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THE LIFE
OF
JOHN HUNTER.

see 3117

By JESSÉ FOOT, SURGEON.

SINE IRÁ ET STUDIO, QUORUM CAUSAS PROCUL HABEO.

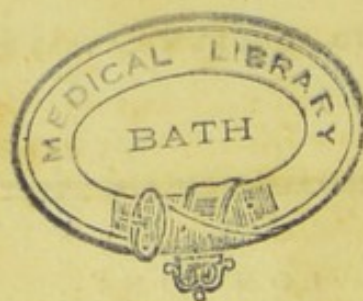
TACITUS.

LONDON:

PRINTED FOR T. BECKET, PALL-MALL.

M.DCC.XCIV.

1894



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ADVERTISEMENT.

I HAVE arranged this Performance into the four following Parts—besides the Introduction—in order to procure, a Rest, for the Reader—and to establish, for myself, a System in the Execution of it.

INTRODUCTION. - - - page 1

PART I.

FROM THE TIME OF HIS STUDY IN THE SCHOOL OF ANATOMY, AND CONSEQUENT TRANSACTIONS, TO THE YEAR 1760 - - - page 9

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SERIES OF TRANSACTIONS, FROM 1770, TO THE CLOSE OF HIS LIFE; WITH AN ACCOUNT OF THE PROGRESS AND ARRANGEMENT OF HIS MUSEUM, - - - page 239

*London, Dean-street, Soho,
April 6, 1794.*

THE

ADVERTISEMENT.

I HAVE written the following into the form of a letter to the friends of the cause, in order to present a full and complete view of the state of the cause, and to show the necessity of a more extensive and efficient system of education.

INTRODUCTION.

PART I.

FROM THE TIME OF HIS DEATH IN THE YEAR 1793, AND CONSEQUENT REFORMATION, TO THE YEAR 1800.

PART II.

FROM THE TIME OF HIS REFORMATION IN THE YEAR 1800, TO THE YEAR 1810.

PART III.

REMARKS ON THE STATE OF THE CAUSE, AND THE REFORMATION IN THE YEAR 1810, AND THE YEAR 1820.

PART IV.

REMARKS ON THE STATE OF THE CAUSE, FROM THE YEAR 1820, TO THE YEAR 1830, AND THE YEAR 1840.

London, 1844.

THE

THE LIFE

OF

JOHN HUNTER.

INTRODUCTION.

HE who engages to write the life of an eminent professional man, when he has closed an active scene, and descended into the vale of eternal rest, must be considered as attempting an important undertaking, increasing the stock of human information, and furnishing another example for directing the conduct of those now walking upon the face of the earth, and of ages yet to come.

It does not necessarily follow, that he alone is an object for historical inquiry, who has been eminent in dispensing an increase of happiness to mankind, conferring useful knowledge in a greater

proportion, or carrying the grand career of genius into practice: the best and brightest examples of men should alone be the models for imitation; but yet it will be ever found necessary, for the purpose of inculcating their true value, to form critical comparisons with characters that have betrayed a contrary inclination.

The progressive improvement of science cannot be more purely marked, nor the particular faculty of him who is supposed to have been of importance in its cause, cannot be better ascertained, than at the moment when his powers are ceasing to act, and his personal influence is withdrawing. This is the immediate point of time which is most favourable for shewing, what has been done on the same given subject by others, and what was done distinctly by him; what was known before, and in his own time to others, and what was made known distinctly by him.

The historian is better prepared for doing an act of justice, for demonstrating truth to the public eye, and must necessarily expose himself to detection by living witnesses, if he aim to pervert either. He cannot plead ignorance from want of information; he cannot say, that his documents have been carried down the rolling current

rent of time, and swallowed up for ever in the gulph of oblivion ; with his memory fresh, and his attention awake, he can neither be forgetful of the professional points then acquired by another, nor lessen the value of discoveries made before the year 1794, by what might be in progress hereafter. The advancement of science will be more accurately traced, by noting periods distinctly ; and that fair title to his own invention, which every man of genius has the right of claiming, will not be so liable to be confounded nor impeached.

It is a lamentable consideration, that characters desirable to posterity should be so commonly deferred, from a habit of indolence, a system of delicacy, or a motive of fear. Prominent features, which can be but barely recollected, are thus defaced ; and the memory being incapable of recalling what has never been deeply impressed, admits of fabulous conceits, which are thus so frequently found to be intruding upon the places of truth.

An historian of the time present rarely likes to be seen unravelling a character, for the purpose of demonstrating what has been with studied art concealed ; of distinguishing betwixt an

inclination for fame, by direct or indirect pursuits ; he will shrink from the office at the very time he should have undertaken it, and more especially, when the splendour of reputation intimidates his mind, increases his awe, and mocks his resolution. But this is the very time that independent judgment would decide for the challenge of inquiry ; whilst the impression is yet fresh, which reputation has made upon opinion, whilst the undertaking cannot be said to be less just, because it is bold ; nor less honest, because it is conscious.

There will always be found a ready race of panagyrists, who may be said to be distinct from the true historian ; who draw their portraits in miniature, and are smooth and flattering, telling rather what a man ought to have been, than what he was. These are so little resembling the originals, as to entitle them to no rank in the scale of common humanity ; both incapable of informing, or instructing, these seem to have no relative concern with the business of life, doing no credit to the head, nor honour to the heart, neither promoting the cause of truth, nor advancement of knowledge. These are found to be so useless, as rarely to command the shortest notice ; and therefore, amidst the thousands that have been pro-

produced, but few have exceeded the life of an ephemeron. Their common lot is an unbeneficed oblivion, neither finding their way into any public libraries, nor becoming tenants of the attention of mankind, as long as tomb-stones of the same personages in church yards.

Of the professional life of JOHN HUNTER, whose celebrity hath attracted my attention in common with the rest of the world, I shall not open the account with a boast of uncommon encomium. I must be content with telling, that I write more to inform than to praise, more for example than glory; that I intend to reason from consequences, rather than strike the mind with splendid attractions of admiration for the character I am about to display.

