymphatic veffels, muft be hiftorical, and muft be fupported by fufficient vouchers. That circumftance has rendered the firft part of this work more tedious than I could have wifhed, The latter part, I flatter myfelf, will be found both more entertaining, and more ufeful, as it is interfperfed with many curious experiments and obfervations, particularly on the fubject of abforption, and on the ftate of the *teftis* in the *fœtus*, which were made, and communicated to me, by my brother.

I have added an Appendix, containing all that was publifhed in the Critical Review, relating to this controverfy, before Dr. Monro wrote his pamphlet, that the reader may be able to confult thofe papers with eafe, when he finds them quoted.

If the world fhould think that I ought to have publifhed this defence fooner, I beg they may recollect that when my friends firft engaged me in it, I only promifed to do it *with proper opportunity* *. It required a good deal of time, and I had little to fpare: the fubject was unpleafant, and therefore I was very feldom in the humour to take it up: Dr. Monro's performance did not hurt me with my friends, and I was lefs folicitous what others might think of me: no perfon could fuffer by the delay, but myfelf: and, as I well knew that I could make every thing very clear, I thought it of no confequence whether I appealed to the public a little fooner or later,

* Critical Review, Vol. VI. p. 316.

CHAP. I.

Of the Injections of the *Testis*.

IN the year 1757, I informed the public * what I had done upon this subject, in the following words.

" 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 method filled the whole *Epididymis*, 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 become gradually more turgid and heavy, " for some time after the external parts were compleatly filled. I " shewed this preparation next night at my public 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. Do- " nald Monro, then at Edinburgh, by a letter from Dr. Garrow, phy- " sician at Barnet, some time in the same month.

" In some such time as a week or fortnight, after the first public " demonstration, my brother made the trial, and succeeded. He shewed " me the *Testis* opened, and the tubular internal substance very generally " filled with mercury. This preparation, which I still preserve, I " shewed at my public lecture, that very evening, with marks of being " pleased with the discovery. In my next course of lectures, viz. Feb. " &c. 1753, and in every course since that time, I have shewn the " same, and some other preparations of the same kind; and always gave " the history of the discovery, to avoid taking that share of it from my " brother which belonged to him."

Whether this was a fair relation of facts, though unpleasant to Dr. Alexander Monro jun. or whether it was false, and consequently most impudent and weak, because it could have been disproved by an hundred living witnesses, the reader will determine, when he considers the fol-

 lowing

* Critical Review for Nov. 1757, Art. IX. See Appendix, No. III.

lowing teſtimonies *. That he may better underſtand them, I beg leave to obſerve, that I have always given two courſes of lectures every winter, each courſe laſting about three months. The firſt, and what is called the Autumn courſe, is finiſhed before New-years-day; and the ſecond, called the Spring courſe, begins about the 20th of January.

Mr. Galhie, of Spital-ſquare, ſurgeon, the gentleman abovementioned, in the account he gives me under his hand, ſays;

"Having made no memorandum of the *Teſtis*, in which your brother "had filled the internal tubes with mercury, I cannot be poſitive as to "the preciſe time it appeared at lectures; I remember, indeed, that the "preparation was made *within* a few weeks after you had injected the "firſt *Teſtis*; and, as it was ſhewn to the pupils the ſame evening it "was finiſhed, *it muſt without doubt* have been publicly demonſtrated "ſometime in the autumn courſe, 1752.

Spital-ſquare,
Nov. 24, 1758.

R. GALHIE."

Mr. Watſon, of Marlborough-ſtreet, ſurgeon and reader of anatomy †, has given the following account of the matter.

"In the beginning of the Autumn courſe of the year 1752, Dr. "Hunter got a *Teſtis*, filled it, and ſhewed it at public lecture. In "this preparation, the whole *Epididymis*, and the ducts between it "and the body of the *Teſtis* were all diſtended with mercury; and, al-"though the mercury had paſſed very probably into the body of the "*Teſtis*, yet the Dr. did not care to run any hazard of ſpoiling ſo fine "a preparation to demonſtrate this circumſtance. I cannot charge my "memory with the exact time; but I very well recollect, that it was "*but a few days* after when I ſaw the whole *Epididymis*, the communi-"cating *Tubuli*, and the *Tubuli* in the body of the *Teſtis*, finely filled "with mercury. I remember perfectly well, that I firſt ſaw this ſecond "preparation

* Let me once for all do juſtice to the character of every gentleman, who has done me the favour of giving his evidence in this diſpute, by declaring, that none of them were officious witneſſes; and that none of them meant to take part with either ſide, but to ſay what they knew to be truth, when application was made to them for that purpoſe.

† I take the opportunity with pleaſure of doing this gentleman the ſame juſtice, that I did him at my lectures with regard to his obſervations upon the *Teſtis*, by declaring that he firſt ſhewed me the ducts coming out from the *Teſtis* to form the *Epididymis*, in a preparation where he had traced them by diſſection with great accuracy.

" preparation in the Dr's ſtudy, one evening after lecture. Both theſe " preparations were produced and demonſtrated by the Dr. in the " Autumn courſe of the year 1752; the one *within a few days* of the " other. In this relation of facts, I have ſtrictly adhered to truth; nor " have I declared more than I was an eye-witneſs to.

Great-Marlbro' Street, Oct. 26, 1758. HENRY WATSON."

Mr. Davenport, of Norfolk-ſtreet, ſurgeon, ſays;

" I declare that I attended both the firſt and ſecond courſe of lectures, " of the winter, 1752-3, with Dr. Hunter, and remember diſtinctly, " that it was *in one and the ſame courſe*, that Dr. Hunter firſt ſhewed " the *Epididymis*, and the internal *Tubuli Teſtis*, filled with quickſilver *. " To the beſt of my remembrance, and from conſulting my notes, it " was in the courſe of October, November, and December, 1752.

London, Oct. 24, 1758. R. DAVENPORT.

Mr. Davies, of King-ſtreet, ſurgeon, gives me the following atteſtation.

" I attended Dr. Hunter's courſe of lectures of October, November, " and December, 1752, and can declare with the ſtricteſt truth, that, " *in that very courſe* he ſhewed his pupils firſt the *Epididymis*, and " then the internal ſubſtance of the *Teſtis*, filled with quickſilver.

King-Street, Covent-Garden, Oct. 26, 1758. GARLAND DAVIES."

Mr. Pile, of Parliament-ſtreet, Weſtminſter, writes as follows;

" I attended Dr. Hunter's courſe of lectures of October, November, " and December, 1752, and remember diſtinctly, that *in that time* he " ſhewed publicly firſt the *Epididymis* filled compleatly with quick- " ſilver, and then the internal *Tubuli* of the *Teſtis* likewiſe filled with " quickſilver.

Parliament-ſtreet, Oct. 25, 1758. DOM. PILE."

* This is as much to our purpoſe, as if he had remembered the very month and day, becauſe Dr. Garrow's letter (admitted by Dr. Monro) proves that the firſt preparation was ſhewn in the Autumn courſe, 1752.

Mr. Nicolay, of Green-ftreet, Leicefter-fquare, furgeon, gives me the following teftimony.

"I attended Dr. Hunter's Autumn courfe of lectures of October, "November, and December, 1752, and feveral other courfes afterwards, "and do declare, that I was prefent when he firft fhewed the *Epididymis* filled with quickfilver, and likewife when he, *fome days afterwards*, "fhewed the internal *Tubuli* filled in the fame manner. Both were "exhibited in one and the fame courfe *.

"So far as relates to what was done in public, I do atteft the truth "of what he has faid upon this fubject, in the ninth article of the "Critical Review for November, 1757.

Green-ftreet,
Nov. 24, 1758.

CHRIS. NICOLAY."

It is time to conclude this chapter. The candid reader is left to make his own reflections upon it; he will fee that it is a full anfwer to all the reafoning in Dr. Monro's pamphlet, relative to this part of the difpute; and the Doctor will obferve that I have been difpofed to oblige him. He defired me †, *to produce the teftimony of fome few of the number who faw the preparation in queftion, in my Autumn courfe, for the year* 1752.

C H A P. II.

Of the Origin and Ufe of the Lymphatic Veffels.

THE bufinefs of this chapter is indeed a little more complex than that of the preceding; yet a reader who is converfant with anatomical and phyfiological fubjects will find the demonftration as clear, and the teftimonies as ftrong.

The authors of the Critical Review, in their account of Dr. Alexander Monro's book upon the Lymphatics ‡, did me the honour of writing the following paragraph, which the reader will find neceffary to be premifed for underftanding this part of the difpute.

"Dr.

* This likewife is the fame thing as faying that it was in the Autumn courfe, 1752.

† In the fixteenth page.

‡ Critical Review for Sept. 1757, Art. VIII. See Appendix, No. I.

" Dr. Monro ſays, the diſcovery he has made with reſpect to the " lymphatics, was owing to experiments aſcertained four years ago. " Now, Dr. Hunter has read public lectures in anatomy eleven years, " and, in every courſe, made the following obſervations on the lymphatic " veins: That whereas the moſt generally received opinion was, that " they were a continuation of lymphatic arteries; he, on the contrary, " believed them to be *the ſyſtem of abſorbing veſſels*, and that they began " from all the internal and external ſurfaces of the body. This belief " he founded on theſe reaſons: Every body allows, that all the ſurfaces " of the body are bibulous, or provided with abſorbent veſſels, by " which, mercury applied to the ſkin, collections of water in the breaſt, " belly, or in the cellular membrane, &c. are occaſionally taken up, " conveyed into the circulation, and ſtrained off again by ſecretion. " That the lymphatic veins perform this office, ſeems probable, from " the following remarks: I cannot inject them, as other veins, by filling " the arterial ſyſtem; ſo that, in all probability, they are not continua- " tions of the arteries. 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 membrane. This looks, as if " they took their riſe from thoſe cells, like the veins in the ſpungy part " of the *Penis*. 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*? But the moſt ſtriking argument, 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 through the lymphatic or con- " globate 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 doctrine explains the uſe of valves in the " lymphatics. In other veins, whether large or ſmall, the fluid is ſup- " poſed 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 onward towards their termination. This doctrine

" of

" of the lymphatics is farther confirmed by the abforption and progrefs " of the venereal poifon. The lacteals were difcovered, traced, and " their ufe afcertained from the circumftance of a manifeft and parti- " cular colour in their contents, upon fome occafions at leaft. We have " not the fame advantage, with refpect to the lymphatics; but, in them, " what we cannot trace with the eye, we find out by the effects of this " poifon. We know from obfervation, that this *virus* may be taken in " at any particular part of the body, and thence diffufe itfelf over the " whole conftitution. We muft fuppofe it abforbed by the fame " veffels that abforb its antidote mercury, or any thing elfe, that is " carried into the mafs of blood by abforption. Thefe things being " of a more inoffenfive nature, pafs unobferved: but this poifon, from " its irritating and deftructive quality, is apt to raife difturbance in its " paffage, before it reaches far enough to mix with the blood. Hence " the lymphatic glands, through which every abforbed liquor muft " pafs, are fo often the parts firft affected by the venereal taint, when " it is fpreading its contagion through the conftitution. This is the " theory of the venereal *bubo*. If the infection be received in the moft " common way, the bubo happens in the groin, becaufe the lymphatics " of the genitals pafs through the inguinal glands; but, if the in- " fection be received at the hand, (a cafe that fometimes occurs) the " *bubo*, for the like reafon, is formed in the arm-pit: when the difeafe is " communicated by the lips, the glands of the neck inflame and tumify.

" This is the very effence of Dr. Monro's treatife; and thefe obfer- " vations have been publicly made by Dr. Hunter to his pupils, for the " fpace of eleven years, &c."

This piece of criticifm occafioned an anonymous letter being fent to the authors of the Critical Review *, which Profeffor Monro jun. tells us was written by his father †: upon which I was defired by thofe gentlemen to furnifh them with a concife account of the facts relating to the controverted point. I drew up fuch an account, and they publifh- ed it with the abovementioned anonymous letter, in the Critical Re- view for November 1757 ‡. It was as follows, " Ever fince I firft read " anatomical lectures, in 1746, among other things a little out of " the common way of thinking, I have advanced the doctrine of the " lymphatics being the fyftem of abforbing veffels, and have fupported " my

* See Appendix, No. II.

† See his note at the bottom of the fecond page.

‡ See Appendix, No. III.

" my opinion by fuch arguments and experiments as are mentioned in " your Review of laft September. This appears by the MS fyllabus " of my lectures, which I have ufed in public from the beginning; " by many MSS of my lectures in the hands of thofe who have ftudied " with me; and by the general teftimony of thofe who have done me " that honour. I have many vouchers in my poffeffion from gentlemen " who have attended my lectures, and I appeal particularly to the " following gentlemen, who are all profeffors or readers of anatomy " now living; Dr. Collignon, profeffor of anatomy at Cambridge; " Dr. Smith, reader of anatomy at Oxford; Mr. Hamilton, profeffor " of anatomy at Glafgow; Mr. Cleghorne, reader of anatomy at Dub- " lin; Mr. Watfon, reader of anatomy in London; Mr. Galhie, of " London, and demonftrator or diffector for the profeffor of Cambridge. " So that the fact of my having taught this doctrine, and fupported it " by fuch arguments, for a number of years at my public lectures, " cannot, I think, admit of a difpute.

So I thought, and fo I fuppofe every reader, but the two Edinburgh profeffors, will think. Now, to remove all poffibility of doubt upon this fubject, I fhall fubjoin fome paffages of letters fent to me upon this occafion, and for the moft part taken literally from MS notes, written by gentlemen for their own ufe, while they attended my lectures. I would not wafte the reader's time, by printing their letters at full length, but fhall extract juft fo much, as to fhew that every argument, which I alluded to in the preceding hiftory, was not only delivered by me at public lectures, but *is now to be found in manufcripts written from my lectures, before Dr. Alexander Monro pretends to have taken up the thought, or made the difcovery:* for it was not till (or after) the fummer, 1753 *, that he received " the firft hint, that the lymphatics were not " continued from the arteries; but that they came from the cellular " membranes, and confequently were abforbents †."

In

* See page 22, 23, and 24, of his pamphlet.

† Shall we call the year 1753, *fortunate* or *unfortunate* for Alexander Monro, jun. Profeffor? Surely it was a *remarkable* year. He was then a *ftudent* of anatomy, and in *that one year* made *three difcoveries*; viz. he filled the *Tubuli Teftis* with quickfilver, found out that the lymphatics were abforbents, and faw the orifices, and introduced briftles into the ducts of the lachrymal gland in the human body. If he goes on at the fame rate, he will become a prodigy. But it was rather *unfortunate*, that Dr. Hunter fhould have done, and publicly taught, the very *fame three things* before that time; and that he fhould be able to prove that a MS of his lectures was *at that time in the hands of ftudents at Edinburgh*, in which there was fomething more than a *hint* about the lymphatics, as we fhall fee hereafter.

In juftice to the gentlemen, who have favoured me with the following extracts, I muft beg the reader to remember that they were not written for the prefs, but as memorandums for private ufe: therefore the moft ample allowance is to be made for the drefs in which they appear. One of thefe gentlemen, after tranfcribing what he finds in his notes, fays, "I hope you will excufe any inaccuracy in thefe notes, as you well "know how difficult it is for young anatomifts to carry off the particu-"lars of any lecture, in which both their ears and their eyes are em-"ployed." Another fays, "This is all I can find in my journal, re-"lative to the topics mentioned in your letter; it is very poffible, I "may have mifapprehended you in fome points, and omitted material "arguments to prove others, as I was abfent fome days, and my notes "were all taken from memory after lecture, and thrown together in a "hafty carelefs manner." In the laft place it muft be fuppofed, that my lecture was not always precifely the fame. The longer I confidered the fubject, I fpoke with more firmnefs, and collected more proofs of the doctrine; and as I always fpoke principally from memory, we may imagine the arguments, brought at different times to fupport it, were not always the fame, nor ranged in the fame order.

Mr. Symons of Exeter, furgeon, attended my firft courfe of lectures, in 1746, and the three following courfes, and likewife affifted me in diffections. He writes to me from Exeter, June 13, 1759, thus: "I "fhall endeavour from recollection, and the affiftance of notes taken at "your lectures, to fet forth the doctrine you taught, concerning the "lymphatics, the two firft years you read.----- and then told us you "thought the lymphatics were abforbent veffels, and that valves are "neceffary where there is no propelling force.----When fpeaking of the "*Teftis*, you told us if we made a hole, through the *Tunica Albuginea*, "broke or bruifed the *Tubuli*, we might diftend the lymphatics by "blowing into its fubftance *. You affured us that the *Receptaculum* "*Chyli*

* This experiment I learned from Dr. Nicolls, when I attended his lectures, and I have ftill a preparation of a horfe's *Teftis* begun by him and finifhed under his direction, in which a vaft number of lymphatics are filled by this method. Mr. Weftbrook, of Dartmouth-ftreet, very well remembers the preparation. He was prefent and affifted when it was made; fo was Mr. Young, furgeon, of Soho-fquare. I afterwards filled the lymphatics in a calf's fpleen by extravafation. Thefe things firft made me think they were as to origin and ufe like the lacteals, which they refemble fo much in other circumftances: and this opinion was every day more and more confirmed by innumerable anatomical experiments and obfervations, in which I was then wholly employed. The abforption and progrefs of poifon as a proof

"*Chyli* might be filled, before the ſeparation of the *Teſtis* from the "body by the above method."

Dr. Collignon, profeſſor of anatomy at Cambridge, attended my lectures in the beginning of the year 1747, but not till the courſe had been ſome time begun. In his letter to me from Cambridge, Nov. 13. 1757, in anſwer to a paragraph in my letter to him about this diſputed doctrine, he ſays; "I always imagined, that the doctrine of the "lymphatics, as deſcribed by you in the third paragraph, and which I "held as orthodox, was delivered by you at your lectures; but the length "of time elapſed ſince I had the pleaſure of attending them, my having "omitted to takes any notes, and the frequent intercourſe which I have had "with many of your pupils for ſome years, make it impoſſible for me "to recollect with certainty, whether I firſt had it from your own "mouth, or collected it from ſucceeding pupils."

Mr. Hamilton, profeſſor of anatomy at Glaſgow, who attended my lectures in 1748, and again in 1749, in a letter to me, dated Glaſgow, Nov. 21. 1757, ſays; "From what I heard in your lectures, and from "ſome converſations with your brother, I learned the method of demonſtrating the lymphatics by blowing into the excretory ducts of "the glands and ſubſtance of the *Teſtis*. I have by me a preparation, "which I had from your brother, where, from an extravaſation, a "number of veſſels are filled, which appear to be lymphatics, and "which I keep to demonſtrate them. Your demonſtration of their "ſimilarity to the lacteals, I have adopted ſince I read here; though "always with the candour of owning you as the author of that, and "a number of other things."

Mr. Watſon, reader of anatomy, and ſurgeon to the Middleſex hoſpital, has favoured me with a long letter on this ſubject, dated

C Aug.

proof of this doctrine firſt came into my mind upon reading what Mr. Freke ſays upon the cauſe and cure of the venereal *bubo*, ſometime after the publication of his Art of healing in 1748. I made enquiry among ſurgeons that were much employed in venereal caſes, and in inoculation; and all their obſervations confirmed my hypotheſis. From that time, I uſed the argument drawn from the abſorption of poiſons. In the ſpring, 1753, I attended a caſe with Dr. Pitcairn, which confirmed this doctrine, as I obſerved to him at the time: And much about the ſame time, Dr. Macaulay had a very painful inflammation in his hand, from ſcratching his finger in opening a morbid dead body; a red and painful line ran up from his finger towards the armpit, and there terminated in a painful ſwelling: he aſked me, what I thought it was, if a nerve, or what? My brother and I, upon ſeeing it, gave our opinion, that it was a lymphatic veſſel inflamed by the poiſon which it had taken in. The Doctor remembers this circumſtance particularly well.

Aug. 27. 1760, of which I fhall give fuch parts only as are directly to the points in queftion.

" ---- I keep to no particular dates, as not being material; fince " from my firft attending your anatomical lectures in the year 1748, to " within a year or two I believe of Dr. Monro jun. his attending them, " I have heard you deliver yourfelf, over and over again, fometimes " in a fhorter way, and fometimes much more fully on the fame fub- " ject.----You fpoke indeed at firft with all that modefty a man would " do, who thought he had reafon to difagree with the common received " opinion, and you grew more pofitive only as you had ftronger " conviction.

" ----You could not fay what was their *(Lymphatics)* precife be- " ginning.---You believed they were not continuations from arteries, " but *a particular fyftem of veffels by themfelves, the true abforbents.* In " fome fucceeding courfes, you declared you was fully convinced of " this, becaufe you had never been able to inject them. If continuations " of the arteries, why not inject them, fince we do inject the lymphatic " arteries?---You never could fill them by injecting the artery, till you " had made an extravafation in the cellular membrane. You thought " them the *abforbing veffels,* becaufe they have valves, as the lac- " teals have, which are known to be *abforbing veffels.*---Why fhould " they *(Lymphatics)* have valves in the *Vifcera,* when the veins have " none? Which you anfwered by obferving that lacteals have no *im-* " *petus* from the arteries, as the veins have; therefore they are fur- " nifhed with valves, and for the fame reafon thefe lymphatics have " valves.----You particularly defired us to attend to what happens with " regard to the progrefs of the venereal poifon.---It was readily ab- " forbed by the *Penis,* and conveyed to the firft neighbouring " lymphatic gland: thus from the *Penis* to the *Inguen:* but if re- " ceived by the finger, it would then be as readily conveyed to the " axillary glands; thefe being the two places, where the *Bubo* is " commonly formed.

" You told us---You had traced the lymphatics from the *Teftis* " of a dog into the thoracic duct *.---You informed us, the lymphatics " might

* The reader will better underftand the reafon of this obfervation, when I have told him that, in fhewing the lymphatics diftended with air in the horfe's *Teftis*, I ufed to fay that in the preparation indeed it might be doubted, whether they were lymphatics or veins; but that I was certain they were lymphatics, becaufe I had inflated them by the fame method

" might be demonftrated either by blowing into the artery, or by " making a ligature on the emulgent vein in a living dog; for in each " way they will be diftended: but you obferved at the fame time, " that thefe experiments were no proofs of their being continued from " arteries, as the cafe might be the fame as in injecting.---You pro- " duced before us, a preparation of the *Teftis* from a horfe, in which the " artery was injected red, and the lymphatics dried hollow, diftended " with air, which you told us had been thrown into them by inflating " the cellular membrane.---I think I could recollect fome few more " particulars upon this fubject: but I fhall give nothing from memory, " not caring to truft to it, at this diftance of time. What I have here " faid is a mere relation of facts, extracted from the notes I have taken " at your lectures."

Dr. D'Urban, in his letter to me from Richmond, Nov. 12. 1757, fays, " I have been looking into the notes I made from your lectures, " in the beginning of the year 1749, and---fhall tranfcribe the para- " graph juft as it ftands.

" Lymphatics] A preparation of the lymphatics of a horfe's *Teftis* *, " ---I do not believe them continuations of the feriferous arteries, but " abforbent veffels placed in every interftice of the body, which take up " any fluid, thrown into the *Abdomen* or any other cavity; as is feen " from daily experience. I have injected the fpleen, which is full of " lymphatics, with the moft fubtle injections, filled every branch of " the artery or vein, when after tying the vein, and forcing the in- " jection till the veffel burft, immediately on the extravafation the " lymphatics became filled. This I have tried more than once, with " the fame fuccefs. Hence I conclude they are not a continuation of, " nor have any communication with the arteries †".

method in a dog, when the *Teftis* was not feparated from the body, and traced them all the way up to the thoracic duct.

* The preparation above-mentioned, where the lymphatics are filled by blowing into the fubftance of the *Teftis*, and which I have generally produced at my lectures both as a fpecimen of thofe veffels, and as a proof of the doctrine. The reader is defired here to obferve, that I had then a preparation, and fhewed it, of the lymphatics of the *Teftis* filled in this manner; and Profeffor Hamilton had from my brother a preparation of the lymphatics filled by extravafation. But, notwithftanding all this, Profeffor Monro has the modefty to affert, (pag. 43.) that " Dr. Hunter never had made any fuch experiments or preparations, nor " even imagined the thing poffible."

† This gentleman (Dr. D'Urban) ftudied at Edinburgh, in the winter 1752-3, after having attended my lectures, was acquainted with Alexander Monro jun. then a ftudent,

Dr. Smith, reader of anatomy at Oxford, gives me the following extract from notes taken at my Autumn courfe of lectures, 1750. " If " you blow or throw water into an artery or vein in the liver or fpleen, " &c. you can raife the lymphatic veins: hence alfo they were thought " to be continuations of the lymphatic arteries; but I doubt it, for on " injecting the fpleen, I could not throw the injection into the lym- " phatics

and received *particular civilities* (as he expreffes himfelf in his letter to me) from Profeffor Monro fenior. The reader might now admire the clofenefs and neatnefs of Dr. Monro's reafoning, in his 35th page, &c. He there " evidently proves" that what I faid fo ftrongly in Dr. D'Urban's prefence, in the beginning of the year 1749 was " gleaned from his Inaugural Differtation" in the year 1755. The Profeffor writes thus, page 35, " But I " fhall evidently prove, that before that time, (viz. January, 1756) he never made the moft " material remarks, and the only ones which lay the ground-work for a juft and allowable " conclufion; but that he gleaned them from my *inaugural differtation*, which I prefented " to him on my coming to London.—Such are the two firft experiments, with which he " fets out, by means of which only, what had paffed for pofitive and direct proofs of " lymphatic arteries, can be refuted.

" *That the lymphatic veins perform this office* (viz. of abforption,) *feems probable*, fays Dr. " Hunter, *from the following remarks.*

" 1. *I cannot inject them as other veins, by filling the arterial fyftem; fo that, in all probability,* " *they are not continuations of the arteries.*

" 2. *I have fometimes obferved in injecting, that they were immediately filled with wax when the* " *arteries burft, and the wax was effufed into the cellular membrane. This looks as if they took* " *their rife from thefe cells, like the veins in the fpungy part of the Penis.*"

Let the reader, for the fake of amufement, compare thefe two arguments with Dr. D'Urban's and the other teftimonies, and then read Dr. Monro's book from page 36 to page 43, and he will fee by this fpecimen, what fort of a profeffor we have to deal with. " Thefe experiments therefore being fully explained and infifted upon in my *Inaugural* " *Differtation*, (viz. in 1755.) which the Doctor had perufed, it is poffible he might have " firft learnt them from it," (very poffible indeed to have extracted the knowledge of 1749 from that of 1755.) " That he did collect them in this way only, and never had made " or imagined any fuch experiments before, the fequel does not allow us to doubt. In the " firft place, the Doctor's dry manner of relating them, &c." After a chain of *fenfible* and *correct* reafoning, he concludes thus, (page 45.) " It is therefore moft evident, that Dr. " Hunter never had made any fuch experiments or preparations, nor even imagined the " thing poffible: and confequently he firft learned from my inaugural differtation, and " from the one I publifhed (afterwards) at Berlin, that the common experiments offered " as direct proofs of lymphatic arteries could be refuted by experiments. Hence he is, in " this refpect, not only guilty of a felf-convicting plagiarifm from me; but, by attempting " to turn my own experiments and words againft myfelf, as ftolen from him, has added " an abufe to injury."

Give us leave to add, that by the fame reafoning *it is moft evident*, that our poffeffor is not only a clofe and clear reafoner, and a fair and candid enquirer after truth, but an elegant and delicate writer.

" phatics till the artery was ruptured, and then it got into the lympha-
" tic veffels. Befides, why fmall valves in the lymphatics, if veins, when
" other veins of the *abdomen* have none? They feem to be abforbents
" of a fine fluid, to be conveyed into the receptacle of the chyle, for di-
" lution, &c. We find the lacteals, which are allowed to be abforbents,
" have valves; and the reafon is, that on the leaft motion, the progreffive
" circulation may be accelerated. The lymphatics therefore feem to
" begin from cells and furfaces." His notes upon the lymphatics of the *Teftis*, run thus, " Dr. Nicholls cut through the *albuginea* into the
" fubftance of the tefticle, and blowed with a blow-pipe: He raifed
" the lymphatics along the tefticle, and running up the fpermatic rope;
" and as a proof that they are not veins, *I have traced them in a dog*
" *up to the receptacle of the chyle*."---He adds, " That I was quite fatis-
" fied with your account about the nature and ufe of the lymphatics,
" and therefore taught it ever fince I began reading here, is not to be
" made a queftion. The firft courfe of anatomy that I read for myfelf,
" began November 21, 1753."

Mr. Davenport of Norfolk-ftreet, attended my courfe of October 1751, and feveral fucceeding courfes. He gives me the following literal tranfcript of notes, which he took down in that firft courfe.

" Some affert, that thefe lymphatic veffels alfo originally take rife from
" the extremities of arteries; which, they tell you, may be proved in the
" dead fubject by tying the vein, and then inflating the artery, as they enter
" the fpleen or kidney, for inftance; or in the living, by making your li-
" gature; either of which methods, fay they, will make thefe veffels
" very confpicuous. But, both thefe experiments are fallacious, and
" prove nothing; for this appearance never comes out in either, till the
" air or fluids have made themfelves a paffage, by deftroying other ten-
" der parts that naturally oppofed them *.------The tefticles have alfo
" lymphatics,

* Profeffor Monro (pag. 20, 21.) by way of introduction to his fubject, tells us the arguments by which former anatomifts were perfuaded that lymphatics were continuations of arteries (particularly air, water, and quickfilver, paffing readily from the arteries into them) and then adds, " Without therefore accounting in fome other way for thefe experiments, and " refuting the arguments drawn from them, to propofe a contrary opinion as a remarkable " difcovery, is certainly betraying a very weak and precipitate manner of hurrying to con- " clufions, contradicted by premifes." Dr. Hunter, as we fee by this evidence, accounted for thefe experiments in another way, viz. by extravafation. Dr. Monro did the fame feveral years afterwards. I will truft the reader for feeing the force of this; and now beg of him to read Dr. Monro from p. 50 to the end of that chapter: but left he fhould not be able to give himfelf that entertainment readily, from not having Dr. Monro's pamphlet immediately at hand, I will do

" lymphatics, though too fmall to be demonftrated otherwife than by " Dr. Nicolls' experiment of inflating them with air, conveyed by a " blow-pipe."

He tells me the following was an additional note, taken at a fubfequent lecture, to the beft of his recollection and belief in the year following, viz. 1752.

" As the veins called Bartholine's lymphatics are too minute to be " traced to their origin, anatomifts are not agreed from whence they " arife. The generality take them to be continuations of fmall arteries; " and their proofs are by no means trivial, if it be true, (as it is afferted), " that by making a ligature upon the vein proceeding from any gland, " you will fee the lymphatic veffels of that gland greatly diftended, as of " the kidney for example. And they tell us too, that by driving air into " an artery in the dead fubject, you inflate thefe veins; and thus demon" ftrate the truth of this doctrine. Notwithftanding thefe fpecious ar" guments, however, Mr. Hunter is clearly of opinion, that thefe veins " are rather the abforbents arifing from all the different cavities of the " body, as the *vefica urinaria*, *fellis*, *veficulæ feminales*, &c. &c.-----A " ftrong prefumptive argument at leaft in fupport of this notion, is the " clofe analogy they in many circumftances bear to the lacteals; for thefe " rife like the lymphatics, have little elfe than the kneading motion in " refpiration to promote the progrefs of the chyle, and are furnifhed with " valves at due diftances, that the chyle may be always fure of paffing " on to the *receptaculum chyli*, and *ductus thoracicus*.-----The fame place " of termination have all the lymphatics; in like manner are thefe pro" vided with innumerable little valves; their coats are thin and fine as " the lacteals; they anaftomofe frequently like them; and in fhort, feem " in

do him the pleafure of tranfcribing one paragraph (from page 53,) which in every line fhows an eafe in writing, and a firmnefs of mind, almoft beyond conception. " I have, however, " *clearly proved*" fays the profeffor, " that Dr. Hunter never had fhewn any preparations, " nor made experiments on the lymphatics, from which any conclufion relating to their " origin could poffibly be drawn: and that, fo far from *pretending* to explain or refute the " experiments of *Nuck*, *Cowper*, *Lifter*, &c. in proof of lymphatic arteries, he never fo " much as *mentioned* them. Confequently Dr. Hunter's pretenfions to even the fmalleft " fhare of what he is pleafed to call," (which by the bye I never did) " *the important difco-* " *very, that the valvular lymphatic veffels are a fyftem of abforbent veins*, are evidently founded " on a *declaration* or conclufion contradicted by premifes. And this conclufion, fo far from " meriting praife, can only be faid not to deferve cenfure, on the fuppofition, that he was " ignorant of what had been done on the fubject."

" in nothing to differ from each other, but in the nature of the fluids " they convey, &c."

Mr. Cleghorne of Dublin, reader of anatomy, who attended my lectures in the beginning of the year 1751, fends me the following extract from his notes.

" The Lymphatic Veins.

" Wednefday, Jan. 23. The origin commonly affigned to the lym- " phatics feems to be falfe.----Small veins that arife from correfponding " arteries have no valves, the lymphatics numerous valves, (though not " at fuch regular diftances as authors paint them). Hence, it is probable " they are made up of the fmall abforbent veins that come from the " different parts of the body; and therefore have the fame occafion for " valves as the lacteals, &c.----

" N. B. The lymphatics demonftrated in a calf's milt, by cutting the " external coat and throwing in air.

" Thurfday March 7. The lymphatics appear in livers long kept, " being diftended with air generated by putrefaction."

Mr. Davies of King-ftreet, Covent-garden, furgeon, fays, " The " following is a literal extract of the notes I made when I had the plea- " fure of attending your courfe of lectures January 1750-1; fpeaking " of the lymphatics.-----According to Mr. Hunter, their origin more " probably from all the cavities and interftices of the parts of the bo- " dy.-----From the common method of raifing them in the fpleen and " *teftes*-----from analogy with the lacteals.-----Hence Mr. Hunter will " have them to be the abforbents.

" As this was the firft courfe of anatomy that I had attended, I was " not able to write my notes fo fully; in the next courfe (to wit, in Oc- " tober 1751) I find that, upon the fubject of lymphatics, I have been " a little more particular.----My notes begin thus: There are two kinds " of lymphatic veins: the one kind, are thofe in which the lympha- " tic arteries terminate, and whofe contents go with the blood, as thofe of " other veins, into the heart; the other kind are the lymphatics of Bartho- " line, which are, &c. After the defcription of them in my notes, comes " this paragraph: Mr. Hunter's own private opinion, that thefe lymphatics " of Bartholine are nothing but the true abforbents of the body; and that " they have their origin from all the cavities, furfaces, and interftices " of the parts of the body: His reafons are, 1. the common method " of raifing them in the fpleen and *teftes* with a blow-pipe, which " could never raife the other lymphatics.-----2. Thefe lymphatics of

" Bartholine

" Bartholine have valves in the *vifcera*; whereas, no other veins in the " *vifcera* have valves.-----3. Thefe lymphatics are never injected from " the arteries.-----4. The analogy they bear to the lacteals, which " have alfo valves.----5. Thefe veffels end for the moft part, (if not " all of them) in the thoracic duct or route of the chyle; whereas, the " other lymphatics end in the blood veffels.

" After your courfe of October 1751, I attended feveral fucceeding " courfes, but was fo much engaged as houfe-pupil at the Lock-hofpital, " that I had not time to write; but, I remember well your explaining " the venereal *bubo*, upon the principle of abforption by the lympha- " tics, and your afking me one night after lecture, if I did not find " your doctrine confirmed by the different feats of the *bubo*, in the pa- " tients of that hofpital."

Mr. Galhie of Spittle-fquare, fays, " As I attended your lectures fo " early as the year 1751, I fhall, agreeable to your defire, tranfcribe from " my notes, what you then declared as your opinion, with regard to the " origin and ufe of the lymphatic veins. That they were not continua- " tions of the arteries, as generally believed, but abforbing veffels, be- " ginning imperceptibly from the different cells and furfaces of the bo- " do, you was fully perfuaded, 1. Becaufe you could not inject them " as other veins, by filling the arterial fyftem. 2. In injecting, you " fometimes obferved, that they were fuddenly filled when the arteries " burft, and the injection was effufed into the cellular membrane; a cir- " cumftance which you imagined fufficiently explained, and refuted " thofe anatomifts who pretended to have filled the lymphatics directly " from the arteries or veins: 3. As a farther confirmation of this doc- " trine, you mentioned the valvular ftructure of the lymphatic veffels, not " obfervable in other veins of the *vifcera*; the great likenefs of the lym- " phatics to the lacteals in their coats, valves, courfe, and terminations; " and laftly, the abforption and progrefs of the venereal poifon, and ino- " culated matter of the fmall pox."

Dr. Abernethie of Edinburgh firft attended my lectures in the Spring, 1752. The extract which he has fent me from his notes runs thus: " Lymphatics (fays the MS.) are of two kinds; one, which carries the " thinner part of the blood back from the arteries, and fo are continu- " ations of the lymphatic arteries; the other abforbs the lymph, which " is feparated in all the different parts of the body for the lubrication " of the parts, and conveys it into the conftitution, or vafcular fyftem, " through the *Receptaculum Chyli* and thoracic duct, or into the liver,

" and

" and fo are properly the abforbents of the body. In the abforbents " are many valves irregularly placed, and at different diftances; and in " all refpects they are like the lacteals, thin tranfparent veffels. The " abforbents do not arife as the firft kind of lymphatics juft now men- " tioned, but from the interftices of the fibres in all parts of the " body *."

I fhall add the teftimony of Dr. Hadley, phyfician to St. Thomas's hofpital. The paragraph upon the lymphatics in his manufcript is this. " Lect. 4th. Jan. 31, 1754. The lymphatics are fo fmall, and full of a " liquor fo colourlefs, that their beginnings are difficult to be found out. " They are moft vifible in the fpleen of a calf.

" There are two forts of lymphatic veins, the one merely the conti-

* That ftudents of anatomy were conftantly going from London to Edinburgh, and from Edinburgh to London, and giving accounts of their ftudies, and of what was paffing at one place to their fellow-ftudents at the other.——That it has been a cuftom with the more ftudious to write notes of the anatomical lectures, and to lend their manufcripts to be perufed or tranfcribed by others, &c. are things fo notorious, that I need not prove them.——That the profeffor's fon, who was educated with a view of being himfelf a profeffor of anatomy, who was to be fent to London to do me the honour of attending my lectures; and was intimate with many who had attended them, and with fome who had manufcripts of them, which they lent out among the ftudents at Edinburgh;—I fay to fuppofe that in thefe circumftances, he had not the curiofity or opportunity to know any doctrines or improvements that were believed to be peculiar to me, would be a *very good natured* fuppofition in the prefent cafe. Upon enquiry I found, that manufcripts of my lectures were very common among the ftudents at Edinburgh, about the time that Profeffor Monro pretends to have made his difcoveries: I was told of two in particular, one written by Dr. Alexander Bruce, who had attended feveral courfes with me, and who by thefe means had got it pretty complete; the other was Dr. Abernethie's. Dr. Bruce was gone to Barbadoes, fo that I had no opportunity of learning what it contained, or to whom he lent it at Edinburgh. I wrote to Dr. Abernethie upon this fubject, and in anfwer he fent me the extract upon the lymphatics as above; and with great candour told me he lent his manufcript at Edinburgh to Mr. Mackbane, for whom part, if not the whole of it, was tranfcribed by his father, in 1755; and, if he was not miftaken, he lent it to Mr. Greenhill in the winter 1753-4; and he has fome notion that Dr. Palmer or Dr. Amory had the ufe of it for a little while the fame year. So that as it is plain there was a very good oppportunity, I think the young profeffor muft for his own credit plead fome unaccountable *incapacity*. His brother fays *(a)*, " I " diffected five years for my father, from 1745 to 1750, and had in the winter conftantly " numbers of the pupils about me, and was intimate with many of them, efpecially of the " Englifh young gentlemen, yet while I remained at Edinburgh, *I never could know* one " thing Dr. Hunter was doing."

(a) Critical Review for Dec. 1757, page 525.

