Hunter and the stag : a reply to Professor Owen from the scientific point of view / [Charles Adams].

Contributors

Adams, Charles.
Society for Protection of Animals from Vivisection (London, England)

Publication/Creation

London : Williams and Norgate, 1882 (London : Pewtress & Co..)

Persistent URL

https://wellcomecollection.org/works/qpn3byyg

License and attribution

Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
https://wellcomecollection.org

HUNTER AND THE STAG:

A REPLY TO

PROFESSOR OWEN.

PROM THE

SCIENTIFIC POINT OF VIEW.

LONDON:
WILLIAMS AND NORGATE,
14, HENRIETTA STREET, COVENT GARDEN;
AND 20, SOUTH FREDERICK STREET, EDINBURGH.
1881.

Price Threepence.

HUNTER AND THE STAG:

A REPLY TO

PROFESSOR OWEN.

FROM THE

SCIENTIFIC POINT OF VIEW.

LONDON:
WILLIAMS AND NORGATE,
14, HENRIETTA STREET, COVENT GARDEN;
AND 20, SOUTH FREDERICK STREET, EDINBURGH.
1881.

Price Threepence.

LONDON:

PRINTED BY PEWTRESS & Co.,

Steam Printing Works,

15, GREAT QUEEN STREET, LINCOLN'S INN FIELDS, W.C.

HUNTER AND THE STAG.

DEAR SIR,

You will remember that in my letter acknowledging your very courteous reply to my enquiry as to the origin of the story of Hunter and the stag I ventured to draw your attention to a letter of mine about to appear in the *Lancet* in reply to your criticisms of my speech at our Annual Meeting. Curiously enough that letter, which had been twice publicly promised insertion, and was at that moment, as I have reason to believe, actually in type—has since been suppressed. I am therefore reduced to the necessity of directly addressing yourself and need, I feel certain no further apology for so doing.

The criticisms to which I refer were contained in your Address on the unveiling of the Harvey Memorial at Folkestone. That Address consisted, as I need hardly remind you, of an eloquent panegyric upon the hero of the day in his especial character as "the Great Vivisector." Its text was a phrase of mine to the effect that "Vivisection while it pandered to scientific curiosity added nothing to practical knowledge." Its argument, an elaborate refutation of that view; firstly by the attribution to vivisectional experiment of Harvey's own grand discovery of the Circulation of the Blood; secondly, by the story of Hunter and the stag to which I have already referred and for which you claim the credit of having originated the

great surgeon's famous operation for popliteal aneurism. Its conclusion, an earnest appeal for the removal of all restrictions from this method of scientific research.*

In a word you attacked on what you maintained to be scientific grounds a position you stigmatised as one of simple sentiment. On scientific grounds I claim the right to reply. I do not for one moment abandon either the "sentimental" or the moral basis of my proposition. The latter of these at all events is still, I am bold to confess, of higher importance in my estimate

---

* "Suppose a Parliament of George II. had decreed that 'no experiment on a living animal should be legal without express permission of the Secretary of State for the Home Department.' John Hunter, at a period when he was known to society only as a rising young surgeon, amusing himself with making an anatomical museum, finds himself compelled to go to Downing Street to obtain the requisite licence to solve the physiological problem then monopolising his cogitations. We may suppose the following colloquy to ensue.

Home Minister: What is the object, Mr. Hunter, of your proposed experiment on the living deer?

Vivisector: I want to know how their horns grow.

H. M.: And what do you propose to do to gratify that desire?

Viv.: For one thing, I propose to cut down upon the carotid artery, and tie it.

H. M.: And what good do you expect to get by inflicting on an unfortunate animal that degree of pain?

Viv.: I have nothing further in view, sir, than what I have stated.

H.M.: And so you would pander to your curiosity in regard to the growth of its horns by subjecting a poor deer to your detestable operation. I can give no sanction to such inhuman vivisection, of which you are unable to foresee any scientific results in relation to your own professional purposes and practice.

The discomforted physiologist departs: and mankind continue to die of a tormenting malady, sometimes with, sometimes without, the added operation of amputation at the thigh."—*Speech at the unveiling of the Harvey Memorial.*

than any to be found in the whole range of Science, false or true. But it is idle to enter upon argument without some basis of common premiss. The question presents itself to you, and is by you presented to your audience, as one of simple science. As such I am for the nonce content to argue it.