Heroics are extremely well suited to the design of him, who is undertaking to draw the character of a warrior, whose renown in battle has struck terror among the inhabitants of the globe; and who has spread desolation wherever he was engaged: there heroics must necessarily be the substitute for truth, whenever he experiences the common lot of human nature. But to trace improvements in the arts of anatomy and surgery, to examine into the result of mental appli-

application, in a pursuit connected with philosophic study, to penetrate into the concealed designs of the heart, and watch its bias, to investigate pre-determined points, and separate them from the open effusions of genius, requires firmness in mind, not volubility in words, settled axioms of truth, not copious streams of fiction.

This undertaking does not consist in a detail of resources in the mind, for obtainment of power, or aggrandizement of fortune; but is chiefly confined to the close investigation of the progress of anatomical and surgical improvements. I am therefore bound to establish my plan upon justice. The charge which I have taken upon myself stands exactly upon a similar basis of a judge, who never acquits nor condemns any one from the narrow motive of partiality, but every one is treated according to the nature of his case.

Nor does it follow, that because I undertake to write the professional life of John Hunter, I am indulging myself upon a subject most suiting to my will, or complying with the bent of my most favoured inclination. I can obtain by this no reputation I had not earned before; I can display no power opportunity had not given me; the

the subject admits of no rhetorical decoration, which my ambition or imagination ever thirsted after. I might be in truth only considered to be pursuing what I began, only compleating what I undertook in his life time, from an intention of doing justice to my own understanding, when some, who were older than me, declined it ; and the younger dared not attempt it. And I will not presume, but assert the privilege of exercising the result of a professional education, and abiding the test of solid judgment in an inquiry after truth.

To allay the tender apprehensions of those, who plaintively expressed their fears and anxieties for me, and who persuaded me to decline the work ; to enlighten the blind admiration of those who never having read a single line he has written, believed him to have been the first Surgeon of his time ; and to inform the implicit, but zealous pupil, who relying upon the truth and integrity of his master, without consulting his own understanding, was persuaded, that the latest discoveries, and newest opinions of John Hunter, could not be found already registered in former authors ; this professional life, if I mistake not, will be found to be not badly calculated.

Thus

Thus as far as they carry conviction to any man's understanding, my labours may be of use ; beyond the evidence it carries with it, I advise him not to follow any man's interpretation.

PART I.

FROM THE TIME OF HIS STUDY IN THE SCHOOL OF
ANATOMY, AND CONSEQUENT TRANSACTIONS, TO
THE YEAR 1760.

IT is not my intention to enter into a minute account of the life of John Hunter; nor to pretend to be nicely accurate in the dates of those domestic transitions, from childhood to that period which terminates the career of human action.

John Hunter was a younger brother of the late Dr. William Hunter, and was born in the county of Lanerk in Scotland, some time about the year 1728.

Nothing that has reached my knowledge till lately, has been said of the transactions of his youth. And as I do not aspire after adding another instance to the natural desire in man for propagating wonders, I shall suppose that when John Hunter was in the arms of his nurse, he was seen exactly like any other child in a similar situation; that he was not discovered in performing any of those romantic feats, which have been

said, by the second sight, to be precursors of future great achievements; that he was neither detected in playing with a serpent, thrusting his hand into the mouth of a lion, nor staring the keen eyes of the eagle through and through, till he forced him to blink at his own, the keenest. Nor shall I attempt to amuse with any anecdotes of young Hunter, during his scholastic education; whether his genius was so unbridled and overbearing, as not to be brought to submit to the trammels of discipline; whether from that time he had fixed the determination, never to read, which he has been declared to continue during his latter days; nor whether he had any education, excepting such as those have, who are bound apprentices to a common trade.

A wheelwright or a carpenter he certainly was, until the event of William Hunter becoming a public lecturer in anatomy, changed the scheme of his future occupations, and determined him to accept the invitation of his brother: to lay down the chissel, the rule, and the mallet; and take up the knife, the blow pipe, and the probe.

The first professional performance of his, was presented to the public eye in William Hunter's Medical Commentaries; as if it had been written

ten by John Hunter in the year 1756. But whether it was published in any other way, before it appeared in the Commentaries, I cannot tell. At any rate, it was said to be drawn up by John Hunter, if not then published, in the year 1756, in justification of some disputes about anatomical discoveries, then agitated, and vehemently conducted betwixt the two Hunters, and the three Monroes of Edinburgh, the father and two sons. These disputes extended to three different discoveries, and involved in them besides, the illustrious De Haller of Gottingen, and the late Percival Pott.

The particulars of these disputes I shall proceed to discuss, in order to discover what was the share that John Hunter took in them; what was the display of moderation and genius, which he had shewn when in the vigour of youth, in the opening of his understanding, and in the dawn of his introduction to that anatomical theatre, where he has been supposed since to have acted, from the superior powers of the mind, a part so conspicuous, as to become in the end the first anatomist, and first surgeon, in the world.

In the year 1746, William Hunter succeeded Sharpe, in reading a course of lectures on anatomy and surgery, to a society of navy surgeons,

at a house somewhere in Covent Garden; in which he gave not only that satisfaction, which fairly promised to him the future fame he acquired as a lecturer, but also induced him to extend the plan of his lectures, so as to bring it to be a perfect school of anatomical instruction.

It was about this time, that John Hunter entered into the dissecting room of his brother, when he was about eighteen years old: and I have no reason to doubt, but he was of great service to his brother, and in as short a time as any young man with the same education could have been. That he was always of a turn to industry, is very clear; and that the pursuits of anatomy are not much retarded by the want of education, may be believed; as whatever was of value that treated on anatomy, has been soon translated into English; so by this, he was enabled, with the assistance of his brother, to select the subject which claimed his closest attention, whenever the same was then attracting the attention of anatomists in other countries.