"nuations of the lymphatic arteries, and of courſe carrying the ſame "fluid contained in thoſe arteries: the other ſort terminate only in the "*Receptaculum Chyli* and thoracic duct. But theſe laſt ſeem only abſor-"bent veins---they are called Bartholine's lymphatics--- they will run "a great length without an *Anaſtamoſis*---they are traceable from the "teſticle of a dog to the thoracic duct---they are very full of valves.

"Mr. Hunter's conjecture of them is, that they are the abſorbent veſſels. "In the *Abdomen* of a dog particularly, and in an hydropical leg, their effects "are very viſible. Their valves ſeem to argue likewiſe for this; for, conſi-"dering the veſſel as a vein, the valves ſeem uſeleſs, but as an *abſorbent vein*, "(i. e. granting that they do not riſe from the arteries) abſolutely neceſſary, "as there is no *impetus* from the arteries to drive the fluid forwards.---The "lacteals begin in the cavity of the inteſtines, and run to the receptacle "of the chyle. Theſe are abſorbent veſſels: when dry they are not "diſtinguiſhable from the lymphatic veins.---In the progreſs of the ve-"nereal poiſon, whenever it gets into the conſtitution, it appears firſt in "ſome of the lymphatics, ſituated near the place where the infection "is received; if (e. g.) it is received at the *Penis*, it will appear in "the glands of the groin, if, by any accident, in the hand, the *Bubo* is "formed in the *axilla*."

If there be any reader, who is not ſatisfied with theſe teſtimonies, I muſt ſuppoſe, either that he does not underſtand the ſubject in diſpute, or that he is reſolved not to be ſatisfied. I will therefore conclude this chapter, and leave Dr. Monro in the full poſſeſſion of all his reaſoning.

CHAP. III.

The HISTORY of the DISPUTE.

IN the two preceding chapters, the reader has been informed what Dr. Hunter had done on the ſubject of the *Teſtis* and lymphatics; previouſly, not only to Dr. Monro's publications, but to the times when he himſelf ſays he firſt made the diſcoveries. Let us now ſee when, and upon what occaſion, Dr. Monro publiſhed theſe diſcoveries, the riſe and progreſs of the preſent diſpute, that the reader may be enabled to paſs his judgment according to the evidence of facts.

Dr.

Dr. Monro publifhed his account of the injection of the *tubuli teftis* in the Edinburgh *Effays and Obfervations*, in 1754. Dr. Hunter took no other notice of this than by declaring to his pupils, that he had fhewn thefe *tubuli* injected at his lectures, before Dr. Monro pretended to the difcovery.

In the fummer, 1755, Dr. Monro publifhed a *Thefis*, (*De Teftibus in var. animal.*) in which that difcovery was improved and extended. In the 12th chapter of this *thefis*, after fome obfervations made upon the lymphatics of the *teftis*, he fays *: "I have explained thefe experiments at greater length, " as they firft incited me to try others on the lymphatic veffels in gene-" ral; and, as I have found that thefe could not only be filled from the " excretory ducts of the glands, but likewife in a manner not hitherto " remarked by authors, viz. by an effufion of fluids into the cellular " membranes and cavities of the body, of which I have already given " feveral examples; and that, without an effufion into the cellular mem-" branes, they never, in my experiments, did admit liquors injected " into the blood-veffels to enter them: thefe, among other things, fur-" nifhed me with arguments of no fmall weight to prove, *That the val-" vular lymphatic veffels, through the whole body, were a fyftem of abfor-" bent veins; and that they did not proceed from the branches of arteries, " as is the common opinion.* But at prefent to propofe all that might be " difputed upon this fubject, would far exceed the bounds of fuch a dif-" fertation; and it will be much fitter to treat of them apart, viz. of " their origin, fabric, manner of acting and ufe, when my time fhall " better permit."

Dr. Donald Monro foon after prefented me with this *thefis*, and the author's compliments, telling me he was coming up to London to attend my lectures. I looked it over at my leifure, and muft own the above-mentioned paffage ftruck me, and gave me fome fufpicion that he was going to treat me unfairly about the lymphatics. However, he was the fon of my old mafter, he bore the character of an ingenious young gentleman, he was appointed conjunct profeffor of anatomy at Edinburgh, and his coming to ftudy with me was furely a particular honour conferred upon me; I therefore wifhed from my heart to procure his friendfhip, and thought it my duty to receive him kindly, and with refpect. It was very natural to wifh all this upon my own account, as it muft be agreeable to any man who reads anatomy in London, to ftand well in

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* I have given his own tranflation of the paragraph from the 36th page of his pamphlet.

the opinion of a profeſſor of anatomy in a ſchool of phyſick, which does ſo much honour to this country. Beſides, I thought that if a youthful eagerneſs for reputation, flattered by paternal partiality, had miſled him in any point with regard to me, I ſhould be more likely to ſet him right, by a friendly and open behaviour.

As I had conceived ſuch hopes, the reader may imagine what kind of reception I gave him. I told him, that Profeſſor Monro's ſon had a right to command any ſervice I could do him in the proſecution of his ſtudies; and it gave me ſome pleaſure to think, that I might be of ſome little uſe to him. Sir, ſaid I, you mean to devote yourſelf to anatomy, and to teach it. You will therefore wiſh for every poſſible advantage. In London we have commonly a greater plenty of ſubjects than at Edinburgh, and for that reaſon perhaps have made ſome progreſs in the practical part of anatomy; particularly in the arts of making preparations *. In the diſſecting room you will find a great deal of that ſort of work going on through the whole winter, under my brother's direction. If you can make any uſe of us, you will do us a pleaſure.

After my lecture, I often ſpoke to him in a familiar way upon anatomical points, and never once ſaid any thing at my lecture, that I thought he could complain of. I there related how, and at what time, I had ſhewn the *tubuli teſtis*, mentioned his having done the ſame thing in a variety of animals; and when I was obliged to ſpeak of the tube, which he ſays he diſcovered riſing from the *epididymis*, I did not name him, becauſe I could not acknowledge it to be a diſcovery; and treated the matter tenderly, becauſe I believed it to be a miſtake. But, however that might be, when I was claiming my juſt right in his preſence, I did not pre-

* Some readers may perhaps think this was a very free ſpeech. It was free, but, I hope, not rude. Profeſſor Monro ſen. has acquired, and undoubtedly has deſerved, great reputation in anatomy; but, in every body's opinion, he might have been a much better anatomiſt, if he had had better opportunities; if he could have been better ſupplied with dead bodies, and had been leſs interrupted by his private practice. Tho' he had written upon injections and preparations, when I attended him, he had almoſt none. In his lectures, his cuſtom was to undervalue preparations (if I underſtood him right) and to inſinuate that they were of no uſe; and indeed the very few he had were well adapted to ſupport the opinion. In London Mr. St. Andre, Dr. Sandys, Dr. Nicholls, and ſome others had improved this branch of anatomy. Their methods were made public, and explained fully (with any little improvement that occurred to me) at a lecture, which it has been my cuſtom to ſet apart for that purpoſe; and now, if I am not miſinformed, even Profeſſor Monro ſen. ſpeaks of preparations with temper and approbation, and his ſon has been much employed in making them.

pretend to more, and therefore ingenuoufly confeffed, that I knew nothing of that duct.

When I treated of the lymphatics in his prefence at lecture, I was pretty full upon the fubject of their being abforbents *, and faid I had taught this doctrine, and fupported it by fuch arguments as I then made ufe of, from the firft of my reading lectures. I faid fo, that I might put him upon his guard. And then I did him the juftice to add, that I found this doctrine laid down or advanced in a general way, in his Inaugural Differtation †. *I was not afraid of truth, and therefore did not fuppress what he had done.*

After all this, he printed his treatife on the lymphatics at Berlin, 1757, and his brother foon after prefented me with a copy, in the name of the author. I read it over, and was aftonifhed to find there the hypothefis I had advanced, fupported by the very arguments which I had ufed for the fame purpofe ‡, with the addition of an introduction, in which he quotes above twenty of the lateft writers, to fhew that the opinion was new. I fay I was aftonifhed, that he could do this without once even mentioning my name.

The reader would be aftonifhed too, if he knew the circumftances minutely, many of which I am under a neceffity of fuppreffing. I will take the liberty of mentioning only one little piece of private hiftory, which

* This Dr. Monro allows in his 35th page.

* This he allows in his 37th page.

‡ He made fome of the experiments indeed with quickfilver, which I had made with air, and with common injection; but the experiment was ftill the fame, and we both drew the fame conclufion from it. I fpeak now of air, injection, or quickfilver, getting into the lymphatics from the cellular membrane. In the argument taken from poifon, I mentioned indeed only the venereal and the variolous, but meant that it might be applied to all poifons which are abforbed; as is plain from the opinion given in Dr. Macaulay's cafe, and mentioned in the laft chapter.

I may take this opportunity of explaining upon the fame principle, a fymptom in venereal cafes, which puzzled me for a long time, and about which I think the learned Dr. Aftruc himfelf has not given much fatisfaction. When the *penis* is affected, efpecially when there are fores in that part, we frequently obferve a hard chord, like a piece of cat-gut, under the fkin, running along that member. Sometimes there is one on each fide. At firft it is only to be felt at the extremity of the *penis* coming from the affected part, but it foon extends itfelf to the root of that organ, and frequently may be traced in the fat of the *pubes* ftretching acrofs towards the groin. From its courfe, it is plain, that it cannot be a nerve, artery, or vein. I have for fome time been well convinced, that it is a lymphatic veffel of the part, indurated by the poifon which it conveys: but I have not yet had an opportunity of being *certain* of the fact by diffection.

which I prefume will be fatisfactory. I knew that Dr. Black, profeffor of medicine at Glafgow, who gave the letter in favour of Dr. Monro *, had ftudied at Glafgow before he went to Edinburgh, where I imagined he muft probably have known fomething of my opinion about the lymphatics before he became acquainted with Dr. Alexander Monro jun. I made fome enquiry, and was informed my conjecture was well founded: then I wrote a letter upon the fubject to Dr. Black, in which I propofed the three following queftions; firft, if he had feen a MS of my lectures at Glafgow or Edinburgh; fecondly, if he knew it was an opinion of mine that the lymphatics were the fyftem of abforbents, previoufly to his feeing Dr. Monro's MS on that fubject; and then, thirdly, if he had faid any thing of this to Dr. Monro when he faw his manufcript. In anfwer to this letter I received the following, which I fhall give at full length.

"DEAR SIR,

"The difpute between you and Dr. Monro has given me a great deal "of concern, and I have often wifhed that my endeavours to prevent it "had been fuccefsful. But fince I was not fo happy as to effect this, and "that I am called upon as a witnefs, the only tafk left me is to do im-"partial juftice to both fides, by attefting thofe facts of which I have "any knowledge, and concerning which my teftimony is demanded. "This I have already done with refpect to Dr. Monro; and fhall now "likewife anfwer thofe queftions which you have been pleafed to put "to me.

"In anfwer to the firft queftion I muft affure you, that I never faw, "nor ever had any knowledge of any manufcripts of your lectures.----- "But I muft declare, in anfwer to the fecond, that I knew it was an "opinion of yours, before I went to ftudy in Edinburgh, that the lym-"phatics were a fyftem of abforbents, and therefore quite diftinct in "their nature and office from the veffels which belong to the fyftem of "the heart. This I learned from Dr. Cullen †; and the argument "mentioned to me, according to the beft of my remembrance, was, "that in making injections, you had obferved that the lymphatics were "commonly filled when the injected matter was extravafated, and not "otherwife; and, if I am not miftaken, he likewife told me, that, agree-"able

* Dr. Monro's pamphlet, page 27.

† At that time profeffor of medicine at Glafgow.

"able to this opinion, you filled the lymphatics of the teſticle with air "by bruiſing its ſubſtance a little, and blowing air into it thro' a hole "in the *Tunica Albuginea*.

"The converſation which paſſed between Dr. Monro and me when "he ſhewed me the manuſcript for his *theſis*, was, according to the "beſt of my remembrance, to this purpoſe. As ſoon as I had read it, "I told him that he muſt ſtrike out intirely the Diſſertation upon the "Lymphatics, becauſe the opinion he there propoſed and ſupported, had "been entertained by you a very long time. I even ventured to tell him, "I could not help ſuſpecting his having got a hint of it, ſome time or "other, from me. He ſeemed ſurpriſed and diſpleaſed, and aſſerted, "that it occurred to him in conſequence of ſome *phænomena* in his ex- "periments, as related in the Diſſertation; that he owed it to no perſon "whatever; and that he was reſolved to publiſh it immediately. I in- "ſiſted that, at any rate, if he did publiſh it then, it would be abſo- "lutely neceſſary for him to mention Dr. Hunter as having been of the "ſame opinion before him, both becauſe I thought politeneſs and can- "dor required ſuch a confeſſion, and becauſe he might expoſe himſelf "to very diſagreeable ſuſpicions by acting otherwiſe; but adviſed him to "delay the publication of it until he had frankly converſed with Dr. "Hunter himſelf, whoſe courſe he propoſed to attend the following "winter, not doubting but that, by ſuch a converſation, all cauſe of "ſhyneſs and diſpute would have been prevented one way or other.

"This, Sir, to the beſt of my remembrance, is the ſubſtance of what "paſſed between us upon this affair, the conſequences of which have "ſince given me a great deal of uneaſineſs.

"I am, my dear Sir, with the greateſt eſteem,

Your moſt humble ſervant,

Glaſgow,
July 1, 1760.

JOSEPH BLACK."

When I ſhewed this letter among my friends, ſome of them ſeemed to think, that, in ſtrict juſtice, Dr. Black ought to have mentioned ſome of its contents in the letter which he gave as a teſtimony for Dr. Monro. I own his conduct did not require any apology with me; I was well convinced of his integrity, and approved of his benevolent tenderneſs. However, I wrote to him again upon the ſubject, and was favoured with his anſwer, which I think it my duty to lay before the reader.

"DEAR

" DEAR SIR,

" I received your letter of the 10th of this month, and cannot oppofe " your intention of printing my former. I am obliged to you for the " tendernefs and delicacy with which you exprefs your concern, left the " world fhould find any difficulty in accounting for my conduct in this " affair. I confefs it has been improper. When I wrote my letter to Dr. " Monro, I ought, no doubt, to have alfo declared thofe circumftances " which feem to favour your fide of the queftion: nor do I pretend to " offer a fufficient apology; but you muft give me leave to tell you, " how I was induced to act as I did. And if I difclofe fentiments which " may not perhaps agree altogether with yours, you muft forgive the " freedom I take, and confider that it is requifite for me to give my " motives for what I did.

" I muft own therefore that I was under no neceffity, but moved by " compaffion for a friend, who had taken a ftep which I no doubt thought " exceffively wrong, but who was threatened with the moft afflicting " and infupportable of misfortunes, the lofs of his character and repu- " tation, and who, after all, might poffibly fuffer this lofs undefervedly " too, through my being forward in publifhing what you have lately got " from me. For tho' I could not help fufpecting, from the circumftance " of our frequently converfing together when I was in Edinburgh, that, " tho' he might afterwards forget it, he might have had the firft idea of " that opinion concerning the lymphatics from me, yet I had no reafon to " be fatisfied of this point. And that he could pick up every particular " of your arguments and experiments in order to publifh them as his " own, is what I could not believe. I knew him to be acute, induftri- " ous, and keen in the purfuit of knowledge; and believed him very " capable of inventing the feveral arguments and experiments which he " has publifhed in his *Thefis* and Differtation, but could not conceive him " to be capable of the other: and I muft obferve, that before I faw Dr. " Monro's papers, tho' I had a general notion of your doctrine upon the " lymphatics, I was not mafter of many arguments in fupport of it;--- " becaufe no doubt I had not attended fufficiently to the fubject, or had " forgot a part of what I had heard, or had not heard the whole: but " this I am fure of, that I read this part of his papers with particular " pleafure, as finding that doctrine rendered much more probable and " interefting than I had conceived it before; for he did not communi-

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" cate his thoughts upon the ſubject to me until he had put them toge-
" ther in writing.

" When I therefore conſidered what he muſt ſuffer, ſhould I add pro-
" bability to the accuſations with which he was charged, and conſidered
" at the ſame time, that I had no reaſon to be ſatisfied that he deſerved
" ſuch diſtreſs, I was perſuaded to give at that time ſuch part of my
" evidence only, as was perfectly direct and concluſive; and to reſerve
" the reſt until you laid me under the neceſſity of declaring the whole.

" You deſire to know in what particular manner Dr. Monro commu-
" nicated to me the method of raiſing the lymphatics by blowing air
" into the glands. So far as I remember, when he ſaid that he could
" raiſe the lymphatics in this way, he told it me as a piece of anatomical
" news, or as a curious anatomical fact, which I imagined was a diſco-
" very of his own, as I had not conſulted Nuck or Cowper upon the
" ſubject; but I cannot ſay, that he either mentioned Nuck or Cowper,
" or ſaid that it was his own.

" I hope you will forgive the freedom I have taken in this letter, and
" believe me to be with the greateſt reſpect,

Your ſincere friend and humble ſervant,

Glaſgow,
Feb. 26, 1761. JOSEPH BLACK."

Theſe letters are ſo ſtrong and clear, that they need no comment.

Upon this friendly admonition, Dr. Monro in the mean time ſuppreſſed the diſſertation, and in the following winter came to London to attend my lectures. His father, Profeſſor Monro ſen. aſſures the public *, that, " he went to London in abſolute ignorance of Doctor Hunter's " having any particular opinion concerning lymphatics------attended his " lectures, and was ſurpriſed when he heard Dr. Hunter teach the doc- " trine of lymphatics being abſorbents." Mercy upon us! Did Profeſſor Monro ſen. really know how things were, and make this public declaration? He ſays, in the ſame paper, " I thought it my duty, as a friend " who knows the facts relating to the preſent diſpute, to ſend you a " fair ſtate of them."

If ſo, I cannot with any decency ſuppoſe but that the ſon muſt have *concealed from his father*, that he knew my opinion of the lymphatics;

E that

* Critical Review for November, 1757, page 432. See the Appendix. No. II. The young profeſſor tells us (pag. 2.) that this was written by his father.

that the father was not trusted with Dr. Black's remonstrance; and that the son informed him from London of his being surprised to hear me teach that doctrine. This supposition excuses the father, but how does it affect the son? If he was fully satisfied that the discovery was honestly his own, as soon as he found that it might, and probably would be disputed with him, it is almost impossible to imagine he should not have consulted his father in so ticklish a situation; that, on one hand, he might not lose his just right, nor, on the other, be suspected of a low plagiarism, which to a generous mind would be still more insufferable *.

I shall now proceed in the narrative. He came to London, attended my lectures, never spoke to me upon the subject, went next winter to Berlin, and there published his dissertation; and though he knew from my own mouth, in presence of a great number of students, that I had taught the same doctrine, and supported it by the *very same* arguments several years before, he did not mention my name, not even in a marginal note.

What made Dr. Reimarus join my name to Dr. Monro's, when he spoke of this doctrine in his *thesis?* Was it not because *it was but just*, because *his conscience did not accuse him?*------What made Dr. Monro avoid it?

The authors of the Critical Review gave an account of Professor Monro's dissertation in the Review for September, 1757, and did me justice upon the occasion. In the Review for November, 1757, they published an anonymous letter, afterwards acknowledged to be from Professor Monro sen. in justification of Professor Monro jun. and a short state of the case from me. Lastly, in their Review for December, 1757, they published a letter from Dr. Donald Monro, followed by remarks which he had given me an opportunity of making; and, at this precise time, the young professor passed through London in his return from Berlin to Edinburgh. Let us here try to guess at the state of his mind, by his behaviour on this occasion.

Had he received no civilities from me, but, on the contrary, insults on

* If it were not to break in upon the reader's reflections, I would now desire him to consider the account that Dr. Monro has given of his thesis, (pag. 25.) and to allow it all the weight and credit which he thinks it deserves: and to consider Dr. Monro's history of his proceedings upon this subject, (pag. 23, 24, and 25.) and believe, if he can, that Dr. Monro would have said nothing of them to Dr. Black, till they were drawn up for publication, if he had been easy in his mind. Young men are not so slow in talking with their friends of their pursuits and discoveries. But if he felt any forebodings that Dr. Black might be unpropitious, it was no wonder he kept him ignorant as long as he could.

on himſelf and his friends, as he pretends *, and had I behaved in his abſence ſo as to deſerve the load of infamy and reproach which he preſently after threw upon me in an abuſive pamphlet; ſurely it would have been indecent meanneſs of ſpirit to ſhew me any reſpect in paſſing through London. Yet he tells us †, "he called at my houſe, in company with his brother, and, not finding me at home, left his name with my ſervant, and deſired him to tell me, that *he would have called more than once*, but that he was only paſſing through London in his way to Scotland, and was to ſet out next morning."

Did he call upon me in civility, and leave ſuch a reſpectful meſſage after ſuch my behaviour to him; and after he had been in London ſome days, when he muſt have been informed of all that had paſſed in his abſence; and the evening before he ſet out for Edinburgh, where he ſoon after wrote the furious pamphlet againſt me? No body will believe it. But when I have told the reader one material circumſtance about that viſit, he will begin to conceive the intention. He called upon me indeed at my houſe in Jermyn-ſtreet, but it was *when I was reading my lecture at Covent Garden*; that is, *when he was certain I was not at home*, for he knew my hours of reading, and he knew that nothing but the moſt urgent buſineſs could prevent my being at lecture at the uſual hour; an accident which he knew would as certainly prevent my ſeeing him, if I ſhould not be at lecture ‡.

Now if the reader believes that I had been really civil to Dr. Monro, when he attended my lectures, he will ſee a reaſon for his thinking it right to ſhew me this reſpect: in the next place, if he believes that Dr. Monro had not properly returned my civilities, and had acted diſingenuouſly

* Page 17. † Page 3.

‡ In this diſpute, I appeal to facts, and therefore ſhall here annex the affidavit of my ſervant.

"I, James Duncan, do voluntarily make oath, that ſome time laſt winter, (I believe it was on the 13th of December,) Dr. Alexander Monro jun. in company with his brother, called at Dr. Hunter's (my maſter's) houſe in Jermyn-ſtreet, while he was at his lectures in Covent Garden, at the uſual hours. Dr. Alexander Monro aſked if Dr. Hunter was at home: I anſwered no; and he left his name, and deſired me to tell Dr. Hunter he had called. This was all he ſaid to me. He did not ſay, that he would have called more than once, nor that he was only paſſing through London, nor that he was to ſet out next morning. When my maſter came home, I told him, and he ſaid that Dr. Monro could not ſuppoſe he was at home at that hour. JAMES DUNCAN."

Middleſex, to wit, ſworn before me this 26th day of October, 1758. J. FIELDING.

nuouſly in his publication, he will ſee a reaſon for his chuſing a time, when he was ſure not to find me. In the laſt place, the reader will not perhaps err greatly, if he ſuppoſes that Dr. Monro, returning to Edinburgh, found that he would infallibly ſuffer in his reputation, if he did not write a bold defence of himſelf, and make the world believe, that Dr. Hunter had not one grain, either of knowledge, candour, or veracity. This ſuppoſition is the leſs improbable, as every man who has ſtudied at Edinburgh for theſe thirty years laſt paſt, muſt know that he would naturally fall into the hands of a ſtrenuous adviſer, who has been long practiſed in exhibiting the characters of anatomiſts.

C H A P. IV.

REMARKS upon ſome EXTRAORDINARY PARAGRAPHS in Dr. Monro's Pamphlet.

IN the three foregoing chapters I have given the authenticated facts relating to the principal points in queſtion, together with the riſe and progreſs of the controverſy. In theſe, ſome reflections and inferences have been made; but deciſion and judgment was left to the intelligent and impartial reader. It might therefore be expected that I ſhould now have done with the ſubject. I certainly ſhould, if the Doctor had kept to the points in queſtion between us. My firſt and only argument was, that having been prior to him in both the improvements or diſcoveries, I had a right to be mentioned, in a marginal note at leaſt, in his treatiſe of the lymphatics, as having been of the ſame opinion. This neglect was the charge that was brought againſt him. But, inſtead of a plain and candid anſwer, his *defence* turns out, almoſt in every page, a violent *accuſation* of me; and he has introduced ſeveral things foreign to the diſpute, as if he wrote, not to juſtify himſelf, and to get the better of the argument, but to deſtroy the reputation of his antagoniſt. It is therefore incumbent upon me, after having furniſhed the reader with materials for judging between us in the original diſpute, to conſider ſome of theſe injurious reflections, leſt they ſhould be ſuppoſed unanſwerable. I ſhall take them in order, as they occur.

§ I. In his introduction, pag. 2, 3, and 4, he complains very much of a poſtſcript which I had written, and pretends to think it *highly probable, my conſcience muſt have rejected what my pen there affirmed, &c. &c.* The

fact

fact was this: Dr. Donald Monro wrote a letter (See App. No. IV.) to the authors of the Critical Review, in defence of his brother, December 7, 1757, which they ſent to me on the next day, and I immediately wrote ſome remarks upon it. In two days after this, Dr. Alexander Monro came to town in his return from Berlin. I then imagined that ſome alterations might be made, or that the paper might be recalled, in conſequence of the two brothers having conſidered the affair together; I gave notice of this to the authors of the Review, and begged the favour of them to defer, as long as they could, printing his letter, and my remarks. On the 22d day of the ſame month, that is, eight days after Dr. Alexander left London, I was informed by the authors of the Review, that they had heard no more from Dr. Donald Monro, and that they could no longer defer the printing. I then dated my remarks; and, as Dr. Donald's letter was concluded with obſerving, that it was *doing juſtice to a brother who was not preſent to anſwer for himſelf*, I put down this poſtſcript, viz. *Dr. Alexander Monro jun. 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.* Was not the fact true, and the inference both natural and fair? Muſt not we believe that his brother told him what he had juſt done; and, if he did not approve, why did he not ſtop the proceeding? Why ſhould my *conſcience reject what my pen here affirmed?* Why of all things ſhould it be conſtrued *calling Dr. Donald's veracity in queſtion?* But, as I would leave as much as poſſible to the reader's determination, I will only beg the favour of him now to read what the Doctor has ſaid upon this ſubject in his firſt four pages. He will then ſee what I was to expect from ſuch a ſpirit of miſapprehenſion or miſrepreſentation.

§ II. I muſt next give a large quotation of four paragraphs, which are of a very ſingular nature, and ſeem to deſerve the reader's attention. He ſays, pag. 17.

1. " The Doctor has thought proper to mention the reſpect and civi-
" lities he ſhewed me in London, with the appearance indeed of compli-
" ment; which he has, however, ſo miſplaced, that ſome think it ra-
" ther implies a reproach of my ingratitude *.

" Now, as I am not conſcious of deſerving this reproach, I ſhall ex-
" plain to the reader the real ſenſe of theſe civilities, by an example re-
" lating to the preſent ſubject; and aſſure him, that I could produce ſe-
" veral

" * Critical Review, pag. 438.

" veral others ſuch like, if not to myſelf, to thoſe at leaſt in whom I
" muſt think myſelf intereſted.

2. " 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 *epididymis*,
" and affirmed that they were ſeen frequently*; but that, for his part,
" he had made a conſiderable number of experiments, and that he 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 deſcrib-
" ed; but which, with his never citing Dr. Haller, plainly ſhewed at
" whom he levelled. Moſt unluckily, however, for the Doctor, when he
" handed about his preparation, I evidently ſaw in it one of theſe very veſ-
" ſels, 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.

3. " 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 obſerva-
" tions, and circumſpection in drawing concluſions.

4. " 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."

In the laſt chapter I have ſaid with what a diſpoſition I received Dr. Monro, and how I *meant* to behave to him at the lectures. If the reader believes what I ſaid, he will eaſily believe what I muſt have felt in reading theſe four paragraphs. I ſhall examine them in order.

1. The only remark which I ſhall make upon the firſt is, that I am ſorry I cannot altogether clear him from what he calls the implied reproach. But if he thought it any breach of the reſpect due from me to him, or to his father, that I differed from both of thêm in ſome anatomical opinions,

" * I before mentioned my having firſt painted ſuch a veſſel in the Philoſophical Eſſays of
" Edinburgh, Vol. I. I had afterwards given three or four figures of it from different ſub-
" jects, in my Inaugural Diſſertation."

ons, I grant that he muſt have met with offence. But why was he ſo unreaſonable? Upon all theſe occaſions I *meant* to be as tender as poſſible; and never then named either of them, that the point itſelf might be conſidered, and not the author of the opinion.

2. In the ſecond paragraph he gives an inſtance of an unmannerly inſult received from me at my lecture. I aſſure him, and the reader, upon my honour, that I meant nothing leſs. Thoſe who know me will not, I hope, readily believe that I was very unmannerly before a numerous aſſembly to a perſon who behaved like a gentleman, who was known by the company to be the ſon of my old maſter, and who, by the rules of the place, was not at liberty to ſpeak in his own defence. I do not pretend to ſay that Dr. Monro did not feel what he expreſſes. Where there is a ſore, there will be ſome tenderneſs: but I declare again, that I did not mean to hurt him. I was therefore ſhocked at the imputation; and aſked a number of gentlemen, for the ſatisfaction of my own mind, if they could recollect, whether what I then ſaid had appeared to them as it ſeems it did to Dr. Monro. All of them aſſured me that there was no appearance of diſreſpect to the author of the opinion, that is, to Dr. Monro. Very fortunately, there happens to be the moſt unqueſtionable evidence in my favour, that the nature of the thing could admit of. It is the teſtimony of a gentleman of undoubted underſtanding and honour, who was preſent; and who, from ſome circumſtances which I ſhall preſently relate, muſt have been without prejudice when his judgment was formed, and who could not poſſibly forget what that judgment was. I ſpeak of Dr. Warren, phyſician to St. George's hoſpital. The account which he gave me, when aſked was this: he happened not to know who the anatomiſt was that had deſcribed the duct in queſtion; and on the next day, while my expreſſions and manner were yet freſh in his memory, he was aſked by a friend to Dr. Monro, whether he did not think what I then ſaid was in a ſlighting way. He gave that gentleman his opinion freely, but in the negative; and ſays now, that he is ſure his judgment was unprejudiced, becauſe he did not know at that time who was meant; and is ſure that he has not forgot, becauſe the next day's converſation with Dr. Monro's friend made a deep impreſſion upon his memory. I preſume the reader will here make a reflection, or two; and therefore I will not diſturb him with any of mine.

I come next to conſider the laſt part of the ſecond paragraph, viz. "Moſt unluckily, however, for the Doctor," &c. Some, I preſume, will

will not think it very probable that Dr. Monro, with one glance of his eye, ſhould plainly ſee in my preparation, when incloſed in a bottle, what neither I, nor my brother, who had injected, diſſected, ſtudied, and handled the part, knew any thing of. Dr. Monro did indeed mention his opinion about that preparation to my brother; but in a very proper manner, and received, in a very decent way, my brother's anſwer, viz. *That it was not a duct going from the epididymis, but a little proceſs or projecting part of the epididymis itſelf.* That it was ſo, I am certain from ſeveral circumſtances, and that ſuch a proceſs is no very extraordinary thing, Dr. Monro will probably know when he has had ſome more experience. But that I might anſwer Dr. Monro with facts, rather than opinions, my brother took the preparation out of the bottle before proper witneſſes, examined the part by diſſection, and gave me the following account: " I " took the preparation in queſtion out of its bottle, in preſence of Mr. " Jones and Mr. Blount, October 9, 1758, and carefully examined by " diſſection the little appendage of the *epididymis*, which Dr. Monro " had ſpoke of to me. I found it to be (what I always took it for) a lit- " tle part of the *epididymis* projecting beyond the reſt, as a little proceſs " or lobe. I unraveled the greateſt part by diſſection, and obſerved that " it was made up of the convolutions of the common excretory canal. " And when it was thus unraveled, we ſaw plainly that this portion of " the canal came out from one part of the *epididymis* and went into an- " other; ſo that it could not poſſibly be a tube or duct going off from " the *epididymis* to any other part of the body.

" What made me very clear in my opinion from the firſt, was, that in " injecting this *teſtis* (the firſt in which the internal *tubuli* were filled " with mercury) I attended very particularly to the appearance of the " quickſilver, as it was running along the meanders of the *epididymis*, " and obſerved this proceſs fill in regular progreſs before any of the mer- " cury got beyond it, or higher: and without doing any thing more than " continuing the preſſure, the mercury ran afterwards through all the reſt " of the *epididymis*, and into moſt of the internal *tubuli*. Now, if it had " been a duct going off from the part, it was ſo large, and admitted the " quickſilver ſo readily in the beginning of the proceſs, that the mer- " cury muſt have run out, or filled this duct ſome way up the ſpermatic " cord, much ſooner than run thro' the ſlender and long winding tubes at " the upper end of the *epididymis* and in the body of the *teſtis*."

3. After what has been ſaid, it will be unneceſſary for me to make

any

any remarks upon the third paragraph of the quotation from Dr. Monro. I will only beg of the reader to *make* his own *obſervations* and *draw* his own *concluſions*.

4. I ſhall now anſwer the fourth and laſt paragraph, viz. " 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 re- " marked 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." The ſhort and plain anſwer is, that this aſſertion is directly, and altogether contrary to truth and fact. I never ſaw ſuch veſſels in my life, (I mean any other than lymphatics) I never ſhewed, nor pretended to ſhew them in my life, either at lecture, or any where elſe; and I always was inclined to believe, and am ſo to this day, that no ſuch veſſels exiſt; and that Dr. Monro had taken a common lymphatic of the part for ſome remarkable tube, which hitherto had eſcaped the purſuit of anatomiſts. In my autumnal courſe of 1758, ſoon after Dr. Monro's pamphlet was publiſhed, I did myſelf juſtice with regard to this accuſation, in preſence of ſeveral gentlemen who had attended all my courſes ſince Dr. Monro's going to Berlin; and called upon them to witneſs (which they readily then did) that in every courſe during that time, I ſpoke of thoſe veſſels in the manner I have juſt related. The only excuſe I can poſſibly ſuggeſt for this bare-faced aſſertion is, that Dr. Monro was ſo informed. But even upon that ſuppoſition he was inexcuſable for aſſerting it *abſolutely*, as it affected another perſon's character ſo materially. He ſurely ought to have conſidered that his information might be falſe, either from ſome miſtake at firſt, or in the conveyance; or that it might be ſome *malevolent miſrepreſentation*. If I am not miſinformed, he knows inſtances of the laſt kind with regard to me. For example, he knows, I believe, that a young profeſſor attended my lectures, and ſome time afterwards read lectures himſelf *. Among other inſtances of diſinge-

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* To underſtand this anecdote, the reader ought to know that there is among my preparations a tongue, one half of which is injected and the other not; ſo that the one half is red and the other white. The line of diviſion is ſtrait, and exactly in the middle. The *foramen cæcum* is very conſpicuous. I have often ſent it round at lecture, as a ſpecimen of that *foramen*; and judging it uſeful to illuſtrate diſeaſes, and to ſtamp them upon the memory of ſtudents, by objects of ſight, whenever it can be done, I uſed generally then to ſpeak of the *hemiplegia*, in the following manner. *If we ſuppoſe the nerves of each ſide to terminate exactly in one half of the body, we ſee how in the palſy of one ſide the ſenſible and inſenſible halves will be divided by an exact middle line.*

nuous behaviour, he ſhewed his pupils a tongue, one half of which was injected, the other not, and ſaid, (as I was informed) "Dr. Hunter "ſhews ſuch a preparation as this in his lectures to explain the *hemiplegia*;" and with a ſneer added, "but I have always looked upon the *hemiplegia* "as a diſeaſe of the *nerves*, not of the *blood-veſſels*."

§ III. The next thing to be conſidered in Dr. Monro's pamphlet, is what he ſays of the lymphatic glands, pag. 38, 39, and 40. I ſhould have paſſed it over, with ſome other things of the ſame kind, if Profeſſor Monro ſen. * had not deſired to know what I had done upon this ſubject.

I had made no ſatisfactory obſervations upon the lymphatic glands for ſeveral years after I had read lectures, and therefore never took upon me to decide between Nuck and Ruyſch, whether they were cellular or only vaſcular. All this, as well as the manner in which the lacteals and lymphatics paſs through them, I profeſſedly gave from authors, and not from my own obſervations. My brother found out, to the beſt of my recollection, in the year 1753, or 1754, that he could fill theſe glands uniformly, and the lymphatic veſſels going from them, by puſhing a pipe into their ſubſtance, as Dr. Nicholls had done in the *teſtis*. When examined in this way, they have exactly the appearance that Nuck deſcribes. After I had ſeen this experiment repeated to my ſatisfaction, I mentioned it in my lectures, and then confirmed what Nuck had ſaid, from my own obſervation. Having found out ſo eaſy a method, my brother then intended *to have diſcovered or aſcertained the ſtructure, and, if poſſible, the uſe of the lymphatic glands*; *to have traced the lymphatic veſſels all over the body*, *and to have given a compleat deſcription and figure of the whole abſorbing ſyſtem*. This he propoſed to accompliſh, as his other employments ſhould permit. He occaſionally filled theſe glands with air, with mercury, and with ſoft wax. They always appeared to be cellular, and the lymphatics to paſs through them in the manner that was commonly ſuppoſed. To ſee more exactly how theſe things were, he injected ſome with wax, and then ſteeped them in ſpirits of ſea-ſalt for corroſion: but he learnt nothing of them by this experiment; for in waſhing they all crumbled to bits, not only the ſuppoſed cellular part, but the viſible branches of the lymphatic veſſels: which was occaſioned, as he imagined, by the frequency or number of valves in them, interſecting the column of wax. As he wiſhed not to be anticipated, I treated the

* Critical Review for November, 1757. See Appendix. No. II.

the fubject lightly at my lectures, and to the beft of my remembrance, only mentioned his manner of filling the glands, and the eafy method of raifing the veffels wherever there are fuch glands, and his opinion of the thoracic duct climbing fo far as the upper *cava*, inftead of terminating immediately into the lower, viz. that the chyle was carried a great way before it was poured into the blood, probably for the fake of being firft mixed with almoft all the lymph of the body. Both thefe obfervations I made as from my brother, when Dr. Monro attended me; and when the hurry of diffections was pretty well over in the fpring, my brother fat about a preparation, which he propofed as a bafis for his intended defcription and figure of the *abforbing fyftem.* Dr. Smith of Oxford happened to be in town at that time, and being much pleafed with the intention and with the preparation, was frequently in the diffecting room while my brother was diffecting, and while Mr. Riemsdyk was making the drawing *: fo were many gentlemen of our acquaintance, befides ftudents.

In that preparation and figure, the lymphatic veffels from the ham upwards to the thoracic duct were feen, as well as the inguinal and lumbar glands, all the larger lacteals at the root of the mefentery, the *receptaculum chyli*, (or what is fo called) and the thoracic duct, all I fay, finely filled with mercury. So far my brother had gone. A very indifferent ftate of health, the effect of too much application to anatomy, which obliged him to be much in the country, other unavoidable avocations, Dr. Meckel's publication upon the lymphatic glands, and a diflike of having any difpute with Dr. Monro, which, by his father's letter in the Critical Review feemed to be threatened, all thefe things, I fay, have from that time made him lay afide the fcheme; and he will hereafter finifh it, or not, as he may think proper.

* In feveral parts of his book Dr. Monro has proved logically, that my *conclufions* were *contradicted by premifes*, and therefore from me they were unjuftifiable: yet thefe very *conclufions* were found to be right, when he had taken a great deal of pains and drawn them from *proper* premifes. He muft furely wonder how I had hit upon fuch truths, without making any obfervations and experiments, and without any ideas of relation, connection, and inference. Call it a power, a gift, fecond fight, or what you pleafe, it is fomething that has fet me right feveral times. I fhall give an inftance of its influence. At the time my brother was working upon this preparation, I told Dr. Smith, (but in confidence as he remembers) that Dr. Monro would next undertake the *lymphatic glands*. This prediction was to be fure at that time againft all human reafon or probability; it was *a conclufion contradicted by premifes*, and yet the very thing happened. See his note, pag. 40.