In the present letter I propose to deal only with the question of John Hunter and his discovery. And this for two reasons. First; when a fair discussion of any point is really desired there is nothing like keeping it clear of all foreign topics. I am most earnestly desirous that this question of the gains from Vivisection to "suffering Humanity" should if possible be for once decided not by mere clamorous assertion but by simple scientific argument and proved historic fact. Second; this particular case of Hunter's vivisectional experiment and its assumed result affords, as you have so promptly recognized, precisely the grounds upon which such an argument may most effectively be carried out. You have yourself selected it as a typical instance of the beneficial results of the practice you so urgently advocate. I am quite prepared to accept it as an equally typical example of its utter barrenness. When that question shall have been fairly argued out I shall be quite ready, should you so desire, to deal in similar fashion with your other contention as to Harvey and the Circulation of the Blood. For the present let us confine ourselves exclusively to the story of Hunter and the stag.

How, then, in the first place, does the case stand with regard to this asserted origin of Hunter's invention regarded from the historical point of view as a mere matter of evidence?

And here let me once more express my sense of the

prompt courtesy with which you replied to my enquiry as to the source from which the anecdote had reached yourself. In one form or another—and some of its forms were not a little eccentric—it had of course been familiar to me ever since I first took up the question of Vivisection. In the evidence for instance of Sir W. Gull—an even warmer advocate of Vivisection than yourself, for he, with Caiaphas* boldly maintains the principle of human sacrifices†—it presents itself in the form of Sir Astley Cooper, tying the aorta of a dog. But under no form could I ever succeed in tracing it a solitary step further back than its original publication by you. And when, finally, a careful search through Hunter's own Museum, failed still, with all the aid of its polite officials, to bring me any nearer to the object of my desire, I ventured a direct appeal to yourself. Permit me, for accuracy's sake to recall the precise terms of your reply.

"British Museum, August 24th, 1881.

"My Dear Sir,—The account of Hunter's Experiment on the Fallow-buck I had from Mr. William Clift, F.R.S., then Conservator of the Hunterian Museum, and the last pupil of John Hunter; he had the account from William Bell, a former assistant of Hunter's, and aiding in the Experiment and subsequent dissection. I was, when so informed, engaged in describing the preparations illustrative of the growth and shedding of the antlers, for the 'Physiological

---

* "Caiaphas . . . said unto them ye know nothing at all, nor consider that it is expedient for us that one man should die for the people."—S. John xi., 49, 50.

† *Sir W. Gull.*—" . . . Would it be right to take an unwilling man and an innocent person and slaughter him for the public safety? Most certainly it would."—"Evidence before the Royal Commission on Vivisection," page 267, question 5489.

Catalogue of the Hunterian Collection' (4th). I have, on former occasions, alluded to it, and the account has appeared in 'Reports' of my Addresses, in Contemporary Medical Journals, as at the Hunterian Society of London; the dates I don't remember.

"Believe me, faithfully yours,

(Signed) "RICHARD OWEN."

The story, then, is one of purely oral tradition. Professor A. is told it by Dr. B., who had it from Mr. C., who was at all events in a position to hear it—if it were ever told—from the great D. himself, the author of the invention in question. Surely a questionable basis this, even for a story which D. might possibly himself have had an interest in suppressing? But this story is precisely one which, had it ever occurred to D., would certainly have been told by him with special zest. We have his own published account of the transaction in question. We have besides the still more elaborate account of Sir Everard Home, his son-in-law, assistant, *alter ego* and fellow-vivisector. And neither account contains, from first to last, one solitary word which can, by human ingenuity, be construed or twisted into the very faintest suggestion of any story of the kind.

I ask you, Sir, respectfully but seriously, is this third-hand hearsay—flatly discredited by direct and indisputable documentary evidence—the kind of basis for scientific dogma which you, the Senior Scientist of England, are prepared deliberately to accept? Is there a conceivable fact, or theory, or assumption, however inherently insignificant, at which, resting on premises such as these, you would, in dealing with any other subject, condescend to cast so much as a glance?