His mind was led with the easiest inclination, to pursue that study his inquiries had approved; and with probably more sincerity and ardour, than if his education had been of a more liberal nature. He had found an unexpected path, directly

rectly leading to fame, opened before him, and that without another choice; for an uneducated man is undoubtedly restrained in his election of the occupations of life; he cannot wander without a guide; some one there must be to instruct him; and John Hunter, thus conditioned, wisely availed himself of his brother's assistance, for conducting him to eminence in the profession of an anatomist.

As soon after his initiation to the anatomical school, as the progress in science will ever permit, he was found to be conducting such experiments, as tended to be useful in the lecture room; and to be advancing the knowledge of some anatomical doctrines, which, at the same point of time, were occupying, or had occupied, with an equal degree of diligence and fervor, the attention in the anatomical schools of Göttingen, Berlin, Leyden, and Edinburgh.

The principal subjects of inquiry, which arrested the attention of all the anatomists of that time, were

1. Of the Injections of the Testis :
2. Of the Origin and Use of the Lymphatic Vessels and Absorbents by Veins :
3. Of

3. Of the Discovery of the Hernia Congenita.

I do not profess a desire to be esteemed by those philosophers, who call themselves the foundest, from having discovered that all the actions of mankind tend to a necessary use: as if man were not endowed with a conscience, and had not a discretionary privilege of discerning right from wrong. Naturalists may tell us, if they please, that in the operations of nature, there is nothing useless; and, as the assertion is general, I have no desire to disturb it by contradiction. The plea of necessity would be but poor, when urged by one, who contradicts another without his improving the subject which gave cause for the contradiction. Whenever the intention is just, reason will not permit the will to be inactive.

These reflections arise from a strong indignation against those who presume, that because men differ in opinion, they must necessarily be turbulent in their dispositions; as if it were impossible for a difference in opinion to be conducted by reason; or for a spirit of emulation to be raised, without a consequent destruction of those who engage in it. The praise or blame arising out of every dispute is relative in its nature, and
solely

solely dependant on the motive for which it was pursued.

I shall explain these disputes in the order I have placed them, by beginning with that on the injections of the testis; and which originated with William Hunter, in the manner that it is seen by the account of it which appeared in the Critical Review for November 1757, under the title of "Facts relating to the Dispute between Dr. Hunter and Dr. Monro," and to which I refer my readers, or to Dr. William Hunter's Commentaries.

I. OF THE INJECTIONS OF THE TESTIS.

WILLIAM Hunter has there asserted, that some time, as a week or a fortnight, after this first public demonstration, his brother, John Hunter, made a trial of injecting the testis, and it succeeded. And here we behold the first fruit of this young anatomist, offered up as a sacrifice at the shrine of disputation; which was only the beginning of many more contests, and which gave cause to an anatomical war upon paper, not finally concluded during five years—half the time of the siege of Troy.

From the statement of the case, as given there by the parties, I shall directly proceed to draw my conclusions.

It is of the utmost importance, for ascertaining the degree of ability in any man, to distinguish betwixt an invention, which is the mere result of industry, and one which is solely dependant upon the powers of the imagination.

The subject which claims my present investigation, could not have been decided by the mere exercise of the imagination; but it must have

have been first suggested, and afterwards proved by experiment.

The suggestion of it in thought was no mark of genius in any one; it was a common and familiar piece of knowledge, which required the art of injection to demonstrate; and that demonstration was the only difficult obstacle in the question.

Every one must have known the purpose of the testis, and that what was elaborated by it was in coitû discharged through the penis. The common operation of stoning lambs, to prevent them hereafter from copulating, proves the knowledge to be in vulgar practice. Therefore, the most important object of injecting the testis, was nothing more than surmounting a difficulty, by demonstrating what was already known, its tubular vascularity. Who first undertook to make the experiment by injections, who first succeeded in it, and who first imparted that success by publication;—this was the pith and marrow of the contention.

It appears, by the answer of Donald Monro, that his father, the professor, had prosecuted the idea as far back as the year 1747; and that de Haller had not only been prosecuting the same
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idea by experimental injections, but had also given the result of them in the Philosophical Transactions; and that professor Monro had likewise given the result of his success in the Medical Essays.

It appears, that the idea was not only first suggested by de Haller and professor Monro; but that experiments were commenced by them before William Hunter had begun to teach anatomy, and before the brother John had begun to learn it.

It appears, that William Hunter was far behind the two professors, de Haller and Monro, in beginning to prove, by ocular demonstration, a connection of the ducts, coming out of the testis, to form the epididymis: as in a note annexed to the evidence of Henry Watson, for confirming the complete preparation of an injected testis, shewn by William Hunter in the autumn course of the year 1752, he says, "I take the opportunity with pleasure of doing this gentleman the justice that I did at my lecture, with regard to his observations upon the testis, by declaring, that he first shewed me the ducts coming out of the testis to form the epididymis in a preparation where he had traced them by dissection with great accuracy."

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It appears, that, after the suggestion of the idea of injecting the testis, and after the experiments to confirm it had been made, and were making by de Haller and the Monroes, the Hunters were active and indefatigable in the same prosecution; and that they succeeded as early, as those who had the advantage of them, by beginning sooner.

It appears, that young Monro, when he announced by publication his success, in completely injecting the body of the testis, was not apprized of the like success by any other anatomist: as the first, supposed to have been so completely done by William Hunter, had not been divided, and consequently not proved; and as the event of the success of the second, done by John Hunter, had not come to his knowledge, when he published concerning the success of his own injection. But whether the Hunters had succeeded in both their injections, or neither, it matters not in the question before us, in point of right or reason. If both the Hunters had successfully injected, and if both had confirmed it; if the Monroes, both professor and sons, if de Haller also, nay, if all the professors in Europe had been apprized of the fact, yet the two preparations belonging to them could not have produced a third, belonging to another.

These two preparations were not Monro's; he had made that which he assumed the right in, he had not only made it, but had demonstrated it; and having so done, he had the common right of publishing it, without the least imputation of theft, and of which he was accused by the Hunters.