After giving this account of what was publicly done concerning the lymphatic glands, I ſhall make ſome remarks on this part of the Doctor's pamphlet. He quotes, (pag. 38.) that experiment of blowing or pouring mercury into the gland, and then ſays, "Dr. Hunter's aſſum-" ing this experiment as his own, is certainly either the moſt undeniable "proof of ignorance of what had been already done upon the ſubject, or "the moſt palpable invaſion of the property of our fore-fathers, if the ex-" preſſion can be allowed, that has ever perhaps appeared in print." How does the Profeſſor prove this *ſtrong aſſertion?* He *prates* about Nuck and Cowper, and finiſhes with the following flouriſh: "From which it was "reaſonable to conclude, that he, (Dr. Hunter) had not made any ſuch "experiment; and that he was but the echo of Nuck or Cowper." Now, do Nuck and Cowper ſay that they blowed or injected mercury by a pipe introduced into the lymphatic gland? No, neither of them ſays ſo; neither of them ever did it, or knew that it could be done, ſo as to fill the lymphatic veſſels, and what they call the cells of the gland. They both injected the mercury by ſmall pipes introduced into the lymphatic veſſels. What a ſhameful miſrepreſentation is this in Dr. Monro, after ſuch a direct and full aſſertion that "My taking the experiment as my "own was either the moſt undeniable proof of ignorance of what had "been done upon the ſubject, or the moſt palpable invaſion of the pro-" perty of our forefathers, that has ever perhaps appeared in print." That he ſhould expoſe himſelf thus to the world! That his father ſhould ſuffer him to do ſo!

In his next page, (viz. 39.) he appears with great advantage, (as he ſeems to think) in *proving* that this experiment is no evidence of the lymphatics being abſorbents, and that therefore *Dr. Hunter had better omitted to mention it in proof of the general doctrine of the lymphatics being abſorbents.* Dr. Hunter never thought it any proof of that doctrine, nor ever mentioned it as ſuch. He mentioned it in his lectures, as a *new experiment* that might be uſeful, and as an eaſy method of exhibiting the lymphatics: and the authors of the Critical Review mentioned it without any direct application. A word to the wiſe, they ſay, is enough; but Dr. Monro would not take a hint. The experiment was mentioned, we may ſuppoſe, that he might not (among other things) take it to himſelf. Yet he has done ſo in the note of his 39th page; and it appears that he did ſo at Edinburgh in 1755, for he then told Dr. Black the experiment, and has got Dr. Black's certificate of having done ſo. Would he have done this, if it had been an experiment to be found in Nuck or Cowper?

Cowper? Did he then learn this experiment (among many other things) at Edinburgh from Dr. Hunter's pupils? Can we doubt, if we trace him a little upon the ſubject? It is plain that in the ſummer 1753, he did not know this experiment, or eaſy method of filling the lymphatics. "In ſummer, 1753," ſays he, pag. 22. "I firſt attempted to fill the lym-"phatic veſſels with quickſilver, introduced by pipes put into openings "made in 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:----but, af-"ter ſeveral experiments, I was convinced that this was impracticable. "----*But, as I greatly wiſhed to have ſome preparation of theſe veſſels*, I "next endeavoured, in imitation of Nuck, Cowper, &c." (Reader attend!) "to fill them from the arteries----but with no other ſucceſs.--- "At laſt I thought of employing what the painters call ſize, (a thin "glue) from which I flattered myſelf with great expectations·----but "was likewiſe diſappointed in ſeveral trials which I made with it," &c. To make the ſtory ſhort; he at length filled them in a *teſtis*, from the artery by extravaſation.

Well then, the Profeſſor allows that in ſummer 1753, though *he greatly wiſhed to have ſome preparations of the lymphatic veſſels*, and made all the uſe he could of Nuck, Cowper, &c. yet he could not inject them either with air, quickſilver, or thin glue. He did not then know (and Nuck and Cowper could not tell him) *the eaſy method of exhibiting and injecting lymphatics by throwing air or mercury into the lymphatic glands.* So far is clear. Now let us ſee when he firſt diſcovered it: for after ſo many difficult and unſucceſsful *methods*, ſurely he would be much pleaſed when he found out this *eaſy method.* But he does not venture to tell us *how* or *when* he found it out; would have had it paſs quietly for his own; and when he finds it challenged by Dr. Hunter, gives it to Nuck or Cowper, as you ſhall ſee.

In the year 1755, he knew this experiment (from Dr. Hunter's pupils ſuppoſe) and procured Dr. Black's certificate in the following words (pag. 28.) "You told me at the ſame time, that an eaſy method of ex-"hibiting 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." From which it is plain, that he was communicating it as a new experiment, and as his own; and it

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was underſtood ſo by Dr. Black, as appears by his letter to me on that ſubject, in which he ſays: "So far as I can remember when he ſaid "that he could raiſe the lymphatics in this way, he told it me as a piece "of anatomical news, or as a curious anatomical fact, which I imagined "was a diſcovery of his own." In his 39th page, he ſays in a note: "Tearing the outer membrane of the conglobate glands, and breaking "their ſubſtance and pouring in mercury, had been my common way "of ſhewing the lacteals of the ſecond order, or lymphatics going for-"wards from them, as Dr. Black obſerves in his letter." You ſee, he ſays it had been his *common way*. Now it was not his *common way* in the year 1753, as we have proved from himſelf; he does not venture to tell us how long this had been his *common way*, or how he found it out, but would have had it paſs for his own obſervation (in his *common way)*; and when he found that Dr. Hunter took it to himſelf, (or rather gave it to his brother) he tells us, with a good aſſurance, (his *common way)* that it was "either the moſt undeniable proof of ignorance of what was done "upon the ſubject, or the moſt palpable invaſion of the property of our "fore-fathers, that has ever perhaps appeared in print;" then lays down his facts or proofs (in his *common way)* of a cock and a bull, and Cowper and Nuck, and concludes (in his *common way)* that *Dr. Hunter had not made any ſuch experiment*, *and that he was but the echo of Nuck or Cowper.*

The next thing that occurs in Dr. Monro's pamphlet will require a whole chapter for its diſcuſſion.

C H A P. V.

Of ABSORPTION by VEINS.

DR. Monro has taken a great deal of pains to diſcredit me upon a variety of ſubjects, which he might have known I ſhould not chuſe to diſpute with him. Upon theſe occaſions he miſrepreſents the facts, puts what words he thinks proper in my mouth, gives out opinions as mine which he ſhould have known to be not ſo; and then ſhews his parts and his reading in confuting them. He would have loſt nothing had he diſputed with more modeſty, and learnt to bear what he thought a triumph with more moderation. We ſhall take his chapter, page 55, *Of abſorption by*

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the branches of the red veins, as an example of this ſort of dealing with me Pray, reader, ſtop here a few minutes, and conſider that chapter carefully: it conſiſts but of a few pages.

He makes me aſſert that the red veins do not abſorb, and then you ſee how he treats me, and triumphs. What foundation has he for making me the father of this hypotheſis? He quotes the Critical Review. Look to the place, and you will find he refers to ſome notes written by the authors of the Critical Review; not to any work of mine. Now, ſuppoſe I had written theſe notes; yet as they appeared under another name, and as he could not prove, or certainly know them to be mine, ſurely he had no right to uſe my name with the freedom he has done. A gentleman would not have done ſo for my ſake; a man of ſenſe would not have done ſo for his own ſake. But the truth is, that I knew nothing of thoſe notes, *directly* or *indirectly*, till I read them in print: the authors of the Critical Review know this to be true, and give me leave to ſay ſo in the ſtrongeſt manner I pleaſe *. To ſhew Dr. Monro in his true colours, I muſt go farther ſtill, and tell the reader that he muſt have known this was not an opinion or hypotheſis that I maintained. He attended all my lectures. I appeal to all the MS notes of my lectures, and to all my pupils. Let them recollect or look into the ſecond lecture; they will find that I ſay of veins, *they begin* 1. *from arteries*; 2. *from ſurfaces or cavities as abſorbents:* and when upon the meſenteric veins, they may remember I explained the common doctrine, and commonly produced the drawing of a little hydraulic machine of Dr. Lieberkuhn, by way of illuſtration; and that I went no farther in oppoſition to the common opinion, than to ſay that I was not ſatisfied, that I had doubts, that all my obſervations were againſt the common hypotheſis, but that I did not take upon me to ſay it was not the truth.

Indeed, in both my courſes of the winter 1759-60, I went ſo far as to ſay, I *believed* that the red veins did not abſorb, and gave my reaſons for thinking ſo: but this was long after Dr. Monro's publication.

In different parts of my lectures I uſed to treat of the tranſudation and abſorption of fluids in animal bodies in the following manner:

" I have

* The Profeſſor has been followed through ſo many mortifying ſituations, that it is high time to ſhew him ſome pity. The reader is therefore intreated to compaſſionate the diſtreſs which he muſt have been in, when he thought of quoting thoſe notes in the Critical Review as *my words*, in order to make *his defence*; which he has done in ſix different places, viz. pag. 19, 37, 53, 56, 58, and 65.

" I have often confidered with myfelf how the interftitial fluid gets
" into the fmaller and greater cavities of our bodies; how the water of
" an *anafarca*, for inftance, gets into the cellular membrane. The com-
" mon opinion, I think, is, that there are every where exhalant arteries,
" which open and terminate on the fuperficies of fuch cavities, and throw
" out the watery fluid which they contain. But for my own part, I
" cannot help believing that it is entirely by tranfudation through the
" coats or fides of the veffels. My reafons for thinking fo are thefe:

" Firft, fo far as I can find, all the arguments of the lateft and beft
" anatomifts, taken from injecting the arterial fyftem in dead and living
" bodies, only prove that a thin fluid paffes readily from the arteries into
" the interftices of parts. They do not prove the exiftence of exhaling
" branches, no more than they prove tranfudation.

" In the fecond place, the phænomena of injections, fo far as I have
" been able to make obfervations, agree better with the notions of tranfuda-
" tion than with that of exhaling arteries. I have had great experience
" of injections, and I have made experiments with all forts of fluids in-
" jected into the arteries and veins of dead bodies. I have always ob-
" ferved that fubtile and penetrating fluids pafs with eafe from the ar-
" teries into the cavity of the inteftine, and into the cellular membrane
" in any part of the body: fuch fluids are water, gum water, whites
" of eggs ftrained, glue, ifinglafs diffolved in water or fpirits, any fluid
" oil, melted butter or axunge, &c. But when thefe fluids were co-
" loured with vermilion, I always obferved that none of the vermilion
" paffed out of the arterial fyftem, but when there were manifeft ap-
" pearances of extravafation and rupture of the veffels: I never obferved
" vermilion pafs into the cavity of an inteftine from the mefenteric ar-
" teries, without feeing a hundred ruptures and extravafations in the *villi*
" of the gut. All this looks as if the fluid oozed through the coats,
" rather than was poured out by the branches of arteries.

" In the third place, I have obferved that the cellular membrane is not
" fo immediately filled by injecting the arteries; it requires fome time,
" and I have plainly feen, when I have let an injected part lie bye a lit-
" tle while, that the cellular membrane became gradually more loaded
" as the arterial fyftem became more empty; a ftrong argument, in my
" mind, that it got out of the arteries by tranfudation.

" In the fourth place, water, and even red blood foaks through all our
" veffels and membranes in dead bodies; as you may fee by fteeping the
" *apex* of a heart well wafhed, or the convolution of a piece of frefh

" in-

" inteſtine in clear water: in both caſes the water will become
" bloody.

" But ſtill it is ſaid that in all theſe caſes the fluids paſs by fine exhal-
" ing veſſels, though theſe veſſels cannot be ſeen. To this I anſwer, that
" if our interſtitial fluid was of a ſtrong marked colour, we ſhould then
" by diſſection be able to obſerve whether it was poured out by ſmall
" arteries, or whether it ſoaked through the natural pores in the coats
" of veſſels. Now very fortunately for us in this diſpute, there is one
" ſuch fluid in the body; it is the bile. Its colour is pretty deep, and
" very different from any thing that lies near the gall bladder. No man
" can have opened any number of bodies, without allowing that the gall
" does paſs in living bodies through all the coats of the gall bladder,
" and pervades the ſubſtance of the neighbouring parts, not by
" exhaling nor by inhaling veſſels, but by manifeſt tranſudation or
" ſoaking.

" It might be aſked, why the red blood does not tranſude through the
" veſſels in living bodies, for I think it certainly does not. In anſwer to
" this it may be ſaid, that our fibres and veſſels have perhaps ſome de-
" gree of tenſion and firmneſs in life, which they loſe with life; and it
" muſt be obſerved too, that in proportion as the blood putrifies it be-
" comes thinner; whence we ſee, in opening a putrid body, all the cavi-
" ties more or leſs filled with a bloody water, and all diſtinction of co-
" lour in the muſcles and cellular membrane quite loſt. But what I
" ſuppoſe to be the principal reaſon that red blood does not tranſude
" through the veſſels in living bodies, is its glutinous quality, its thick-
" neſs while it is equally mixed up with its coagulating part. That
" part coagulates as certainly as the blood ſtagnates even in living bo-
" dies; and when the univerſal ſtagnation happens in death, this part
" of the blood collects itſelf into irregular *polypi* and coagulations all
" over the body, and the reſt of the blood is no longer the thick viſcid
" fluid it was before, but rather a bloody *ſerum*, that will ooze through
" all the veſſels and membranes."

Such were my notions of the *ſource* of our interſtitial fluid. With regard to its *abſorption*, I was of opinion that nature had provided a ſyſtem on purpoſe, viz. the lymphatics. I conſidered theſe veſſels and the lacteals as an appendage to the venal ſyſtem, by which the ſtores were brought in for ſupplying the circulation; and the glands and ſecretory veſſels all over the body, I conſidered as an appendage to the arterial ſyſtem, by which the proper ſeparations were made, and the redundancies thrown off.

My only doubt was, whether the veins did or did not abſorb a certain quantity, eſpecially in the inteſtines. From my own obſervations on injections, I ſhould have concluded that they did not, and that there was no paſſage for liquors between an inteſtine and the meſenteric veins, otherwiſe than by tranſudation. But authors of the beſt credit had given ſuch arguments and experiments in favour of abſorption by veins, that I dared not, even in my own mind, determine the queſtion.

At this time my brother was deeply engaged in phyſiological enquiries, in making experiments on living animals, and in proſecuting comparative anatomy, with great accuracy and application. It is well known that I ſpeak of him with moderation, when I ſay ſo. He took the ſubject of abſorption into his conſideration, and from all his obſervations, was inclined to believe, that in the human body there was one, and but one ſyſtem of veſſels for abſorption: he knew ſo well that many things had been aſſerted by one perſon after another, which were not true, that ſo many miſtakes had been made from inattention, ſo many errors introduced from other cauſes, that he could eaſily ſuppoſe the veins might not perhaps abſorb, after all the demonſtrations that had been given of the fact; and therefore was determined to ſee how far this point could be cleared up by plain experiments and obſervations. With that intention he made the following experiments, in my preſence, and in preſence of a number of gentlemen, who all of us aſſiſted him, and made our own obſervations upon what paſt before us. I ſhall quote the experiments from him, and can bear teſtimony to the fairneſs with which they were made, and with which they are here related.

A N I M A L F I R S T.

" Experiment I. On the third of November 1758 *," ſays he, " I opened the belly of a living dog. The inteſtines ruſhed out im-" mediately. I expoſed them fully; and we obſerved the lacteals filled " with a white liquor at the upper part of the gut and meſentery; " but in thoſe which came from the *ileon* and *colon* the liquor was " tranſparent.

" I tied up the meſenteric artery and vein that was going to about half " a foot

* In the preſence of Doctors Clayton, Fordyce, and Michaelſon, and Meſſ. Blount, Jones, Churchill, and Richardſon.

" a foot of inteſtine, and put a light ligature upon the upper part of " the inteſtine, including a little of the meſentery, then emptied that " part of the gut by ſqueezing it downwards, and put a ſimilar ligature " upon the lower part of the gut. In the next place, I made a ſmall hole " in the upper end of this part of the gut, and by a funnel poured in " ſome warm milk, and confined it by making a third ligature upon the " gut cloſe to this hole. Theſe ligatures prevented the circulation of " blood in this part of the bowel. Laſtly, I punctured the vein beyond " the ligature that had been made upon the meſenteric veſſels, and by " gentle ſtroking with the end of the finger ſoon emptied it of its " blood.

" Exper. II. I immediately after this made the ſame experiment, " and in the ſame manner, on a part of the inteſtine lower down, where " the lacteals were filled with a tranſparent liquor.

" In the firſt experiment the lacteals continued to be filled with a " milky or white fluid: in the ſecond, the lacteals, which before con- " tained only a tranſparent lymph, were preſently filled with white milk.

" In both theſe experiments, we could not obſerve that the leaſt white " fluid had got into the veins. After attending to theſe appearances a " little while, I put all the bowels into the *abdomen* for ſome time, that " the natural abſorption might be aſſiſted by the natural warmth; then " took out and examined attentively the two parts of the gut and me- " ſentery upon which the experiments had been made: but the lacteals " were ſtill filled with milk, and there was not the leaſt appearance of a " white fluid in the veins: on the contrary, what little blood was in " them, was juſt as thick, and as deep-coloured as in the other veins, " and when ſqueezed out from them, coagulated as the blood of other " veins.

" Exper. III. I tied up and filled another piece of the inteſtine with " milk in the ſame manner, but did not make a ligature upon the me- " ſenteric veſſels, leaving a free circulation in the part. We looked very " attentively at the colour of the blood in the veins of that part, both " with our naked eyes and with glaſſes: we compared it with that in the " artery, and in the neighbouring veins, but could not obſerve that it " was lighter coloured, nor that it was milky, nor that there was any " difference whatever.

" Exper. IV. Laſtly, we took that part of the gut which was filled " with milk in the firſt or ſecond experiment, and ſqueezed and preſſed " it very gradually, in order to ſee whether any milk would by theſe

" means paſs into the empty meſenteric veins. This we did gradually " with more and more force till the gut at laſt burſt; but ſtill there was " not the leaſt appearance of any thing milky in the veins.

A N I M A L S E C O N D.

" Experiment I. November 13, 1758 *, I opened the *abdomen* of " a living ſheep, which had eat nothing for ſome days, and upon ex- " poſing the inteſtines and meſentery, we obſerved the lacteals were " viſible, but contained only a tranſparent watery fluid. I made a hole " in the inteſtine near the ſtomach, and by a funnel poured in ſome " thin ſtarch, coloured with indigo, ſo as to fill ſeveral convolutions; " then tied up the hole in the gut, and put all the bowels into the " *abdomen* for ſome time. Upon taking them out after this, we obſerved " all the lacteals of that part filled with a fluid of a fine blue colour. " We thought at firſt, that the blood in the veins of this part was of " a darker colour; but upon comparing it carefully with that in the " other veins it was manifeſtly the ſame.

" Exper. II. I opened a vein upon this part of the meſentery, and " catched a table ſpoon full of its blood. I ſet it bye to congeal and " ſeparate into its *coagulum* and *ſerum*. On the next day, and the day " after that, I examined the colour of the *ſerum*; but it had not the " leaſt blueiſh caſt.

" Exper. III. I fixed an injecting pipe in an artery of the meſentery, " where the inteſtine was filled with the blue ſtarch, and tied up all " communications both in the meſentery and inteſtine, (as in Animal " Firſt, Exper. I.) but left the correſponding vein free; then I injected " warm milk by the artery, till it returned by the vein, and continued " doing ſo till all the blood was waſhed away, and the vein returned a " bright white milk. This was done with a view of ſeeing if the milk " in the vein acquired any blueiſh caſt; but there was no perceptible " difference between the arterial and venal milk.

" Exper. IV. After this I opened the vein with a lancet, and diſ- " charged moſt of the milk, then put a ligature upon both the artery " and vein, and waited ſome time to ſee if they would fill, but they did " not, nor did the remaining contents of the vein acquire the leaſt " blueiſh caſt. Then I opened the gut at this part, but we could not " obſerve

* In the preſence of Doctors Wren, Fordyce, and Michaelſon; and Meſſ. Blount, Tickell, Churchill, Paterſon, and Skeette.

" observe any appearance of the milk having got into the cavity of the
" inteſtine.

" Exper. V. I filled another part of the inteſtine with milk. All
" that we obſerved after doing this, was that the lacteals became
" fuller, though not of a white colour, and the veins remained of the
" ſame complection.

Exper. VI. I fixed a pipe into a vein of the meſentery, and injected
" milk towards the inteſtine, to ſee if any would paſs into the cavity of
" the gut: but preſently innumerable extravaſations happened; ſo that
" the experiment was fruitleſs.

" Exper. VII. I fixed a pipe into an artery, and tied up the vein, and
" all the communications, then injected milk for ſome time into the
" artery till the vein became quite turgid and tight; this was continued
" for ſome little time, and with as much force as we thought the veſſels
" would bear without burſting: then we opened the inteſtine at that part,
" and there was no appearance of milk in its cavity.

Exper. VIII. I took a piece of the inteſtine that was quite empty
" and clean, and filled it with warm water. The returning blood in
" the vein of this part appeared not at all diluted or thinner than in
" the other veins. Then I tied up the artery, and all the communica-
" tions; and attended to the ſtate of the vein for ſome time: it did not
" grow more turgid, nor did its blood become more watery, nor was
" there any appearance whatever of the water's having got into the
" veins.

" The animal was quite alive all the time of our making theſe experiments
" and obſervations, which laſted from one o'clock till half an hour after
" three. I choſe a ſheep rather than a dog, both becauſe the animal was
" much larger, and therefore its meſenteric veſſels were fitter for being
" eaſily injected; and beſides, becauſe it is much more patient and
" quiet. Theſe advantages we were all ſenſible of when we made the
" experiments.

ANIMAL THIRD.

" June 22d, 1759. We repeated moſt of theſe experiments on
" another ſheep, to ſee if the effect would be the ſame: but in this
" animal the viſcera were diſeaſed, inflamed, and thickened in moſt parts,
" ſo that the experiments were much leſs ſucceſsful, leſs ſatisfactory, and
" concluſive. After injecting milk into the meſenteric artery for ſome
" time, and allowing it to return by the vein, we opened that part of
" the

" the inteſtine, which had been previouſly emptied, and found in it a
" watery fluid of a whitiſh caſt, as if a few drops of milk had been
" mixed with it.

A N I M A L F O U R T H.

" In July, 1759 *, I repeated moſt of the experiments related in " article *Animal Second*, upon another ſheep. The effect of all of them " was ſo nearly the ſame that I need not be particular.

" I ſhall only obſerve, that when the inteſtine was filled with ſtarch- " water and indigo, and milk injected by the artery till the vein was " waſhed clean of blood, and a ligature put upon the artery and vein, ſo " as to leave them about half full of pure white milk, after waiting " more than half an hour we could not obſerve that the vein was in the " leaſt more filled or turgid, nor had the milk in the veins acquired the " leaſt of a blueiſh caſt, nor even in the ſmalleſt veins upon the gut itſelf, " where we ſhould ſuppoſe the abſorbed liquor muſt have been appa- " rent, if any had been taken up by the veins from the cavity of the " inteſtine.

" After the animal was dead, I blowed into a meſenteric vein, and " the air found a paſſage into the cavity of the gut, though in making the " experiment when the animal was alive, I could not force the milk by " injection from the vein into the gut.

A N I M A L F I F T H.

" If any animal could be ſuppoſed a fitter ſubject for ſuch experiments " than a ſheep, it would be an aſs. He is not ſo large, nor ſo ſtrong, " but that he may be managed; he is patient in the greateſt degree; his " meſentery and veſſels being larger, it is ſo much more eaſy to fix " injecting pipes, make ligatures, &c.: and, what is a very great ad- " vantage in making ſuch experiments, his meſentery is very thin, with- " out fat, ſo that the veſſels are conſpicuous and diſtinct. Hence it is " eaſy to ſeparate the artery from the vein, to fix pipes, to tie up anaſto- " moſing veſſels by a needle, &c.

" Therefore I got an aſs, and on the 24th of Auguſt, 1759 †, put him " upon

* In preſence of Doctors Macaulay, Ramſey, and Michaelſon; and Meſſ. Edwards and Tomlinſon.

† In preſence of Doctors Macaulay and Michaelſon; and of Meſſ. Edwards, White, and Gee.

" upon his back in an open garden, and tied him faſt to four ſtakes driven " into the ground, then opened his *abdomen*, &c.

" Experiment I. I poured a ſolution of muſk in warm water into " a piece of the inteſtine, and confined it there by two ligatures. In " doing this the animal ſtruggled, and a little of the liquor was ſpilt " upon the outſide of the inteſtine and meſentery.

" After waiting a little while, I opened with a lancet ſome lacteals of " this part, which were full of a watery fluid, and catched a little of " their contents in a ſmall ſpoon. It ſmelled ſtrongly of the muſk; and " though it could hardly be doubted that the muſk had been taken up " from the inteſtine by abſorption, yet as ſome of the muſk-ſolution had " been ſpilt upon the external ſurface of the parts, and as it was im- " poſſible to collect the lymph from the lacteals without reſting the edge " of the ſpoon upon the meſentery, the ſmell of the ſpoon might be " owing to that circumſtance.

" After this I wiped a vein upon the meſentery very clean, and opened " it with a lancet: a gentleman who had kept out of the way of the " muſk, came immediately with a clean ſpoon, and filled it from " the ſtream of blood without touching any part of the animal, and car- " ried it directly off; but it had not the leaſt ſmell of muſk.

" Exper. II. We poured ſome ſtarch water, made very blue with " indigo, into a part of the gut in the ſame manner as in ſome of the " former experiments; tied the vein and artery of this part; then punc- " tured the vein cloſe to the ligature, and preſſed out almoſt all the " blood; then tied up the empty vein, and put all into the cavity of the " belly for a quarter of an hour. After that, we examined the part, " found the lymphatics very turgid, as the fluid could not paſs through " them towards the thoracic duct, on account of the ligatures made upon " the meſenteric veſſels: but we found the veins of this part empty, ex- " cept indeed that a little blood had got into them from the neighbouring " veſſels, which, from the appearance, had evidently paſſed the ligatures " tied round the ends of the gut; a circumſtance which it is very dif- " ficult to obviate.

" Exper. III. I next repeated the *Third Experiment* of *Animal Second* " exactly in the ſame manner, and preciſely with the ſame effect.

" Exper. IV. Then I repeated the *Fourth Experiment* of *Animal Se-* " *cond*; and the effect was ſtill the ſame.

" N. B. It may not be amiſs to obſerve, that the lacteals continued to " abſorb the blueiſh liquor all this time; even at the part upon which

" this

" this *Fourth Experiment* was made, where the nerves muſt neceſſarily
" have been tied up with the artery.

" Exper. V. I ſqueezed a piece of the inteſtine ſo as to empty it as " entirely as well might be, then tied up all the lateral communications " of the veſſels, and injected warm milk into the meſenteric vein till it " returned by the artery, and continued this operation for ſome time af- " ter all the blood was waſhed out. Then I opened that part of the in- " teſtine through its whole length, and found it quite empty.

" I made this experiment again upon another part of the inteſtine, in " the ſame manner, and exactly with the ſame ſucceſs."

Here is a new doctrine propoſed in phyſiology, viz. *that the red veins do not abſorb in the human body*. The fair enquirer after truth will be convinced, by the obſervations which occurred to me, that the common opinion is ſupported by ſome proofs that are at leaſt doubtful or equivocal, and that the other opinion is not without plauſibility; and he muſt allow that my brother's experiments render it highly probable.

But we may preſume from what Dr. Monro has argued upon the doctrine of the lymphatics, that he will ſay I had no right to ſet up this hypotheſis, except *I could refute, or otherwiſe explain* the experiments and arguments of the beſt and lateſt phyſiologiſts in ſupport of the common opinion. I will therefore ſhew how little Dr. Monro has been able (with all the aſſiſtance which he may have had) to eſtabliſh the old opinion, or to diſprove the other.

The firſt thing that he urges (pag. 56) is an inconſiſtency between two expreſſions which he is pleaſed to call mine. My anſwer is, that I wrote *one of them*, and the authors of the Critical Review wrote *the other*. Yet there appears to me no inconſiſtency. For, if we ſuppoſe that no ſmall veins do begin as abſorbents from the cavity of the inteſtines, &c. may not veins, as inſtruments of the circulation, begin from the cells of the *penis?* And though the chyle or lymph could not well paſs through the abſorbent ſyſtem without valves and lateral preſſure, may not blood return eaſily by the veins, where there are cells between the terminations of the artery and beginnings of the vein? With regard to the motion of the contained fluid, theſe cells are to be conſidered, as enlargements of the canal, as ſomething of an intermediate nature between an aneuryſm and a *varix:* ſo that the blood is preſſed from the cell into the vein, with the ſame force with which it was thrown from the artery into the cell. Hence it is that the *penis* remains in a ſtate of ſyſtole, ſo long as the reſiſtance to the venal

ſtream

ſtream does not overcome the natural contraction of the part; but as ſoon as it does, the extenſion or *diaſtole* of the part neceſſarily enſues, and muſt continue as long as that reſiſtance is continued. I ſhall prove this by an experiment which my brother made upon a living dog, and which I ſhall give in his own words. " In April 1760," ſays he, " in preſence of " Meſſ. Blount, &c. I laid bare the *penis* of a dog almoſt thro' its whole " length, traced the two veins that come from the *glans*, which in this " animal makes the largeſt part of the *penis*, and ſeparated them from the " arteries by diſſection, that I might be able to compreſs them at plea- " ſure, without affecting the arteries. Then I compreſſed the two veins, " and found that the *glans* and large bulb became full and extended. I " then irritated the veins, in order to ſee if there was any power of con- " traction in them, which might occaſionally ſtop the return of the blood; " but could not obſerve any ſuch appearance. Then I pricked the *erec-* " *tores penis*, upon which they contracted; but their contraction had no " ſenſible effect upon the veins or upon the *penis*."

Dr. Monro's next argument (page 57) is ſuch as can have no weight againſt the new hypotheſis. It is a long chain of premiſes, each of which may, or may not be true; and at laſt there is a *concluſion* which is very agreeable to our hypotheſis, and a preſumption about a point that is not in queſtion. It runs thus: *Lacteals have not been certainly obſerved in oviparous animals*, and yet, God knows, they may have them.---Dr. Monro is *convinced they have not lymphatic veſſels*, and we all know he may be miſtaken;-----*therefore in them the red veins abſorb*, or ſomething elſe does it.-----*But as we cannot obſerve that in them the veins differ in ſtructure from the ſanguineous veins in man*, and yet it muſt be granted they may differ, and nobody has yet proved whether they do or not. *Hence*, (true or falſe, right or wrong, according to the profeſſor's logic) *it is not neceſſary that abſorbent veſſels ſhovld have the valvular ſtructure of the lymphatics*. No ſurely: capillary veſſels, for inſtance, will abſorb without valves; but veſſels like the lymphatics or lacteals, that are not only to abſorb, but to *convey* fluids a conſiderable way unaſſiſted by gravitation, require valves, or ſomething that ſhall have the ſame effect, ſuch as the muſcular periſtaltic motion of the great traductory veſſel that goes from the mouth to the anus. *And it is alſo to be preſumed*, ſays our author, *that the ſtructure of the branches of the red veins in man is ſuch as renders them capable of abſorbing*. We, on the other hand, *preſume* that if they were capable of doing it, they would actually do it; which does not appear to be the caſe; and we *preſume* if their ſmall branches do not open into the cavities, that they may not be ſo capable of doing it.

Our author's next argument (page 57 and 58.) is drawn from the allowed abſorption of the *placenta*.------" Yet," ſays he, " whatever di-" ligence 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. Hun-" ter to prove the exiſtence of valvular lymphatic veſſels in the *placen-" ta*: 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." Surely, notwithſtanding all the Profeſſor's *diligence*, there may be ten thouſand ſhort lymphatics in the *placenta*, terminating in the branches of the vein. Suppoſing there were, muſt the Profeſſor have ſeen them? Perhaps not with all his diligence.

Now let us conſider his *more direct proofs*, (pag. 58.) " Fluids," ſays he, " 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 *; which evidently ſhows that many of their branches begin " from theſe; and hence muſt be inhalent." Here is an aſſertion, and an inference. The aſſertion conſiſts of two parts, which muſt be conſidered ſeparately. *Fluids injected into the veins ſweat out upon the ſurface of the ſkin.* Dr. Haller ſays no ſuch thing in the place referred to †; and Kaau Boerhaave ſays nothing like it in his 3d, 4th, and 5th chapters, where

" * See the elegant treatiſe of Kaau Boerhaave *De Perſpiratione*: Or, *Element. Phyſiolog.* " of the illuſtrious Dr. Haller, Lib. ii. S. 2. § 22, 23, 24."

† On the contrary, all he ſays there of the cutaneous veins is this, *Inhalationem, ſive humoris per cutem reſorptionem, proprio loco oſtendemus,* and then in a note deſires us in the mean time to conſult his Commentary on Boerhaave, Tom. III. n. 416, where no ſuch experiment is mentioned; but in the following, n. 417, after ſpeaking of the arteries which may be demonſtrated by injections to terminate on the ſurface of the ſkin, he ſays: *De venis experimentum non novi, ob valvulas, ſed analogia demonſtrat, eſſe quæ reſpondeant arteriis.* So that to prove an anatomical experiment, our Profeſſor quotes Baron Haller, who ſays he knows no ſuch experiment. Pleaſe to obſerve, I ſpeak here of an anatomical *fact*, not of an *opinion*. It is Baron Haller's, and every body's *opinion* that inhaling veins begin from the ſurface of the body; and accordingly in another part of his writings, *Prim. Lin.* § 444, he ſays: *Demonſtrat has venas injectio anatomica, quæ per venas perinde, ut per arterias exſudat, ſi aquoſa & tenuis fuerit*: and what that *exſudatio per arterias* is, he tells us, § 435, &c. *Guttulæ ſub epidermide effuſæ* (quæ) *hanc in veſicas elevant.* But if Dr. Monro had found out theſe two paſſages in Haller, they would not have anſwered his purpoſe, except he could have proved that *bliſtering* was the ſame thing as *ſweating*.

where he profeſſedly treats of the cutaneous diſcharges; and therefore we preſume he no where ſays ſo. We will venture to ſay the aſſertion is not true, and that the Profeſſor will find it to be without any foundation, if ever he ſhall make a fair trial. This aſſertion then of the Profeſſor cannot be allowed to have any weight: nor can we allow much to what he ſays in the following page, (59) *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 into the cavity of the guts*, &c. The Profeſſor does not ſeem to have practiſed injections with ſufficient dexterity and circumſpection; without which they will often miſlead us egregiouſly. In *ſuch trials* as he made, injections paſſed more readily, &c. Why? In plain Engliſh, becauſe they ſooner burſt, which gives a *ready outlet* enough. By *ſuch trials* he may find a *ready outlet* from the right auricle of the heart into the cavity of the *pericardium*, and from the larger veſſels of the brain into its ventricles, and ſeveral others which no anatomiſt ever yet dreamt of. That our author's *trials* were frequently *ſuch*, appears too from what he ſaid in page 23, viz. *In injecting the arteries indeed, for example the meſenteric, I had often obſerved that the injected matter paſſed more readily by the lateral branches into the cavity of the guts, than into the correſponding veins.* From all which, thoſe who are much converſant in injections, muſt ſee how he was going on.

The paſſage of fluids from one veſſel or cavity into another, muſt not always be taken as a proof that they have natural communications. Experiments of this kind made with water, air, or quickſilver, are particularly unſatisfactory. Water tranſudes readily through, perhaps, all parts of the human body, except the cuticle; and air and mercury are very apt to produce rupture in the veſſels, long before they have run into ſuch minute branches as are eaſily filled with glue, &c. even when coloured with the powder of vermilion. Hence we ſee that in the dead body, air blown into the *trachœa* not only readily paſſes into the pulmonary vein, but finds a *ready outlet* at a thouſand places through the external coat of the lungs. Any of the above-mentioned fluids readily paſs from the emulgent vein into the ureter; and mercurial injections into the arteries and veins of moſt parts of the body, produce rupture and extravaſation long before ſuch minute branches, as we can eaſily inject by other means, are filled. This is not owing to the want of fluidity in the mercury, but probably to the ſtrong attraction among its parts, which reſiſts its being drawn out into fine threads: and hence it runs ſo well through the excretory fyftem of the *teftis*, where the coats of the tubes are fo much thicker and ftronger *.

From what has been now faid, and from what was advanced upon the fubject of tranfudation in the former part of this chapter, and from my brother's experiments, the reader muft judge of the experiments which Dr.

* Anatomical injections have certainly improved phyfiology, and yet they have fometimes led anatomifts into great blunders. From the fimplicity of the art of injecting, any man who can fix a pipe and melt wax, may fet about experiments of that kind; and may draw conclufions about the delicate and complex operations in the living body, from the event of a blundering experiment made upon a dead one. If his wax breaks through the veffels at any part, it will be a *direct proof* that there was a natural paffage; and if by efcaping at this *ready outlet*, it does not run into the fmaller veffels, it proves clearly that no fuch fmall veffels exift, and that thofe anatomifts who have feen them, and who have injected them, muft have miftaken one thing for another. Our young Profeffor is one of thofe who make bold injections and bold conclufions. His father has the fame pretenfions to injections, and has the fame knack of going on in the argument, whether the wax ftops or runs. I fhall give a fpecimen. He twice, in a bitch, injected the uterine veffels with mercury, and found that he had not thereby filled any veffels in the *placenta (a)*; thence he concludes that in that animal the uterine veffels do not ramify through the *placenta:* and yet I find it as eafy to prove by injections that they do, as that the mefenteric veffels go into the coats of the inteftines. Having in this way eftablifhed his fuppofition, that the uterine veffels do not in the human fubject pafs into the *placenta* and *chorion*, he afterwards found it would be neceffary in fupport of his opinion to take off Noortwyk's authority *(b)*, and therefore tells us that Noortwyk muft have made a *miftake*, and is perfuaded he will alter his opinion, *when the miftake is pointed out to him*; and, what is ftill better, concludes that Noortwyk *has afforded him a very pretty proof of there being no* anaftomofis *between the veffels of the* uterus *and fecundines.* Profeffor Monro would only have you fuppofe that Dr. Noortwyk had miftaken the *uterus* for the *placenta* and membranes; and, in order to give his argument fome plaufibility, he has *egregioufly mifreprefented* the Doctor by fubftituting *one* word for *another* of a quite different meaning, in two of his quotations, and in a third has *coined a whole fentence* for him; &c. &c. Now the fact is, that Noortwyk was not miftaken, and is, we may prefume, of the fame opinion ftill, as it is impoffible he fhould not believe his own eyes. I have feen his preparation, and can vouch for the truth of the greateft part of what he advances, not only from having feen that preparation, and from having converfed with him upon the fubject, but *from my own experiments and obfervations on the human* uterus, *and thofe often repeated.*

I went into this digreffion to fhew a little of the genius of our two Profeffors; to fhew their eafy and dictatorial way of writing under a weight of diftreffing circumftances. It was for fuch a fpirit in Dr. Monro's whole pamphlet that I defpifed it; and it was fuch a dictatorial ftile, and fuch a parade of appealing boldly to facts, that made my friends think that the performance might impofe upon fome unexperienced readers.

(a) Med. Effays (Edit. 1752.) Vol. II. pag. 130. *(b)* Ibid. pag. 118.

Dr. Monro has quoted as authorities againſt the doctrine which I have been proving and defending. I grant that he has produced ſome of the greateſt *names* in anatomy; but it is an argument that may be brought againſt every new doctrine, and will have little weight when put into the ſcale againſt facts.

C H A P. VI.

Of the Veſſels of CARTILAGES, and of the Ducts of the LACHRYMAL GLAND.

DR. Monro (pag. 61.) has given me an opportunity, which I embrace with pleaſure, of correcting in print one of my own errors in anatomy. When I was a young man, and a very young anatomiſt, I wrote a paper concerning Cartilages, which was publiſhed in the Philoſoph. Tranſ. Vol. 42. In that little eſſay I advanced that the veſſels of the articulating cartilages paſs, ſecure from friction and preſſure, between the griſtle and the bone. This appearance I ſaw diſtinctly in ſeveral injected joints: but afterwards I found out that the veſſels, which I had ſeen, did not belong to the ſuperficial cartilaginous cruſt which remains in the adult, but to what lies immediately under it, in growing animals, before the oſſification is complete. My pupils will bear me witneſs, that I confeſſed and corrected this error, in every courſe of my lectures; and ſo far as I know, it was never taken notice of except by myſelf, till Profeſſor Monro jun. did it that honour, after he had attended my lectures.