But I will not rest my refutation of your argument solely upon the utter absence of anything like evidence in its support. I join issue with you boldly, not only on the historical, but on the plain surgical and anatomical facts of the case. I maintain that you are absolutely in error, not only as to the real basis of John Hunter's famous improvement; but as to the very nature of that improvement itself and the mode of treatment previously pursued. I maintain—and by this I do not mean that I assert, but that I am prepared to prove—that not only was this discovery of his wholly and absolutely unconnected with any vivisectional experiment of any kind upon any animal, but that its essential conditions, from first to last rendered, *ex hypothesi*, any argument from any such premises altogether irrelevant and absurd.

And, first, with regard to the misconception under which you are evidently labouring with regard to the treatment of aneurism before John Hunter's time, and the improvement introduced into it by him.

"At Hunter's Hospital" you tell us "cases of popliteal aneurism* were not uncommon. Now, Hunter, turning over in his mind the phenomena he had observed and caused in vivisecting the deer,

---

* As this letter may probably reach the hands of many less instructed than yourself in matters of anatomy and of surgery I may mention here that a popliteal aneurism is a tumour in the hollow between the two hamstrings at the back of the knee occasioned by the giving way of the artery under pressure of the blood current. The position of the legs in driving—especially in the case of gentlemen's coachmen—who sit down to their work instead of practically standing up to it like stage-coachmen and omnibus-drivers—is specially provocative of this complaint, the sharp bend in the artery checking the blood-current, while the air, the movement and the muscular exertion all combine to stimulate it.

thought thus, Suppose, instead of amputating the man's limb, I were to cut down and tie the femoral artery, it might stop the flow of blood into the aneurismal tumour long enough at least to allow the blood there to coagulate and form a natural plug; and if the human capillaries should behave like the cervine, a man's leg may become nourished* independently of the popliteal channel." Now, I think I am justified in saying that this, if it means anything, means (1) that previous to Hunter's time the only known method of treating popliteal aneurism was by amputation; (2) that the novelty of Hunter's mode of treatment consisted in tying the artery in which the aneurism had occurred without removal of the portion of the limb beyond the ligature; (3) that the basis of this innovation was the idea, then first started by himself, that the nourishment thus cut off from the limb by the obliteration of its natural channel of supply might not impossibly be afforded by what is now known as the collateral or supplementary circulation; that is to say by the enlargement, under pressure of the diverted current, of certain smaller channels branching off from the obliterated artery above the point of obstruction in something of the manner in which the traffic of a main thoroughfare, obstructed by the bursting of a gas main, is carried on by means of ordinarily unfrequented back streets and lanes; (4) that this idea, previously unknown to surgery, was suggested to him by the accidental discovery of a similar natural makeshift in the case of the vivisected deer?

The argument is clear, simple and straightforward. The only question is as to the premises on which it is based.

* I quote *verbatim* from the report of your speech in the *British Medical Journal*.

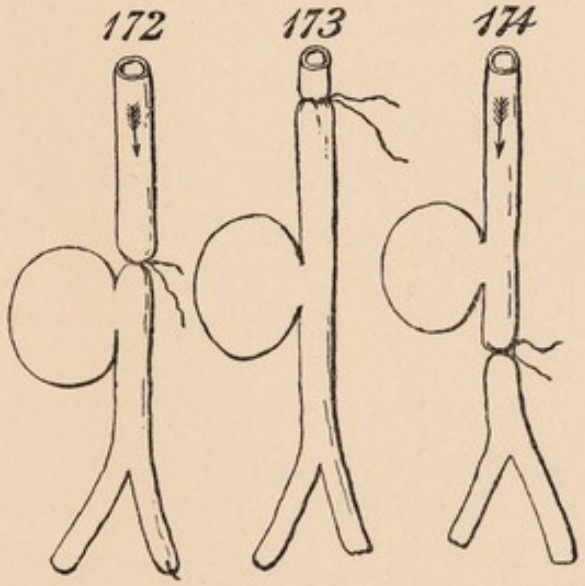
Now, what are the facts?