It appears, that the Hunters, who of all men cannot, nor ever could have been suspected of being negligent in maintaining their anatomical rights, nor diffident in meeting the eye of the public, have not only in this dispute, but throughout their lives, founded their complaints upon an unreasonable plea; whether intentionally, or from a weakness in their nature, I will not hastily decide. They have uniformly expressed themselves, upon every cause of dispute, (and the whole of their history is composed of disputes,) as if they conceived that, what had been read or demonstrated by them in the lecture-room, was virtually and actually published; that it was, *bona fide*, equal to, and had all the right and effect of an open publication.

What they were desirous of being considered as a publication, was only a demonstration. A preparation cannot be published, but a plate from it may. They both seem to be convinced
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of the impossibility of uniting the differences upon certain occasions: as whenever they found it their interest not to be reduced to the necessity of so feeble a resource in argument, as whenever they chose to claim a right, which they could claim by common means, they always discovered strong signs that they did know there was a palpable distinction betwixt demonstration in a lecture-room, and a printed publication: for no anatomists have published more than the Hunters; and no anatomists have swelled their publications with such varieties of anatomical plates, not only taken from the original preparations, which they demonstrated, but also from the appearances of parts which they had dissected; and to demonstrate which in their successive courses, if they had not thus published their plates, they must have dissected the same over and over again.

It appears, that there was a trial of the art of injecting the testis, pursued in common, by de Haller, the Monroes, and the Hunters; and that the success in it was become proper to de Haller and the Monroes, by their publications: but that the Hunters, until they came forth with their complaints of what others had done, had never published. But if the Hunters had published their success in the experiment ever so late,

late, without impeaching that which fairly belonged to de Haller and the Monroes by their having published more early; if they had left their reputation for diligence in anatomical pursuits to stand upon the merit it really possessed, they would not have been undervalued; nor, for the first time, exposed themselves to strong suspicions of their naturally possessing the torment of jealousy; the strongest symptom of a weak understanding.

And lastly, it appears, now the art of injecting is improving, that the difficulty of success is only surmounted, by attending to the proper subject for injection: and that it should be only attempted on a testis of a subject which was in a state of perfect health, and whose age was favourable for a perfect secretion of seminal fluid; who had not undergone emaciation from sickness, and whose seminal vessels had been emptied a short time before his death; and that it succeeds to a greater certainty on the testis of a large animal than a small one.

Before I close this subject, I beg to be indulged in bestowing my tribute to the memory of Henry Watson, in whose collection I have seen many injected testes. He paid his last debt to nature a few days after John Hunter. And
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it is due to his reputation, that his name should find a record in this page, lest his modest merit might have otherwise, passed away in silence.— He was Surgeon to the Middlesex Hospital, at its first commencement; but resigned that for the Westminster Infirmary, at which he continued to the day of his death. Some time ago, being rendered infirm, through a paralytic stroke; his end was hastened by the alarm of a fire in Rathbone-place, in the vicinity of his house. He died very far advanced in years.

He had formerly read lectures on anatomy in the Borough, and possessed a very extensive well chosen collection of anatomical preparations. He was Fellow of the Royal Society, and published many papers in the Philosophical Transactions, and in the London Medical Journals. He had been a very good operator, and a surgeon of sound judgment; very easy of access, and modest in his communications.

He was one of the examiners at the Surgeon's Hall. When in his duty, he never contracted the frowning brow, to confound the diffidence of youth; but by the placidity of his demeanour, solicited a display of the knowledge they possessed. He had a considerable taste for music, and filled up his leisure hours with the solace of harmony.

2. THE DISPUTE BETWIXT THE HUNTERS AND MONROES, ON THE ORIGIN AND USE OF THE LYMPHATIC VESSELS, AND OF ABSORPTION BY VEINS.

THE part which John Hunter appears to have taken in the dispute upon the present subject, is, by his attempting, out of date, to prove, by experiments made on five animals, that there was one, and but one system of vessels for absorption. But if these experiments, made by him, had come forth before the publication of young Monro upon the same subject, he would have then produced them in proper season, for entitling him at least to a share of claim in the discovery, properly belonging, so far as it relates to the Hunters, to Monro solely.

These experiments on five living animals were began by John Hunter, in November 1758, and finished in August 1759: they are concluded with the following emphatic words: "Here is a new doctrine proposed in physiology, viz. that the red veins do not absorb in the human body."

On the appearance of young Monro's treatise, the conduct of the Hunters went no farther than to say, that they had taught, at their lectures,

lectures, what young Monro had pretended to discover: and that what he had published as his own discovery, they had not only taught, but he had stolen from them. They did not then say that they had published any thing upon the subject, as they most assuredly had not; but they relied upon the plea they had before been in the habit of practising, namely, that what they taught and demonstrated in their lecture-room, was virtually equal to a printed publication.

But it does not yet appear, that the Hunters were serious in this assertion; for if they had been, John Hunter would not have gone about experiments, beginning in November 1758, and ending in August 1759, and by them to prove to the world, that he knew what young Monro claimed as his discovery, before young Monro published it,—and to prove that he knew what young Monro had published in the beginning of the year 1757, by what he had discovered by experiments conducted betwixt November 1758 and August 1759.

Strange as it might appear, this is the sort of argument offered in their vindication by the Hunters. And by their argument, it appears, that if they had been the authors of the dis-

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covery

covery claimed by Monro, but which had been stolen from them, why had they not proved their title in the fairest way, and made it out in an intelligent manner? Whereas the experiments of John Hunter contradict their own assertions; as they were made to ascertain the truth of young Monro's discovery, nearly two years after his publication of it. So that the triumphant words with which his experiments are concluded, at so late a season as August 1759, when compared with the result of the knowledge imparted by the publication of young Monro's treatise, in the beginning of the year 1757, appear to have lost all the effect of that ingenuity which, among men of erudition, commands a competent admiration.