I grow ſo tired of my author, that I will only add one criticiſm more. It ſhall be upon his laſt Chapter, viz. *Of the Lachrymal Gland and its Ducts:* a new ſubject, which ſhould not have been brought into the diſpute: but by his manner of treating it, we ſee how little he is diſpoſed to do me juſtice, and how readily he will expoſe himſelf to diſagreeable ſuſpicions, for the ſake of being thought a diſcoverer in anatomy. He tells us that *ſeveral of the moſt celebrated anatomiſts, particularly* Morgagni, Vater, Haller, *and* Zinn, *had exhauſted their patience to no purpoſe in queſt of ſuch ducts in the human ſubject, &c.* Then he comes to his diſcovery, viz. In the ſummer in 1753, (the ever memorable year ſtill) he ſought after theſe ducts, diſcovered their orifices, and introduced briſtles into them; and ſhewed this to his father, who always *mentioned it* in his lectures ſince that time, &c. &c.

By

By this time, I fancy none of my readers will be ſurpriſed when I tell them that I had done the ſame thing, and that I had taught it in every courſe of lectures ever ſince the year 1747. If it were neceſſary to prove a thing ſo notorious, I could produce many teſtimonies, particularly that of Mr. Symons of Exeter, who attended my firſt courſe of lectures, and of Dr. Smith of Oxford, who both remember that they ſaw me introduce briſtles into the ducts * of that gland in the human ſubject, and that they aſſiſted me in doing it.

Is it not very ſingular that the young ſtudent ſhould have made ſo many diſcoveries in the year 1753 at Edinburgh, and that all of them ſhould have been publiſhed before that time at my lectures in London? Is it not pretty plain how he came by them, when we are informed, that every winter there were ſome ſtudents at Edinburgh who had attended my lectures; and about this very time ſome of theſe gentlemen belonged to a ſociety, of which he was a member, inſtituted by the ſtudents for talking and diſputing upon medical ſubjects? Does it not agree with, and ſupport this inſinuation, that he publiſhed ſuch things as his own, after he had heard them from my mouth before a numerous audience, without doing me the juſtice of putting my name into a marginal note? It would have been but juſtice, if he had really and truly made the diſcoveries himſelf; and if he had, would not he have done ſo?

After all that has been ſaid and proved, the reader will ſurely be entertained with our Profeſſor's concluſion †, which I think a ſpecimen of

* I make no doubt but that all, or much the greateſt part of the tears come from the *glandula lachrymalis*. That they do, and that this gland, ſmall as it is, can ſecrete a conſiderable quantity of liquor in a ſmall ſpace of time, will appear from the following experiment which my brother exhibited to my ſatisfaction. He inverted the upper eye-lid in a living ſheep, and we could diſtinctly ſee the tears or water guſhing in conſiderable quantity from the orifices of the ducts of the gland. Every time the part was wiped dry, the guſh was very manifeſt, and when it was confined for a moment by preſſure, the ſucceeding guſh of water was in greater quantity, and more violent.

"† 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 con-"cluſion from theſe facts was altogether improper; and that he farther 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

of the moft eafy behaviour under fuch aukward circumftances, that has ever been exhibited to the public.

To the beft of my knowledge, I have ftated the facts with the moft perfect regard to truth. Were I, in my turn, to draw conclufions, I might perhaps be wrong. I will therefore leave that to others; and leave Profeffor Monro jun. to enjoy himfelf, and his difcoveries; and to fay and to print whatever he may think proper upon this difpute without moleftation. He has eftablifhed a character for ever with me, and I am refolved to take no farther notice of him.

CHAP.

" But even upon the fuppofition that thefe obfervations ufed by Dr. Hunter had not been " borrowed, and that his conclufion from them had been well founded; ftill there was no " reafon whatever for me to make the leaft mention of the Doctor, as I learned nothing from " him; for thefe and many other obfervations, with various experiments, were remarked " and fully explained, and the conclufion, that the lymphatics were a fyftem of abforbents, " was drawn from them in my treatife *(a)*, previous to my acquaintance with him, or " knowledge of his arguments.

" Were I, before quitting the fubject, in return for the profufion of the Doctor's good " wifhes for my fake, to offer him my beft wifh, it would be, that he had not attacked me at " all; for, by that means, he has forced me, contrary at leaft to my intention, if not to my " inclination, to bring to light many circumftances neceffary for my own defence, from which " Truth would not allow me to draw conclufions greatly to his honour: and for which " therefore he has himfelf only to reproach.

" If, however, the Doctor fhall ftill perfevere to alledge, that his caufe is not fo defperate " as I have reprefented it, it is to be expected he will endeavour to make this appear in a plain " way, by facts well vouched and conclufions fairly deduced from them.

" For if, inftead of thefe, he fhall anfwer truth by exclaiming againft me for telling it, " becaufe it happens to gall him; fhall wrangle about words and expreffions; fhall affect not " to comprehend, what the reft of the world may think but too plain; fhall again infult the " patience of the public by making prefumptions upon prefumptions, confeffing, at the fame " time, that there is no certain knowledge in the cafe *(b)*; in fhort, fhall anfwer facts by " fuppofitions; arguments and plain conclufions by evafion and perplexity, and an attempt " at a fort of wit, which, efpecially in an affair of this nature, muft ever recoil upon him " that ufes it: the difcerning part of the readers, I prefume, will allow, that I do the " Doctor no injuftice in concluding, that he gives up his caufe and filently avows his con- " viction; and that he is labouring to raife duft, in order to fcreen himfelf and get off, " like what ftory tells us of fome of the combatants of old, who, when worfted, efcaped " in a cloud."

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

" *(b)* As in Critical Review, page 531."

C H A P. VII.

An Examination of what Profeſſor Monro ſen. publiſhed as a Defence of his Son.

HAVING taken my leave of the ſon, I find myſelf under the diſagreeable neceſſity of ſettling ſome matters with the father, my old maſter in anatomy. He took an early part in the diſpute, as an anonymous author *; and if he had pleaded his ſon's cauſe with moderation and candour, he would have ſhewn a dignity of mind worthy of his years and of his rank in the profeſſion. But this was ſo far from being the caſe, that I have the mortification to find him led away by his paſſions to miſrepreſent facts, and deſcend to quibbling, in ſupport of his arguments and purpoſes; and this not only in that firſt attack, when he was leſs informed and his reſentment therefore keener, but on many ſubſequent occaſions, as I have been credibly informed, and particularly at his public lectures, when I ſhould think he muſt have known a little more of the grounds of the diſpute. It is true, that after what I have ſaid in the former part of this work, it may ſeem unneceſſary to take notice of what the ſenior Profeſſor has advanced in that anonymous performance; yet there being ſome indirect accuſations in it, which I could not ſo well bring into the former part, I hope the candid reader will indulge me with a little more of his time and patience.

In the firſt ſeven paragraphs our author gives an hiſtorical account of the young Profeſſor's diſcoveries and publications, to ſhew that he could not have learned any thing concerning the lymphatics from me. I have already given the hiſtory of the diſpute, with *proofs* of all the material points; and Profeſſor Monro ſen. has forfeited all reputation as an hiſtorian, by telling us, in the end of the firſt paragraph, that he *knows the facts relating to the preſent diſpute, and ſends a fair ſtate of them*; and then by aſſuring us in the ſixth paragraph, that *Dr. Monro went to London in abſolute ignorance of Dr. Hunter's having any particular opinion concerning lymphatics ;----attended his lectures, and was ſurpriſed when he heard Dr. Hunter teach the doctrine of lymphatics being abſorbents.* Dr. Black's letter ſhews what credit is due to theſe aſſertions.

I ſhall

* Critical Review for November, 1757. See Appendix, No. II.

I ſhall therefore paſs over this part, and indeed all that is contained in the letter, except the three laſt paragraphs, which I ſhall conſider, under the three following articles.

1. Why I do not anticipate others by an early publication.

2. How far the right belongs to him, who firſt puts a diſcovery into print.

3. How far it is true that I have *perpetual diſputes with Pott, Haller, Albinus, Monro, or perhaps twenty more, that will prove very troubleſome, and will at laſt redound very little to my honour.*

When Dr. Monro aſks why I do not anticipate others by an early publication, I own I do not perhaps underſtand the meaning of the queſtion. If he means to adviſe me to be ſilent on any thing, which I may think a diſcovery or improvement, till I have put it into print, leſt it ſhould be ſtolen from me, and occaſion a troubleſome diſpute; I am ſurely much obliged to him for ſuch affectionate attention to my peace of mind and reputation. But if he means that I ſhould print my obſervations before I am prepared; I cannot take his advice.

He that is in a hurry to publiſh diſcoveries, will often have occaſion to repent of his haſte. Reflection, and more favourable opportunities of making inquiries, will at length bring us back to truth, if we have been miſled; and will confirm and improve our inventions, if they be right. Had I printed my thoughts about the lymphatics in the firſt years of my reading lectures, they would have been more imperfect, becauſe the proof drawn from poiſonous abſorption had not then occurred to me; and I am not ſo partial to the doctrine at this day, as not to ſee that ſomething ſtill remains to be done. The argument drawn from extravaſated fluids paſſing readily into the lymphatics in dead bodies, is not as yet concluſive, though Dr. Monro has built ſo much upon it; for in this caſe there may be ruptures in the lymphatics, and the fluids may get into them by ſuch means, rather than by their abſorbing orifices. And this poſſibility will come nearer to probability, when we reflect, that the lacteals cannot, by any means now known, be injected or filled from the cavity of the inteſtine in the dead body: and if the lymphatics be ſimilar in other reſpects to the lacteals, we ought to preſume they are ſo likewiſe at their beginnings or abſorbing orifices.

But the great thing ſtill wanted upon this ſubject is to prove, by experiments made on living animals, that the lymphatics *do actually and certainly abſorb*. We muſt throw milk, or other coloured fluids into the cheſt, *abdomen*, and into the cellular membrane of the limbs in living animals; and open them after a proper time, in order to fee if the lymphatics be actually filled by thefe fluids. My brother and I have already made feveral experiments of this kind, as occafion offered: but the appearances have not as yet been clear and fufficiently fatisfactory. It will likewife be neceffary to watch morbid appearances, particularly in glandular difeafes, dropfies, *emphyfemata*, and in contufions with extravafated blood. Accurate obfervations and diffections of that kind would probably throw great light upon the fubject. By way of illuftration I fhall give a cafe, which will confirm our theory of the lymphatics, and at the fame time fhew that it may be ufeful in practice.

In the fpring 1759, a gentleman about 40 years of age, who had a very good conftitution, and who lived temperately, confulted me about an inguinal tumour. It was manifeftly a fwelling of one or more of the glands, nearly as big as a pullet's egg, and almoft perfectly indolent. It began without any known caufe, and increafed gradually. Though there was no probability, yet as there was a poffibility of fome infenfible venereal abforption, I advifed him to be put into a gentle mercurial courfe, and to wait the event. The fwelling became larger every day, was hardly at all painful, and at length had a plain fluctuation. He was fo much afraid of the lancet and cauftic, that my brother, who was his furgeon, left it to burft of itfelf. It did fo, and the difcharge, which was extraordinary great, was principally a thin watery fluid. From the indolence of the tumour, and from the immenfe difcharge for fome days, we fufpected it might come from the loins, or fome other internal part; but upon the moft accurate examination which we could make, this did not appear to be the cafe. His health was otherwife perfectly good; the fore healed flowly and gradually, without fhewing the leaft mark of a venereal taint; and the difcharge, which was always watery, was in fuch a quantity, that it wetted through every thing he could cover it with, in a very little time, even when the wound was to appearance almoft healed. Then the difcharge became lefs by degrees, and at laft ftopped fuddenly. The wound clofed at the fame time, and continued well ever after. But when the water began to flow in lefs quantity, the limb began to fwell; and as the ftream from the wound diminifhed, the bulk of the limb increafed. The fwelling of the leg was anafarcous, and it was general, and attended with no pain or inflammation, but with a confiderable degree of weaknefs, weight, numbnefs, and unfitnefs for motion. His general health was perfectly good, and the other leg quite found. The nature of the cafe feemed to be very clear, upon the fuppofition of lymphatics being abforabſorbents: thus, whatever had been the cauſe, the glands had been obſtructed, and, in conſequence of the ſuppuration, the principal lymphatics of the limb were ſo affected, that they could not tranſmit their contents to the thoracic duct: ſo that while the wound remained open, the abſorption went on, and the lymph was diſcharged that way, as the *ſaliva* ſometimes is by a wound in the cheek; but as that vent ceaſed, the abſorption was checked, and the whole leg neceſſarily ſwelled from the accumulation of extravaſated lymph. In this way I explained the caſe as well as I could to the patient himſelf; and told him farther, that as the wound had cloſed of itſelf, I could not doubt but that ſome paſſage ſtill remained open for the lymph, which in time would become more and more free: and that I apprehended nothing more was to be done but to uſe a good deal of friction, eſpecially upwards; and when he was in bed, to keep his leg higher than the reſt of his body. I obſerved to him likewiſe that he might try purging, ſweating, diuretics, bliſters, fomentations, &c. but that in my opinion he was principally to truſt to time and patience. However, I adviſed him to have a conſultation. To make the ſtory ſhort, Dr. Taylor was conſulted, and confirmed my opinion; and the event was likewiſe agreeable to it: for in about ſix weeks the leg was gradually reſtored to its full health and vigour.

I have often obſerved in women, who do not give ſuck, and in nurſes, after they leave off ſuckling, that the axillary glands become painful, ſwell, and ſometimes ſuppurate. Is not this owing to the acrimony which the milk has acquired by long ſtagnation in the breaſt, and affecting the gland through which it muſt paſs in abſorption? I have obſerved too that they are at the ſame time liable to little fevers of the intermitting kind, but very irregular in their return, which come on with a *rigor*, and go off with ſweat. Are not ſuch fevers raiſed by the abſorption of acrid milk?

In December 1760, a male ſubject was brought to me, which, from the appearance of the ſtump, I ſuppoſed to have died two or perhaps three weeks after an amputation below the knee. The fleſh of the ſtump ſeemed to have been in a bad condition, being putrid, and ſeparating from the bone. The lymphatic glands of that ſide, at the upper part of the thigh, the groin, and upon the iliac veſſels within the *abdomen*, were much ſwelled: and two of them, when cut into, were found compleatly ſuppurated: yet in no other part of the body, (which was injected and diſſected for a demonſtration of the blood veſſels at my lectures) could we obſerve the leaſt diſorder in the lymphatic ſyſtem. Was not this diſorder of thefe glands owing to the putrid abforption from the ftump? And perhaps, if we had known the circumftances of the cafe, we fhould have had reafon to believe that this abforption raifed and fed the fever which carried off the patient.

Difcoveries and improvements in the arts are not commonly brought to any tolerable degree of perfection in a little time; efpecially when they fall to the fhare of men who are much employed about other things, and when they require opportunities that feldom happen *; and with the beft opportunities, and when managed by the ableft men, it has been thought better to throw them out firft in a more private way, in order to take the opinion of other people, and hear the reafoning of thofe whofe prejudices may ballance the partiality of an author. The GREAT HARVEY proceeded upon this cautious plan in the difcovery of the

* For example, I have for many years taught the following new doctrine about luxations, viz. That when a diflocation is produced by violence in an healthy ftate of the joint, the capfular ligament is always lacerated, not fimply ftretched. I proved it to be highly probable from the anatomy of the joints, and from experiments made upon dead bodies; and fhewed that the difficulty of reduction, in fome cafes, does not depend on the imaginary contraction of mufcles, nor the impoffibility in others, on the imaginary infpiffation of the *fynovia*; but that in a fimple diflocation, the facility or difficulty of reduction may probably arife from the nature of the laceration; and that the impoffibility of reducing an old diflocation is owing to the union of all the lacerated with the neighbouring parts. In my lectures, I always fignified a defire of feeing fuch cafes. Mr. Gataker was kind enough, fome years ago, to gratify my curiofity upon one fuch occafion; and Mr. Thomfon lately did me that favour, in a cafe that proved, in a moft fatisfactory manner, every circumftance which I had advanced; at leaft as far as one cafe could prove any general doctrine. Surely men of fenfe muft think that it was right to give my thoughts and obfervations upon this fubject at my lectures. It was doing juftice to my pupils to give them every idea, however imperfect, that might be ufeful to them in their profeffion. Accordingly Mr. White of Manchefter, foon after he had attended my lectures, applied this doctrine to practice in treating diflocations; and with great fuccefs, as appears by his paper in the fecond volume of Medical Obfervations, publifhed at London; and in the letter which accompanied that paper, he did me all the juftice that I could expect.

On the other hand, whatever may be Dr. Monro's opinion, I cannot but think it was right to fufpend being an author upon this fubject, till I had actually diffected fome cafes of diflocations, and could prove the doctrine in a more unexceptionable manner. At length, this doctrine has been publifhed with advantage in the fame volume by Mr. Thomfon, who had too much fenfe and too much candour to fupprefs my name. But ftill it will be neceffary to examine a number of cafes, before it can be known with certainty whether the doctrine be in general well or ill-founded; and probably it will require the examination of a great variety of cafes to explain the various circumftances, which may occafion particular exceptions to the more general rule.

the circulation *. When Profeſſor Monro recommends example, does he think there is any one more reſpectable?

Another thing that kept me from writing upon the lymphatics, and upon ſome other ſubjects too, was the want of leiſure to examine what had been already ſaid of them by authors of reputation. If a man writes freely upon any ſubject, without knowing what has been ſaid by others, he riſques being made the object of ridicule or cenſure. I have ſeveral times met with my own obſervations in books, after having long believed them peculiar to myſelf. It muſt be the caſe with every man who is more entertained with nature than with books. The preſent diſpute has given me a freſh inſtance of it. Gliſſon having been quoted, I conſidered what he had advanced upon this ſubject, and had the pleaſure and mortification to find that he gave † exactly the ſame account both of tranſudation and of abſorption: ſo that I can no longer call it, what I really believed it to be, a *new opinion*, but *Gliſſon*'s *revived and confirmed*; for in him it was *mere opinion*, and accordingly was overlooked or rejected by his ſucceſſors, as happened to the doctrine of the circulation in the writings of Servetus and Cæſalpinus.

I come next to conſider our author's ſentiments as to this queſtion: *How far the right belongs to him who firſt puts a diſcovery into print?* I know of no act of parliament, indeed, about the matter; but will venture to ſay, that all men will think alike upon the queſtion. And I cannot drop the ſubject without ſmiling at the weakneſs of quoting precedents for eſtabliſhing injuſtice.

I am come to the laſt article; and every thing is of a piece. "Perpetual diſputes with Pott, Haller, Albinus, Monro, or perhaps twenty more, will prove very troubleſome, and will at laſt redound very little to his honour."

Profeſſor Monro has been pleaſed to put his ſon into very good company, but he muſt give me leave to make ſome diſtinction.

Pray how came the names of Haller and Albinus here, two names reſpected by all the world, and by nobody more than myſelf? To mortify me ſtill more, he might have added Morgagni and Winſlow; and

* See his dedication to the college. "Meam de motu & uſu cordis & circuitu ſanguinis ſententiam, E. D. D. antea ſæpius in prælectionibus meis anatomicis aperui novam, &c."

† Anat. Hepat. Lond. 1654. pag. 402. 448. & 456.

and then every body that does not know me, would have fuppofed that I had treated all thefe great men in a manner very unbecoming one who profeffes himfelf to be their admirer, not their rival. If, in a few inftances, I have ventured to diffent from fuch great authorities, my diffent proceeded from nothing but freedom of enquiry and conviction of mind. Surely then, if Profeffor Monro alludes to any thing of this kind, he does me an injury. Baron Haller, and Profeffor Albinus, ever diligent and fuccefsful in ftudy, have, in their late publications, anticipated me in fome things. I have faid fo in my public lectures, where it was well known what doctrines I had taught, and how long I had taught them. But I never made a difpute of thefe things with them, nor they with me: and I never did accufe either of them of treating me unfairly in any fenfe, nor ever had reafon to think that either of them had done fo. If then Profeffor Monro alludes to thefe anticipations, when he draws and publifhes a character with fuch freedom, he certainly acts unjuftly. When I think of this, and of his years, I feel fomething different from refentment, and would therefore wifh to have done with him: but in juftice to myfelf, I muft give an account of fome parts of my lectures, which mifreprefentation might ftile difputes, and which I prefume were pointed at by Profeffor Monro fenior.

C H A P. VIII.

Of the Difcovery of the MEMBRANA PUPILLARIS, and of the Infenfibility of Tendons, &c.

NAtural curiofity makes ftudents wifh to know fomething of the hiftory of anatomical difcoveries and opinions. It is not only entertaining but ufeful to fee by what purfuits and fteps an improvement was made: it gives clearer ideas of the fubject, makes a ftronger impreffion upon the memory, fhews the moft probable road to improvement in fimilar enquiries, and raifes emulation. Upon thefe accounts the hiftorical part is introduced into anatomical lectures: and if a teacher thinks he has made any improvements himfelf, he will naturally give an account of them for the reafons above-mentioned, and for another reafon too, which I need not mention.

In treating hiftorically of the *membrana pupillaris* in my lectures, I have generally

generally given the honour of the difcovery to Dr. F-----s S------s. At the fame time I did juftice to Wachendorf, Haller, and Albinus, and do fincerely believe that they all made the difcovery fairly. I did that juftice, indeed, to Albinus before he publifhed any thing upon the fubject, and told my audience, that when I paid my refpects to him in Holland, he not only fhewed me a preparation, but an engraved figure of that vafcular membrane, and then told me that he had known it many years *. However, upon inquiry, I underftood that Dr. S---- had found it out before any of thefe gentlemen, and had fhewn it to all his acquaintance who had any curiofity for fuch fubjects. 'Tis true, he never put it into print, but it is not the lefs true that he found it out, and that he has long had that and all parts of the eye finely prepared and preferved, and elegantly expreffed in drawings.

I have

* Speaking of this membrane (Academ. Annotat. lib. 3. pag. 82.) he fays, "Equidem "fcio, reperiffe me —— —— in infante illo, in quo primum vafa cryftallini implevi." Now, in juftice to this great anatomift, I muft declare that I believe this, both becaufe he afferts it, and becaufe I know from the circumftances, it was hardly poffible he could mifs taking notice of it in that child. I have always obferved, both in the human body, and in the quadrupede, that there is a great refemblance to one another in the veffels of the *capfula cryftallini* and of the *membrana pupillæ*. In an injected *fœtus* I always find both nearly in the fame ftate: if one be filled only with the blood that is driven before the injection, fo is the other; if one be filled partly with the injection, and partly with blood, the other is in the fame condition; if one by good fortune he finely and minutely filled by injection, the other is fo too; if one be hurt by extravafations, the other is commonly in the fame ftate; and when the *fœtus* is fo near its full time, that the one cannot be injected, neither can the other. Therefore I fay, I know that the *membrana pupillæ* was injected by Albinus in that child; and, if fo, I cannot think that it could efcape his intelligent and inquifitive eye.

I do not know that any perfon has taken notice of a circumftance relating to the veffels of the *membrana pupillæ* and of the cryftalline *capfula*, which I have obferved and can demonftrate by injections, both in the human *fœtus*, and in that of the quadrupede; and as I am upon the fubject, I will give the obfervation here in a few words.

The artery of the cryftalline *capfula* does not terminate at the great circle of that humour. Its fmall branches pafs that circle, and run a very little way on the arterior furface of the cryftalline humour before the points of the ciliary proceffes; then they leave the humour, and run forwards, fupported on a very delicate membrane, to lofe themfelves in the *membrana pupillæ*.

The artery, therefore, that paffes through the body of the vitreous humour, goes firft to the cryftalline *capfula* and then to the *membrana pupillæ*.

The *membrana pupillæ* receives two different fets of arteries, one larger from the *iris*, and the other much fmaller, but very numerous, from the cryftalline *capfula*.

When the *membrana pupillæ* exifts, there is a fine vafcular membrane all around, which paffes in the pofterior aqueous chamber, from near the edge of the cryftalline humour to the edge of the *pupilla*.

I have chofen this inftance to fhew what muft naturally be paffing at anatomical lectures, where there is a regard for truth: and it is the more to my purpofe, as it is *well known here* that I could have no motive but the love of truth for introducing this part of anatomical hiftory in my lectures. And was any perfon to reprefent fuch an anecdote as *a difpute with Albinus* or *Haller that would redound little to my honour*, it would only prove his ignorance of the fubject, or malevolence towards me.

But if it be right to do juftice in this way to another perfon, furely it cannot be wrong to do the fame fort of juftice to one's felf. Anatomifts of the faireft character in every age have done fo; and among living authors, Albinus and Haller are refpectable authorities, and a fanction to the practice.

In my lectures I commonly took notice that Profeffor Albinus had anticipated me in fome things, and particularly with regard to the fecond fet of teeth. It was a fact as well known here, and as capable of being proved as that I had read lectures ever fince the year 1746; and therefore it can never *redound to my difhonour*, nor could I on this account be reprefented, but by malevolence or ignorance, as having any difpute with Profeffor Albinus, who poffibly might, and I fuppofe, really did know thefe things long before me.

Ever fince I read lectures, I have been of opinion that the *periofteum*, *dura mater*, tendons, and ligaments, were altogether infenfible, or at leaft were endowed with a very fmall degree of fenfibility; and have always taught that doctrine. I confidered this queftion as may be feen in the MS of my lectures, when I treated of the *periofteum* and of the tendons; and particularly when of the ligaments. In the firft two years, viz. 1746 and 1747, I propofed the opinion with referve, well knowing the oppofition that prejudice raifes againft new doctrines; but ever fince the year 1748, I have fpoken of this opinion with more firmnefs. The account which I ufed to give was a little different at different times, as it was for the moft part given from memory, but was always to the following purpofe.

Periofteum. " Authors generally fuppofe the *periofteum* to be extreme-
" ly fenfible, and therefore plentifully fupplied with nerves *. But I be-
" lieve no anatomift can confirm this doctrine by diffections of the
" nerves; and fome cafes in furgery, which I have attended to, feem
" to

* Among thefe authors I might have mentioned Profeffor Monro fenior; and indeed, I believe, fometimes I did mention him.

"to difprove the fuppofition. I have often feen the *periofteum* cut and "fcraped, both on the fcull and in the limbs, without the patient's "feeming to be at all in pain. The argument drawn from the tendons "diffufing themfelves into the *periofteum*, and furnifhing it with nerves "for fenfation, will have no weight, if it be found as difficult to trace "nerves into the tendons as into the *periofteum*, and if the tendons them-"felves fhould be found to be infenfible. The argument taken from the "excruciating pain in fome nodes is inconclufive, as we frequently fee "large ones that are perfectly indolent. And the fuppofition that the "fenfibility of the *periofteum* was neceffary, confidering it as a guard to "the infenfible bone which it inclofes, cannot have any weight when "we reflect that both are inclofed by the fkin, one of the moft fen-"fible parts of the whole body. Whoever has felt a fmart pinch "of the fkin, will readily allow that there is no occafion for any thing "befides the fkin to keep us from wantonly bruifing or breaking our "own bones. I imagine that all the white, gliftening, inelaftick fub-"ftances, viz. *periofteum*, tendons, ligaments, and *dura mater*, are of "the fame nature; and therefore I fhall more fully confider this pro-"perty of fenfibility when treating of *ligaments*.

Tendons. "They have been fuppofed to be extremely fenfible. On "the contrary, I believe that in a healthy and natural ftate they have "little or no feeling. But we fhall take this matter into confideration "when we come to *ligaments*.

Ligaments. "Thefe too have been thought to be abundantly fupplied "with nerves, and to be fenfible in living bodies: yet, from my own "obfervation and reflection, I cannot help being of opinion that they "have hardly any fenfibility at all. Ligaments and tendons are made "of the fame fort of gliftening, inelaftick fibres; and differ only in "this refpect, that ligaments bind bones to bones, whereas tendons "bind mufcles to bones; fo that tendons are in effect ligaments. Thus "for example, in the knee it is the fame fort of fubftance that binds "the *mufculi extenfores* to the *patella*, and the *patella* to the *tibia:* fo "much the fame, that many parcels of fibres are common to both, "being continued over the furface of the *patella*, from the one, to "make a part of the other: yet it is called tendon above the *patella*, "and ligament below it; and between thefe, where the fibres cover "the furface of the bone, it is called *periofteum*. Tendons, ligaments, "*aponeurofes*, *periofteum*, and *dura mater*, are fo evidently of the fame "nature, that we muft fuppofe them endued with nearly the fame fhare

" of feeling *. In wounds of the hands and feet I have often feen " tendons partly divided or cut quite through, where the patient com- " plained of no remarkable pain, either at the time of receiving the " injury, or afterwards in the courfe of dreffing and wiping the fore. " I have known the *tendo achillis* torn quite afunder without pain. I " was confulted by an Italian gentleman, who had a gradual and par- " tial rupture of the *tendo achillis*. It happened to him at feveral " different times, and firft of all when he was walking in the mall. " He was always fenfible of fomething giving way at the time, but " never had any pain or any other fenfation, except a kind of weaknefs " and aukwardnefs in the motion of the part. The moft unexception- " able proof that I can give of this doctrine, is the cafe of Mr. Ser- " jeant Ranby. He was very near lofing his life, as moft of you may " know, from puncturing his finger with a pair of fciffors †. When " the part was laid open, the tendons of the *flexores* were expofed, and " appeared to be perfectly found. He cut them both through with his " own hand at one ftroke of a pair of fciffors; and when I afked him " about his feeling he affured me, that except the jarring of the inftru- " ment, which hurt him a little on the lips of the wound, he had no " more pain or feeling of any kind, than if he had cut a cord or any thing " that was not a part of his own body. He thought my opinion well " grounded, and faid his own cafe had given him the ftrongeft convic- " tion that tendons were infenfible, and that it was a circumftance which " he had particularly attended to. Now if we fuppofe a tendon pof- " feffed of any degree of feeling, we can hardly conceive any thing " more likely to give pain, than the bruifing pinch of a pair of fciffors, " in cutting fo thick and fo hard a fubftance.

" That the *dura mater* has little or no feeling I am convinced, from " having feen it, in two different patients, laid open by a crucial incifion " in

* When I was a ftudent in St. George's hofpital, in the year 1741 and 1742, I had the pleafure of attending Dr. Nicholls's courfes of anatomy. All his pupils muft remember his " divifion of ligaments into *inelaftic*, *inelaftic-degenerated*, and *elaftic*. The *inelaftic*, when ftretched, " breaks before it be fenfibly lengthened; is of a white colour, and has a peculiar gliften " like pearl or polifhed filver. It lofes that colour by boiling, and then becomes elaftic; " but grows fhorter in the fame proportion. Of this fubftance are tendons, *aponeurofes*, " the *periofteum*, *dura mater*, and many of the ligaments of the bones." Thefe ideas I firft received from that ingenious and elegant anatomift, and my own diffections and obfervation confirmed his doctrine. It was therefore very natural for me to conclude that all thefe parts in the body were *infenfible*, as foon as I had obferved that any one of them was fo.

† This was in the year 1748.

" in the operation of the trepan. Though both of them complained " very much when fcalped, neither of them fhewed any fymptoms of " being difturbed when the *dura mater* was cut.

" Whenever there has been extraordinary pain at the time of the " operation from bleeding in the arm, I take it for granted that the " lancet has touched a nerve, and not the tendon, as is commonly fup- " pofed: and we all know that the alarming fymptoms which fometimes " happen after bleeding, are oftener owing to fome circumftances of the " patient's conftitution, than to any unfortunate or unfkilful manner of " executing the operation.

" If we allow that tendons, ligaments, &c. have any, the fmalleft, " degree of fenfibility, we can eafily imagine from analogy that in- " flammation, and other circumftances of a difeafe, may raife that fen- " fibility to a very high degree. A certain degree of cold, we know, " exafperates pain; and heat mitigates it in all parts of the body. " There may be many other things lefs known that have the fame or " much greater influence on pain, and that may act in particular parts " of the body only, or be pretty equally diffufed over the whole.

" Without having taken the pains of actually tracing the hiftory of " the opinion, I think I can guefs how it came about that tendons, &c. " were thought to be very fenfible. In the firft place, it muft have " been obferved in furgery, that all fuch parts are apt to fuppurate in " an unkindly manner; to produce floughs generally before they gra- " nulate when expofed to the air; and that frequently dreadful in- " flammations and fevers come on from apparently flight injuries in " fuch parts. Thence it would feem natural to conclude, that thofe " parts are very irritable, fenfible, and full of nerves, the organs of " fenfation. Another thing that would naturally miflead people into " the opinion was this: all thefe parts were conftantly by anatomifts " called *nervous* parts. The Greeks gave us our anatomical language, " and ufed the word νευρον, not only to fignify what we now call a *nerve*, " but a *tendon* likewife and a *ligament*. It was the name given to the " *genus* of which there were three *fpecies*; and, for this reafon, the term, " *nervous parts*, in Greek writers, fignifies equally *nervous*, or *tendinous*, " or *ligamentous* parts of the body. Thus tendinous expanfions were " called *aponeurofes* by the Greek writers, and by the moderns too. " It is from the fame fource that we have taken our expreffion *a ner-* " *vous arm*, &c. fignifying finewy, or ftrong, and *nervous ftile*, expreffing " force and energy."

The first time I ever knew of Baron Haller's having any particular opinion upon the subject, was on paying a visit to Dr. Peter Shaw, who gave me Castell's *thesis*, printed in 1753; and some time after, I had the pleasure of reading the Memoire in the second volume of the *Acta Gotting*. published in the same year *. From that time, at my lectures, I treated the insensibility of tendons, &c. as my own observation, expressed my satisfaction at its being confirmed, and extended to other parts by Baron Haller; and after giving a general account of his observations with commendation and respect, I commonly made remarks to the following purpose.

REMARK 1. " After all I suspect that Baron Haller has perhaps gone " too far in concluding that these parts have absolutely *no sense* of feel- " ing. Experiments made on brutes cannot ascertain the fact. It must " be done by observations made on living human bodies, where we can " be informed of the more obtuse and gentle, as well as of the more " painful and exquisite sensations. The case of Mr. Ranby seems to " me more conclusive than any hitherto related of the human body; " and

* As I really have not, nor ever had, a dispute with Baron Haller, and as I believe he would upon every occasion be ready to do me justice, I would avoid every thing that could be construed disputing with him. Yet the reader would no doubt wish to see here in a few words how this opinion of insensibility stands as to time betwixt Baron Haller and myself.

I conceived the opinion in 1741 or 1742, when I attended St. George's hospital; taught it in my lectures in 1746; confirmed it by serjeant Ranby's case in 1748; and first learned that Baron Haller was nearly of the same opinion in 1753.

Baron Haller conceived the first opinion about the sensibility of tendons in May, 1748 (*a*); and the first experiment which he made on purpose to determine the point was in November, 1750 (*b*).

The first experiment which he mentions on the *periosteum* was made in November, 1750 (*c*).

His first upon ligaments was in December, 1750 (*d*).

The first observations which he made upon the insensibility of the *dura mater* were with Zinn (*e*), when they were making experiments upon the *corpus callosum*. Baron Haller says this was in January, 1748. Perhaps there may be some mistake or typographical error in this date; for from the following *exordium* of Zinn's *thesis*, we should naturally believe that the experiments had been made late in the spring, or in the summer, 1759. *Cum mihi scribendum esset tempore a dissectionibus anatomicis libero —— —— decrevi experimenta quædam capere circa corpus callosum, cerebellum & duram matrem.*

The first experiments that were made expressly in order to try the sensibility of the *dura mater* were in November, 1750 (*f*).

(*a*) Mem. sur la Nat. sensible, &c. Vol. I. p. 130. (*b*) Ibid. p. 116. (*c*) Ibid. p. 139. (*d*) Ibid. p. 140. (*e*) Ibid. p. 151, 152. (*f*) Ibid. p. 152.

" and yet, when all is laid together, I would not venture to pronounce " fuch parts to be abfolutely void of fenfe in a found ftate, or incapable " of more acute feeling in particular difeafes.

REMARK 2. " Baron Haller appears to be led into an error in furgery. " From his experiments and obfervations he feems to think that wounds " and punctures of tendons and ligaments, and penetrating wounds in " the joints, will be found to be attended with as little danger, and to " heal as kindly as fimilar wounds in flefhy parts. The danger of a " wound will not be found to be proportioned to the pain which the " patient feels when the wound is inflicted; that pain being momentary " for the moft part, or tranfient, and of little confequence. And cures " that happen in the body of the quadruped muft be applied with great " caution to the human body. From many cafes that have come under " my own obfervation, as well as thofe which I have learned from " others, I am fo well convinced of the great danger of punctures in " tendinous and ligamentous parts, more efpecially about joints, that I " think it my duty to put you upon your guard: and particularly " to caution you againft cutting into the cavity of a joint, unlefs " there be very urgent reafon. Sometimes, indeed, thefe wounds will " heal up very kindly; and fuch favourable cafes have made fome fur- " geons look upon them as not particularly dangerous; but more ge- " nerally, even when the habit appears to be healthy, they are followed " with violent pain and inflammation, and with a very bad fpecies of " fever, and with large fuppurations. At this time patients are often " delirious; often convulfed. Sometimes they are foon carried off by " the violence of the fever: fometimes death is more flow, as if it were " partly brought on by the fever, and partly by the immenfe difcharge. " And when they do recover, it is almoft always with the lofs either " of the limb, or at leaft of the motion of the joint: and generally " the whole habit is fo much vitiated, that it is a long time before " they recover perfect health *.

" Perhaps from the obvious ftructure of a joint one reafon may be " affigned for an inflammation being more painful and more mifchievous " in its confequences here, than in many other parts; there is a want of " room for the parts to fwell, and yet to be tolerably at their eafe. The " cavity of a joint is fo fully poffeffed in the natural ftate, and the li- " gaments fo tight and fo ftrong, that when the deep-feated parts inflame

" and

* Here cafes were mentioned.

" and ſwell, they compreſs one another in the ſame proportion: and that " the compreſſion of an inflamed part will aggravate pain, and give fury to " the diſeaſe, needs not be explained in a country where tight bandages " have been ſo happily exploded."

If all this ſhould be reckoned *a diſpute with Haller*, if it ſhould *prove very troubleſome, and at laſt redound very little to my honour*, as Profeſſor Monro gives out, anatomiſts muſt be very cautious in their improvements and communications.

But I have the pleaſure to find that Baron Haller has not taken his opinion of me from the two Profeſſors; for in the very laſt piece that I have ſeen of his writing, where he is treating of the diſpute about the ſenſible and inſenſible parts, and where he is complaining with great freedom of ſome other men, he mentions my name, and my opinion, in a very different manner *. And I am the more proud of the good opinion which he is pleaſed to entertain of me, as I have never taken any pains to clear myſelf of the aſperſion. On the contrary, I rather avoided a fair opportunity that fell in my way; and I did ſo, becauſe I felt myſelf mortified at the thought of being obliged to juſtify myſelf againſt an accuſation ſo ill grounded, and ſo ſtrongly marked with partiality.

C H A P. IX.

Of the Rupture, in which the *Teſtis* is in contact with the Inteſtine.

THE only diſpute, which Profeſſor Monro ſen. can poſſibly accuſe me of having with Mr. Pott, relates to that particular ſpecies of rupture, in which the inteſtine is found in contact with the *teſtis*; and, I preſume, is what the Profeſſor is afraid may redound to my diſhonour. I ſhall endeavour fairly to repreſent the caſe; and ſhall then moſt readily ſubmit to the judgment of the public.