Let me refer you in the first place to either of those easily accessible and universally recognised authorities, *Bryant's Surgery*, and *Erichsen's Surgery*, at Vol. I. p. 430 of the former of which works,* and at p. 544 of the latter† you will find figured, side by side, three methods of treating aneurism by simple ligature—"Hunter's," "Anel's," and what Bryant distinguishes as "Wardrop's," and "Brasdor's as practised by Wardrop," whilst Erichsen condenses them into one which he styles simply "the distal operation." So far as our present argument is concerned all three operations stand of course upon precisely the same footing. But it is well to avoid all unnecessary complications and I will therefore confine my observations to the two former. They differ, as you will observe, and as the illustration which I have taken the liberty of borrowing is expressly introduced by Mr. Erichsen to show, simply in respect of the precise locality at which the ligature is applied. The operation in either case is of general application, or rather, to speak more correctly, is restricted in its application only by the more or less of accessibility in the particular artery involved. A restriction, it will be observed to which Hunter's operation would obviously be more often obnoxious than that of Anel. The most favourable case of all for Hunter's method, as affording the widest choice of situation, is the popliteal, which besides being in itself the commonest of all forms of aneurism next to that of the aorta, was, no doubt as you justly observe, especially common among the gentlemen's coachmen in Hunter's hospital in Grosvenor Place. In this case then, Anel's plan places the

---

* Edition of 1876. † Edition of 1861.

ligature on the popliteal artery itself close down upon the aneurismal sac; Hunter's a few inches higher up upon that portion of the same vessel distinguished for convenience sake by the name of the femoral artery, just as, to recur to the analogy already used, one portion of the great thoroughfare between west and east is known as Oxford Street and another as Holborn. Carrying out that analogy, and assuming the injured gas main to have burst opposite Mudie's Library, Anel erected his barriers at Meux's Brewery and Hunter opposite the Pantheon.



Anel's Operation. Hunter's Operation. Distal Operation.

Now if Hunter's operation depended—as it unquestionably did depend—for the nourishment of the leg below the knee, upon the capacity of the collateral, or back street circulation for taking up the work formerly carried on by the femoral artery, upon what would Anel's depend for the performance

of that naturally belonging to the popliteal? The peroneal and tibial arteries by which the lower limb is nourished are simply prolongations of the latter, just as it is itself simply a prolongation of the former channel. So far as concerns their dependence upon this principle of collateral circulation the two operations are as the accompanying diagrams show, simply identical. Without it, in the one case as in the other, the limb deprived of its sustenance must inevitably gangrene and die. Without the knowledge of it, in the one case as in the other, no surgeon out of Bedlam would dream of attempting an operation the failure of which was a foregone and inevitable conclusion.

You will say perhaps that even though this be so your position is unaffected, for that Anel simply acted upon the principle discovered and pointed out by Hunter. I venture to think otherwise. And for this, amongst other reasons. *Anel died the year after Hunter was born.*

But if this should not be considered sufficiently conclusive let us go back a little further.

Guillemeau, like Anel a Frenchman, was a pupil of the great Ambroise Paré, barber-surgeon to Henry II., Francis II., Charles IX. and Henry III. of France. He too like Anel treated aneurism by ligature; like him too placing the ligature close down upon the sac. Whether Paré himself practised the operation I cannot satisfactorily ascertain. But as one of his chief claims to immortality—claims so far acknowledged in his own day as actually to preserve him from premature mortality on the fatal night of S. Bartholomew—was the revival of the ligature in place of the actual cautery for arresting the hæmorrhage resulting from wounds it is at least probable that he may have revived its use

in this direction also. For it would at all events have been a revival. Passing by Albucasis, the thirteenth century Arabian whose claim is not quite clear, let me go straight to the Alexandrian Aëtius who in the earlier half of the sixth century maintained the superiority of the double over the single ligature, an opinion shared, according to Miller,* by Abernethy. And further back still to the Roman Antyllus who in the fourth century applied his ligatures both above and below the aneurismal sac; or to Rufus the Ephesian whose operation for brachial aneurism in the first century might, but for the mere difficulty of dates, have been a copy of Anel's in the eighteenth. And finally to Celsus, to whom appears to be really due the credit of having discovered that the hæmorrhage resulting from the opening of the aneurismal sac might be arrested by means somewhat less "heroic" than the application of red-hot irons; and who flourished, if my chronology be correct, not very far from "the Year One."