In the Critical Review for September 1757, the reviewers concluded their account of Dr. Monro's treatise, *De Venis Lymphaticis Valvulis*, with very severe remarks. These were made evidently with the concurrence of the Hunters. For the charge of the Hunters, and the reply of professor Monro, I refer my readers to that Critical Review, or to William Hunter's Commentaries.

Professor Monro has there said, that what John Hunter and his brother have done, since the
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the beginning of the year 1755, when young Monro's thesis was published, declaratory of the publication that followed in the beginning of the year 1757 at Berlin, is out of the present question.

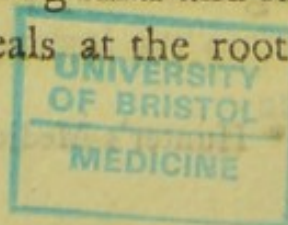
But lest the reader should be at a loss to know what John Hunter was doing all this while; and how it came to pass that the subject either slept with him, or that he was distanced in the race he attempted to run with the young Monro, I think I can satisfy him as to that point, by proving, from William Hunter's own words, that John Hunter would have prosecuted the investigation sooner, if he could; and that he positively did not prosecute it, notwithstanding any assertion by them to the contrary.

"I had made," says Dr. Hunter, "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 go-

ing 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

to be anticipated, I treated the subject lightly at my lectures, and to the best of my remembrance, only mentioned his manner of filling the glands, and the easy method of raising the vessels wherever there are such glands, and his opinion of the thoracic duct climbing so far as the upper cava, instead 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 sake of being first mixed with almost all the lymph of the body. Both these observations I made as from my brother, when Dr. Monro attended me; and when the hurry of dissections was pretty well over in the spring, my brother sat about a preparation, which he proposed as a basis for his intended description and figure of the absorbing system. Dr. Smith of Oxford happened to be in town at that time, and being much pleased with the intention and with the preparation, was frequently in the dissecting room while my brother was dissecting, and while Mr. Riemsdyk was making the drawing: so were many gentlemen of our acquaintance, besides students.

“In that preparation and figure, the lymphatic vessels from the ham upwards to the thoracic duct were seen, as well as the inguinal and lumbar glands, all the larger lacteals at the root of the



the mesentery, the receptaculum chyli, (or what is so called) and the thoracic duct, all I say, finely filled with mercury. So far my brother had gone. A very indifferent state 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 dislike of having any dispute with Dr. Monro, which, by his father's letter in the Critical Review seemed to be threatened, all these things, I say, have from that time made him lay aside the scheme; and he will hereafter finish it, or not, as he may think proper."*

By this quotation, it clearly appears, that the Hunters were never prepared with their figures for publication; and that Nuck, Meckel, de Haller, and Monro, had anticipated all their procrastinated projects, and made discoveries for them.

From the true statement of the question, one can hardly find a single justifiable cause of complaint from the Hunters; and one is at a loss for a reason, why the appeal to the public was so clamorously made, for what had been done by others; whilst the choice was open to them, for doing what they pleased, and for exhibiting their drawings

* Hunter's Medical Commentaries, p. 34—35.

drawings when they pleased. I shall not dwell upon the insidious manner, by which they designed to reserve their information, as they have betrayed of themselves as much of that part as I am desirous of being made known. But ought investigation to wait upon their leisure, and be suspended by the rest of mankind, for them to reassume it whenever they chose? Is nothing right or just, which is performed by another, when it had not previously their approbation? If any production anticipated them, the cry directly was, that they were forestalled; if any anatomical question was agitated by them, and another at the same time, the cry directly was, that they were robbed; or if their names were not mentioned by an author, although it were impossible for the author to know that they were engaged in the same pursuit, or even their sentiments about the subject he had written upon, the cry directly was, that they were most flightingly and indecently treated.

Proud as we are of the inventive faculties of our countrymen, the investigation of the absorbent system is much more belonging to foreign anatomists than our own: and I am afraid almost to assign my reason for the sentiment; but I believe it to be a just one. I believe that the abilities of Europeans in general are equal;
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and that the success of the foreign anatomists beyond our own, is not derived from superior faculties, but because they can afford to pursue a study, which requires the use of time and brings in return no profit, better than we can. The German professors have all of them provisory incomes, and are established by the government with liberal conveniencies.

This observation particularly arises out of the present subject; as it is proved to be of that arduous nature, as to have attracted the attention of the most eminent anatomists for the two last centuries.

The perfection of that discovery, which ascertains the structure and use of lymphatic glands, which traces the lymphatic vessels all over the body, which gives a complete description and figure of the whole of the absorbing system in every class of animals, and which proves it independent of branches of sanguineous veins and arteries, brings with it an increase of knowledge creative of wonder: it is such a discovery, as no single man, endowed with the greatest patience, strongest mind, keenest eye, quickest sense, and utmost longevity, could have compleated.

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To prove that the system of absorbents must have been brought to perfection by the progressive advances of many anatomists, in many ages, and that it could not have been perfected by any single genius, is easily done by referring to the progressive history of the discoveries. But independent of this, I might say, with a moderate and rational definition of genius;—that, if it consist in an intense application of a mind fitting by capacity for this particular study, and concentrated to this object alone,—such a genius could never have completed the doctrine of the absorbent system, as we now find it. The nature of the discovery must pre-suppose an accumulation of mental ability, a patient exercise of human labour.

The lacteals had been seen by ancient anatomists, before their offices were made known: Herophilus and Erasistratus take notice of white vessels, but were unacquainted with the use of them. Casper Asellius, in the year 1622, appears to have been the first discoverer of the purposes of the lacteal vessels: and soon after, when dissection of living animals became the general practice of anatomists, Rudbeck and Bartholine discovered the lymphatic vessels: and this progress was farther improved by Picquet,
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who discovered the thoracic duct, which had been traced before by Eustachius in a horse.