Some time about the year 1748, Mr. Sharp aſked me in converſation, if, in diſſecting ruptures, I had ever found the inteſtine in the ſame bag, and in contact with the *teſtis*. I told him, I never had found it ſo, and did not think it poſſible. He ſaid he had met with it three times, if he

* Mem. ſur les Part. ſenſib. & inſenſib. Tom. IV. p. 37.

he was not very much deceived; but, that, in two of thoſe inſtances, he could not be ſo poſitive about the fact, becauſe the obſervation was made in performing the operation for the bubonocele on a living body. He did me the honour to deſire that we might examine together the firſt ruptured ſubject that either of us ſhould meet with. A few weeks after this a ſubject was brought to me, which was ruptured on both ſides. The ſize and ſhape of the tumour were almoſt exactly the ſame in both, and the protruded bowels were fallen down ar far as the loweſt part of the *ſcrotum*. I examined them in Mr. Sharp's preſence and under his direction; and as ſoon as the hernial ſac of the right ſide was laid open, we ſaw the *teſtis* lying bare, in the bottom of its cavity. The *tunica albuginea* and the naked *epididymis* were ſeen ſo diſtinctly, that there was no room for a moment's doubt. Then we diſſected the rupture of the left ſide; and there it was as indiſputable that the bottom of the hernial ſac was ſituated upon the outſide of the *tunica vaginalis propria*, or, in other words, that theſe two bags were diſtinct, and without any communication; and that the inteſtine in ſuch a rupture could not have come into contact with the *teſtis*, unleſs a laceration had been produced both in the hernial ſac, and in the *tunica vaginalis propria*. We therefore concluded that ſuch a laceration of thoſe bags had actually happened to the rupture on the right ſide, and that it muſt happen in all ruptures where the *teſtis* is found in contact with the inteſtine.

At this time both Mr. Sharp and myſelf were conſidering the parts concerned, only as they really are in ſound adult bodies: and whenever a rupture is produced under ſuch a ſtate of thoſe parts, it cannot be otherwiſe; that is, either the hernial ſac and *tunica vaginalis propria* muſt be diſtinct cavities, or the bowels muſt have forced their way through both bags, and thus have come into contact with the *teſtis*. Mr. Sharp taught this doctrine afterwards in his writings *, as I did in my lectures. I preſerved the two ruptures, and occaſionally ſhewed them to illuſtrate the doctrine in my lectures, and always told my audience what I have been now relating, as a piece of juſtice to Mr. Sharp.

* Critical Enquiry, Lond. 1750. pag. 3. " It is evident to me, that notwithſtanding " the *peritonæum* may at firſt fall down with the *viſcera*, yet in length of time it may alſo " be ruptured, becauſe I have found the *inteſtine* and *omentum* within the *tunica vaginalis* of " the teſticle, and in contact with the teſticle itſelf, which they could not poſſibly have " been, if they were inveloped in a portion of the *peritonæum*: however, this circumſtance " occurs but rarely, for we uſually find, &c."

Soon after this, Mr. Chefelden, who was then compofing his remarks on Le Dran's Surgery, faw thefe two ruptures, and defired to have a drawing, or rather indeed a fketch of them, which he engraved and publifhed. In the explanation of the figures, he did me the honour to mention me, and declared himfelf to be of the fame opinion, as to the manner in which the inteftine had got into contiguity with the *teftis* *.

In the latter end of the year 1755, when I firft had the pleafure of reading Baron Haller's obfervations on the *hernia congenita* †, it ftruck my imagination that the ftate of the *teftis* in the *fœtus* and its defcent from the *abdomen* into the *fcrotum* would explain feveral things concerning ruptures and the *hydrocele*, and particularly that obfervation which Mr. Sharp had communicated to me, viz. that in ruptures the inteftine is fometimes in contact with the *teftis*. I communicated my ideas upon this fubject to my brother, and defired that he would take every opportunity of learning exactly the ftate of the *teftis* before and after birth, and the ftate of ruptures in children. We were both convinced that the examination of thofe facts would anfwer our expectation, and both recollected having feen appearances in children, that agreed with our fuppofition, but faw now that we had neglected making the proper ufe of them.

In the courfe of the winter, my brother had feveral opportunities of diffecting fœtufes of different ages, and of making fome drawings of the parts; and all his obfervations agreed with the ideas I had formed of the nature of ruptures, and of the origin of the *tunica vaginalis propria* in the *fœtus*. But till thofe obfervations were repeated to his fatisfaction, and were fufficiently afcertained, he defired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the *teftis*, and of the fituation of the hernial fac, &c. I only put in this temporary caution, that I was then fpeaking of thofe things as they are commonly in adult bodies, and not as they are in the *fœtus*: and at laft, when I was concluding my lectures for that feafon in the end of April, 1756, with a courfe of the chirurgical operations, I gave a very general account of my brother's obfervations, and fhewed both the drawing of Fig. II. which was then finifhed, and the fubject from which it was made.

Some

* Le Dran's Operations, tranflated by Mr. Gataker, with Mr. Chefelden's remarks, Lond. 1749, page 463. " E, the fac of the *hernia inteftinalis* which had communicated itfelf to " the tefticle———The prefent cafes I had from Mr. Hunter, &c."

† Alberti Halleri Opufcul. Patholog. Laufan. 1755, 8vo. pag. 53, &c.

Some time in May or June following, Mr. Pott prefented me with his Treatife of Ruptures. In the preface I found that he had done me the honour of adding my name to a very refpectable lift; and I imagined that this compliment was meant as a very kind return for the refpect which I had wifhed to fhew him upon every occafion, and particularly for what had paffed between us fome time before, at a meeting for examining the nature of ruptures, and the ftate of the parts concerned: for, when he began to compofe his treatife (as I prefume, becaufe, though he faid nothing to me of fuch an intention, I foon after heard of it among his friends) he defired that we might examine thofe things in the firft proper fubject which I could apply to that purpofe. My brother diffected the parts on both fides of a body, in the fame manner as they were commonly prepared for my lecture, and fo as to demonftrate my ideas as clearly as poffible. We examined the parts with attention, both in the frefh fubject, and in fome preparations of *herniæ*; and thofe who have attended my lectures may imagine what I demonftrated, and what I faid. So far as I could judge, I had the fatisfaction of finding that we thought alike upon moft points.

In perufing the book itfelf, I was forry to meet with the following paffage, (page 13) " If the tefticles of a *fœtus* were down in the *fcro-*
" *tum*, dependent from the fpermatic chord, as they are in an adult,
" they would in fome poftures and difpofitions of the child at the time
" of parturition be very liable to be hurt; to prevent which, and poffi-
" bly for other reafons alfo, the tefticles of a *fœtus*, during its refidence
" in the *uterus*, lie within the *abdomen*, behind the *peritonæum*, defended
" by the bone.

" Soon after birth, when the lungs come to be diftended with
" air, and prefs on the diaphragm, when the mufcles of refpiration act,
" and thofe of the *abdomen* begin to fqueeze the contents of the belly,
" the tefticles are pufhed out through the apertures in the abdominal
" mufcle (called the rings) into the upper part of the *fcrotum:* this
" paffage of the tefticle from the belly into the *fcrotum*, I take to be the
" principal caufe of the ruptures of infants; for the ring or aperture being
" by this means dilated, a portion of caul or gut has an opportunity of
" flipping through, before the aperture has had time to contract itfelf
" again, and which protrufion will be forwarded by the continual efforts
" of the child in crying.

" This has always appeared to me to be the cafe-----"

I fay, I was forry to fee this paffage in my friend's performance, but took no notice of it to the author, when I thanked him for his prefent, or to any other perfon. The fubject appeared to me to be too delicate for converfation; and the reader will no doubt think fo, when I tell him in what light it appeared to me. The firft part is certainly erroneous with regard to the fact which is advanced; and the reafoning is not fo folid as might have been expected. *The teftes* of a *fœtus* do not lie within the *abdomen* till after its birth; they fall down before that period: and furely we muft allow that the *teftes* are more in danger from accidents after, than in the time of birth; and that, if their fituation was to be determined by *protection* and *danger*, they ought to be going up, juft at the time when they are coming down.

The fecond part of the paffage feems to be taken from Haller; at leaft it happens to be precifely what that author had publifhed in the preceding year. His words are " Neutram caufam (fc. herniarum) " exclufam velim, profundius tamen fæpe latere radices, ex his obferva- " tionibus adparebit, ex quibus conftat in ipfo fœtu non valde raro jam " natam herniam reperiri, vacuam equidem, fed quam ex minimis caufis " inteftino repleri oporteat.------In fœtu enim---teftes in cellulofa tela " lumborum fedent, proxime renum---Defcendunt inde---fenfim &---in " fcrotum adveniunt, femper retro peritonæum, &c. Caufa hujus pro- " greffus videtur in refpirationis vi, & in mufculorum abdominis poteftate " poni.---Herniarum, ni fallor, congenitarum, modus hinc elucefcit, quo " generantur. Patulus eft proceffus peritonæi, &c. Cum autem his in " corporibus teftes eodem cum inteftinis facco omnino contineantur, " nihil eft fingularis five inexpectati, fi ea in apertum faccum, a levi vi " depreffa fuerint----Annon id fuadet notiffima obfervatio, longe plerof- " que herniofos in ætate infantili id vitium contraxiffe----Nullum adeo " fere mihi dubium fupereft, quin prima in origine teftis in abdomine " fedeat, deinde vi refpirationis, clamoris & nixuum paulatim, &c." Now if the reader will confider that Baron Haller's *Opufcula Pathologica*, which contained this curious obfervation, were publifhed in the year preceding the publication of Mr. Pott's treatife, and were in every body's hands here *, he will not be furprifed that I felt fome uneafinefs for my friend, at reading the laft part of the paffage quoted, viz. " This has " always appeared to me to be the cafe."

Upon

* This work of Haller had not only been generally read in the original, and approved of here, but even its Englifh tranflation was advertifed on the 20th of February, 1756, that is, fome months before Mr. Pott's publication.

Upon reading a few pages more, I found that whether he had always been of that opinion, or had forgot that it was likewiſe Haller's, he could not then account for the contiguity of the *teſtis* and inteſtine in ſome ruptures. "The caſe quoted by Mr. Sharp," ſays he, page 21, "of the "inteſtine being found in contact with the teſticle being an accidental "thing, and to be ranked as ſuch, or as one of the *luſus naturæ*." Therefore I concluded that what I had taught at my lecture in the latter end of April, relating to the *hernia congenita*, could not have come to his knowledge before the firſt part of his book was printed.

My brother continued his inquiry, and by the autumn had aſcertained what I ſhall next preſent to the public in his name. Afterwards I ſhall reſume my narrative.

"Obſervations on the State of the *Teſtis* in the *Fœtus*, and on the *Hernia Cogenita*, by Mr. JOHN HUNTER.

"Until the approach of birth, the *teſtes* of the *fœtus* are lodged within the cavity of the *abdomen*, and may therefore be reckoned among the abdominal *viſcera*.

They are ſituated immediately below the kidneys, on the forepart of the *pſoæ* muſcles, and by the ſide of the *rectum*, where this inteſtine is paſſing down into the cavity of the *pelvis*: for in the *fœtus* the *rectum*, which is much larger in proportion to the capacity of the *pelvis*, than in the full grown ſubject, lies before the *vertebræ lumborum* as well as before the *os ſacrum*. Indeed the caſe is pretty much the ſame with regard to all the contents of the *pelvis*: that is, their ſituation is much higher in the *fœtus* than in the adult; the ſigmoeide flexure of the *colon*, part of the *rectum*, the greateſt part of the bladder, the *fundus uteri*, the fallopian tubes, &c. being placed in the *fœtus* above the hollow of the *pelvis*, in the common or great abdominal cavity.

At this time the ſhape or figure of the *teſtis* is much the ſame as in the adult, and its poſition or attitude is the ſame as when it is in the *ſcrotum*: that is, one end is placed upwards, the other downwards; one flat ſide is to the right, the other to the left; and one edge is turned backwards, the other forwards. But as the *teſtis* is leſs connected with the ſurrounding parts while it is in the loins, its poſition may be a little variable. The moſt natural ſeems to be when the anterior edge is turned directly forwards; but the leaſt touch of any thing will throw that edge either to the right ſide, or to the left, and then the flat ſide of the *teſtis* is turned forwards.

It is attached to the *pſoas* muſcle all along its poſterior edge, except juſt at its upper extremity. This attachment is formed by the *peritonæum*, which covers the *teſtis* and gives it a ſmooth ſurface, in the ſame manner as it invelopes the other looſe abdominal *viſcera*.

The *epididymis* lies along the outſide of the poſterior edge of the *teſtis*, as in older bodies, but is larger in proportion, and adheres backwards to the *pſoas*. When the *fœtus* is very young, the adheſion of the *teſtis* and *epididymis* to the *pſoas* is very narrow; and then the *teſtis* is more looſe, and more projecting: but as the *fœtus* advances in months, the adheſion of the *teſtis* to the *pſoas* becomes broader and tighter.

The veſſels of the *teſtis*, like thoſe of moſt parts of the body, commonly riſe from the neareſt larger trunks, viz. from the *aorta* and *cava*, or from the emulgents.

The artery riſes generally from the forepart of the *aorta*, a little below the emulgent artery; and often from the emulgent itſelf, eſpecially in the right ſide of the body; which may happen the rather, becauſe the trunk of the *aorta* is more diſtant from the right *teſtis* than from the left. Sometimes, but much more rarely, the ſpermatic artery ſprings from the phrenic, or from that of the *capſula renalis*. Beſides the artery which riſes from the *aorta*, or emulgent, &c. the *teſtis* receives one from the hypogaſtric artery, which is ſometimes as large as the other. It runs upwards from its origin, paſſing cloſe to the *vas deferens*, in its way to the *teſtis*. The ſuperior ſpermatic artery ſometimes paſſes before the lower end of the kidney. Both theſe arteries run in a ſerpentine direction, making pretty large but gentle turnings; both are ſituated behind the *peritonæum*, and both run into the poſterior edge of the *teſtis*, between the two reflected *laminæ* of that membrane, much in the ſame manner as the veſſels paſs to the inteſtines between the two reflected *laminæ* of the *meſocolon* or meſentery.

The veins of the *teſtis* are analogous to its arteries. The ſuperior ſpermatic vein (to begin with its trunk) riſes commonly in the following manner; on the right ſide from the trunk of the *cava* a little below the emulgent, and on the left ſide from the left emulgent vein. The reaſon of this difference between the right and left ſpermatic vein, no doubt, is becauſe the *cava* is not placed in the middle of the body: ſo that by the rule of ramification, which is obſerved in moſt parts of the body, the *cava* is the neareſt large vein of the right ſide, and the emulgent is the neareſt

neareſt large vein of the left ſide. But the difference is inconſiderable; and accordingly we ſometimes find the right ſpermatic vein coming from the right emulgent vein, and ſeveral other varieties, which, ſo far as I can obſerve, follow no preciſe rule. There is likewiſe a ſpermatic vein, which riſes from the internal iliac, and runs up to the *teſtis* with the inferior ſpermatic artery. Both the ſpermatic veins run behind the *peritonæum* with their correſponding arteries, and go into the poſterior edge of the *teſtis*, where they are loſt in ſmall branches.

The nerves of the *teſtis*, like its blood veſſels, come from the neareſt ſource; that is, from the abdominal plexuſes of the intercoſtal; eſpecially the inferior meſenteric *plexus*. They run to the *teſtis*, attending upon its blood-veſſels, and are diſperſed with them through its ſubſtance. The *teſtis* therefore, with reſpect to its nerves, may be reckoned an abdominal *viſcus*; and this obſervation will hold good, when applied to the full-grown ſubject, as well as to the *fœtus*: for thoſe branches of the lumbar nerves, which are commonly ſaid to be ſent to the *teſtis*, paſſing through the tendon of the external oblique muſcle, in reality go not to the *teſtis* itſelf, but to its exterior coverings, and to the *ſcrotum*.

The *epididymis* begins at the outer and poſterior part of the upper end of the *teſtis*, immediately above the entrance of the blood-veſſels. There it is thick, round, and united to the *teſtis*; as it paſſes down, it becomes a little ſmaller and more flat, and is only attached backwards to the *teſtis*, or rather indeed to its veſſels, for it lies looſe againſt the ſide of the *teſtis* forwards: and at its lower end it is again more firmly attached to the body of the *teſtis*; ſo that in the *fœtus* there is a cavity or pouch formed between the middle part of the *teſtis* and the middle part of the *epididymis*, which is more conſiderable than what is commonly obſerved in full-grown ſubjects. As the body grows, the *epididymis* adheres more cloſely to the ſide of the *teſtis*. The greateſt part of the *epididymis* is made up of one convoluted canal, which becomes larger in ſize and leſs convoluted towards the lower end of the *epididymis*, and at laſt is manifeſtly a ſingle tube running a little ſerpentine. That change happens at the lower end of the *teſtis*, and there the canal takes the name of *vas deferens*.

This duct is a little convoluted or ſerpentine in its whole courſe, but is leſs ſo as it comes nearer to the bladder; inſtead of running upwards from the lower end of the *teſtis*, as it does at a more advanced period of life, in the *fœtus* at this age it runs downwards and inwards in its whole courſe; ſo that it goes on almoſt in the direction of the *epididymis*, of which

which it is a continuation. It turns inwards from the lower end of the *epididymis*, under the lower end of the *teſtis*, and behind the upper end of a ligament or *gubernaculum teſtis*, which I ſhall preſently deſcribe; then it paſſes over the iliac veſſels and over the inſide of the *pſoas* muſcle, ſomewhat higher than in adult bodies; and at laſt goes between the *ureter* and bladder towards the *baſis* of the proſtate gland.

At this time of life the *teſtis* is connected in a very particular manner with the *parietes* of the *abdomen*, at that place where in adult bodies the ſpermatic veſſels paſs out, and likewiſe to the *ſcrotum*. This connection is by means of a ſubſtance which runs down from the lower end of the *teſtis* to the *ſcrotum*, and which at preſent I ſhall call the ligament, or *gubernaculum teſtis*, becauſe it connects the *teſtis* with the *ſcrotum*, and directs its courſe in its deſcent. It is of a pyramidal form; its large bulbous head is upwards and fixed to the lower end of the *teſtis* and *epididymis*, and its lower and ſlender extremity is loſt in the cellular membrane of the *ſcrotum*. The upper part of this ligament is within the *abdomen*, before the *pſoas*, reaching from the *teſtis* to the groin, or to where the ſpermatic veſſels begin to paſs through the muſcles. Here the ligament runs down into the *ſcrotum* preciſely in the ſame manner as the ſpermatic veſſels paſs down in adult bodies, and is there loſt. The lower part of the round ligament of the *uterus* in a *fœtus* very much reſembles this ligament of the *teſtis*; and may be plainly traced down into the *labium*, where it is imperceptibly loſt. That part of the *ligamentum teſtis*, which is within the *abdomen*, is covered by the *peritonæum* all around, except at its poſterior part, which is contiguous to the *pſoas*, and connected with it by the reflected *peritonæum*, and by the cellular membrane. It is hard to ſay what the ſtructure or compoſition of this ligament may be. It is certainly vaſcular and fibrous, and the fibres run in the direction of the ligament itſelf. It may be muſcular; and I am inclined to believe that it is in part compoſed of the *cremaſter* muſcle turned inwards, and running upwards to join the lower end of the *teſtis*. The following obſervations ſeem to render this hypotheſis probable.

In the hedge-hog the *teſtes* continue through life to be lodged within the *abdomen*, in the ſame ſituation as in the human *fœtus*; and they are faſtened by the ſame kind of ligament to the inſide of the *parietes* of the *abdomen* at the groin. Now, in that animal, I find that the lowermoſt fibres of the internal oblique muſcle, which conſtitute the *cremaſter*, are turned inwards at the place where the ſpermatic veſſels come out in other animals, making a ſmooth edge or lip by their inverſion; and that then

they

they mount up in the ligament to the lower end of the *teſtis*. Sometimes in the human body, and in many other animals, and very often in ſheep, the *teſtes* do not deſcend from the cavity of the *abdomen* till late in life, or never at all. In the ram, where the *teſtis* is come down into the *ſcrotum*, the *cremaſter* is a very ſtrong muſcle; and, though it be placed more inwards at its beginning, it paſſes down pretty much as it does in the human body, and is loſt on the outſide of the *tunica vaginalis*: but in the ram, whoſe *teſtis* remains ſuſpended in the abdominal cavity, I find that the ſame *cremaſter* exiſts, though it is a weaker muſcle; and inſtead of paſſing downwards, as in the former caſe, it turns inwards and upwards, and is loſt in the ligament which attaches the *teſtis* to the *parietes* of the *abdomen*, and which in this ſtate of that animal is about an inch and a half in length. In the human *fœtus*, while the *teſtis* is ſuſpended in the cavity of the *abdomen*, the *cremaſter* is ſo ſlender that I cannot trace it to my own ſatisfaction, either turning up towards the *teſtis*, or turning down towards the *ſcrotum*.

The *peritonæum*, which covers the *teſtis* and its ligament or *gubernaculum*, is firmly united to the ſurfaces of thoſe two bodies; but all around, to wit, on the kidney, the *pſoas*, the *iliacus internus*, and the lower part of the abdominal muſcles, that membrane adheres very looſely to all the ſurfaces which it covers. Where the *peritonæum* is continued or reflected from the abdominal muſcles to the ligament of the *teſtis*, it paſſes firſt downwards a little way and then upwards, ſo as to cover more of the ligament than what is within the cavity of the *abdomen*. At this place the *peritonæum* is very looſe, thin in its ſubſtance, and of a tender or gelatinous texture; but all around the paſſage of that ligament the *peritonæum* is conſiderably tighter, thicker, and of a more firm texture. When the abdominal muſcles are pulled up ſo as to tighten and ſtretch the *peritonæum*, this membrane remains looſe at the paſſage of the ligament, while it is braced or tight all round; and in that caſe the tight part forms a kind of border or edge around the looſe doubled part of the *peritonæum*, where the *teſtis* is afterwards to paſs. This looſe part of the *peritonæum*, like the *intro-ſuſcepted* gut, may, by drawing the *teſtis* upwards, be pulled up into the *abdomen*, and made tight; and then there is no appearance of an aperture or paſſage down towards the *ſcrotum*: but when the *ſcrotum* and ligament are drawn downwards, the looſe doubled part of the *peritonæum* deſcends with the ligament, and then there is an aperture from the cavity of the *abdomen* all around the forepart of the ligament, which ſeems ready

to

to receive the *teſtis*. This aperture becomes larger when the *teſtis* deſcends lower, as if the pyramidal or wedge-like ligament was firſt drawn down, in order not only to direct but to make room for the *teſtis* which muſt follow it. In ſome fœtuſes I find the aperture ſo large, that I can puſh the *teſtis* into it, as far as the tendon of the external oblique muſcle.

From this original ſituation within the *abdomen* the *teſtis* is afterwards moved to its deſtined ſtation in the *ſcrotum*. It is the more difficult to aſcertain the exact time of this motion, as we hardly ever know the exact age of our ſubject. According to the obſervations which I have made it ſeems to happen ſooner in ſome inſtances than in others, but generally about the eighth month. In the ſeventh month I have commonly found the *teſtis* in the *abdomen*, and in the ninth I have as commonly found it in the upper part of the *ſcrotum*.

At the abovementioned period, the *teſtis* moves downwards till its lower extremity comes into contact with the lower part of the abdominal *parietes*. By this time the upper part of the ligament, which hitherto was within the *abdomen*, has ſunk downwards, lies in the paſſage from the *abdomen* to the *ſcrotum*, and thus dilates that paſſage for the reception of the *teſtis*. The place where the ligament is moſt confined, and where the *teſtis* meets with moſt obſtruction in its deſcent, is the ring in the tendon of the external oblique muſcle: and accordingly I think we ſee more men who have one *teſtis*, or both, lodged immediately within the tendon of that muſcle, than who have one, or both, ſtill included in the cavity of the *abdomen*.

After the *teſtis* has got quite through the tendon of the external oblique muſcle, it may be conſidered as poſſeſſing its determined ſtation; though it commonly remains for ſome time by the ſide of the *penis*, and by degrees only deſcends to the bottom of the *ſcrotum*. And when the *teſtis* has deſcended intirely into the *ſcrotum*, its ligament is ſtill connected with it, and lies immediately under it, but is ſhortened and compreſſed.

Having now given an account of the original ſituation of the *teſtes*, of the time of their deſcent from the *abdomen*, and of the *route* which they take in their removal to the *ſcrotum*; I ſhall in the next place deſcribe the manner in which they carry down the *peritonæum* with them, and then explain how that membrane forms the ſac of the *hernia congenita* in ſome bodies, and the *tunica vaginalis propria* in others.

When

When the *teſtis* is deſcending, and when it has even paſſed into the *ſcrotum*, it is ſtill covered by the *peritonæum*, exactly in the ſame manner as when it was within the *abdomen*; and the ſpermatic veſſels run down behind the *peritonæum* there, as they did when the *teſtis* lay before the *pſoas* muſcle; and that *lamella* of the *peritonæum* is united behind with the *teſtis*, the *epididymis*, and the ſpermatic veſſels (beſides the *vas deferens)* as it was in the loins; and the *teſtis* is fixed backwards to the parts againſt which it reſts, and is unconnected and looſe forwards, as it was when in the *abdomen*. In coming down, the *teſtis* brings the *peritonæum* with it; and the elongation of that membrane, though in ſome circumſtances it be like a common hernial ſac, yet in others is very different. If we can imagine a common hernial ſac reaching to the bottom of the *ſcrotum*, and covered by the *cremaſter* muſcle, and that the poſterior half of the ſac covers, and is united with, the *teſtis*, *epididymis*, ſpermatic veſſels, and *vas deferens*, and that the anterior half of the ſac lies looſe before all thoſe parts, it will give a perfect idea of the ſtate of the *peritonæum*, and of the *teſtis* when it comes firſt down into the *ſcrotum*. The *teſtis* therefore in its deſcent does not fall looſe, like the inteſtine or *epiploon*, into the elongation of the *peritonæum*; but it ſlides down from the loins, carrying the *peritonæum* with it; and both itſelf and the *peritonæum* continue to adhere by the cellular membrane to the parts behind them, as they did when in the loins. This is a circumſtance which I think may be eaſily underſtood; and yet I ſhould ſuppoſe that it may not be ſo very intelligible, becauſe I find ſtudents very generally puzzled with it, and imagining that, when the *teſtis* comes firſt down, it ſhould be looſe all around, like a piece of the gut or *epiploon* in a common *hernia*. The ductility of the *peritonæum*, and its very looſe connection by a ſlight cellular membrane to the *pſoas*, and to all the other parts around the *teſtis*, are circumſtances which favour its elongation and deſcent into the *ſcrotum* with the *teſtis*.

It is plain from this deſcription, that the cavity of the bag, or of the elongation of the *peritonæum*, which contains the *teſtis* in the *ſcrotum*, muſt at firſt communicate with the general cavity of the *abdomen*, by an aperture at the inſide of the groin. That aperture has exactly the appearance of a common ſmall hernial ſac: the ſpermatic veſſels and *vas deferens* lie immediately behind it, and a probe paſſes readily through it from the general cavity of the *abdomen* down to the bottom of the *ſcrotum*. And if this proceſs of the *peritonæum* be laid open through its whole length on the forepart, it will be plainly ſeen to be a continuation of the *peritonæum*; the *teftis* and *epididymis* will be feen at the lower part of it, without their loofe coat, the *tunica vaginalis*; and the fpermatic veffels, and the *vas deferens* will be feen covered by the pofterior part of the bag, in their whole courfe from the groin to the *teftis*.

Thus it is in the human body, when the *teftis* is recently come down; and thus it is, and continues to be through life, in every quadruped, which I have examined, where the *teftis* is in the *fcrotum*; but, in the human body, the communication between the fac and the cavity of the *abdomen* is foon cut off: indeed I believe that the upper part of the fac naturally begins to contract, as foon as the *teftis* has paffed through the mufcles. This opinion is grounded on the following obfervation. I have feen an inftance, where, from the age of the *fœtus* and from every other mark, it was probable that the *teftis* was very recently come down, and yet the upper part of the fac was very narrow: I pufhed the *teftis* upwards, in order to fee if it could be returned; the attachments of the *teftis* eafily admitted of its afcent, and fo did the aperture in the tendon of the external oblique mufcle; but the orifice and upper end of the fac would not, by any means, admit of the *teftis* being pufhed quite up into the *abdomen*. However this may be, the upper end of the fac certainly contracts, and is quite clofed, in a very fhort fpace of time; for it is feldom that any aperture remains in a child born at its full time. The lower part of the fac remains open or loofe, even in the human fubject, through life, and forms the *tunica teftis vaginalis propria*, the common feat of an hydrocele. This contraction and obliteration of the paffage feems to be a peculiar operation of nature, depending upon fteady and uniform principles, and not the confequence of inflammation, or of any thing that is accidental: and, therefore, if it is not accomplifhed at the proper time, the difficulty of bringing about an union of the part is much greater; as in children who have had the fac kept open by a turn of the inteftine falling down into the *fcrotum* immediately after the *teftis*. This looks as if nature, from being baulked when fhe was in the humour of doing her work, would not, or could not fo eafily do it afterwards. I fhall readily grant that what has been advanced here as a proof of the doctrine, may be explained upon other principles. This at leaft is certain, that the clofing of the mouth, and of the neck of the fac, is peculiar to the human fpecies; and we muft fuppofe the final caufe to be the prevention of ruptures, to which men are fo much more liable than beafts, from their erect ftate of body.

What

What is the immediate cauſe of the deſcent of the *teſtis* from the loins to the *ſcrotum?* It is evident that it cannot be the compreſſive force of reſpiration, becauſe commonly the *teſtis* is in the *ſcrotum* before the child has breathed; that is, the effect has been produced before the ſuppoſed cauſe has exiſted. Is the *teſtis* pulled down by the *cremaſter* muſcle? I can hardly ſuppoſe that it is; becauſe, if that were the caſe, I ſee no reaſon why it ſhould not take place in the hedge-hog, as well as in other quadrupeds.

Why do the *teſtes* take their blood-veſſels from ſuch diſtant trunks? Thoſe phyſiologiſts, who have puzzled themſelves about the ſolution of this queſtion, have not conſidered, that in the firſt formation of the body, the *teſtes* are ſituated, not in the *ſcrotum*, but immediately below the kidneys; and that therefore it was very natural that their blood-veſſels ſhould riſe in the ſame manner as thoſe of the kidneys, but a little lower. The great length of the ſpermatic veſſels in the adult body will no doubt occaſion a more languid circulation, whch, we may ſuppoſe, was the intention of nature.

The ſituation of the *teſtis* in the *fœtus* may likewiſe account for the contrary directions of the *epididymis* and of the *vas deferens* in adult bodies, though theſe two in reality make only one excretory canal. In the *fœtus* the *epididymis* begins at the upper end of the *teſtis*; and it is natural, conſidering it as an excretory tube, that it ſhould run downwards. And it is as natural that the reſt of the tube, which is called *vas deferens*, ſhould turn inwards at the lower end of the *teſtis*, becauſe that is its moſt direct courſe to the neck of the bladder. Thus we ſee that in the *fœtus* the excretory duct is always paſſing downwards. But the *teſtis* is directed in its deſcent by the *gubernaculum:* and this is firmly fixed to the lower parts of the *teſtis* and *epididymis*, and to the beginning of the *vas deferens*, and thence muſt keep thoſe parts invariable in their ſituation with reſpect to one another: and therefore in proportion as the *teſtis* deſcends, the *vas deferens* muſt aſcend from the lower end of the *teſtis*; and it muſt, from the paſſage through the abdominal muſcles down to the *teſtis*, run parallel with the ſpermatic veſſels.

The *teſtis*, its coats, and the ſpermatic chord, are ſo often concerned in ſome of the moſt important diſeaſes and operations of ſurgery, particularly in the *bubonocele* and *hydrocele*, that their ſtructure has been examined and deſcribed by the ſurgeons, as well as by the anatomiſts, of every age. Yet the deſcriptions of the cleareſt and beſt writers upon

M 2 the

the fubject differ fo much from one another, and many of them differ fo much from what is obvious and demonftrable by diffection, that it would feem difficult to account for fuch a variety of opinions. The very different ftate of the parts in the quadruped, and in the human body, no doubt, muft have occafioned error and confufion among the writers of more ancient times, when the parts of the human body were defcribed from diffections and obfervations made principally upon brutes: and the circumftances in the ftructure of the parts, which are peculiar to the *fœtus*, having been imperfectly underftood, we may fuppofe, has likewife contributed to make perplexity and contradiction among authors.

Baron Haller, in his *Opufcula Pathologica*, has obferved that, in infants, fometimes the inteftine falls down into the *fcrotum* after the *teftis*, or along with it, and occafions what he calls the *hernia congenita*. In fuch a cafe the hernial fac is formed before the inteftine falls down, as that ingenious anatomift has obferved. There are befides two very peculiar circumftances in a rupture of this kind; the inteftine is always in immediate contact with the *teftis*, and there is no *tunica vaginalis propria teftis*. The ftructure of the parts in the *fœtus* explains, in the moft fatisfactory manner, both thofe circumftances, however extraordinary they muft appear to a man, who is only converfant with the ftructure of the parts in fubjects of a more advanced age: and indeed it is fo clear that it needs no illuftration. I may obferve, however, that the *hernia congenita* may happen, not only by the inteftine falling down to the *teftis* before the aperture of its fac be clofed, but perhaps afterwards: for when the fac has been but recently clofed, it feems poffible enough that violence may open it again.

It muft likewife be obvious to every anatomift, who examines the ftate of the *teftis* in children of different ages, that the mouth and neck only of the fac clofe up, and that the lower part of the fac remains loofe around the *teftis*, and makes the *tunica vaginalis propria*. Whence it is plain that this tunic was originally a part of the elongated *peritonæum*: and as that tunic is undoubtedly the feat of the true *hydrocele*, it is alfo plain that the *hernia congenita* and the true *hydrocele* cannot exift together in the fame fide of the *fcrotum*; for when there is a *hernia congenita*, there is no other cavity than that of the hernial fac; and that cavity communicates with the general cavity of the *abdomen*.

The obfervations, contained in the two laft paragraphs, occurred to my brother upon reading Baron Haller's *Opufcula Pathologica*, and gave rife

to

to my inquiries upon this ſubject. That the deſcriptions which I have given may be better underſtood I have annexed three figures that were carefully taken from nature.

The firſt figure repreſents the *teſtes* within the *abdomen*, in an abortive *fœtus* of about ſix months. All the inteſtines, except the *rectum*, are removed; and the *peritonæum* in moſt places is left upon the ſurfaces which it covers, ſo that the parts have not that ſharpneſs and diſtinct appearance, which might have been given to them by diſſection.

A The upper part of the object, covered with a cloth.

B B The thighs.

C The *penis*.

D The *ſcrotum*.

E The flap of the integuments, abdominal muſcles, and *peritonæum*, turned back over the right *os iliûm* to bring the *teſtis* into view.

F The flap of the ſkin and cellular membrane of the left ſide diſpoſed in the ſame manner.

G The flap of the abdominal muſcles and of the *peritonæum* of the left ſide turned back over the ſpine of the *os iliûm*. The lower part of this flap is cut away, in order to ſhew the ligament of the *teſtis* paſſing down through the ring into the *ſcrotum*.

H H The lower part of each kidney.

I The projection formed by the lower *vertebræ lumborum*, and by the bifurcation of the *aorta* and *vena cava*.

K The *rectum* filled with *meconium*, and tied at its upper part where the *colon* was cut away.

L That branch of the inferior meſenteric artery which was going to the *colon*.

M The lower branch of the ſame artery, which went down into the *pelvis* behind the *rectum*.

N The lower part of the bladder, that part of it which is higher than the *oſſa pubes* in ſo young a *fœtus* being cut away.

O O The hypogaſtric or umbilical arteries cut through, where they were turning up by the ſides of the bladder in their way to the navel.

P P The *ureter* of each ſide paſſing down before the *pſoas* muſcle and iliac veſſels, in its courſe to the lower part of the bladder.

Q Q The ſpermatic arteries running a little ſerpentine.

R R The *teſtes* ſituated before the *pſoas* muſcles, a little higher than the *inguina*. In this figure the anterior edge of the *teſtis* is turned a little

little outwards, to fhew the fpermatic veffels coming forwards to the pofterior edge of the *teftis*, in the duplicature of the *peritonæum*: which duplicature connects the *teftis*, inclofes its veffels, and gives it an external fmooth coat, much after the fame manner as the duplicature of the mefentery connects the inteftine, conveys its veffels, and gives it a polifhed covering.

The beginning of the *epididymis* is feen at the upper end of the *teftis*, from which it runs down on the outfide (and therefore in this view behind the body) of the *teftis*.

S S The *vas deferens* of each fide paffing acrofs, in a ferpentine courfe, from the extremity of the *epididymis* at the outfide of the lower end of the *teftis*, and then before the lower part of the *ureter*, in its way to the *veficula feminalis*.

T T What I have called the *gubernacula* or ligaments of the *teftes* in a *fœtus*. On the left fide this ligament is intire, fo that it is feen going down from the lower end of the *teftis*, through the ring of the mufcle, into the *fcrotum:* but on the right fide its upper and forepart is cut away, that the continuity of the *epididymis* and *vas deferens* may be feen; and no more of the ligament is exhibited than what is fituated within the cavity of the *abdomen*.

N. B. The lower part of the ligament, as it is feen in the right fide of this figure, lies fo loofe in the paffage through the mufcles, and is there fo loofely covered by the *peritonæum*, that, when the *teftis* is pulled up, more of the ligament is feen within the cavity of the *abdomen*, and then the *peritonæum* is made tight and fmooth at that place; but, on the contrary, when the *fcrotum* is pulled downwards, the lower part of the ligament is dragged fome way down through the paffage in the mufcles, and the loofe *peritonæum* is carried along with it; fo that then there is a fmall elongation of that membrane, with an orifice from the cavity of the belly, like the mouth of a fmall hernial fac, on the forepart of the ligament.

The Second Figure reprefents nearly the fame parts in a *fœtus*, fomewhat older, in order to fhew the ftate of the *teftes* when they have recently defcended from the *abdomen* into the *fcrotum*. The fmall inteftines are removed, and the large inteftines are left in their natural fituation.

A A The liver, in out-lines.

B B The thighs, unfinifhed.

C The *penis*.

D The

D The middle part of the *ſcrotum*; on each ſide of which the forepart of the *ſcrotum* is cut away, that the *teſtes* may be ſeen.

E E The two flaps of the ſkin and of the cellular membrane diſſected off from the lower part of the *abdomen*, and turned down upon the thighs.

F The *inteſtinum cæcum*.

G G The *appendicula cæci vermiformis*.

H The arch of the *colon*.

I The turn of the *colon* under the ſpleen.

K The *colon* paſſing down on the outſide of the left kidney.

L The laſt turn of the *colon*, commonly called its ſigmoeid flexure, which in adults is ſeated quite in the cavity of the *pelvis*.

M The beginning of the *rectum*.

N Part of the abdominal muſcles of the right ſide, with the ſmooth inveſting *peritonæum*, turned back over the ſpine of the *os iliûm*.

O O The lower part of the *obliquus externus* muſcle of the left ſide.

P The lower part of the *rectus* muſcle on the right ſide, turned outwards, and towards the left ſide, ſo that the epigaſtric artery is ſeen going to the inſide of that muſcle.

Q The forepart of the bladder.

R The *urachus*, as it is called.

S The crural veſſels coming into the thigh from behind the *ligamentum Fallopii*.

T The external appearance of the ſpermatic rope of the left ſide.

U The external appearance of the *teſtis*, when its *tunica vaginalis*, or proceſs of the *peritonæum*, is a little diſtended with air or water poured into it from the cavity of the *abdomen*.

V The right *teſtis*, brought fully into view by laying open the proceſs of the *peritonæum* in its whole length.

W The *epididymis* of the ſame ſide.

X X The ſpermatic veſſels.

Y The *vas deferens*. N. B. The *peritonæum* lies before the ſpermatic veſſels and *vas deferens*, or covers them within the *abdomen*; and its proceſs or elongation covers them in the ſame manner all the way from the abdominal muſcles downwards; ſo that if the inteſtine ſlips down after the *teſtis* in a *fœtus* it muſt be placed before the ſpermatic veſſels and *vas deferens*.

Z The *ureter*.

& The

& The remains of the *gubernaculum* or ligament which bound and conducted the *teſtis* to the *ſcrotum* *.