Nor, for the purposes of my argument is it in any way needful to stop even here. If an artery be but once fairly obliterated it matters little, so far as concerns its future performance of its normal function of nourishing the limb beyond the point of obliteration, whether that obliteration be effected by a catgut ligature of the latest Listerian pattern or by the plain hot poker of the pre-Celsian period. Nor would that lack of acquaintance with the system of collateral circulation which may no doubt be fairly

---

* "Principles of Surgery," by James Miller, F.R.S.E., F.R.C.S.E., Surgeon in Ordinary to the Queen in Scotland, Surgeon in Ordinary to H.R.H. Prince Albert of Scotland, Professor of Surgery in the University of Edinburgh, &c., &c., &c. Edinburgh: Adam and Chas. Black. 1853. p. 541.

enough inferred from their general ignorance of the existence of any circulation at all, interfere in any way from the practical dependence upon that collateral circulation of the limbs on which they operated and which knew as much about the philosophy of their own sustenance then as now. Absolute ignorance of the whole circulatory system no doubt gave to Hippocrates as to his predecessors, a boldness in handling the knife which a partial knowledge, covering only the main whilst it missed the collateral circulation, would have lessened if not destroyed. But when the knife was once at work its influence was at an end. If after the obliteration of its main artery any single limb was saved through all the centuries, that limb was saved by the agency of the collateral circulation and by that alone.

And for Anel, at all events, the Circulation of the Blood was part of his professional A.B.C. The one link in the chain still left missing by Harvey, Malpighi's microscope had long since made the common property of every elementary textbook. Anel knew to a line the region nourished by the channel he was about to cut off. He knew just as precisely the result which must inevitably follow were there no other channel at hand by which that nourishment could be supplied. And if, knowing both, he nevertheless proceeded with his operation just as boldly as those who had performed it in blissful ignorance of both those vital facts the inference is surely tolerably clear that he had also a third compensatory knowledge, the knowledge of those collateral channels by which the interrupted circulation might as a question of theory and would as a matter of fact be supplied.

Let us see how far these indisputable and historical

facts justify—or otherwise—the four heads of your proposition. You say:

1. That before Hunter's discovery the only method known of treating popliteal aneurism was by amputation.

ANS. *Aneurisms were treated by simple ligature of the artery scientifically for years—ignorantly for centuries—before Hunter's birth.*

2. That the novelty of Hunter's mode of treatment consisted in tying the artery without removal of the distal portion of the limb.

ANS. *In this respect Hunter's operation, and that at all events of Anel, who died when Hunter was yet a baby in arms, are absolutely identical.*

3. That the basis of this innovation was the discovery by Hunter of the principle of the collateral or supplementary circulation.

ANS. *Anel's operation depended on the collateral circulation precisely as did that of Hunter; who could hardly have made, and could certainly not have communicated its discovery before he was born.*

4. That this discovery of Hunter's was the result of his vivisection of the deer.

ANS. *Omne majus in se continet minus. If the "accident of birth" prevented Hunter from making the discovery at all, à fortiori it could not have been the result of any particular operation of his, even were it one of those early experiments upon the legs and wings of flies in which the scientific proclivities of the embryo vivisector commonly find their first development.*

I think, Sir, I have here shown, with something of clearness (1) that, as I ventured to assert at starting,

you were fundamentally in error with respect to the nature of John Hunter's discovery. (2) That whatever may have been the novelty actually involved in it that novelty was not the substitution of ligature for amputation. (3) That this treatment by ligature was not, and could not possibly have been suggested by the experiment to which you have so confidently referred it and upon its origin in which you based your triumphant refutation of my assertion that vivisection "while it pandered to scientific curiosity added nothing to practical knowledge," and your eloquent appeal for powers of haphazard mutilation of highly sentient animals as unrestricted as those under which the Californian placer-digger drives spade and pick into the soil in hopes of an occasional nugget here and there.

But I have not yet done with John Hunter and his discovery. I undertook to prove not only that, as a matter of fact, that discovery was not based upon the particular vivisectional experiment to which through an entire misconception of its nature you had erroneously attributed it, but that it was essentially of such a kind as to be incapable of aid or illustration from any vivisectional experiment of any description. Let us see then in the second place what this famous discovery of Hunter's really was.