So far a foundation of this wonderful system in the animal œconomy was laid, when Nuck produced his complete treatise on the absorbent vessels; and the patient eye of Lieberkuhen distinguished the anatomical structure of the *villi* of the intestines,—of the beginnings of the lacteal vessels, from the internal surface of the villose coat and their orifices;—termed by him, *ampullulæ*: and so far as these discoveries had advanced to his time, I shall refer the reader who may be solicitous of pursuing the investigation, to the seventh volume of the illustrious de Haller.

When the subject of enquiry had been eagerly conducted by these anatomists, who may be deemed to be of the class, in point of time immediately behind the present; among the names of which, I shall take notice of Nuck, Ruysch, Duvernai, Lieberkuhen, de Haller, Cowper, Mekel, Nichols, Akenfide, Monroes, and the Hunters,—all of whom enriched the theory, by partial researches into comparative appearances of the system of absorbents, throughout the general race of animals;—it was for a while pursued no farther. By some it was discontinued,
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from an arrest by death; by many exhausted from a tedium of the mind; and by others abandoned, partly from disgust, partly from anticipation, but principally because the study produced no emolument for recompence from failure of success, nor reparation for loss of time. The subject appeared to be sleeping, whilst John Hunter was upon the wing, to quit the lecture room for the army; and whilst the brother William found out from inference, that he could employ his time to a better account, both in point of profit and in point of honour:—the laurels which both the brothers gained in this contest not being worth the scramble they made for them.

I am greatly at a loss to point out, what advances the Hunters have made in this branch of anatomical science. There is no trace apparent of any thing done by them, but the experiments of the five living animals, made by John Hunter, betwixt November 1758 and August 1759; and by which, it is said, that he discovered the system of absorbent vessels to be independent of any absorption by sanguineous veins and arteries.

If these experiments were made at the time they were dated, how came they not to be then published; especially as the design of them was

to justify the Hunters, in asserting, that what young Monro had published of the system of absorbents being independent of sanguineous veins and arteries, was a thought stolen from them?—If these experiments, made at least eighteen months after the publication of Monro, were not published as soon as they were made, to reclaim that right he had forestalled them in—how much less could they hope to establish that right to themselves by publishing them, for the first time, at the time that they did, which was in the year 1764, after John Hunter's return from the war,—five years after they were made,—and seven years after the supposed theft which was the justifying cause of their being made? And although I have given each of them a niche in the temple which Fame has erected to the progressive discoverers of the absorbing system, yet my reason will hardly suffer me even to admit, that what was published by them on the subject, has barely earned the situation.

At this period of anatomical history, when the prosecution of this branch of the study was somewhat suspended by others, Hewson enriched it by discoveries, additionally to those who had gone before him. By innumerable laborious dissections, instituted on living and dead animals, he discovered and injected the whole
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of the absorbent system in birds, *amphibia*, and fishes; and by these important discoveries, he proved the universality of the system of absorbents; and by demonstrating their existence in the various classes of animals, he rendered it more than probable, that the veins even in them do not absorb. These discoveries of the immortal Hewson, were considered as so important in anatomy and physiology, that they were rewarded by the Royal Society of London with Sir John Copley's annual gold medal. But his grand career of modest merit and patient professional labour, was prematurely stopped in the year 1783, when he fell the victim of a putrid fever.

That eclipse which overshadowed the anatomical honours of the English nation from the public loss of Hewson, was a short time after dissipated by the succession of Sheldon; the present professor of anatomy to the Royal Academy. The last work of Hewson had illustrated, by figures, the lymphatic vessels of the extremities and trunk: but he had left no representation of the lacteals, nor of the lymphatics of the different viscera of the thorax or abdomen: the lymphatic vessels of the viscera, and the lacteals in the human subject, were but imperfectly known, even at the late period when we were deprived of him.

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To supply this deficiency, Sheldon has corrected the error of former authors, who had published on this subject, and given many elegant figures from nature, of those parts of the system in the human body, where they have been falsely described, and where they have been recently discovered. But this does not comprehend the whole of his work; its value is very extensive; his unreserved discovery of the art of injecting, to which the great progress of anatomical science is chiefly indebted, claims a tribute of praise for ingenuous candour, and ought, by its example, to induce anatomists in future to publish, as he has done, whatever they know respecting anatomical investigations, which have been derived from injections. The progress of the science has been much impeded by a mystery among anatomists, who have generally concealed the composition of their injections, and their methods of dissecting, injecting, and preparing the different parts: a mystery unworthy the character of a philosopher.

I am now to tell the reader how the high spirit of the sublime author of "the Pleasures of Imagination," became somewhat ruffled by this dispute, insomuch as to have drawn from him a smart reply indeed, but such a one as demonstrated who it was that was called upon in vindication

dication of the feelings proper to a refined scholar, and an accomplished gentleman :

—— ingenuas didicisse fideliter artes
Emollit mores nec finit esse feros.

Young Monro, in a postscript to his anatomical observations, had taken notice of what he called inaccuracies, in a paper published in the Philosophical Transactions, for the year 1757, written by Akenfide, on the origin and use of the lymphatic vessels of animals. In reply to this, Akenfide published notes on the postscript, and animadverted smartly, and indeed with seeming justice, on young Monro, for affirming that he only hinted as a conjecture, in the Gullstonian lecture, what in fact, from premises supported by argument and experiment, himself assures us, he described as the very next thing to a physical certainty ; and likewise for insinuating, that Akenfide's paper owed its appearance to Monro's treatise *de Glandulis Lymphaticis*.