N. B. It is evident that part of the *peritonæum*, which in this figure, is carried down in the form of a hernial ſac to a little below the *teſtis*, lies before the *teſtis*, *epididymis*, ſpermatic veſſels, and *vas deferens*, and that it covers thoſe parts in the ſame manner as it covers the abdominal *viſcera*, viz. the poſterior part of the ſac, (ſuppoſing the ſac to be cut lengthways into two halves) is united with them, and gives them a ſmooth ſurface, while the anterior half of the ſac lies looſe before them, and may be removed to ſome diſtance from them, as when the ſac is diſtended with water.

The Third Figure repreſents the *teſtes*, &c. in the ſame ſubject; all the parts above the *oſſa iliûm* being cut away, and the abdominal muſcles and the bladder being turned downwards.

A A The thighs, unfiniſhed.

B The *penis*.

C The middle part of the *ſcrotum*, its lateral parts being removed to ſhew the *teſtes*.

D D The ſkin and cellular membrane of the *abdomen* turned down over the thighs.

E E Part of the abdominal muſcles and *peritonæum* turned down at each groin.

F F The *peritonæum* covering the *iliacus internus* muſcle of each ſide.

G The *inteſtinum rectum* filled with *meconium*.

H The bladder, with the umbilical artery on each ſide of it, turned a little forwards over the *ſymphyſis* of the *pubes*.

I I The ureters paſſing over the *iliac* veſſels to the *pelvis*.

K The right *teſtis* expoſed, as in Fig. II. V. W. XX. Y.

L The left *teſtis* incloſed in the proceſs of the *peritonæum*, See Fig. II. U.

M The ſpermatic veſſels of the left ſide, ſeen through the *peritonæum* which covers them, in their deſcent through the abdominal muſcles at the groin.

N The left *vas deferens* ſeen through the *peritonæum*, in its paſſage from the mouth of the ſac to the poſterior part of the bladder.

O The mouth or aperture of the proceſs of the *peritonæum*, whereby its mouth or cavity communicates with the general cavity of the belly. This aperture cloſes up, and the membrane becomes ſmooth

* The letter-engraver forgot to put the mark (&) of reference into the plate: it ſhould ſtand where a ſtrait line falling perpendicularly from the letter V would meet a ſtrait line drawn from E to D.

ſmooth at this place, when the *fœtus* grows a little older; unleſs when the gut falls down after the *teſtis*, and keeps it open. In that caſe it makes the mouth of the hernial ſac.

P The left epigaſtric artery branching upon the inſide of the *rectus* muſcle, which is here turned downwards and outwards. This artery is always ſituated, as in this figure, on the inſide of the mouth of the hernial ſac, or paſſage of the ſpermatic veſſels."

Thus far my brother. Now I reſume my narrative-----

In my autumn courſe of lectures, 1756, (and indeed in every courſe, which I have read ſince that time) I demonſtrated the principal things contained in my brother's account of the *teſtes* in the *fœtus*; and I particularly explained that ſpecies of rupture in which the inteſtine is found in contact with the *teſtis*. This circumſtance of the diſeaſe, which had puzzled Mr. Sharp and Mr. Cheſelden as well as myſelf, and which even Mr. Pott regarded as a *luſus naturæ*, was now rendered perfectly intelligible. The diſcovery was become the novelty of the time among ſtudents in London, and other inquirers after anatomical improvements; and many gentlemen of my acquaintance deſired to ſee the preparations which my brother had made; and among the reſt my friend Mr. Pott did us that honour, one day during that courſe of lectures. I was not preſent. My brother ſhewed him the preparations with great readineſs, and explained to him my hypotheſis of the contiguity of the inteſtine and *teſtis* in ſome ruptures. Mr. Pott ſaid nothing at that time of an intention to write upon the ſubject; but, ſome weeks afterwards, it appeared, by a public advertiſement, that he was ſoon to publiſh a treatiſe on that ſpecies of rupture. I was much ſurpriſed; however, I thought it proper to ſay but little, till I ſhould ſee how he treated the ſubject. The treatiſe came out in the month of February or March, 1757. It aſtoniſhed me, if poſſible, more than Profeſſor Monro's account of the lymphatics had done. It hardly contained one new idea. It was what any of my pupils might have written; (for the caſes given in the end, ſupported only an unconteſted fact) and yet neither my brother's name, nor mine was mentioned. It bore ſtrong marks of *ſecond-hand* obſervation, and of a *time-ſerving* hurry in the compoſition. I complained of this at my lectures: every perſon to whom I mentioned the ſubject expreſſed his ſurprize: and the authors of the Critical Review made ſome reflections which could not be pleaſing to Mr. Pott, and which, one would have thought, muſt have brought on ſome kind of juſtification.

I hope Mr. Pott can, and, if ſo, I think he ought to clear up theſe diſagreeable appearances. If he does it in a candid manner, he muſt allow that I have not wantonly ſought a diſpute with him: and if it ſhall appear that I have miſunderſtood or miſinterpreted any part of his conduct towards me, he ſhall not find me wanting in my endeavours to do him juſtice.

APPEN-

APPENDIX,

CONTAINING

What was publiſhed in the Critical Review, before Dr. Alexander Monro jun. wrote his *Eſſays Anatomical and Phyſiological*, &c.

No. I.

In the Critical Review for September 1757, *the Reviewers concluded their account of Dr. Monro's Treatiſe* De Venis Lymphaticis Valvuloſis, *with the following remarks.*

HAVING given a brief account of this performance, common juſtice demands that we ſhould make a few obſervations; which, perhaps will, in the reader's opinion, invalidate the Doctor's claim to this important diſcovery: obſervations, which were communicated to us by a perſon of probity, who engages to confirm the truth of his aſſertions by the teſtimony of above an hundred unexceptionable evidences. In the mean time Dr. Monro will pleaſe to obſerve, that we are altogether neutral in the diſpute, and ſhould be ſorry to incur the diſpleaſure of a gentleman, for whoſe extraordinary talents we have a ſingular veneration.

Doctor Monro ſays, the diſcovery he has made with reſpect to the lymphatics, was owing to experiments aſcertained four years ago. Now, Doctor Hunter has read public lectures in anatomy eleven years; and, in every courſe, made the following obſervations on the lymphatic veins: That whereas the moſt generally received opinion was, that they were a continuation of lymphatic arteries; he, on the contrary, believed them to be *the ſyſtem of abſorbing veſſels*, and that they began from all the internal and external ſurfaces of the body. This belief he founded on theſe reaſons: Every body allows, that all the ſurfaces of the body are bibulous, or provided with abſorbing veſſels, by which mercury applied to the ſkin, collections of water in the breaſt, belly, or in the cellular membrane, are occaſionally taken up, conveyed into the circulation, and ſtrained off again by ſecretion. That the lymphatic veins perform this office, feems probable, from the following remarks: I cannot inject them as other veins, by filling the arterial fyftem; fo that, in all probability, they are not continuations of the arteries. I have fometimes obferved in injecting, that they were immediately filled with wax, when the arteries burft, and the wax was effufed into the cellular membrane. This looks as if they took their rife from thofe cells, like the veins in the fpungy part of the *penis*. If they were continuations of arteries, why fhould they be fo plentifully provided with valves, which are not found in the other veins of the *vifcera?* But, the moft ftriking argument is the analogy between the lymphatics and lacteals. Thefe two fyftems are, to all appearance, the fame in their coats, in their valves, in their manner of ramifying, in their paffage through the lymphatic or conglobate glands, and in their termination, viz. in the route of the chyle. As they are perfectly fimilar, in every other refpect, we muft fuppofe them to be fo in their origin and ufe. The lacteals are known to begin from the furface of the inteftines, and to be the abforbents of thofe parts. There is no difference but the name. The fame veffels are called *lacteals* in the inteftines, and *lymphatics* in the other parts of the body. This doctrine explains the ufe of valves, in the lymphatics. In other veins, whether large or fmall, the fluid is fuppofed to move onwards by an *impetus* received in the arterial fyftem: but, the cafe is not the fame in veffels that fuck up a fluid from a furface. Thefe require valves, that every lateral preffure upon them may have the effect of an impulfe at the beginning of the canal, in driving the fluid on towards their termination. This doctrine of the lymphatics is farther confirmed by the abforption and progrefs of the venereal poifon. The lacteals were difcovered, traced, and their ufe afcertained, from the circumftance of a manifeft and particular colour in their contents, upon fome occafions at leaft. We have not the fame advantage, with refpect to the lymphatics: but, in them, what we cannot trace with the eye, we find out by the effects of this poifon. We know from obfervation, that this *virus* may be taken in at any particular part of the body, and thence diffufe itfelf over the whole conftitution. We muft fuppofe it abforbed by the fame veffels that abforb its antidote mercury, or any thing elfe that is carried into the mafs of blood by abforption. Thefe things being of a more inoffenfive nature, pafs unobferved; but, this poifon, from its irritating and deftructive quality, is apt to raife difturbance in its paffage, before it reaches far enough to mix with the blood. Hence the lymphatic glands, through which every abforbed liquor muft pafs, are fo often the parts firft affected

by

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 occurs) 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.

This is the very eſſence of Dr. Monro's treatiſe; and theſe obſervations have been publickly made by Dr. Hunter to his pupils, for the ſpace of eleven years. In the end of the winter, 1755-6, Mr. John Hunter made ſeveral injections of the lymphatic glands and veins with quickſilver; and, in the month of May, 1756, Mr. Riemſdyk finiſhed a fine drawing of them, with the receptacle and duct (from a preparation which the doctor ſtill preſerves) in preſence of many pupils and occaſional viſitants.

Dr. Reimarus, who attended Dr. Hunter's courſe in 1755 and 1756, quotes him for this doctrine of the lymphatics, in his theſis publiſhed this year at Leyden. Dr. Monro publiſhes his treatiſe at Berlin, without mentioning Dr. Hunter's name, though he attended his lectures with Dr. Reimarus; and though Dr. Hunter was particularly full on the ſubject of lymphatics during that courſe, on the ſuppoſition that his claim to this diſcovery would be anticipated. Among other things, he obſerved, that the lymphatics were raiſed by blowing or pouring mercury into the conglobate glands. We think Dr. Hunter has ſome reaſon to complain of this omiſſion, as Dr. Monro ſeems to have referred to every other author that ever treated on that ſubject.

No. II.

The Eighth Article of the Critical Review for November, 1757. The Letter was afterwards acknowledged to have been written by Dr. Alexander Monro, ſenior.

ALTHOUGH the authors of the Critical Review never intended to take cognizance of any production not previouſly publiſhed, they are nevertheleſs willing to inſert the following letter, as an inſtance of their candour; and this ſtep they are the more inclined to take, as they will have an opportunity to acquit themſelves of the imputation of partiality, which this gentleman endeavours to fix upon them. Neuters they

they ſtill profeſs themſelves to be in the diſpute between Drs. Hunter and Monro, as far as conviction, and the right they have aſſumed to themſelves to give their opinion of all literary productions, will allow them to be neuters: when they preſume to judge in any diſpute, they hope the unprejudiced part of mankind will allow them to be unbiaſſed in their deciſion.

That they might throw all the lights they could acquire on this unfortunate controverſy between two gentlemen of merit, for whom they have all due regard, they no ſooner received the following letter, than they had recourſe to Dr. Hunter for a real ſtate of the facts, which gave riſe to the conteſt; and though he could not be ſuppoſed to anſwer paragraph by paragraph a paper which he had not ſeen, he has furniſhed us with a detail of ſome incidents and remarks, which in juſtice to him we ſhall inſert at the end of the letter; and we cannot help thinking that they will not only ſerve as an anſwer to the letter, but alſo terminate the diſpute.

To the authors of the Critical Review.

GENTLEMEN,

1. "After giving an account of Dr. Monro junior's treatiſe *De venis* " *lymphaticis valvuloſis*, inſerted in your Review of September laſt, you " add ſome obſervations and reflections which have been thought inju- " rious to Dr. Monro; but as you declare yourſelves altogether neutral " in the diſpute, you will do him juſtice by inſerting immediately ſome " obſervations of a friend of Dr. Monro's, before it be too late, to pre- " vent the prejudices which your reflections may have occaſioned againſt " him. As he is at preſent abroad proſecuting his ſtudies, I thought it " my duty, as a friend, who knows the facts relating to the preſent diſ- " pute, to ſend you a fair ſtate of them.

2. " Dr. Monro, in winter 1752-3, attempting to fill the *epididymis* " with quickſilver, in the manner directed by his father both in medi- " cal eſſays, vol. 5. art. 20. § 29. and in a manuſcript wrote in the " year 1747, on the method of diſſecting and of making preparations fit " for a regular courſe of anatomy, obſerved in ſome of his experiments " that the quickſilver returned by the lymphatic veſſels without entering the " ſanguiferous veins, he wondered how or from whence this happened*; " he ſhewed the preparation to ſeveral of his acquaintance, and was very " importunate with them to give their opinions what this phænome-

" non

* De vaſis lymph. valvul. p. 1.

" non depended on. Not ſatisfied with their anſwers, he made numer-" ous more experiments, and conſulted all the books on this ſubject he" could procure. More than four years ago, I, with many others, ſaw" the preparations, which led him to the general doctrine of the lym-" phatics being a ſyſtem of abſorbents.

3. " Dr. Monro, having the deſign of taking the degree of doctor in" the univerſity of Edinburgh, early ſet about compoſing ſuch a diſſer-" tation as the laws of that univerſity require candidates for this degree" to publiſh and defend, and prepared the one *De teſtibus & ſemine in*" *variis animalibus*, in which the treatiſe on the lymphatics was includ-" ed, but the part relating properly to the *teſtes* being rather longer" than moſt ſuch inaugural diſſertations, he was prevailed on to with-" hold from the preſs the part treating particularly of the lymphatics," but allowed Dr. Black of Glaſgow, Dr. Reimarus *, and ſeveral" others of his friends, the peruſal of it.

4. " Before Dr. Reimarus ſaw it, I read it, and ſo far as I can re-" member, the arguments and experiments were the ſame as are con-" tained in the treatiſe *De venis valvuloſis lymphaticis*, which he publiſhed" at Berlin in the beginning of the year 1757, and about which the" preſent diſpute is.

5. " In October 1755, Dr. Monro's diſſertation *de teſtibus*, &c. was" printed and publiſhed at Edinburgh. Some hundreds of copies were" given to ſtudents, and ſent to anatomiſts in moſt countries in Europe." ---In this diſſertation he promiſes a treatiſe on lymphatic veſſels in" general---he deſcribes and gives figures of the inſtruments he made" uſe of in his experiments---tells what are his injecting materials, and" how the injection is to be made---gives ſeveral figures of the lymphatic" veſſels of the ſpermatic chord filled with quickſilver---and in ſhort" gives the ſubſtance of his general ſyſtem of lymphatics.

6. " Soon after the publication of this diſſertation, Dr. Monro went" to London in abſolute ignorance of Dr. Hunter's having any particular" opinion concerning lymphatics. Immediately upon his arrival he" waited on Dr. Hunter, and gave him a copy of his diſſertation, at-" tended his lectures, and was ſurpriſed when he heard Dr. Hunter" teach the doctrine of lymphatics being abſorbents.---In ſummer 1756," he went abroad for his further improvement, and at the intreaty of

" ſome

* See Reimarus's theſis, page 3, note *g*.

" ſome friends at Berlin, to whom he ſhewed his treatiſe on the lym-
" phatics, he publiſhed it there in the beginning of 1757.

7. " Let every reader judge from this ſimple narrative of facts, the " truth of which can be proved by the teſtimony of hundreds, to what " or to whom Dr. Monro owed his knowledge of the lymphatics being " a ſyſtem of abſorbents. Could it be to Dr. Hunter?

8. " You may perhaps ſay, that whatever way Dr. Monro had his " knowledge, Dr. Hunter has a right to the honour of the diſcovery, " for he has publickly read lectures in anatomy eleven years, in every " courſe of which he made the obſervations inſerted in your Review, " which are ſaid to be the eſſence of Dr. Monro's treatiſe; and you ap- " peal to Dr. Reimarus's theſis, where Dr. Hunter is quoted for his " doctrine of the lymphatics.

9. " I dare ſay your obſervator has given Dr. Hunter's doctrine very " full, as it is now, but as you tell us that Dr. Hunter was particularly " full on the ſubject of lymphatics during that courſe, to wit, in the win- " 1755-6, that is, after he had Dr. Monro's diſſertation *de teſtibus*, " and had converſed with Dr. Reimarus, I muſt beg leave to doubt, " whether in the preceding years Dr. Hunter was ſo explicit as your " obſervator repreſents.

10. " I ſhall ſurpriſe you in the following aſſertion, that your obſerva- " tions are neither the eſſence of Dr. Monro's doctrine, nor indeed a " ſufficient proof of the general doctrine of the lymphatics being ab- " ſorbents.

11. " Dr. Hunter is ſaid to believe the lymphatics to be the ſyſtem of " abſorbing veſſels, and aſſerts that every abſorbed liquor muſt paſs " through the lymphatic glands.---Dr. Monro proves, that the lympha- " tics are a ſyſtem of abſorbing veſſels, but allows that only a portion of " abſorbed liquors paſſes into the lymphatics, while the inhalant branches " of the ſanguiferous veins take alſo a ſhare of the abſorbed liquors *.

12. " You

* Dr. Hunter not only believed the lymphatics to be the ſyſtem of abſorbing veſſels, but gave very ſufficient reaſons for that belief. Vide Critical Review, No. XX. 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 inhalant branches for the ſame purpoſe, has been ſuppoſed: but nature would hardly form two ſyſtems for the ſame operation. Such a ſuppoſition is inconſiſtent with the ſimplicity, uniformity, and perfection of her works.

12. "You and your obſervator both have omitted the principal, I had "almoſt ſaid the only convincing proofs which Dr. Monro has brought of "the lymphatics being a ſyſtem of abſorbents, to wit, his numerous ex-"periments of throwing quickſilver into different organs; and of the "lymphatics being filled when parts are macerated in water, and of "their being filled in injecting the excretories of glands, and many more "ſuch experiments, as well as morbid caſes, and the effects of medi-"cines mentioned every where through his little book.

13. "I ſhould have thought it incumbent on you and your obſerva-"tor to have told us of numerous ſuch experiments done by Dr. Hunter, "previous to Dr. Monro's having wrote this treatiſe on the lymphatics, "before you had endeavoured to rob Dr. Monro of any little honour "there may be in a diſcovery of this kind, or before throwing out any "indiſcreet hints of his having ſtolen it from Dr. Hunter *.

14. "The arguments, as well as facts, mentioned by your obſerva-"tor in proof of the doctrine, are all taken notice of in books, and "were generally known long ago, and therefore if Dr. Hunter in his "lectures related no new experiments, nor ſhewed any preparations that "had not been made by Nuck and others, I can ſee no neceſſity Dr. "Monro was under of taking any notice of Dr. Hunter; he might in-"deed have mentioned him as an old maſter, but he could never quote "him as one from whom he had learned any thing new relative to the "lymphatics. Perhaps at this diſtance from London I may be doing "great injuſtice to Dr. Hunter, he may have done numerous experi-"ments, and made many preparations before the year 1754, though "your obſervator has omitted them; but as I hear Dr. Hunter intends to "publiſh ſomething on this ſubject, we ſhall then know from himſelf "what experiments he had made, and how far he had proſecuted "the diſſection of the lymphatic veſſels before Dr. Monro wrote his "treatiſe.

15. "Why does your obſervator quote Reimarus for having attended

O "Dr.

* That Dr. Hunter has made numerous experiments, is not to be doubted; but ſurely we were under no obligation to particularize theſe experiments, eſpecially as the arguments we borrowed from his lectures, were, in our opinion, concluſive. But, granting he had made no experiments, he firſt declared that the lymphatics were the ſyſtem of abſorbents: he ſupported this declaration with ſolid and ſatisfactory reaſons, and therefore he has a good claim to the diſcovery. He may plead that thoſe experiments mentioned by Dr. Monro were no other than a ſuperſtructure built upon his foundation; and doubtleſs had reaſon to expect that Dr. Monro would have taken notice of him in his treatiſe publiſhed at Berlin.

" Dr. Hunter's lectures along with Dr. Monro, and for having menti-
" oned in his thesis Dr. Hunter's teaching this doctrine, without telling
" us at the same time that in the very same note Dr. Reimarus tells us
" that he had read Dr. Monro's treatise before he came to London?

16. " Why does your observator make Dr. Hunter to suppose that his
" claim to the discovery would be anticipated, but because Dr. Monro
" had already done it in his inaugural dissertation, where he has the fol-
" lowing passage: *Hæc inquam, inter alia, argumenta non levia suppedi-*
" *tarunt vasa lymphatica valvulosa per totum corpus, venarum absorbentium*
" *systema esse, neque, uti vulgo fertur, ab arteriarum surculis emanare. Sed*
" *omnia nunc, quæ de hac re disputari possunt, proponere, longe ultra talis*
" *dissertationis limites excurreret: & multo aptius de iis, de eorum scilicet*
" *origine, fabrica, agendi ratione & usu, si quando per otium licuerit, seor-*
" *sim agetur.* Dissert. inaugur. *pag.* 55, 56.

17. " After reading this quotation, I suppose our readers will be a
" good deal surprised at your observator's having thrown out any hint of
" Dr. Monro's having clandestinely stole any thing from Dr. Hunter,
" especially when he is told that a full year and a half elapsed be-
" tween his promising the separate treatise on the lymphatics, and his
" publishing it; and that Dr. Hunter knew this, and was in the mean
" time writing and publishing on other subjects, in which no body could
" have anticipated him for want of the drawings with which they are
" illustrated.

18. " What Mr. John and Dr. Hunter have done since the beginning
" of the year 1755, is out of the present question; nor do I see what
" the design can be of mentioning the preparations made by them in the
" year 1756, unless it be to anticipate whatever shall be in the treatise
" and figures which Dr. Monro promises at the end of the one *de vasis*
" *lymphaticis valvulosis*, and to claim the honour of whatever may be
" contained in it. Since Dr. Hunter's figures were made more than a
" year ago, why don't he prevent his rival by an early publication of a
" treatise on the lacteal and lymphatic vessels, and the lymphatic glands,
" for Dr. Monro's has been ready some time, and will probably be soon
" published *?

19. " Dr.

* No person has a right to ask, why does not Dr. Hunter publish his discoveries? He may be so engaged in other more necessary avocations, that he cannot spare time sufficient to superintend a publication that requires accuracy and precision: perhaps he postpones publication until he shall have made further progress in his researches, and brought his dis- coveries

19. "Dr. Hunter keeps himſelf unneceſſarily in a diſagreeable ſitua-
" tion by the diſcoveries or improvements publiſhed by other people, be-
" ing claimed by him or his friends as of right pertaining to him. Per-
" petual diſputes with Pott, Haller, Albinus, Monro, or perhaps twenty
" more, will prove very troubleſome, and will at laſt redound very little
" to the doctor's honour. Rudbeck appealed to ſcholars and friends
" for his having knowledge of the lymphatic veſſels before Thomas
" Bartholin publiſhed his deſcription of them, but could not perſuade
" anatomiſts to take the honour of the diſcovery from Bartholin.---Van
" Horn called his ſcholars to witneſs, and had the declaration of a very
" great man, Swammerdam, an eye-witneſs, in his favour; ſtill howe-
" ver Pecquet is regarded as the diſcoverer of the receptacle of the chyle
" and thoracic duct, and De Graaf is eſteemed for his treatiſe on the or-
" gans of generation *.

20. " Dr. Hunter has ſtrong calls to avoid theſe inconveniencies to
" eſtabliſh his own honour, and to do ſervice to the community by
" publiſhing the improvements and diſcoveries which he thinks he has
" made in ſound or morbid bodies. He has the example of the greateſt
" men in the anatomical way for this practice. If he neglects to follow
" it, he may poſſibly find as few to acknowledge their having profited
" by him, as he hitherto owns himſelf to have learned from others, in
" either his writings or lectures."

No. III.

The Ninth Article of the Critical Review for November, 1757.

Facts relating to the Diſpute between Dr. Hunter and Dr. Monro.

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 method filled the whole *epididymis*, and the

O 2 tubes

coveries to perfection, being loth to follow the examples of thoſe writers on anatomy, who from an eager deſire of ſeeing themſelves in print, have raſhly uſhered into the world productions that were imperfect and erroneous.

* If Rudbeck and Van Horn were defrauded of the diſcoveries they had made, they certainly were injured, and had reaſon to complain. But a piece of injuſtice in one inſtance will never warrant iniquity in another: theſe may be precedents, but not a juſtification of any future fraud or plagiariſm. Dr. Hunter never had any diſputes with Haller or Albinus: thoſe he has had with other authors have been his misfortune, not his fault. a man who finds himſelf injured, will naturally complain.

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 become gradually more turgid and heavy for ſome time, after the external parts were completely filled.

I ſhewed this preparation next night at my public 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 Edinburgh, by a letter from Dr. Garrow, phyſician at Barnet, ſome time in the ſame month.

In ſome ſuch time as a week or fortnight after this firſt public demonſtration, my brother made the trial, and ſucceeded. He ſhewed me the *teſtis* opened, and the tabular internal ſubſtance very generally filled with mercury. This preparation, which I ſtill preſerve, I ſhewed at my public lecture that very evening, with marks of being pleaſed with the diſcovery. In the next courſe of lectures, viz. Feb. *&c.* 1752, and in every courſe ſince that time, I have ſhewn the ſame, and ſome other preparations of the ſame kind; and always gave the hiſtory of the diſcovery, to avoid taking that ſhare of it from my brother which belonged to him.

Dr. Alexander Monro jun. printed the ſame diſcovery firſt in the Edinburgh eſſays, vol. 1. pag. 396, in 1754, and then more fully in his inaugural theſis in October 1755, without taking any notice of what I had done upon the ſame ſubject. Upon inquiry I found that he had injected the internal *tubuli* in the latter end of January, or in February 1752; that is, two or three months after I had publiſhed it at my lectures, and after Dr. Garrow's letter above-mentioned to Dr. Donald Monro his brother. This is atteſted by Dr. Donne, who was, that winter, a ſtudent at Edinburgh, and a companion in anatomical ſtudies with Dr. Alexander Monro jun.

Here you will obſerve it admits of no diſpute that I, or my brother, was the firſt who injected, and publiſhed the injection of the *tubuli teſtis*, and that Dr. Alexander Monro jun. was the firſt who printed it,

I wiſh it were poſſible to prove this negative, that he did not learn it of me at ſecond hand: I wiſh it for his ſake, becauſe it would clear him, and take nothing from me. But conſidering that letter of Dr. Garrow, and the conſtant intercourſe between the ſchools of anatomy at London

and

and Edinburgh, the presumption must always be against him. Such are the facts concerning the first subject in dispute between us: with regard to the lymphatics, the facts are as follow.

Ever since I first read anatomical lectures, in 1746, among other things a little out of the common way of thinking, I have advanced the doctrine of the lymphatics being the system of absorbing vessels, and have supported my opinion by such arguments and experiments as are mentioned in your Review of last September. This appears by the MS. *syllabus* of my lectures, which I have used in public from the beginning, by many MSS. of my lectures in the hands of those who have studied with me, and by the general testimony of those who have done me that honour. I have many vouchers in my possession from gentlemen who have attended my lectures, and I appeal particularly to the following gentlemen, who are all professors or readers of anatomy now living. Dr. Collignon, professor of anatomy at Cambridge; Dr. Smith, reader of anatomy at Oxford; Mr. Hamilton, professor of anatomy at Glasgow; Mr. Cleghorne, reader of anatomy at Dublin; Mr. Watson, reader of anatomy in London; Mr. Galhie of London, and demonstrator or dissector for the professor of Cambridge. So that the fact of my having taught this doctrine, and supported it by such arguments, for a number of years at my public lectures, cannot, I think, admit of a dispute.

Dr. Alexander Monro jun. advanced this doctrine in a general way, in 1755, in his *thesis* above-mentioned, and hinted an intention of treating it more fully upon some future occasion: immediately after which he came to London, and did me the honour of attending my course; and I hope I shewed him the respect that was due to his own merit, and to the son of one of my first masters in anatomy, who has done honour to his country as a professor of the art.

Here again you will observe there can be no dispute that I was the first who published this doctrine about the lymphatics, and that Dr. Alexander Monro jun. was the first who printed it.

He had been unfortunately exposed to the suspicion of plagiarism in his first setting out as an author; and when I saw by his *thesis* that he was opening another field, where he would be in as great danger of being suspected or censured, I took the first opportunity I could, with decency, of putting him upon his guard, by delivering this doctrine fully at my lecture in his presence, and by adding that I had done so in every course of lectures since the beginning.

That

That ſtudents of Edinburgh have many opportunities every winter of knowing what paſſes in London, is obvious, becauſe every winter ſome of them go to ſtudy at the one place, after having ſtudied at the other.

That the ſubſtance of what is new in Dr. Monro's treatiſe on the lymphatics, is the very doctrine I have taught, every impartial man who underſtands the ſubject will allow.

That two perſons engaged in the ſame ſtudies ſhould light on the ſame diſcovery, is no ways improbable; but that they ſhould ſupport it by a number of arguments and experiments intirely the ſame, though it be poſſible, is ſurely ſo improbable, that I could wiſh Dr. Monro had for his own ſake mentioned me in a marginal note.

Jermyn-Street,
Nov. 23, 1757.

WILLIAM HUNTER.

No. IV.

The Eighth Article of the Critical Review for December, 1757.

A Letter from Dr. Donald Monro, to the Authors of the Critical Review *.

GENTLEMEN,

YOU'LL excuſe the freedom I take of ſending you this, and inſiſting upon its being inſerted in the next number of your Review, without either notes or commentary, ſince in your laſt you put in a letter from Dr. Hunter, in which I am mentioned as an evidence of my brother's having ſtole the diſcovery of the ſeminal veſſels of the teſticle from him, Dr. Hunter, by alledging that I had received a letter from Dr. Garrow of Barnet, informing me of Dr. Hunter's having done it long before my brother; and I think myſelf obliged, in juſtice to a brother, to let the world know the true ſtate of thoſe facts. But although I exclude you from making any notes or commentaries of your own, yet I allow you to ſend

* Though this gentleman has precluded us from the privilege of making notes upon his letter, we cannot help taking notice of the cavalier manner in which we are treated. *He inſiſts*, and *he excludes*, and *he allows*, as if fate had ſubjected the authors of the Critical Review, to the ſovereign authority of Dr. Donald Monro. Now we muſt take the liberty to tell him, that our compliance with his demand is not owing to any regard we have for his injunctions, nor to any obligations we are under with the public, but to a ſincere and earneſt deſire of doing juſtice, which, in this caſe, has prompted us to deviate from our plan, ſo far as to take notice of a diſpute that never was printed.

ſend immediately a copy of this to Dr. Hunter, and to inſert after it whatever anſwer he ſends you, with his name put to it. (A.)

Doctor Garrow of Barnet, in December 1752, wrote to me that Dr. Hunter had thrown in quickſilver into the *vas deferens*, that he ſaw it in the *epididymis*, but did not chuſe to cut the preparation to ſee where the quickſilver had gone till he had made another ſuch. In anſwer to this, I wrote to Dr. Garrow on the 14th of December (by the return of the poſt) that Dr. Hunter's preparation of the teſticle was a common one, and that he would get the quickſilver to go no further than the *epididymis*. This anſwer Dr. Garrow carried to Dr. Hunter, and aſked him if he knew whether the quickſilver had penetrated any further than the *epididymis*, for that he intended ſoon to write to Dr. Donald Monro, and would acquaint him with it if he had. To which queſtion Dr. Hunter made no anſwer, but begged of Dr. Garrow that he would write no more to Dr. Donald Monro; which requeſt Dr. Garrow complied with, and I never received after this, while at Edinburgh, any letter from Dr. Garrow, or from any other perſon, in which Dr. Hunter was mentioned, nor heard of Dr. Hunter's having injected the ſeminal tubes of the teſticles till months after it was done by my brother; and by having received no anſwer from Dr. Garrow, I was convinced that Dr. Hunter had got the quickſilver to go no further than the *epididymis*, and therefore never mentioned Dr. Garrow's letter to my brother, as it contained nothing new nor more than had been publiſhed the year before by Dr. Haller in the 494th number of the Philoſophical Tranſactions; nor did my brother know that ever ſuch a letter had been wrote till he came to London in November 1755, when he heard of Dr. Hunter's having mentioned it in his lecture (B.)

It may be aſked why did I write to Dr. Garrow that Dr. Hunter's preparation was a common one. My reaſons for it were theſe (C)

In medical eſſays, vol. 5. art. 20. ſec. 29, my father has the following paſſage,—" It has been doubted whether the *vas deferens* and *epidi-*
" *dymis* were continued tubes. To be ſatisfied in this, cut the *vas defe-*
" *rens* through where it lies on the inſide of the *veſicula ſeminalis*, and
" take it and the teſticle away from the body; preſs the *epididymis* from
" its larger towards its ſmaller extremity, and from that to the cut end
" of the *vas deferens*, till you have ſqueezed out all the liquor you can,
" taking care by ſqueezing with moiſt fingers not to let theſe parts dry
" too much in doing this; then put a long pipe into the *vas deferens*,
" and

" and through it pour quickſilver; the weight of ſuch a high column of " mercury, aſſiſted by your fingers preſſing from time to time towards " the teſticle, will make the quickſilver go forward in the tortuous ca- " nal about half the body of the *epididymis*, beyond which I never could " make it paſs, being, I ſuppoſe, ſtopped there by the liquor of which " the canals were full."—And in a manuſcript wrote in the year 1747, on the method of diſſecting and of making anatomical preparations, he orders this ſame preparation to be made; and as he ſuſpected that the quickſilver might be made to paſs further than he had got it, he often deſired me, as well as Mr. John Campbell and Dr. Thomas Fraſer, who then ſtudied under him, to attempt pouring in quickſilver into the *epididymis*; which I did, but not ſucceeding after a few trials better than my father had done, I attempted it no more (D.)

In the year 1751, I ſtudied under Dr. Haller at Gottengen, where I found that he had been attempting to make this ſame preparation, and that he had ſucceeded better than my father or I, and had got the quickſilver to paſs quite through the *epididymis* into the beginning of the ſeminal veſſels of the teſticle, but could not get it to go further (E.)

In May 1752, 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 *epididymis* and ſeminal veſſels of the teſticle proſecuted by diſſection, much further than thoſe can be ſeen before the teſticle is cut (F.)

After reading this narrative of facts, I fancy Dr. Hunter himſelf, and every other perſon, will be convinced that I could have no knowledge of Dr. Hunter's having injected the ſeminal tubes of the teſticle from Dr. Garrow's letter, and I believe that to this day Dr. Hunter himſelf does not know whether theſe veſſels are injected in that teſticle of which Dr. Garrow wrote to me, for I am told that he never opened it, but ſtill keeps it whole; and therefore I do affirm, both from the experiments of others and ſeveral I have ſince made myſelf, that nobody can even now ſay that the ſeminal veſſels of that teſticle are filled with the quickſilver: it is as probable that the quickſilver is extravaſated within the *tunica albuginea* before it went ſo far as it did in the teſticle, of which Dr. Haller has given a figure in number 494, of the Philoſophical Tranſactions. (G.)

Having thus ſhewn what grounds Dr. Hunter has for alledging that his diſcoveries were ſtole, I ſhall next give ſome account of my brother's

injecting

injecting thofe veffels, which I believe will convince every impartial reader that it was not owing to any hint from Dr. Hunter, but to his father's orders and inftructions that he attempted it (H.)

My father finding upon my return to Edinburgh in October 1752, that I had not made all the anatomical preparations that were wanting, defired my brother to make as many as he could according to the directions given in his manufcript on diffections and anatomical preparations, as he had time that winter by my taking the trouble of diffecting for the public demonftrations; and accordingly, amongft others, he attempted this on the 9th of January 1753, the parts of generation having been demonftrated to the pupils on the 8th. Upon finding that the quickfilver had gone much further than he expected, he cut open the tefticle that night, and Mr. Donne, Dr. d'Urban, and a great many more faw it next day with the feminal tubes filled, as reprefented in the figures in the Edinburgh phyfical effays, and in his inaugural differtation.—At this time neither I, nor any other perfon I know of, had received any intelligence at Edinburgh of Dr. Hunter's having fucceeded in filling thefe tubes with quickfilver, nor do I know to this day that Dr. Hunter did it before that time; for the filling the *epididymis* I count as nothing, my father and Dr. Haller had done it before him, and he can only be faid to have injected the feminal tubes, or to have known any thing about them after having cut open a tefticle where they were filled with quickfilver. *Quæritur*, on what day, or in what week, did Dr. Hunter demonftrate them? It muft have been fome time after the 19th of December; or why did he not anfwer Dr. Garrow's queftion? (I.)

What Dr. Hunter means by faying he publifhed this difcovery before my brother, is what I do not comprehend; if he means by publifhing, fhewing it in his private colleges, he puts a meaning upon the word *publifhing* different from what I underftand by it, for I diffected five years for my father from 1745 to 1750, and had in the winter conftantly numbers of the pupils about me, and was intimate with many of them, efpecially of the Englifh young gentlemen, yet while I remained at Edinburgh, I never could know one thing Dr. Hunter was doing; and fince I have been in London, I have not heard more, except fome few things, relative to the prefent difpute, and what I heard accidentally from the Doctor himfelf in private converfation (K.)

Dr. Hunter alledges, that from the intercourfe there is between the fchools of anatomy at London and at Edinburgh, that my brother muft certainly have known of and ftole the difcovery from him; but whatever ſtreſs he may lay upon this, I declare upon my honour, that I never knew, and to the beſt of my knowledge no other perſon then at Edinburgh knew, of Dr. Hunter's having filled the ſeminal tubes with quickſilver; and I hereby beg it as a favour, that if any gentlemen wrote to me, or to my father, or to my brother, or acquainted any of us before the 9th of January 1753, with Dr. Hunter's having filled the ſeminal tubes of the teſticle, that ſuch gentlemen would let Dr. Hunter know it by a letter, and if Dr. Hunter can produce one ſuch well-vouched evidence, Dr. Monro will freely give up all claim to the honour of the diſcovery, but till that time he muſt always look upon himſelf as the firſt who injected and publiſhed figures of theſe ſeminal tubes of the teſticle (L.)

I hope I have fully ſatisfied Dr. Hunter's moſt ardent wiſhes, and proved that Dr. Monro jun. did not learn *at ſecond-hand from him* any thing about theſe veſſels of the teſticle; and for the truth of what is here inſerted, I refer him to Dr. Garrow of Barnet, to Mr. Donne, who now lodges in his brother's houſe, and to Dr. d'Urban of Richmond, and to every gentleman who attended the anatomical lectures at Edinburgh in winter 1752-3 (M.)

I don't ſee how any gentleman can be cenſured for plagiariſm by publiſhing any thing new he diſcovers by his own induſtry in the arts and ſciences, even though a hundred, nay ten thouſand, ſhould have known it before him; if they neglect publiſhing, and letting the world know it, it is their fault, and none of his.—In the preſent caſe, Dr. Alexander Monro jun. injected the ſeminal tubes with quickſilver the 9th of January 1753; in April or May drawings were made from them, and a copy of them ſent in a few weeks after to Dr. Peter Shaw, phyſician to his majeſty, which Dr. Hunter ſaw ſoon after their arrival at London. Dr. Monro did not publiſh either figures, or any account of them, till Autumn 1754; nor did he know that Dr. Hunter, or any other perſon, diſputed the honour of the diſcovery with him till November 1755, that he came to London, and attended Dr. Hunter's lectures. (N.)

What Dr. Hunter means by ſaying, *that by my brother's theſis, he ſaw he was opening another field* (meaning the lymphatics) *where he would be in as great a danger of being ſuſpected or cenſured,* is what I do not underſtand. I thought the human body had been a field open to all phyſicians, and more particularly to thoſe who made anatomy their profeſſion. My brother had of himſelf ſtarted that ſubject, and

perhaps

perhaps had carried it a much greater length than Dr. Hunter, before he came to London, or knew of Dr. Hunter's having any opinion about thofe veffels; but I fhall not fay more of this at prefent, but refer our readers to the letter inferted in your Review for December 1, 1757, which was wrote by one who knows every ftep my brother took in profecuting thofe veffels, much better than I do (O.)