Before his time aneurism had, as we have seen, been treated in two different ways. *First*—let us say*—by amputation. Which, when it succeeded, of course left

* Whether, historically speaking, the "heroic" cure by chopping off the limb and dipping the stump into a kettle of boiling pitch preceded or followed the equally "heroic" treatment by knife and red-hot poker I confess myself unable to determine without more research than, on the whole, the question seems to be worth.

the patient a cripple. And which as a rule did not succeed; the ligature of the stump commonly coming away too soon and the man dying from secondary hœmorrhage. *Second*, by tying the artery close down upon and again immediately below the aneurismal sac, opening and cleaning out the sac itself and trusting to the collateral circulation for the nourishment of the lower portion of the limb. Which operation also commonly failed and from the same cause as the other, the failure that is to say, not of the collateral channels, capillary or otherwise, by which the circulation was to be carried on, but of the ligatures by which it was to have been cut off.

Now in spite of the vivisectional follies into which he was betrayed as naturally as, living a few centuries earlier, he would have followed the Philosopher's Stone, or the Elixir Vitæ, or whatever else might be special scientific wildgoose of the time, John Hunter was a man who thought. And this constant failure in one particular class of cases of an operation the success of which was the uniform daily experience in others, set him thinking. Why did these ligatures, which in a normal condition of the vessel might be placed in precisely the same situation with very tolerable certainty of holding on at least as long as their presence was needed, in contact with an aneurism lose all their holding power, and rapid as the process of obliteration is, come away before it was effected? And so the idea dawned upon him that the failure of the ligature arose precisely from the presence of the aneurism. How? Very simply, when once the solution has been discovered. The aneurism itself is a giving way of the coats of the artery under the pressure of the blood driven through it by the heart. That failure arises from

local weakness; that local weakness from local disease. In Anel's operation the ligature is placed carefully as closely as possible to the sac and therefore, in all probability, on the diseased patch; which, having already demonstrated, by the fact of the aneurismal rupture itself, its inability to bear the ordinary pressure of the passing current, is thus called upon to resist the far severer strain of its absolute obstruction by tying.

Go back beyond the diseased patch. Place your ligature upon a sound part of the artery where it is still in possession of its normal powers of resistance and the normal results will follow. In this case the popliteal artery is diseased. Ligature the femoral artery, which is sound, and your ligature may be safely trusted to maintain its hold till the diseased popliteal shall have been finally obliterated and the circulation established through the collateral channels.

This was John Hunter's "innovation," and a brilliant innovation it was. None the less brilliant, I venture to think, in that it was the result not of any mere happy-go-lucky hacking at the throat of an unfortunate stag, but of legitimate argument and logical deduction.

That it was so the conditions of the case itself prove at once "beyond a peradventure." *The whole theory of the improved method is based upon the assumed diseased condition of the affected part.* In the vivisected animal no such diseased condition exists, or can be induced. The only mention of vivisection by Hunter himself in his vindication of his theory is that of an abortive attempt to induce that condition by scraping away the coats of the carotid artery of a dog.

But I will go further even than this. I have shown that with Hunter's brilliant discovery in its original inception

vivisection as a matter of fact had, and as a matter of science could have, nothing whatever to do. Bear with me a moment longer while I show how, in its ultimate development, it was actually directed towards dispensing in great measure with the aid of that collateral circulation to the imaginary accidental discovery of which by Hunter you have erroneously attributed the origin of his invention.

As first designed, the tying of the femoral artery by Hunter's plan was simply, like the tying of the popliteal on Anel's, a first step in the operation, which was only completed by the opening and emptying of the aneurismal sac.* And one of the great objections