Akenfide evinces, that Monro, in most of his remarks upon his paper, either misunderstood or misrepresented his meaning. In reply to Monro's objection, that the lymphatics are not called veins on account of their valvular structure, but because the fluid in them moves from the smaller to the larger branches, and towards the heart,

heart, Akenfide observes, that they could not be called veins on this account, because at this rate the pancreatic and biliary ducts might have been called veins also. Here however, he seems to have forgot, that the fluid in the pancreatic duct moves not towards the heart, but into the ductus communis, and thence into the duodenum. Akenfide has however invalidated the force of several of Monro's objections, particularly that of inconstitence, with which he is charged in admitting a communication between the blood-vessels and lymphatics. He shews that he did not suppose that such a communication between the arteries and the nascent extremities of the lymphatics subsisted, but at the places of their termination into the veins; the probability of which he confirms from experiments by Cowper. Upon the whole, though Akenfide plainly discovers, that he is offended with young Monro, yet he expresses it like a gentleman and a man of genius, and concludes with the following :

“ And such at last are those slips, as Dr. Monro styles them, which he is pleased to own may *perhaps* be thought *venial* in one who does not make anatomy his particular study. In return for which equitable concession, he may (not *perhaps* but certainly) be assured, that Dr. Akenfide has so much partiality to a liberal ambition

bition in those who are entering upon the world of letters and science, that into whatever *slips*, or forward disputes, or overweening conclusions, they may be drawn by it, in asserting their own pretensions to any thing praise worthy; he shall think them all venial, *except want of candour*: nor would he have troubled himself, or any one else, with a word in answer to Dr. Monro's treatise, but for the passage quoted from it in the first of these notes."*

* Vide Monthly Review, October, 1758.

3. OF THE DISPUTE BETWIXT THE HUNTERS AND
PERCIVAL POTT, ON THE DISCOVERY OF THE HERNIA
CONGENITA.

THIS dispute was originally began in consequence of Percival Pott's publication of his discovery of that species of rupture, called *hernia congenita*. It appears, that at the time the enquiry into the various species of ruptures was conducting with zealous ardor by Percival Pott, the same had also occupied the attention of de Haller at Gottingen; and it also appears, that de Haller was the first pioneer in anatomy, who virtually and positively explored the true nature of the *hernia congenita*: that is, de Haller was the first of all anatomists, of all countries, who published the discovery; and it was de Haller who also gave it the name of *hernia congenita*: a name which defines the nature of the rupture; when the intestine in an Infant falls down into the *scrotum* after the *testis*, or along with it, producing thereby what he called *hernia congenita*.—Or according to the explanation of Pott, it is that particular kind of *hernia*, in which the portion of intestine or *omentum*, which occasions the tumour, instead of being found alone in the hernial sac, (as in a common rupture,) is found in contact with the naked testicle; the bag containing

taining it being formed by the *tunica vaginalis testis*.

In the year 1755, Albertus de Haller published his account of the *hernia congenita*, and confirmed by it, that idea which had been previously floating from the imperfect observations of others; particularly from those made by Sharpe, as remote as the year 1748. The remarks of Sharpe had called the attention of english anatomists and surgeons to this object; and if such a rising character as Pott had not given the subject his closest attention, he certainly could not have been said to be what he then promised, nor what he afterwards was acknowledged,—as a man by habit indefatigable, and endowed by genius, education, and honor, to merit the title in a superlative degree, of being the most eminent surgeon of the age:—a man naturally so disposed, as to be as far above the narrow sollicitations for dispute, as an expansive mind could possibly elevate him.

De Haller's paper on the *hernia congenita*, appeared in the *Opuscula Pathologica*, and was translated into English in February 1756. The publication by Pott most undoubtedly appeared some time after, and it was at that immediate point of time when the dispute commenced.

The Hunters took two grounds to make out their accusation against Pott; one, that he had stolen the discovery from de Haller; another, that he had stolen it from them. They thought proper to make out two inditements against him, that if he should be acquitted of one, there might be a chance, at least, of his being convicted upon the other.

They first attacked him, for having stolen the discovery from them; but at the same time abused his performance,—abused that very performance which was said to contain a valuable discovery belonging to them, and if not their's, was de Haller's;—abused that performance which, if (as they called it) ignorant, dull, or useless, was not an object for invidious contention;—not worthy to be claimed by them, from whom the discovery it contained was stolen,—nor to be defended by him who was accused of having committed the theft.

“The treatise,” (says William Hunter), “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 pupil of mine might have written; (for the cases given at the end, supported only
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an uncontested fact) and yet neither my brother's name nor mine was mentioned. It bore strong marks of second hand observations, 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 surprise; 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."

Such was the style of the attack of the Hunters; and to say the truth, if Pott had not made a reply to it, he must have been more than a stoick; he must have confined the common passions of nature by something more than philosophy has suggested for binding them. Pott submitted to a defence; and so far as it is necessary to repel the dishonourable charge of his having stolen the discovery, by a previous conversation he had with John Hunter, I shall give an extract of his own words:

"I do not," (says P. Pott) "remember that the *congenial bernia* was once mentioned by either of us during my short visit, notwithstanding the Doctor has said that his brother shewed me his preparation with great readiness, and explained to me his (the Doctor's) hypothesis of the

the contiguity of the intestine and testicle. Our conversation turned entirely on the passage of the *testes* from the belly into the *scrotum*; and, as far as I could perceive, (for he spoke with the most cautious, apprehensive reservedness) 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 visit 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 intention 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 the honour of the discovery, it would not have given me any concern at all.

“ That is a short and true account of the fact; this is the thing for which I have been traduced in print. The manner in which I attained my knowledge I have already most faithfully related. But excepting that single circumstance of not having related the short conversation which passed between his brother and me, and from which I did

did not derive the least degree of information. 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 prosecuted my enquiries,—as I should have known that the subject was in so able hands: I want no reputation of that sort."

This visit was paid (by John Hunter's confession) in the autumn of the year 1756; and Pott has proved an historical account of his discovery, by an appeal to names above the power of suspicion. He says, that as the thing gave him much pleasure, he procured a number of subjects, examined carefully, noted appearances, drew conclusions, made preparations, and shewed both the papers and preparations to his friends. And what the Hunters must think very remarkable, as they never had been used to any such possibility, most of those friends belonged to the *same Hospital*. Perhaps that was the reason why they could not bring themselves to believe him. Among the rest were Sainthill, Nourse, Webb, and Hawkins. And when he had examined a
great

great variety of subjects, he enlarged his notes, digested them into better order, and shewed them again to the same persons.