I am forry to have been obliged to appear in this difpute between a brother and a friend; I had declared myfelf neutral, and was refolved never to have appeared in it at all; but fince, contrary to my inclination, my name has been brought in, and I appealed to as a witnefs by Dr. Hunter, I hope he will excufe this, nor think I have done him any injury by doing juftice to a brother who is not prefent to anfwer for himfelf.

London,
December 7, 1757.

DONALD MONRO.

P. S. Never imagining that there would be any difpute about this preparation, I did not preferve Dr. Garrow's original letter; but in the beginning of September laft, when I heard of Dr. Hunter's mentioning it as a proof of my having early intelligence of his having injected the feminal tubes of the tefticle, I wrote to Dr. Garrow, and defired him to let me know by a letter how far he had ever wrote to me on that fubject; to which I had the following anfwer:

DEAR MONRO,

'Tis impoffible for me to charge my memory with the exprefs words in my letter to you, with refpect to Dr. Hunter's preparation of the tefticle. I fearched long for a copy, but in vain; however, I found your anfwer (contrary to my expectations) the words of which are, "*Mr. Hunter's preparation of the tefticle, is a common one; he will get* "*the quickfilver to go no further than the epididymis.*"

To the beft of my remembrance my words were, That Mr. Hunter had injected the *vas deferens*, that the quickfilver was feen in the *epididymis*, that he believed it had penetrated further, but did not care to cut the preparation till he had made another fuch.

I cannot be pofitive to the above paffage, but declare upon my word and honour that I never after wrote to you on that fubject.

Barnet,
Sept. 11, 1757.

W. GARROW.

Having wrote to him ſince to know the date of my letter, which he ſtill preſerves, I had the following anſwer:

DEAR SIR,

The Date of your letter to me from Edinburgh, is December 14, 1752; that of mine to you I do not remember, having no duplicate; but think it muſt have been a week or ten days before.

Barnet,
Dec. 4, 1757. I am yours, &c. W. GARROW.

One day about a month ago, I met the doctor in town, and aſked him why he did not anſwer my letter from Edinburgh? when he told me, that upon ſhewing it to Dr. Hunter, he deſired him to write me no more upon that ſubject.

No. V.

The Ninth Article of the Critical Review for December, 1757.

To the Authors of the Critical Review.

GENTLEMEN,

IN your laſt Review I gave an account of the facts, ſo far as they had then come to my knowledge, relating to a diſpute between Dr. Alexander Monro, jun. and myſelf. This I did with ſuch regard to juſtice and truth, that I did not think I ſhould have had occaſion to give you or the public any further trouble. But the paper which you have ſent me from Dr. Donald Monro, lays me under the neceſſity of making ſome further remarks. He honours me with the name of friend, yet keeps very clear of the appearance of partiality to me. He aſſumes the character of an evidence, who is ſorry to be called upon; yet takes the cauſe upon himſelf, ſays more than there was occaſion for, and more than his cauſe will bear. Through the whole paper he ſeems to be in a miſtake, as to his two material points. Firſt, he ſeems to have thought that an evidence to an improbable fact will be more readily believed if he ſhews warmth and prejudice to that ſide of the queſtion, and adopts the cauſe as his own. Secondly, he ſeems to treat me, as if I had wiſhed to convict his brother of plagiariſm. The public will tell him, the firſt was ill judged; and, I do aſſure him, the laſt was very far from being true. From the beginning of the diſpute I propoſed doing

doing myfelf juftice, fecuring to myfelf what I knew was, and what I could prove to be my own: but was very far from defiring to fix difhonour upon his brother; which will be the more readily believed, as it is plain I could reap no advantage from it.

Allow me now to proceed to the particular remarks, which I fhall note with letters of reference.

(A.) I could wifh to make fome obfervations on the following paragraph in his introduction, if I were fure I underftood it, *viz. fince in your laft you put in a letter from Dr. Hunter, in which I am mentioned as an evidence of my brother's having ftole the difcovery of the feminal veffels of the tefticle from him Dr. Hunter, by alledging that I had received a letter from Dr. Garrow, of Barnet, informing me of Dr. Hunter's having done it long before my brother.* If the reader would take the trouble of looking into my letter in the laft Review, he would fee that I did not *mention Dr. Donald Monro, as an evidence of his having ftole*; I ufed no fuch ungentleman-like expreffion; nor did I lay claim to *the difcovery of the feminal veffels of the teftis:* I knew they had been difcovered long before I had exiftence; nor did I fay, that Dr. Garrow's letter *informed him of Dr. Hunter's having done it* (i. e. ftole) *long before*, &c. I muft own, however, that I perhaps miftake the meaning of this paffage. Let me fee again---*evidence of having ftole---the feminal veffels---from him Dr. Hunter---alledging---a letter---informing---of Dr. Hunter's having done it long before my brother.* After all, I certainly do not underftand it: for it does not inform me which of us ftole, and which of us loft the feminal veffels.

(B.) In the fecond paragraph, he gives an account of Dr. Garrow's letter, and (I am almoft afhamed to take notice of it) leaves out the principal part of the information. If the reader will turn to the account which Dr. Garrow has given of his own letter, he will fee thefe words, *that he* (Dr. Hunter) *believed it had penetrated further*, viz. than the *epididymis*. Now, why was this material paffage fuppreffed in the beginning of *a true ftate of thofe* facts?

He fays, Dr. Garrow's letter was written in December; I had faid *fome time in November*. Now it feems it was written in the very beginning of December. The reader, who makes himfelf mafter of the fubject, will eafily fee that it does not affect the argument, whether we put it in November or December; as the intelligence it contained muft have arrived before the 9th of January, when Dr. Alexander Monro jun. firft injected the *teftis* with mercury.

I wifh

I wifh Dr. D. Monro had not been fo particular in his account of my converfation with Dr. Garrow, as he was not prefent. The converfation was very fhort, and not greatly in his favour; but it was a private converfation.

The reafon he gives for not telling his brother of Dr. Garrow's letter, fhews confummate prudence.

(C.) His words were, as he now publifhes them from Dr. Garrow, *Mr. Hunter's preparation of the tefticle is a common one*; *he will get the quickfilver to go no further than the epididymis*. Now let us hear his reafons for faying fo.

(D.) The firft is a curious one. Let us abridge it, that we may fee its full force, thus: My father had told us in print that he could never make mercury pafs further than half-way through the body of the *epididymis*; and informed me by a MS. that he fufpected the quickfilver might be made to pafs further than he had got it, and often defired me and Mr. ------, and Dr. ------, to attempt it; which I did, without fucceeding better than my father, and attempted it no more. *Ergo*, When I was told that Dr. Hunter had done it, I faid it was a common preparation.

(E.) His next argument runs thus: Dr. Haller indeed had fucceeded better than my father, or I; but had not filled the *tubuli teftis* with quickfilver. *Ergo.*

(F.) His laft argument is in itfelf very fhort. Dr. Haller had traced thefe veffels within the coat of the *teftis* by diffection. *Ergo*, Dr. Hunter's preparation was common, and he could not fill them with mercury.

(G.) Dr. Monro received information at firft that I had filled the *tubuli* as far as could be known before the *teftis* was cut open, and that I believed them to be filled within the *teftis*, but that I would not venture to open the preparation till I had got another of the fame kind Ought he then to have treated this preparation as a common one, and to have taken upon him to fay, that I would get the quickfilver to go no further than the *epididymis?* Does it not appear the moft natural fuppofition, when fuch a letter was received in the beginning of the winter, while the father and his two fons were fetting out jointly in the anatomical bufinefs of the feafon, that he fhould tell his father of it; as neither he nor his father had ever been able to do it; and tell his brother of it, whofe employment that winter was to be the making fuch preparations as they were deficient in? And, does not this fuppofition agree very well with what happened afterwards? his brother made the experiment

upon

upon the very firſt ſubject, *viz.* January 9th. He ſucceeded; did what Dr. D. Monro had been told that I believed I had done. It was publiſhed immediately as a diſcovery, at his father's lectures; and afterwards in print, when he (at leaſt) might have known that I had demonſtrated the ſame thing at my public lectures in the month of November, or December, preceding that 9th of January. Was not it natural for me to be diſſatisfied with all this?

What wild diſpute does the doctor introduce here, about that *identical* firſt *teſtis?* have not I ſaid in the ſtate of the facts publiſhed in the laſt Review, that I ſhewed the *tubuli* filled in another, which was injected by my brother at my deſire?

(H.) Some readers perhaps may ſay here, we are already convinced how it was.

(I.) I allow it, becauſe I believe it, to have been on the 9th of January. In my narrative, indeed, I ſaid, *upon enquiry I found that he had injected the internal tubuli in the latter end of January, or in February,* 1752-3. Dr. Donald Monro had referred me to Mr. Donne for this information, who gave it me in preſence of ſome gentlemen from the beſt recollection he could make, in the following words, *that it could not be ſooner than the middle of January:* but he allows, with great candour, that he might be miſtaken in eight or ten days, after ſo many years, as he had nothing but his memory to direct him; and the reader will be ſenſible that the 9th of January is the ſame thing to my argument as the 9th of February.

Nor do I know, ſays the doctor, *to this day, that Dr. Hunter did it before that time.* Does he aſſert this becauſe it was my brother who injected the ſecond *teſtis?* or does he doubt my veracity in aſſerting that I ſhewed the internal *tubuli* injected in my autumn courſe of lectures, 1752? Was not this a public tranſaction before a great number of witneſſes now living?——*For the filling the epididymis I count as nothing; my father and Dr. Haller had done it before him.* In anſwer to this 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.———*He can only be ſaid to have injected*, &c. I had told him that my brother injected and cut open the *teſtis*, and that I ſhewed it in my autumn courſe of lectures, in 1752. I took no note of the day. He knows, I preſume, that my courſe begins in October, and ends in December. Let it be any day in theſe three months that he pleaſes.

---- *Or why did he not anſwer Doctor Garrow's queſtion?* I treated his anſwer

anſwer to Dr. Garrow as I then thought it deſerved, and therefore deſired Dr. Garrow to drop the ſubject. I am ſorry to have had occaſion to put Dr. Garrow's name ſo often in print. I muſt at leaſt do him the juſtice to declare, that it is not meant diſreſpectfully, and that ſo far as I know, he has behaved in this diſpute with honour and integrity.

(K.) *What Dr. Hunter means*, &c. By *publiſhing* I only meant *making publickly known:* that is, by my lectures, which are honoured with ſtudents from all parts of Great-Britain. The inſtitution indeed is private, but if I have not been flattered, its influence has been more extenſive.

----*Yet while I remained at Edinburgh I never could know one thing Dr. Hunter was doing*. What ſhall we ſay to this, after he has told us that he received a letter from Dr. Garrow, telling him what I had done, and was doing about the *teſtis?*

(L.) *Dr. Hunter alledges---That my brother muſt certainly have known of and ſtole the diſcovery from him*. I ſhould not have alledged that a gentleman *muſt certainly have ſtole*, without being able to prove it. I had no *certain knowledge* in this caſe, but ſuch preſumption, or grounds of ſuſpicion, as the reader has been made acquainted with. I gave the hiſtory of the diſpute about the *teſtis*, and lymphatics firſt at my lectures, and then printed it in your laſt month's Review, (being invited ſo to do) in order to ſecure to myſelf what is my own. At my lectures I communicate every thing freely for the benefit of ſtudents. For ſome time I have been too much engaged to have leiſure for printing my obſervations: many of them are ſtill crude, and I am flattering myſelf that I ſhall be able to bring them, perhaps, to ſome perfection. I am vexed when I ſee them printed by other people; eſpecially if there be reaſon to ſuſpect that I have not been fairly treated. Now let the reader put himſelf in my place, and ſay, whether the hiſtory which I gave of the facts was not as tender as might be expected.

I declare upon my honour. I have ſuch a reſpect for *honour*, that I would not have it offended even in thought: and therefore I wiſh Dr. D. Monro had kept to the only point which concerned him as an evidence in this diſpute, and declared that he had not told his brother of Dr. Garrow's letter. Let *honour* appear with as much dignity and force, as poſſible, when he does appear.

---*But till that time he muſt always look upon himſelf as the firſt who injected and publiſhed figures of the tubes*. Many have given figures of theſe tubes before him: but to avoid wrangling about an expreſſion, and to come to the point: whatever Dr. Alexander Monro jun. may have

known

known in January 1753, he certainly now knows that my brother injected these tubes before he did: therefore I apprehend it is not in his power to look upon himself as the first who did it.

(M) *For the truth of what is here asserted I refer him to*, &c. These gentlemen will probably desire to be excused giving their names as evidences to some things contained in this paper. When we appeal to witnesses, we should specify what they will answer for: else the appeal is mere sound. Neither these gentlemen, nor any body else, can positively say he did not learn it of me at second-hand. The affirmative, from its nature, admits of proof: the negative he may be conscious of; but he never can prove it.

(N) *I don't see how any gentleman can be censured*, &c. However it may appear to Dr. Monro, it is certain the public will not give up their right of censuring when they find cause. They are ever just in giving applause where it is due, and seldom fail to shew their resentment when they are insulted by ill-founded pretensions.

(O) *What Dr. Hunter means by saying——is what I do not understand.* Why write about it then? Especially as it is a dispute between a brother and a friend, where he has not the pretence of speaking as an evidence. As he shews an inclination to be in it, let us try if we can make it intelligible to him by a familiar allusion. A field may be *common*, and yet admit of robbery. Suppose two men have a right to gather sticks in a common; if one of them takes what the other had gathered, he is censured with justice, if he knew it when he took them; and if he did not know it, he is dishonest, and will be censured, if he refuses giving them up, when it is proved that they were become another person's property.

He concludes with referring the reader to the last Critical Review. The reader cannot oblige me more than by considering attentively what is there said upon both sides of the question.

Jermyn-Street,
Dec. 22, 1757.

WILLIAM HUNTER.

P. S. Dr. Alexander Monro jun. 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.

END OF PART I.

A

SUPPLEMENT

To the FIRST PART of

MEDICAL COMMENTARIES.

By Dr. HUNTER.

First Printed in the Year MDCCLXIV.

THE

INTRODUCTION.

PERHAPS it may be as found philosophy to say, that all the actions of men are directed to some good end, as it is to subscribe to an opinion which has prevailed among naturalists, that, in the works of nature, nothing is absolutely without its use. Literary disputes are disagreeable to the greatest part of mankind; and the disputants are, for the most part, condemned by the world. Yet it is reasonable to think, that even these disputes answer some good purpose. By engaging the passions of men more warmly, they rouze a spirit of emulation, and give a spur to enquiry.

It is remarkable, that there is scarce a considerable character in anatomy, that is not connected with some warm controversy. Anatomists have ever been engaged in contention. And indeed, if a man has not such a degree of enthusiasm, and love of the art, as will make him impatient of unreasonable opposition, and of encroachments upon his discoveries and his reputation, he will hardly become considerable in anatomy, or in any other branch of natural knowledge.

These reflections afford some comfort to me, who unfortunately have been already engaged in two public disputes. I have imitated some of the greatest characters, in what is commonly reckoned their worst part: but I have also endeavoured to be useful; to improve and diffuse the knowledge of anatomy: and surely it will be allowed here, that, if I have not been serviceable to the public in this way, it has not been for want of diligence, or love of the service.

It has likewise been observed of anatomists, that they are all liable to the error of being *severe* on each other in their disputes. Perhaps from being in the habit of examining objects with care and precision, they may be more disgusted with rash assertions, and false reasoning. From the habit of guarding against being deceived by appearances, and of finding out truth, they may be more than ordinarily provoked by any attempt to impose upon them; and for any thing that we know, the passive submission of dead bodies, their common objects, may render them less able to bear contradiction.

But,

But, to be more ſerious, we muſt allow that the language and manner of literary war ſhould be adapted to the circumſtances. Injuries, diſregard of truth, and mean artifices, in one party, will, and ought to be treated with ſome degree of indignation, by the other. In order, therefore, to judge properly of the manner, we muſt enter into the cauſe, and ſift it to the bottom, that we may ſee and feel the ſituation of the writer; and then, perhaps, what ſeemed, upon a ſuperficial view, too keen, will appear to be very gentle.

In the ninth chapter of the *Medical Commentaries*, I defended myſelf againſt a reproach thrown upon me by profeſſor Monro, ſenior, of Edinburgh, by giving a clear and conciſe account of a diſpute, which I was unfortunately involved in with Mr. Pott. The account was indeed unfavourable to Mr. Pott; but the circumſtances were fairly ſtated, ſo far as I could be informed; and I had taken ſome pains to procure information. I concluded that account by ſuppoſing that it was poſſible that I had miſunderſtood his conduct towards me; and declared, that if ever I ſhould ſee reaſon to think that to have been my caſe, he ſhould find me ready to do him juſtice. Here the affair reſted till laſt October, when he publiſhed a ſecond edition of his general Treatiſe on Ruptures. In that he added a chapter on the *Hernia congenita*; and took the opportunity of giving the public his account of our diſpute. I read it, and found that we differed very widely in ſtating the facts upon which the whole diſpute between us depends. I remembered the promiſe I had made, and reaſoned thus in my own mind: "Had I been convinced of being in the wrong, I ſhould certainly have excuſed myſelf in the beſt manner I could; but I ſhould as certainly have done juſtice to Mr. Pott's character, by owning my error, and aſking pardon of him, and of the public. Whoever reads his account, and ſuppoſes that there are no miſtakes in it, muſt think that it is my duty to do ſo immediately. Yet, now that I have got all the light which he has given me; when I read over both accounts, and compare them together, I am ſtill conſcious that mine is exactly true in every particular; and that in his there are ſuch miſtakes and inaccuracies, as could not have been expected from a man of his underſtanding and abilities, whether one conſiders him as a ſurgeon, or as an author. Yet theſe miſtakes happen to be in the great points upon which the diſpute turns, and totally change the nature of the caſe: therefore, juſtice to the public, as well as to myſelf, obliges me to clear up the matter."

The diſpute between us owed not its riſe to jealouſy, private pique, or malevolence, on either ſide; we lived in common, though not intimate friendſhip; and ſo far as I knew, neither of us had the leaſt cauſe of complaint againſt the other, till the occaſion of this diſpute. What I ſaid in my lecture, or in print, was not in the hurry of paſſion; but with reflection and meaning: And as to the manner of telling his ſtory, I muſt be ſo candid as to confeſs, that if the circumſtances had been exactly as he has repreſented them, I ſhould have thought myſelf deſerving even of a more ſevere rebuke from him. He has treated me, for the moſt part, with the language of a gentleman, for which I thank him. I have, indeed, received ſome *inciſion* at his hand, but little *butchery*; and I have been ſo much uſed to meet with the latter, that I am the more ſenſible of his lenity.

My purpoſe in the following pages is to prove the truth of the accuſation, which, in my own defence, I brought againſt Mr. Pott, in the ninth chapter of the Medical Commentaries. To ſpeak my opinion freely upon the whole diſpute, I muſt firſt declare, that, after having duly conſidered the defence which he has made in the ſecond edition of his Treatiſe on Ruptures, publiſhed laſt October, I am ſo far from repenting of what I ſaid, that I cannot wiſh to retract one ſyllable of the accuſation. And now I ſhall enter upon the particulars.

SECT. I.

Of a fuppofed plagiarifm from Baron Haller.

THE firft point in order of time, is, whether Mr. P. borrowed a remarkable paragraph from Baron Haller, and gave it to the world as his own, in the firft edition of his general Treatife on Ruptures. He avers (p. 149 of his defence) that he never had feen, read, or heard of, that work of Baron Haller, either in Latin or Englifh, till twelve months at leaft after his publication. By way of a fhort introduction to this declaration, he fays, " To fave the reader's time, and " to cut fhort this part of the difpute"---Is there any argument in this way of cutting a difpute fhort? The fact is of too much importance to be cut fo fhort; and I fhall, in the fequel, prove, however refpectable his veracity may be, that his memory frequently mis-leads him, where one would think it impoffible to be mis-led, and betrays him into moft difagreeable fituations. But furely no man is heard as evidence in his own caufe. Evidence muft be drawn from the teftimony of credible witneffes, (not of parties) or, for want of fuch teftimony, from circumftances.

Let us confider the evidence which he brings. He avers; but does not name one witnefs. He publifhed a new, a curious, and an ufeful doctrine of the moft common caufe of *Herniæ*; and added, " This has always been " my opinion ;" which, by the bye, is an officioufnefs that gives ftrong fufpicion. It looks like a confcioufnefs, that people would immediately fay, " This is the opinion which Haller has publifhed within thefe few " months." Yes, fays he, but *it has always been mine*. This, however, is digreffion. I was faying, he publifhed a new doctrine, which would have done honour to any man of the profeffion, and faid " it had always " been his opinion ;" yet now, when that fact is difputed, he cannot, it feems, *for he does not*, bring any one friend, pupil, or acquaintance, to teftify, that it was his opinion before the time of Baron Haller's publication. Is it not amazing, that he fhould not have taught that curious doctrine to his apprentices and pupils? that he fhould not have mentioned it to me, when we were confidering *Herniæ* in a dead body diffected for that purpofe? that he fhould never have mentioned it to fuch gentlemen as Meff. Hawkins, Sainthill, Nourfe, and Webb; to whom, he tells us,

(p.

(p. 145) he communicated his other new idea? If he had, they would not have forgotten ſo curious an opinion; nor would they have refuſed him the juſtice of giving their teſtimony to truth. "It had always been his "opinion;" but, it ſeems, he never mentioned it to any mortal. Can we account for ſuch *cautious, apprehenſive reſervedneſs*, to uſe his own words, in a gentleman, who is now ſo very communicative; who writes a book every year for the inſtruction of the profeſſion, and advertiſes the contents of all his works, almoſt every day, in every public paper?

His having brought no teſtimony muſt then appear as a ſtrong preſumptive proof againſt him. Let us next ſee, what degree of probability he has been able to draw from the circumſtances of the caſe. Now let us remember the caſe; it is allowed by himſelf to be thus: He publiſhed a curious doctrine in ſurgery; *viz.* that the deſcent of the *Teſtes* from the loins into the *Scrotum* is the moſt common cauſe of *Herniæ*, as his own, after B. Haller's book, which contained that doctrine, had been even tranſlated into Engliſh; yet he inſiſts ſtill, that the doctrine was his own, that it had always been his opinion; and that he had not ſeen or heard of the Baron's book (which was frequently advertiſed in our news-papers) till about a year afterwards. That I may do all juſtice to the arguments brought in proof of this *extraordinary* and *improbable* fact, I ſhall relate the whole in his own words, and interſperſe ſome remarks, that the reader may the better feel the force of theſe arguments.

"But (p. 149) ſetting aſide whatever pretenſion I may have to be "believed upon my bare aſſertion, is it probable that if I had ſtolen "my opinion from the Baron's book, that I ſhould have given ſo ſhort, "ſo imperfect, and, indeed, ſo erroneous an account of what he has "ſo fully explained, or, at leaſt, ſo clearly pointed out?" Whoever will take the trouble of comparing the paſſages quoted from the Baron and from Mr. Pott †, will ſee that, if Mr. P. did ſteal at all, he ſtole the whole ſubſtance; and that no man could venture upon a more literal tranſlation, with any chance of concealing the plagiariſm. The name, and other little circumſtances, for good reaſons, were left out; and B. Haller might perhaps ſay, *Hic quidem non unam aliquam aut alteram a nobis, ſed totam ad ſe noſtram de herniis congenitis obſervationem tranſtulit. Atque, ut reliqui fures, earum rerum, quas ceperunt, ſigna commutant: ſic ille, ut ſententiis noſtris pro ſuis uteretur, nomina, tanquam*

† Medical Commentaries, part I. p. 73 & 74.

rerum notas mutavit. But, to diſcuſs this point in plain Engliſh, ſurely Mr. P.'s doctrine being *ſhort* and *imperfect*, is no proof that it was not taken from Haller; for Haller's account of it is both *ſhort* and *imperfect.* It was a new obſervation, and required careful and repeated examinations; therefore Haller, at firſt, talked as became a true philoſopher, with diffidence; and, at laſt, had hardly a doubt left:—*cauſa videtur poni—ni fallor—ſuſpicio nondum matura—non ſufficiunt experimenta—Hæc omnino merentur conſiderari a viris gnaris & veri cupidis & per experimenta repeti —Hactenus dubius—nullum fere dubium ſupereſt.* Theſe expreſſions ſhew, that this ſketch by the great phyſiologiſt, though *ſhort* and *imperfect,* was not ſtruck off at once, and at random, but was the reſult of obſervation and patient enquiry; and if he ſhall be bleſſed with health and long life (which I moſt earneſtly pray for) he will probably favour us with a more full and perfect account of the matter. Here I cannot help obſerving how ſlowly, and with what difficulty, we acquire knowledge by ſtudy; yet how quickly and eaſily it comes by intuition. What Baron Haller took ſo much pains to find out, was—*always Mr. P.'s opinion.*

Mr. P. alſo ſays, that if he had borrowed it from Haller, it was improbable he ſhould have given *ſo erroneous an account of what he has ſo fully explained.* This is indeed a ſpecious argument, as it is propoſed; but, when examined, it is another very unfortunate one, as it proves what it is brought to diſprove. The only error in Mr. Pott's account, that I am aware of, is this; that the *Teſtis* remains in the *Abdomen* till birth, and is then forced down by breathing, crying, *&c.* But this very error is in Haller's book; and therefore ſerves to prove the plagiariſm. It was eaſier to take the whole, than to correct the error. As it was,—*it had always been Mr. P.'s opinion.* The only difference is this: B. Haller publiſhed the opinion cautiouſly, and with heſitation, as it aroſe in his mind from the examination of a few caſes: but Mr. P. took it all without heſitation, and gave his own little bit of a ſort of a reaſon for it; *viz.* It was right the *Teſtis* ſhould be out of the way of danger till after birth.

We have ſeen the force of his firſt argument: It proves what was not intended. He goes on thus: "If I had taken my account of the deſcent "of the *Teſtes* from thence, why did I not alſo learn from thence the "reaſon why the Inteſtine and *Teſtis* are ſometimes found in the ſame "*ſacculus?*" Becauſe Baron Haller neither mentioned this caſe, nor gave any reaſon for it. What ſays Mr. P. to this plain anſwer? I preſume he

will

will call it *rude*, and *malevolent*, and *unprovoked*; but he muſt allow that it is a full anſwer to his ſecond argument, and that hitherto, therefore, he ſtands juſtly ſuſpected of plagiariſm from B. Haller. He proceeds to urge his ſecond argument thus: "One of theſe facts was as much the "ſubject of my enquiry, at that time, as the other; and in the *Opuſ-"cula Pathologica* (the book alluded to) are both of them ſatisfactorily "accounted for, and made to illuſtrate each other." The reader will perhaps be amazed when I aſſure him, that the one fact in diſpute, called here *one of theſe facts*, is neither accounted for ſatisfactorily, nor unſatisfactorily; nor made to illuſtrate the other, or to illuſtrate itſelf, or to illuſtrate any thing elſe; it is not ſo much as once mentioned.

Let us go to the next argument: he ſays, "Why ſhould I call the caſe "related by Mr. Sharp a *luſus naturæ?* Why not avail myſelf thoroughly "of the plagiariſm, by giving a true ſolution of the appearance; ſhewing "that it was not a *luſus naturæ*, nor produced by what Mr. Sharp and "Dr. Hunter had thought was the cauſe of it, but by the inteſtine being "puſhed into the open *tunica vaginalis?*" Any man who read Aquapendente's Tract on the valves of the veins, might have availed himſelf thoroughly of it, and explained the circulation of the blood; yet the obvious inference, which had eſcaped Aquapendente, eſcaped every body, till Harvey's keen glance caught it. How ridiculous it is in Mr. P. to aſk why he did not avail himſelf of B. Haller's obſervation, by giving a true ſolution of the appearance! The queſtion proves only, that it ſeems to have required a little more thought and attention than he was pleaſed to give it: which, I preſume his acquaintance will not think very ſtrange. "All this is in the ſame chapter of the ſame book;" not in the ſame, nor in any other chapter of the ſame book. The reader may ſtare, indeed; but the fact is ſo. "From this book Dr. Hunter and his "brother derived all their knowledge of both theſe ſubjects." People naturally judge of others by their own experience of themſelves. No; I beg Mr. P.'s pardon: he knows that a good deal of anatomical knowledge is to be got without books or diſſections. Let any man, for inſtance, who knows but the common things, keep a good correſpondence with ſtudents, or borrow notes taken at lectures, and he may, with very little trouble, become as great a diſcoverer as a modern *junior profeſſor*, or *ſenior ſurgeon*. If the reader will take the trouble of comparing Mr. J. Hunter's account of *both theſe ſubjects*, with B. Haller's, he will ſee what reaſon Mr. P. could have to aſſert, with original ſimplicity, in his defence, that Dr. Hunter and his brother took all their knowlege of both theſe facts from this book. " And this book (if I had read it) muſt have " informed me of both, as certainly as of one. Is Haller's account of " one more plain and intelligible than of the other ?" Haller's account of the one is indeed very plain and intelligible; but he has given no account at all of the other, neither intelligible nor unintelligible. " Or " is it likely that I ſhould read only what related to one, and not what " related to the other, when they were not only in the ſame chapter and " page, but equally parts of the ſubject I was then enquiring into ?" The reader, by this time, may think it very likely, that he read what related to the one, and as unlikely, that he ſhould read what related to the other, becauſe there is nothing ſaid of the other, either in the ſame, or in any other chapter or page of the book.

" Indeed, the ſpirit of criticiſm, or, more properly, the deſire of " finding fault, has in this inſtance got the better of that artful caution, " with which Dr. Hunter moſt frequently either expreſſes or conceals " his ſentiments, has carried him beyond the proper mark, and made " him prove too much." If it is a crime, we muſt not accuſe Mr. Pott of artful caution; and we can eaſily believe, he thinks Dr. Hunter has proved too much. But by-ſtanders obſerve beſt whether the mark be hit or not. " Since, if I had read the *Opuſcula Pathologica* of Haller, " previous to the publication of my general treatiſe in 1756, I muſt " have obtained from thence that very information, which the Doctor " ſays I got from his brother in 1757, at the ſame time when he is ſaid " to have explained to me the Doctor's hypotheſis; for in that book, " as I have already obſerved, are contained both the Doctor's hypothe- " ſis, (as he calls it) and Mr. Hunter's diſcovery." The reader muſt be ſick of all this over and over; and therefore I will tell him, for the laſt time, that my hypotheſis is not contained in that book, nor ever was in any book, till Mr. Pott made a pamphlet of it, and took it to himſelf. My hypotheſis was, that in ſome caſes of *Herniæ* the inteſtine muſt lie on the outſide of the *tunica vaginalis propria teſtis*, and in others within it. Theſe laſt were reckoned unaccountable by Mr. Pott, who conſidered them as *accidents*, or *luſus naturæ*; and Haller has made no compariſon, contraſt, or oppoſition, between the two ſpecies. It is true indeed, that by reaſoning and applying what the Baron ſays of the anatomy of the parts in fœtuſes, it is eaſy to give a ſolution of Mr. Pott's *luſus naturæ*; and accordingly it ſtruck me when I read Haller, but in the way of *inference*; and this I owned in the account which I gave of the matter, as freely as Mr. Pott tells what he read in Lagaranne, and

what

what uſe he made of it. Mr. Pott muſt not pretend, that becauſe the doctrine is contained in Haller *by inference*, therefore I did not make the diſcovery; for if he makes that plea, I ſhall eaſily prove, by the ſame argument, that he had himſelf made the diſcovery, when he called it an *accident*, or *luſus naturæ*; that is, when he did not underſtand it. He called it a *luſus naturæ* in his general treatiſe in 1756; yet, in that very book, and in the paſſage which appears ſo evidently to be taken from B. Haller, he ſays, "This paſſage of the *Teſtis* from the belly into the "*Scrotum*, I take to be the principal cauſe of the ruptures of infants; "for the ring, or aperture, being by this means dilated, a portion of "caul, or gut, has an opportunity of ſlipping through, before the aper-"ture has had time to contract itſelf again." The intelligent reader will ſee that the diſcovery is contained in this; becauſe, if the caul, or gut, takes the opportunity of following the *Teſtis*, before the paſſage contracts itſelf, it cannot be otherwiſe than in contact with the *Teſtis*, which it follows. Yet he owns now, that he could not then account for the contiguity of thoſe parts in a rupture, and therefore called ſuch a rupture a *luſus naturæ*.

But to return to the ſubject of plagiariſm from B. Haller. Mr. P. goes on thus: "I am very willing to allow that Dr. Hunter might "reaſonably preſume," and the reader ſurely cannot now doubt, "that "I had ſeen the *Opuſcula*; but is ſuch a preſumption to be immedi-"ately admitted as a proof;" yet you ſee when it is well examined, it equals demonſtration in the conviction which it gives; "or can it be "thought ſufficient to authorize or vindicate ſo rude and ſo unprovoked "an attack as he has made on me?" Now, after all, this *rude* and *unprovoked* attack, as he would wiſh the reader to believe it to have been, was made upon him in the following manner. I was accuſed by profeſſor Monro, ſenior, of having a diſpute with Mr. P. I *knew* that Mr. P. had taken an obſervation from me, and aſſumed the honour of it to himſelf; therefore my attack was not *unprovoked*: whatever the attack was, it was made on that account. In the introduction to my accuſation of him, I had occaſion to quote a remarkable paſſage, which I was then convinced (and now have proved) was taken from B. Haller: yet all that I ſaid of it was this, that *I felt ſome uneaſineſs for my friend.* Surely that was *gentle*, not *rude*. I appeal to his friends. But if he inſiſts that it was *rude*, I will cut this point very ſhort, by recantation: I beg his pardon for having ſaid ſo; and now declare, with great civility, that I feel no uneaſineſs at all for my old friend. If the reader does, I

muſt

muſt applaud his generoſity; and can ſay, with great ſincerity, I was once in his ſituation; and think it very probable he will come to mine, when he knows his friend a little better.

SECT. II.

The true State of Mr. P.'s Viſit to Mr. J. HUNTER.

TO follow the order of time, the next enquiry ſhould be into the account which Mr. Pott has given of the occaſion and circumſtances of his diſcovering and aſcertaining the nature of the particular ſpecies of *Herniæ*, which made the ſubject of his pamphlet. Here he ſtands accuſed of plagiariſm from my brother and from myſelf. One of the moſt important circumſtances of the tranſaction is a viſit which he paid to my brother. I ſhall begin with that viſit, becauſe it is important; becauſe it will ſerve as a key to other things; and becauſe Mr. P. and I repreſent it in ſuch different lights: it ſhall be the teſt between us, of proper behaviour, of candour, and veracity.

Mr. P. pretends (p. 145) that he called at my houſe in Covent-Garden with an intention of telling me what he had done; that he learnt nothing from my brother, *&c.* "He ſhewed me *one ſingle preparation*," ſays he; "he did not ſhew me any other preparation——nor do I re-"member that the congenial Hernia was once mentioned by either of "us during my ſhort viſit, notwithſtanding the Doctor has ſaid that his "brother "ſhewed me his preparations with great readineſs, and ex-"plained to me his (the Doctor's) hypotheſis of the contiguity of the "inteſtine and teſticle." Our converſation turned entirely on the paſ-"ſage of the *Teſtes* from the belly into the *Scrotum*; and, as far as I "could perceive, (for he ſpake with the moſt cautious, apprehenſive re-"ſervedneſs) our ſentiments were alike.

"My papers were at this time finiſhed, and corrected for the preſs; "——nor did I alter a ſingle ſyllable in them, in conſequence of this vi-"ſit to Mr. Hunter. But had that gentleman been half ſo explicit as "his brother repreſents him to have been; had he been ſo ingenuous as "to have told me, that either he or the Doctor had regarded themſelves "as the diſcoverers; had he ſignified that either of them had any in-"tention to ſay, or to publiſh any thing about it——I would either have "ſuppreſſed my book, or have mentioned their names in it.——And as

"to

" to the honour of the diſcovery, it would not have given me any con-
" cern at all.

" This is a ſhort and true account of the fact; this is the thing for
" which I have been traduced in print.----Page 149. The manner in
" which I attained my knowledge I have already moſt faithfully related.
" ----Page 156. But excepting that ſingle circumſtance of not having
" related the ſhort converſation which paſſed between his brother and
" me, and from which I did not derive the leaſt degree of information,
" ---Page 162. When I publiſhed my tract on the congenial Rupture,
" I had no intention to anticipate either of them, or to prevent either
" of them from enjoying any reputation or honour, which might ariſe
" to them from their labours on this, or any other ſubject: if he (Dr.
" H.) had ſaid, that he or his brother was then enquiring into that part
" of the animal œconomy, I ſhould moſt probably never have proſe-
" cuted my enquiries,--as I ſhould have known that the ſubject was in
" ſo able hands: I want no reputation of that ſort."

Now the reader ſhall judge between us, from poſitive and unqueſtion-able evidence, which the point in queſtion happens to admit of.

My brother gives me the following account of Mr. Pott's viſit.

" One morning, ſome time in the autumnal courſe of lectures 1756,
" Mr. Pott called upon me in Covent-Garden, and ſpoke to me of the
" preparations which I had made relating to the *Teſtes* and *Herniæ* of
" children, and expreſſed a deſire of ſeeing them. I went with him
" into the preparation-room, and we examined them together; and
" ſome gentlemen, who lived with me at that time, were in the room
" with us, or at leaſt were coming and going, for we were ſome time
" together; and after we had examined and talked of theſe matters,
" Mr. Pott came into the parlour with me, and ſat with me ſome time
" longer. I cannot take upon me to ſay which, or what number, of
" thoſe preparations were then examined; but to the beſt of my know-
" ledge, I ſhewed them all; and I had ſeveral at that time. I told
" him what I had done, and told him the uſe you (Dr. Hunter) had
" made of theſe obſervations, in explaining the different ſituations of the
" inteſtine in *Herniæ, viz.* Whether it lies in contact with the *Teſtis*,
" or on the outſide of the *Tunica vaginalis*. I particularly remember
" that he was then of opinion, that reſpiration was the cauſe of the de-
" ſcent of the *Teſtis*, as he had explained it in his book of Ruptures,
" which was publiſhed ſome months before; and that I took the liberty

" of

" of declaring againſt that opinion, and told him I had commonly
" found them out of the *Abdomen* before the time of birth. Mr. Pott
" did not tell me, or give any hint which I underſtood, that he had
" an intention to publiſh upon the ſubject.

" 8th of Oct. 1763. John Hunter."

My brother's verbal account at the time, was enough for my ſatisfaction; becauſe I was as ſure of the truth of what he ſaid, as I could have been by the evidence of my own ſenſes. But ſome time after the Critical Reviewers had taken notice of Mr. P.'s pamphlet, I was told, by a gentlemen of the profeſſion, that Mr. P. had been attacked in company about his diſcoveries, and that he poſitively denied having ever ſeen our preparations. Upon this information, I applied to Mr. Luſcombe, ſurgeon, of Exeter, who was in my brother's houſe at the time of Mr. Pott's viſit; and I deſired my brother to write to Mr. Patch, ſurgeon, then of Exeter, but now of London, who was likewiſe preſent. In anſwer to my application, Mr. Luſcombe wrote to me as follows:

" Sir,

" In the autumnal courſe of your lectures, which I had the pleaſure
" to attend, boarding then with your brother, I perfectly remember that
" Mr. Pott called on him about the latter end of the courſe, (which
" began Oct. 4th, 1756) and that your brother then demonſtrated to
" him the ſituation of the *Teſtis* in the *Fœtus*; the manner of its paſſing
" down into the *Scrotum*; the ſpecies of rupture when in contact with
" the *Teſtis*; and ſhewed its ſituation, and explained the manner of
" its paſſage, with your opinion about that rupture; *viz.* that it was
" produced from infancy, being what is called the Congenial Rupture,
" which was fully explained in the ſame courſe. Returning you my ſin-
" cere thanks, *&c. &c.*

" Exeter, May 5th, 1759.