* I am quite aware that this statement is open to question. "The ligature of the artery on the cardiac side of the aneurism without opening the sac, was first done," says Erichsen, "by Anel in 1710." And he admits that "this operation, though attended with the risk of wounding or inflaming the sac, which was in close proximity to the seat of the ligature constituted a considerable advance in the treatment of the disease." But he goes on to say that "as Anel however performed his operation as a mere matter of convenience in a particular case and without the recognition of any new principle of treatment being involved in it, it attracted little attention at the time and does not appear to have been repeated by any of the surgeons of his day." Miller—commonly trustworthy enough in the matter of dates—places the operation in 1740, but that, as Anel died in 1730, is probably a misprint. At all events the operation, whatever its inception, seems to have remained altogether in abeyance, on the one reckoning for five and forty years, according to the other and more probable calculation for three-quarters of a century. Then, when in 1785 Hunter makes his grand experiment on the bricklayer at S. George's—an experiment, be it observed, which failed but the failure of which did not in the least disturb John Hunter's robust and well-founded faith in his own powers of deduction—we find him arguing the question *de novo* with an entire ignorance of anything of the kind having been attempted by Anel or by any one else which if not real is certainly

advanced against Hunter's invention was that it involved the inflicting of two wounds instead of one. This set Hunter thinking again, and gradually he arrived at a conclusion which involved not a mere modification of the accepted treatment, but an absolute revolution; and which, yet further developed by his successors, has led in our day to a system of treatment which dispenses with the knife altogether, and effects its object by simple compression with the finger.

The artery—argued Hunter—has been unable to resist the full strain of the blood pressure; but it has still some power of resistance. Now, suppose that instead of altogether cutting off the current, and relying exclusively on the supplementary circulation for nourishment of the limb beyond, we only apply to the sound portion of the artery just so much pressure as shall reduce the strain to such amount as the diseased portion shall be able to bear. Will not the current, thus slowed, choke up with its fibrous deposit, the mouth of the aneurismal sac? And will not its contents thus left to stagnate, be quietly absorbed by natural process, without local incision of any kind?

To all which questions John Hunter's clear intellect answered, yes; and so he advanced another stage, what has been justly styled one of the most brilliant achievements of conservative surgery. And beyond this he himself did not go. It was left to his successors—I frankly confess that I am not quite certain which of them—to

---

assumed with most remarkable skill. It is no doubt conceivable that Hunter may have stolen the idea of leaving the isolated tumour to the process of natural absorption from Anel's brachial operation in 1710 but the evidence does not by any means appear to me to bear out the assumption. And even if it were so it was from him that the idea, left fruitless by its originator, received all its practical life and development.

carry out his argument still further and to substitute for the incision and the slack ligature a simple digital pressure on the external surface.

So much then for John Hunter's improvement in the treatment of aneurism; for its origin in the supposed new discovery of the collateral circulation, and for our indebtedness for that discovery to Hunter's experiments on the antlers of the unfortunate Richmond stag. I leave it to you, Sir, as a man who, whatever his devotion to "Science," is not, I hope prepared, as too many of the common crowd of scientists would seem to be, to sacrifice upon its altar his own candour, honour, and self-respect, to say frankly how far I have proved, or failed to prove, my point; how far I am justified in claiming, as I do claim, the case you have put forward in refutation of my position as the strongest possible evidence in its support; in asserting, as I am bold to assert, that that pandering to scientific curiosity with utter barrenness of practical result, which I maintain to be the special characteristic of vivisection, needs and can receive no more unanswerable illustration than this too famous story of John Hunter and the stag.

But one word more and I have done.

We, opponents of Vivisection, are taunted everywhere with ignorance of our subject and incapacity for arguing it. Its advocates are never weary of asserting that the facts and reason of the case are alike wholly on their side. But never yet has a too great confidence in their cause betrayed them even for a moment into the weakness of openly discussing any of the points on which their position is so unimpeachable or of meeting

argument with any riskier answer than assertion and abuse.

I venture, Sir, to hope that in this, as in so many other respects, you will show your superiority to your colleagues. I offer you, with all respect, a scientific argument. Meet it I pray you, as it should be met by a man of science. If it be ill-reasoned, refute it. If it be untrue, disprove it. If it be absurd, demonstrate its absurdity and ridicule it and its author as may seem you good.

But if it have a show of reason or a substratum of fact; if there be in it anything to lead you to suppose that you may possibly have overstrained your own case or overlooked any of its conditions, do not condescend to that "policy of silence" which however befitting to the *Lancet* and the *British Medical Journal* is surely unworthy of one who occupies a position and bears a reputation such as yours.

I am, dear Sir,

Very faithfully yours,

CHARLES ADAMS.

Office of the Society for
Protection of Animals from Vivisection,
1, Victoria Street, London, S.W.