In Mr. Patch's letter to my brother, which is a long one, upon a variety of ſubjects, is the following paragraph: " In anſwer to your en-
" quiry, if I can recollect being preſent at the time Mr. Pott ſaw your
" preparations, I perfectly remember that Mr. Luſcombe, one morning,
" I believe in November laſt, came into my room, and told me that Mr.
" Pott was in the preparation-room with you; on which I went in,
" and

" and ſaw you two looking on thoſe preparations of the *Fœtus*, where " the *Teſtes* are ſeen deſcending into the *Scrotum*, and the *ſacs* or pro- " ceſſes of the *Peritonæum*, that are afterwards to become the *Tunicæ* " *Vaginales*. You then told him, you had taken drawings of thoſe parts; " and that the Doctor, in his lectures of the former winter, had ex- " plained, from theſe preparations, the manner in which a congenial " rupture is formed; which I likewiſe had heard from ſome of the pupils " who attended him at that time." In another part of the ſame letter, ſpeaking of the account given in the Critical Review of Mr. Pott's pamphlet, he ſays, " I can vouch for the truth of all that is there ſaid, except " the quotation from Mr. Cheſelden, and that I do not remember that " the drawings of the parts were ſhewn at lectures, though Mr. Luſ- " combe and I had the pleaſure of ſeeing them among your curious col- " lection of drawings." This letter is dated, " Exeter, June," *viz.* *June* 1758, and ſigned, "James Patch."

Here is ſuch evidence as requires no comment; it ſettles the point in queſtion, and renders all argumentation or declamation equally uſeleſs; it is the concurring teſtimony of two gentlemen of the profeſſion, who underſtand the ſubject, who are independent and diſintereſted; it proves that I had ſhewn theſe preparations, and taught that doctrine of *Herniæ* in my public lectures, even before Mr. Pott's firſt book was publiſhed, at which time he owns that he knew nothing of the *Hernia congenita*, and therefore called it a *luſus naturæ*; it proves that he was informed of all this; it proves that he came as a friend to ſee theſe preparations, and ſaw them, and heard my brother's opinions and mine upon the ſubject; it proves that he knew from my brother's own mouth, that he had made drawings of the parts to illuſtrate the doctrine; and Mr. Pott allows, that he never ſpoke, either to my brother or to me, of his intentions of publiſhing any thing upon the ſubject; yet in a few months after that viſit, he publiſhed the facts and doctrine, as his own, without mentioning our names in any way whatever: he allows too, that the gentle, but determined rebuke which I gave him, for this ſingular behaviour, was extorted from me, when a ſuppoſed diſpute with him was objected to me in reproach: and now after all, and under the weight of theſe circumſtances, he publiſhes a juſtification of himſelf, built upon a *flat* and *poſitive denial* of theſe *unqueſtionable facts*; and holds it out to the face of the whole world, with an air of triumph. By what name ſhall we call this ſpecies of diſorder?

Οὐκ ἐς' ἀνοίας οὐδὲν (ὡς ἐμοι δοκεῖ)
Τολμηρότερον.

If phyſic has no power, and friends no influence; at leaſt, in ſuch a melancholy ſituation, means might be taken to prevent all acceſs to ſuch dangerous and deſtructive weapons as pen and ink.

SECT. III.

The Circumſtances alone ſufficient Evidence.

THOUGH, in conſequence of what has been laid before the reader in the laſt ſection, we may conſider the diſpute between Mr. P. and me to be at an end, I ſhall beg leave to ſhew, that the *circumſtances* of the caſe, without that *poſitive proof*, would be ſufficient to convict him before any impartial tribunal. And in ſuits of this kind, where poſitive proof can ſeldom be had; where no fence can be raiſed to ſecure property; where property itſelf is ſo dear to the firſt poſſeſſor; where it is ſo right for the public to encourage invention and improvements, and to diſcourage, or even to puniſh plagiariſm, it is the duty of all ingenuous men to give judgment from the *circumſtances*; to ſuppoſe that truth is always attended with an ingenuous, conſiſtent, and open behaviour; and that double-dealing, inconſiſtency, or contradiction, and miſrepreſentations of particular parts, are *infallible* marks of an unſound whole. Truth always tallies with, and ſupports truth; and what is not true, may generally be detected by the nature of the prop-work (which muſt be framed of incongruous ſtuff) that ſupports it.

Mr. P. I think, I may ſay, allows that I explained, in my public lectures, what he called a *luſus naturæ*, before he underſtood it; for he does not ſo much as pretend that he knew it before me: he only aſſerts, that he knew it without me, or found it out himſelf; and tells us, (p. 143.) this was (without ſpecifying the time or date) when he examined a *Fœtus*, in company with an inquiſitive young gentleman, at that time his dreſſer at the hoſpital, who had injected it, and brought it to his houſe for examination. This is the fact, which he is pleaſed to fix upon; and I ſhall, *for the preſent*, allow, that it was Lagaranne who put him on the enquiry, with his inquiſitive dreſſer. I muſt, however, beg leave to aſk him, why he did not tell us this inquiſitive gentleman's name?

name ? or was Mr. Pott afraid it would be found out that he was my pupil? for his dreſſers and apprentices did me the honour of attending my lectures in thoſe days. Or was he fearful leſt I ſhould deſire the gentleman to ſay, upon his honour, whether he had not learned the fact from me, or from my pupils, previouſly to his meeting, upon an enquiry about it, with Mr. P.? and whether, at that meeting, he had not a better title to be called *communicative*, than *inquiſitive?* for I have good reaſon to believe that Mr. Pott himſelf was the *inquiſitive gentleman*. If I had known his name, I might have aſked him likewiſe, if ever Mr. P. had ſeen his MS. notes of Dr. Hunter's lectures; and ſome other queſtions of that kind. This is a very ſuſpicious ſetting out. I would aſk any man of ſenſe, if he can believe that Mr. Pott, when he was publiſhing a Treatiſe on Ruptures, did not aſk his dreſſers and apprentices whether Dr. Hunter had any thing new upon the ſubject; or deſire to ſee their notes, that he might judge for himſelf. He allows, that he attended Dr. Hunter's lecture at the theatre, not without hopes of getting ſome hints upon the ſubject; and he does not deny, that he deſired him to explain his ideas upon a dead body, diſſected in private for that purpoſe. Can any man of ſenſe believe, that his apprentices or dreſſers did not immediately tell him of a curious diſcovery, that was made public at a lecture, concerning the ſubject of his book, and explaining a fact which he had been forced to call a *luſus naturæ?* We ſee that he talked with thoſe gentlemen upon ſuch ſubjects. Can he have any reaſonable pretext for not being informed of this diſcovery? Muſt not information have reached him by twenty different channels? could they all fail? can a man with any *decency*, plead ſuch ignorance? He has not even the plea of diſtance; the pitiful plea of the Profeſſor, who pretended to have *found out*, at Edinburgh, what at that time was *publicly taught* in the anatomical ſchools of London, Oxford, Cambridge, and Glaſgow.

So far the matter is very clear: now let us trace him down through his own improbable ſtory. "As the thing gave him much pleaſure, " (p. 144) he procured a number of ſubjects, examined carefully, noted " appearances, drew concluſions, made preparations, and ſhewed both " the papers and the preparations to many of his friends; and, among " the reſt, to Mr. Serjeant Hawkins, Mr. Sainthill, the late Mr. Nourſe, " and the late Mr. Webb. When he had examined a great variety of " ſubjects, he enlarged his notes, digeſted them into better order, and " ſhewed them again to the ſame gentlemen." But all this time he kept his friend Dr. Hunter in the dark. He conſulted his other friends twice, but him not once. Surely there muſt have been ſome good reaſon for this conduct; becauſe, when he was about writing his book on Ruptures, he took ſome pains to get a meeting with Dr. Hunter, and was deſirous of hearing him explain his ideas on a dead body, procured on purpoſe. Perhaps he may ſay, that from the very little ſatisfaction received at this meeting, he was afterwards leſs ſolicitous about having his opinion of any doctrine or obſervation in anatomy. But, as he tells us, he always was pleaſed to *entertain a high opinion of Dr. Hunter's anatomical abilities*, it ſeems ſtrange he ſhould never conſult him once, when he conſulted his other friends twice. Is not this conduct a demonſtration of aukwardneſs, perplexity, and diſtreſs of mind? Accordingly, being ſenſible of the ſuſpicious appearance, he labours to remove any impreſſion of that kind from the minds of his readers, thus; "Hav-"ing always," ſays he, "entertained a high opinion of Dr. Hunter's "*anatomical* abilities, I called at his houſe, deſigning to have told him "what I had done, and to have had ſome converſation with him on the "ſubject: The Doctor was not at home, but his brother, Mr. Hunter, "was, and with him I had ſome talk." Here again is the pitiful pretence of a Profeſſor. Both of them would make the world believe, that they had meant to ſee me; but the Profeſſor called at my houſe in Jermyn-ſtreet, when he knew I was at my lecture-rooms in Covent-Garden; and Mr. Pott called at thoſe rooms in the morning, becauſe he knew I was there only in the evening. He does not pretend that he had made an appointment with me at that time, or deſired a meeting with me afterwards, or ever called at my houſe in Jermyn-ſtreet. All this demonſtrates that he had no particular deſire of ſeeing me, whatever he may wiſh to make his reader imagine. What paſſed between him and my brother, has been related in the preceding ſection; and he does not pretend that he talked of his papers, or dropped any hint of his intention to publiſh: Yet he ſays, in the next page, that his papers were then corrected for the preſs, and he did not afterwards alter a ſingle ſyllable in them. Surely, he expreſſed his own conduct and feelings, when he ſaid of my brother, that *he ſpake with the moſt cautious, apprehenſive reſervedneſs*. That he did alter ſome *ſyllables*, however, in conſequence of that viſit to my brother, is clear; becauſe, in the pamphlet, he gave up, or corrected, his error of reſpiration being the cauſe of the deſcent of the *Teſtis* from the *Abdomen* into the *Scrotum*. The reader muſt now be perfectly ſenſible, that this part of Mr. P.'s conduct with regard to me, upon one ſuppoſition, is very conſiſtent indeed; but, upon

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any other ſuppoſition, is altogether inconſiſtent, dark, and abſurd; and therefore it muſt be a ſtrong evidence, with all impartial men, of deſign, evaſion, and under-hand dealing.

Some months after this viſit to my brother (which he has ſo egregiouſly miſrepreſented, as was ſhewn in the ſecond ſection, and which therefore does not argue, but demonſtrate unfair dealing), without ſeeing or ſeeking me, he publiſhed his pamphlet, and neither named my brother nor me; and this at a time when he knew that all the people of this place, who were converſant with anatomical enquiries, knew that his tract contained nothing material, but what I had made public in my anatomical lectures, before he pretends to have known any thing of the matter; and what I was continuing to make public in ſucceeding courſes of lectures. In this production of Mr. P. the doctrine being tranſplanted from its native ſoil, and nurſed up in the dark, was imperfect; the deſcriptions incorrect in ſome places; no figures of the parts were given for illuſtration; but three caſes from St. Bartholomew's hoſpital were added, to make up a pamphlet of forty pages; a *time-ſerving* compoſition, which was hurried into the world, to ſnatch the only poſſible moment for raiſing reputation; and, if we miſtake not, it has raiſed a reputation which will not eaſily be ſhaken off, or ſoon forgotten.

Almoſt as ſoon as Mr. P.'s tract was publiſhed, (which he ſent me, indeed; for how could he avoid doing ſo?) I complained of him, by name, in the moſt open manner, in my lecture; and the Critical Reviewers charged him with plagiariſm, when they gave an account of his tract. Yet Mr. P. bore all this without replying, or taking any method of public juſtification. Had he been conſcious of having acted an ingenuous part, it is natural to ſuppoſe that he would have juſtified himſelf, while dates and other circumſtances were recent, and proofs eaſily procured: for the defence which he has given, at laſt, is of ſuch a nature as required no great time to be prepared. It contains no teſtimonies. It is barely the account which he is pleaſed to give of the matter: his own aſſertions, without any proof. If his ſtory was true, why did not he, with indignation, anſwer an accuſation made in ſo public a manner? He knew of it; and ſays (p. 162) he *reſtrained* ſome of the ſtudents from ſpeaking of it to me. Why ſhould he *reſtrain* them from following their inclination, if he knew that his conduct had been proper? Would he perſuade the world, that he was afraid it might have hurt my reputation? It is probable, he was afraid it might hurt his own. If I had *ſpoken to himſelf*, he ſays, he would have cleared up the matter;

matter; but as I had ſpoken only to about an *hundred gentlemen*, in a lecture, we may preſume he thought there could be no reaſon for taking any notice of it; and that it could not be ſuppoſed to affect his reputation. But why did he take no notice of what was ſaid in the Critical Review? He tells us, (p. 158) *an anonymous writer has no juſt claim to an anſwer*; and he believes the Reviewers themſelves will think ſo. What, does he really believe that the Reviewers are either ſo humble, or ſo abandoned, as to think that no anſwer is neceſſary, when they openly charge a man with plagiariſm? Reviewers have character and influence, though they have no name; and the more influence, indeed, as they profeſs being impartial: And thence we ſee authors of this, and of every nation, daily defending their characters, when they think they have been unfairly repreſented to the public by Reviewers. Why then ſhould Mr. P. trim ſo nicely, and ſo patiently, the balance of *juſt claim*, when his character was ſo openly attacked? but, at laſt, after more than *five years patience*, he found there was a juſt claim, and publiſhed his defence.

After theſe remarks, it might, perhaps, be thought an affront to the reader's underſtanding, or candour, to offer farther proofs of ſomething extremely like diſingenuity in Mr. P.'s conduct. Yet I will ſuppoſe, either that I may have been partial to my own reaſoning, or that I may have failed in conveying my ideas clearly. Therefore, I will beg leave to offer one proof more; which, indeed, is of the moſt convincing nature: it is this, that the whole ſtory of Lagaranne, which has been ſo circumſtantially related, and upon which Mr. P. reſts his defence, by accident has been found to be *an impoſition upon the public*.

Some time ago, (about eighteen months, if I can truſt my memory) in a converſation upon ſome points of anatomy and ſurgery, and particularly upon the *Hernia congenita*, which I happened to have with Mr. Moffat, ſurgeon to the Middleſex-hoſpital, and reader of anatomy, he aſked me, if I had read De Lagaranne upon *Herniæ?* Upon my ſaying I had not, he told me, there was ſomething in that writer, which was very near to a full account of the *Hernia congenita*, and he offered to lend me the book; adding, that he had ſhewn it to *Mr. Pott, who was a good deal ſurprized and pleaſed with it.* I thanked Mr. Moffat, and told him I had the book, (as well as a thouſand more, at leaſt, which, to my ſhame, I had not read) and that I would certainly look into what the author had advanced.

When

When I obſerved what uſe Mr. Pott had made of Lagaranne, in his defence, the miſrepreſentation diverted me exceedingly. I was very deſirous that the public might know the ſecret; and therefore I wrote a letter to Mr. Moffat, putting him in mind of what he had told me, and complaining of the ill uſe which Mr. Pott had made of his information. I told him, that he could not be angry with me for telling the truth; nor could he, with honour, refuſe to be an evidence in ſupport of it. Then I put ſome queſtions to him upon the ſubject; and he was pleaſed to ſend me the following anſwer.

" SIR,

" If I am called upon, however diſagreeable it may be, truth obliges " me to declare, that I ſhewed to Mr. Pott the paſſage in Lagaranne " relative to the proceſſes of the *Peritonæum*, in which the Congenial " *Herniæ* are formed. He did not, at that time, ſeem to be acquainted " with the book. I lent it to him, and in a few days he returned it, and " told me, that he had long had that book; and intimated, that he had " taken notice of the ſame paſſage, before I ſpoke to him; and rather " wondered that he had not recollected it. *This was after the publica-* " *tion of his tract*; and, I believe, about the time when that number of " the Critical Review was publiſhed, which gave an account of his " tract. I am, *&c.*

" Queen-ſtreet, Nov. 14, 1763. J. Moffatt."

This evidence puts the fiction of *Lagaranne*, and of the *inquiſitive gentleman*, in ſo clear a light, that it requires no comment. What Mr. Pott could ſay for himſelf, in this very aukward ſituation, we ſhall probably never know; for he has declared that he will write no more upon the ſubject; and the world may think the declaration was made at a very proper time, *viz*. When his ſubject was growing intractable and deſperate. But my intention being only to convince the candid reader, I will not dwell upon circumſtances ſo humiliating to an author, and to a man.

SECT.

SECT. IV.

Refutation of abſurd Accusations.

BESIDES the great points in diſpute between Mr. P. and me, which it was *neceſſary* to ſettle, ſome queſtions have ariſen, which would not deſerve an anſwer upon any other occaſion; and yet may, with propriety enough, claim ſome attention, now that the pen is in my hand.

Mr. P. ſeems to exult in thinking it probable, that I was the author of the account, which was given of his pamphlet, in the Critical Review; and then (p. 159) triumphs over this ſuppoſed behaviour, as cowardly and treacherous. In the ſame page alſo, he complains, that I attacked him openly at my lectures, and is ſurpriſed that I was not aſhamed to do it, and aſhamed to confeſs it: *An unmanly method*, ſays he, *and equally unbecoming a man of candour, or a man of ſpirit*. Now it ſeems difficult to conceive, that both thoſe attacks were made by the ſame hand, they are ſo unlike: The one was in the dark, and might be treacherous; the other was open, and could only be impudent, if it was at all wrong. It is ridiculous enough to reckon it unbecoming a man of ſpirit; for, in my mind, an attack made openly, and by name, before a number of gentlemen, and afterwards acknowledged and repeated in print, is not one of the ſtrongeſt and moſt deciſive marks of the want of a decent ſhare of ſpirit. I own I ſhould rather ſuſpect the man who, inſtead of defending himſelf when he is attacked, ſtands complaining of the *unmanly* manner, and wrangles about the juſtice of the *claim*; who diſregards one challenge becauſe it has no name, and another becauſe it has.

But, to examine theſe two inconſiſtent charges a little more particularly, I muſt tell the reader, that the account in the Critical Review was not mine, in any other ſenſe than that it was the language I uſed at the time, both in my lectures, and among my private acquaintance; and therefore the ſubſtance of it was, probably enough, delivered by myſelf to the anonymous perſon who calls himſelf *Pupil*, either in a lecture, or in private converſation. I made no ſecret of the complaint; ſo that it might eaſily have been ſent to the Reviewers, by any friend of mine. And it is no wonder that two little miſtakes ſhould have

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crept into the account, without any intention of miſrepreſenting facts. Accordingly, Mr. P. is there ſaid to have quoted Mr. Cheſelden as well as Mr. Sharp, which is an error: but it is an innocent error; for it is not of the leaſt conſequence in the diſpute; and accordingly Mr. P. who could eaſily have diſproved it, allows it to paſs without notice. The other error is this: it is ſaid in that account, that I had complained of Mr. P. *to himſelf.* This moſt certainly is a miſtake: I never did, and never ſhall. If I had been the author of the account, that error ſhould not have been introduced, for this reaſon, among others, that I ſhould not have wiſhed my friends to believe, that I had had any communication with him, after the publication of his pamphlet. From that time he was not to be of my acquaintance; my opinion was totally *changed*; the grounds I went upon were *certain*; and as I was certain that I was ill treated, complaining to him would have been as mean, as it would have been uſeleſs.

But, after all, if the account given by the Reviewers, or Pupil, (or, to pleaſe him, *by myſelf)* was *falſe*, it was unjuſtifiable, injurious, and infamous: but if it was *true*, as I aver, and have proved it to be, except in the above-mentioned inſignificant articles) pray to what purpoſe is all this wrangling, and accuſation about the author of it? Had not I a right to tell firſt without my name, (if I had thought it proper) what I had before told openly in my public lecture, and afterwards told in a book, to which I put my name? why ſhould not my friends write to the Reviewers? it appears by the Critical Review for June 1757, that Mr. Pott's friends did ſo.

The other complaint urged againſt me is, the telling my *tale* to the young people at my lecture-room. "I am really," ſays he, (p. 159) at a loſs to ſay "which has been moſt ſurprizing to me, the Doctor's having made ſuch "complaint, or his not having been aſhamed to acknowledge it. Why make "an appeal to a ſet of people, who could not poſſibly know any thing of "the matter, or, at leaſt, as it related to me? nor whether the complaint "was well or ill grounded? Why ſhould Dr. Hunter be ſo vain as to "imagine, that his *ipſe dixit* muſt be implicitly believed by all who heard "him? *&c.*" Has Mr. P. really got into ſuch *habits* and *ways* of thinking, that he is ſurprized any body *tells the truth*, and is not aſhamed to acknowledge it? I have proved every article of that appeal to be *true:* Why then ſhould he be ſurprized, either that I made it, or that I was not aſhamed to acknowledge it? Becauſe, ſays he, it was made *to a ſet of people, who could not poſſibly know any thing of the matter, or, at leaſt, as* *it related to him.* It is very ſtrange, indeed, if the gentlemen who attended my lectures *could not poſſibly know* what I had *demonſtrated there.* Several of them had been preſent, and bore witneſs to every part of the tranſaction; and the reſt of them *could not poſſibly* doubt facts, which were of ſo glaring a nature, and ſo well atteſted by their fellow-ſtudents.

Conſcious, no doubt, of the abſurdity of the firſt part of his propoſition, Mr. P. endeavours to give it a little plauſibility by adding, *or, at leaſt, ſo far as it related to him.* This is another phantom; the mere ſhadow of an argument. All thinking men muſt ſee, that the ſtudents could very well know all the material part, even as it related to him. They could read his firſt book, and then they could not but know, that while I was explaining the *Hernia congenita*, he was calling it a *luſus naturæ,* or accident. Was this above their capacity? They could know from one another (for ſome were preſent) that he came, after this, as a friend, and ſaw the preparations which my brother had made, and which I had ſhewn to them in lectures, and heard our doctrines and opinions explained. Was this beyond their comprehenſion? And, as his pamphlet was publiſhed when the complaint was made to them, they could read it, and could ſee that the whole was mine; and yet that he had taken the whole to himſelf, without mentioning my name, directly or indirectly. Was this dark, or intricate, or beyond their reach? Was it neceſſary to know more than thoſe facts, to judge of my complaint, or of his behaviour? or, was it neceſſary, before they could poſſibly know any thing of the matter, that they ſhould wait patiently five long years, and be made acquainted with the inſtructive and delectable hiſtory of *one Gargantua*, and *the inquiſitive gentleman of St. Bartholomew's?* a romance, which it has been already proved, had not an exiſtence, even in the author's fertile imagination, till ſome time after.

But, ſays he, (p. 160) "it was diſingenuous to endeavour to ſet me "in a contemptible light to his hearers, without having once mentioned "the thing to me, or hearing what I had to ſay in my own vindica-"tion." Had his behaviour been only doubtful, I ſhould have endeavoured, ſome way or other, to have found it out, before I had complained of him in public: but he had ſaved me that trouble, by removing all poſſibility of doubt. There was at once an end of our friendſhip, and of my reſpect for him. *Fides, ut anima, unde abiit, nunquam redit.* Hear what he had to ſay! I knew at that time, as well as the reader knows now, that he had nothing to ſay in his vindication, which could be to the purpoſe; and yet, it is my ſincere opinion, that

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he loſt nothing among his acquaintance, by what he is pleaſed to call my endeavours to make him appear contemptible. However, I endeavoured to repreſent him fairly, and as he was: if the figure he made was reſpectable, the merit was all his own; and if it was not, the demerit was not mine.

It is pleaſant enough to ſee the pains he takes, to make the world believe, that I had been babbling to boys, and miſ-leading young minds, who could not judge for themſelves. He affects not to know the kind of aſſembly that he ſpeaks of. There are always a great number of gentlemen preſent at theſe lectures, who are enabled, both by education and age, to judge of more difficult queſtions than any which this diſpute has occaſioned.

I have now anſwered all the charges which Mr. P. has brought againſt me, except what are contained in two notes; and theſe ſhall next be conſidered.

In a note (p. 161) he ſays, " In the Medical Commentary, ſpeaking " of my erroneous account of the time of the deſcent of the *Teſtes*, and " of my ſuppoſed theft from Haller, the Doctor ſays, that the ſubject " appeared to him *too delicate for converſation*. But though it was too " delicate for converſation, even with a man whom he dignifies with " the reſpectable name of *friend*, yet it did not appear too delicate to be " made the ſubject of an anonymous piece of ſatire. What an idea of " *delicacy*, as well as of *friendſhip*, does this convey! *Hic nigræ ſuccus* " *loliginis*; *hæc eſt ærugo mera*."

That the reader may the better underſtand the idea that I meant to convey of *delicacy* with my *friend*, and clearly ſee our author's ſophiſtry, I beg leave to inform him, that in the year 1756 I treated a *very delicate* ſubject (*viz.* my *friend* Mr. P.'s ſuppoſed plagiariſm from Haller) *with ſilence*, becauſe *he was then my friend:* but, after he publiſhed his pamphlet in the year 1757, in which he took from me (till then his *friend*) what he knew was mine, and what he knew I ſhould be ſorry to loſe, without either aſking my conſent, or making any acknowledgment; then, I ſay, he had no right to expect *delicacy* or *friendſhip* from me. Yet, even then, I wrote no *anonymous ſatire*, but *complained openly* of his moſt *indelicate* and *unfriendly* behaviour to me. I imagine the reader will now underſtand the *nigræ ſuccus loliginis*.

——*Quod vitium procul afore chartis,*
Atque animo prius, ut ſi quid promittere de me

Poffum aliud, vere promitto. Liberius fi
Dixero quid, fi forte jocofius; hoc mihi juris
Cum venia dabis.

The other note, which I beg leave to anfwer, is in Mr. P.'s 163d page. It can be a fecret to none of Mr. P.'s readers, that he there reproaches me with having *infulted* him, by fending the Medical Commentaries to him, as it contained fome things which *could not be pleafing to him.* Very certain I am, that no *infult* was *intended*; nor, indeed, was any civility meant. It was thought but juftice, to let the perfon concerned have a copy of his accufation, as foon as the public; that he might fettle the defence he was to make, and be prepared to talk upon a fubject, which was to come into public converfation. This, I am told, is always done. I never complained of my antagonifts at Edinburgh, for fending me their publications; and never heard, or fuppofed, that they were offended at my fending mine to them. But, to avoid all unneceffary argumentation, if Mr. P. was really hurt by my ordering the book to be left at his houfe, as well as at an hundred more in London, I voluntarily give him the fatisfaction which a gentleman thinks fufficient in fuch cafes; *viz.* I affure him, upon my honour, that I did not mean it as an offence or infult, and not only beg his pardon, but promife that I will never again fend him any book that I may publifh.

Thus I have endeavoured to clear up a difpute, which appeared to me to be of confequence. Had the queftion been only about unimportant difcoveries, and infignificant improvements, it could hardly have deferved a line for every page which has been beftowed upon it: But when the characters of men are *ftaked* in a difpute, it grows too ferious and important to be neglected. This confideration made me fend thefe fheets to the prefs fooner, and perhaps more incorrect, than I could have wifhed. Mr. P.'s defence of himfelf, and accufation of me, came upon me in the very beginning of my hurry; in the firft week of my firft courfe of lectures, which is not yet finifhed. If I had had more leifure, I might have put this Supplement into better order, and might have been tempted to touch upon fome other inviting fubjects. Mr. P. has fupplied me with an unneceffary profufion of matter; infomuch that, inftead of having *wantonly fought a difpute with him*, as he would have wifhed the world to believe, I could, for the fake of argument, give up every point that he has defended, and attack him as a plagiary, upon new ground. I might begin with his anatomical defcriptions, particu-

larly

larly with what he ſays of the rings in the abdominal muſcles. He makes a parade upon this ſubject, as if he was really an anatomical obſerver and improver, both in his Treatiſe on the Hydrocele, and in that upon Ruptures; and with as much eaſy aſſurance, as if I had not for many years demonſtrated the ſame things, in a very particular manner in my courſes of lectures; and as if there were not now living many hundreds of gentlemen, who know the truth of what I here advance. But as I have done ſome juſtice to the two principal characters in this diſpute, and can have more uſeful employment for the very few hours that are at my own diſpoſal, I will give Mr. Pott up to the enjoyment of his reputation, as an iugenious and modeſt improver of ſurgery, as a man who is faithful to his friend, and religiouſly obſervant of Truth, upon every occaſion.

Quæ, ſi ſingula vos forte non movent, univerſa certe inter ſe connexa, atque conjuncta, movere debebunt.

Jermyn Street,
Dec. 31, 1763.

POST-

POSTSCRIPT.

IT would give me a very ſincere pleaſure, if I could promiſe myſelf that I am now appearing in controverſy for the laſt time: I heartily wiſh that it may be ſo. I have never attacked any man who treated me fairly, and do promiſe that I never will. This is a ſecurity on my ſide, that will not fail; and, we may hope, that the example of my two friends, will prevent the ſame kind of unfair proceedings from others.

Indeed, my *old maſter*, Profeſſor Monro, ſenior, has ſtill a demand upon me; but he will not permit me to diſcharge my duty to him, and forces me to take this method of endeavouring to prevail upon him. He has honoured me with an *expoſtulatory epiſtle*, and flattered me with the promiſe of publiſhing a comment upon all my works. Yet I cannot perſuade him, in a more private manner, to anſwer two ſhort, and plain, and fair queſtions. Therefore I muſt lay our correſpondence before the public, in hopes that my *old maſter's* friends will uſe their influence with him, in my behalf.

He ſent his *Expoſtulatory Epiſtle* to me, with the following letter:

"To Doctor William Hunter, Phyſician, London.

"SIR,

"In return for your Commentary, I herewith ſend you a copy of
"ſome animadverſions on the part of it immediately relative to me; and,
"as this is too ſmall a compenſation for ſuch an elegant book, I ſhall do
"myſelf the pleaſure to ſend you a larger volume, of the ſame kind, on
"all your publications, in the vulgar ſenſe, and muſt, in the mean
"time thank you for furniſhing ſuch copious materials to

"Your old maſter,

"Edinburgh, Dec. 4, 1762. Alexander Monro."

When I had conſidered his *Expoſtulatory Epiſtle*, I wrote to him as follows:

"To

"To Alexander Monro, ſenior, Profeſſor of Anatomy, Edinburgh.

"Sir,

"I return you my thanks for the new edition of your Oſteology, "which you were pleaſed to ſend me. At the ſame time I received your "*Expoſtulatory Epiſtle*, and a letter in manuſcript.

"You certainly have a right to demand information of the particular "paſſages in Dr. Noortwyk's book, which I charge you with having "miſrepreſented. They are as follow:

"Medical Eſſ. vol. ii. p. 119. The words *moſt ſtrongly* are ſubſtituted "for *quam poſſet proxime*; which tranſlation alters the ſenſe entirely.

"*Ibid.* The word *and* (*moved the knife*) is ſubſtituted for the word "*vel*; which likewiſe alters the ſenſe entirely.

"*Ibid.* p. 124. The following ſentence is coined: *And the ſoft* "*ſpongy internal ſubſtance of the womb is inſinuated into the furrows be-* "*tween theſe knobs.*

"In my turn, I ſurely have a right to demand an anſwer to the two "following queſtions, Who is meant by the *deceaſed benefactor and* "*friend?* who by the *firſt introducer into buſineſs*, mentioned in the 27th "page of your Epiſtle? I flatter myſelf, you will think it proper to "give me a direct anſwer, as ſoon as your leiſure will permit, that it "may not be in the power of malevolence itſelf to accuſe you of *ſtab-* "*bing in the dark.* I am, Sir,

"Your very humble Servant,

"London, Jermyn-ſtreet, Feb. 11, 1763. William Hunter."

I expected an anſwer; and own I was ſurprized at not receiving any. At length I wrote to him again as follows:

"To Alexander Monro, ſenior, *&c.*

"Sir,

"It is now almoſt ten months ſince I troubled you with a letter, to "which I have hitherto received no anſwer. I am inclined to do you "juſtice; yet I cannot well anſwer your printed *Expoſtulatory Epiſtle*, "without knowing who are meant by the *deceaſed benefactor and friend*, "and the *firſt introducer into buſineſs*, as they are repreſented by you in

"the

"the 27th page of your *Epiſtle*. Let me repeat to you, that you cer-
"tainly had a right to aſk, what the paſſages were in Dr. Noortwyck,
"which I affirmed you had miſrepreſented; and accordingly I pointed
"them out to you. I have the ſame right to be informed of the *bene-*
"*factor* and *firſt introducer*, whom you have endeavoured to make the
"world believe I have uſed ill. Will you, Sir, who (p. 2.) value
"yourſelf upon your *candour*, and (p. 28) recommend plain ſpeaking in
"diſputes; who call yourſelf a *blunt, teſty old fellow*; will you, I ſay,
"upbraid me, in the face of the whole world, with having behaved ill
"to my *deceaſed friend and benefactor*, and to my *firſt introducer into*
"*buſineſs*, and yet refuſe to ſtate the fact, in ſuch a way as that I may
"clear myſelf, if innocent: or make the beſt reparation in my power,
"if I have had the misfortune to be ſo much in the wrong? I cannot
"think you will ſtoop ſo low; and therefore I will once more aſk you
"the queſtion, in this private manner, and wait a reaſonable time for
"your anſwer. If you will not favour me with an anſwer at all, you
"muſt not be offended if I apply in another manner, and clear myſelf
"of your *ill-grounded aſperſion*. If you were really ſo informed, you
"were egregiouſſy abuſed, and you will now be glad to clear yourſelf;
"but if you avoid this fair opportunity of doing me juſtice, I muſt
"accuſe you, not only of ſpreading, but of raiſing a *groundleſs calumny*.
"I am, Sir,

"Your humble Servant,

"London, Dec. 3, 1763. William Hunter."

Hitherto the Profeſſor has not condeſcended to take notice of theſe letters; and therefore it is now time to addreſs myſelf to him in print.

To Alexander Monro, ſenior, *&c.*

SIR,

Give me leave to ſend you a *plain letter*, in anſwer to your *Expoſtulatory Epiſtle*. Since the publication of that Epiſtle, I have, again and again, aſked you two plain queſtions, which your friends will probably think, you ſhould have anſwered ſooner. Whether you will now, or not, is perfectly indifferent to me; but, for your own ſake, it might be proper to ſay ——; or whatever you have found to go off moſt ſpeciouſly upon

upon ſuch occaſions. You may conſult with your relation, whom you have gone ſome lengths to ſerve, in his diſtreſs:

Nunc, ſi quid potes aut tu, aut hic,
Facite, fingite, invenite, efficite.

And, in the mean time, I will make ſome ſhort remarks on the reſt of your Epiſtle.

You ſay, (p. 1) " it was really cruel in me to force you to reſume the " pen, eſpecially in controverſy, which you always diſliked ſo much that " you never were the aggreſſor." This piece of declamation muſt have entertained your readers, who all know, that in this very diſpute, you were the aggreſſor: and that you wrote a long paper in the Critical Review againſt me, at a time when I had neither directly nor indirectly brought you into the diſpute; and when I had not printed any thing upon the ſubject. You muſt allow this fact; and your beſt friends muſt allow that it is unanſwerable, You will probably beſt know, what they will ſay upon this occaſion; but I well know what muſt be their real opinion.

Were you never the aggreſſor in another inſtance? Recollect yourſelf before you ſpeak; and tell the world, who was the author of that coarſe attack upon Garengeot, in the Medical Eſſays, which all gentlemen allow to be a diſgrace to the collection.

You tell us (p. 2) that " my late attack in my *Medical Commentaries* " on your candour and veracity, the part of your character which you " always valued moſt, piques you ſo much that you muſt appeal to the " public for redreſs; and that poſſibly, when the ſpirit is thus rouſed, " ſomething more than your vindication will appear." Whatever may afterwards appear, the public, in the mean time, would be glad to ſee your vindication. Your Epiſtle is not of that kind: it is vindictive enough, but it is no vindication.

In your 3d, 4th, and 5th pages, indeed, you ſeem to attempt a vindication; but the attempt ends in nothing. Give me leave to ſtate the caſe to you. I ſaid that you had forfeited all reputation as an hiſtorian, by aſſerting, firſt, that *you knew the facts relating to the diſpute* (between your ſon and me) *and ſent a fair ſtate of them*; and then by aſſuring the public, *that Dr. Monro* (junior) *went to London in abſolute ignorance of Dr. Hunter's having any particular opinion concerning Lymphatics, and was ſurprized when he heard Dr. Hunter teach the doctrine of Lymphatics being*

Abſorbents. Now, Sir, all the world knows, that it has been proved that this was a *direct miſrepreſentation* of a fact. Had not I then a right to ſay ſo, in my defence? and did not I leave you room, for the only defence which *candour and veracity* could make, *viz.* a confeſſion that your antagoniſt was in the right, and that you were in the wrong? Would not the public have applauded you more, if you had frankly owned your fault, and pleaded the excuſe of ignorance? Inſtead of this, you wrangle, and will not even confeſs that it was a fault. Your friends will tell you, that it would have been more proper to deprecate, than to ſhew a ſpirit of revenge, in ſo humiliating a ſituation.

From the 6th to the 16th page of your Epiſtle, you wrangle with me about your diſpute with Dr. Noortwyk. I have told you the paſſages which you have *miſrepreſented*, and the fact is as clear as ſun-ſhine; yet I know that you would wrangle for ever, rather than confeſs that you have been in the wrong. But there is ſtill one way left you, for gaining a victory over me, in this part of our diſpute. State the caſe to Dr. Noortwyk in a letter; you may have an anſwer from Holland, in two or three weeks: You allow that he is *learned and candid*; aſk him if you have tranſlated thoſe paſſages like a man of *veracity and candour*, the part of your character upon which you value yourſelf moſt: Aſk him if he has *altered his opinion*. He is *candid*, you know, and therefore will do you juſtice readily; and as you allow, that he is *learned*, you will not pretend that he does not underſtand the meaning of his own words. Your beſt friends will allow this to be a fair propoſal. Try what Dr. Noortwyk will do for a man of *candour and veracity* in great diſtreſs.

Unexpectedly, Sir, I am obliged to take my leave of you, very abruptly; but, if I live, this ſhall not be my final farewel. You ſhall have the pleaſure of hearing from me frequently, till you have gratified my curioſity with reſpect to my *benefactor* and *firſt introducer*. Then, once for all, I will pay my reſpects to you, aud leave you to enjoy the *ſweets of your calm retreat*. I intended to have made ſome remarks upon the reſt of your *Epiſtle*; but while I was writing this *Poſtſcript*, and correcting the proof-ſheets of what relates to Mr. Pott, I was ſo frequently interrupted, that my printer, and many of my friends, began to deſpair of my finiſhing what had been promiſed. At laſt, on the eleventh of February, I was ſo fortunate as to meet with a gravid *Uterus*, to which, from that time, all the hours have been dedicated which have been at my own diſpoſal. I have been buſy in injecting, diſſecting, preſerving, and ſhewing it, and in planning and ſuperintending drawings and plaiſter

caſts

caſts of it; neither of which can poſſibly be finiſhed, for ſome time. You will not then be ſurprized, that in all this time, I have not once taken up my pen, to finiſh this *Poſtſcript* on the intended plan. Indeed, it would not have been in my power to have finiſhed it, for ſome time to come.

I have been ſo particular in my apology, in order to prevent your thinking me neglectful of you; and likewiſe that you, who have promiſed a comment upon all my works, and have thanked me for furniſhing ſuch copious materials, may have the pleaſure of being informed, that I am preparing more materials for your amuſement, and for your criticiſm. I have already made five very capital drawings from this ſubject. They, and ſome more, ſhall be engraved by the beſt maſters, as ſoon as poſſible; and then the whole ſhall be publiſhed. My firſt and original intention, you know, was to have publiſhed ten plates only; but thinking the work imperfect, I waited patiently for more opportunities of adding ſupplemental figures. Sixteen plates were finiſhed on this plan, ſeveral years ago: But ſtill I was diſſatisfied with the work, as being incomplete; and, in ſpite of the importunity of many friends, in ſpite even of your affectionate and good advice, I kept it from the public. When the additions which have been made, ſhall be publiſhed to the world, I ſhall have an opportunity of learning whether, for the future, I ought to be directed by your conſummate wiſdom and prudence, or go on as well as I can, in my own ſimple and blundering manner. I am, Sir,

Your very humble Servant,

Jermyn-ſtreet, March 15, 1764. William Hunter.