Pott must have been a very bold man, if he had appealed to those names without authority, and he must have been a longer time in bringing forth his discovery than John Hunter has given him, if this appeal of his be true: for his visit to John Hunter was during the autumnal course which finishes with the year, and the treatise was published in February 1757 following.

There will never be an instance produced where any thing was obtained from the Hunters that referred to the improvement of science, which they chose to conceal; and where, from their luxury in imparting information, any advantage of them could have been possibly taken. The reason for their thus guarding all their proceedings, cannot escape him, who is furnished like me, with a clue directly leading to their hearts: who has acquired that masterpiece over their motives as directly to account for all their undertakings, by discerning the intentions which induced them to set about them. This I shall corroborate by the following quotation.

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ascertained, he desired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the *testis*, and of the situation of the hernial sac, &c. I only put in this temporary caution, that I was then speaking of those things as they are commonly in adult bodies, and not as they are in the *fœtus*: and at last, when I was concluding my lectures for that season in the end of April, 1756, with a course of the chirurgical operations, I gave a very general account of my brother's observations, and shewed both the drawing of Fig. II. which was then finished, and the subject from which it was made."

I ask whether it were possible to detect the proceedings of the Hunters, who had previously thus debated the matter of prudence; and who had, from that cause, kept back even what they knew (without de Haller, if any body believes it) from the very pupils who were attending them for information?—I ask what the motive of the Hunters could have been, during the interval betwixt the publication of de Haller, which by this time had appeared, and that of Pott, which did not appear till the February following, for keeping back the important information, published by de Haller, and known to them, from their pupils? It was most assuredly to make themselves appear, in the eyes of the pupils,

pils, *principals* in the discovery. For if they had avowed the discovery, without any illustration of it from their own dissections, suspicion would have directly given the credit to de Haller. It was prudent, therefore, in them to avoid saying any thing of the discovery, until they were prepared;—and during that interval Pott's untimely treatise abruptly appeared.

It is worthy elucidation in philosophy, for the information of all men, from whomsoever the example may be drawn, to dwell upon this infallible axiom: that no one is ever so strenuous for the reputation of genius, as he who has it not; and that no one is ever so careless of that gift of nature, as he who has it. Genius sits easy upon him who intrinsically possesses it: he neither feels importance nor dignity from his own conception of its value, because he cannot say, it belongs to him in any other than a relative sense: he must derive his title to it, from the approbation of those who alone can estimate the fruits of his invention. If ever there was an instance, where two men have been so often disappointed by mistaking themselves, as the Hunters, I know not where to find it. All their diligence, their art, and their contests, only prove, that they struggled indeed for it, but could never obtain

a reputation bearing the smallest resemblance to men of genius.

Thinking they had securely grasped the opportunity of rising into fame, by making de Haller's discovery indirectly their own, they meditated their dissections, when, miserably unfortunate for them, these came too late, as in the mean time Pott's publication came forth. That was the shaft which stuck in their sides :--that was the *lateri lethalis arundo* :—that was the cause of their abusing Pott's publication, and—that was the similar practice to which they had resource, in the disputes of injecting the *testis*, and of the absorbent system. If Pott's publication hardly contained one new idea, the Hunters could not have been injured : or if Pott confirmed the truth of his discovery, by a variety of *cases*, which offered, from time to time, at the hospital he belonged to, and the Hunters did not, they could not possibly lay claim to his discovery ; as without *cases* there could not have been a disease of that nature known or described : nor could such *cases* have been brought forward dependant upon the will, but only when patients afforded the opportunity ; and such *cases* being rare in their nature, there must have been some time, and a patient watching for collecting them. These facts could never have been stolen from
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the Hunters, on Pott's visit to them, within two months of his publishing; and the progress of these facts are attested by the surgeons already appealed to.

Or if Pott had taken up the idea from de Haller's publication, and confirmed it by these *cases*; in that instance, the Hunters prove themselves to have no other concern in the contest, than informing against Pott for something taken from de Haller, and in that they prove too much for their own sakes, as—"Whoever," (says Dr. Hunter) "will take the trouble of comparing the passages quoted from de Haller, and from Pott, will see that if Pott 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."

Pott, in his answer to this, declared that he had never seen, read, or heard of that work of de Haller, either in Latin or English, till twelve months at least after his publication. This he avowed, as he emphatically says, to save the reader's time, and to cut short the dispute. But the part of his reply which was felt most severely, from the truth it contained, was the following:

"Indeed

“ 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: since, if I had read the *Opuscula Pathologica* of de Haller, previous to my 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 hypothesis (as he calls it) and John Hunter’s discovery.”

After the treatise of Pott on the *hernia congenita* had appeared, the consternation in the lecture room of the Hunters must have been distressing to them; as in consequence of the Hunters previous plan of prudence, for keeping back the discovery of de Haller, till they were prepared to make it their own by dissections, the pupils were continued in ignorance of the whole of the discovery. And when that treatise, published by a surgeon belonging to one of the hospitals, and written in the vulgar tongue of their country,

country, could be read by the pupils, and the contents of it loudly whispered in the lecture-room, it was impossible but in the consequence there must have been produced an explosion : —it was too ripe, and too critical to be suppressed : and the Hunters were driven to say any thing against Pott, because they could not say any thing for themselves. It will hardly be credited, that, if their situation could if possible have been more distressing than it was, they would have confessed to the pupils, that they knew of the discovery, but had foreborn to demonstrate it, from those motives of prudence I have already explained.

I am now prepared to consider, if I could, John Hunter's description of the situation of the *testis* in the *fœtus*, with its descent into the *scrotum*. I should have hoped to have considered the importance of this piece of anatomical performance relatively, at the immediate time it was in preparation, for being displayed in the lecture-room, either before his pupils or those of his brother ; but that privilege is denied me ; as the first information we have of it is drawn from William Hunter's Commentaries. And I am very apt to conclude, that this performance took up more time than we know of ; as John Hunter has given us to understand, in the second page of
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of his "Animal œconomy," that, "the following observations on this subject were taken from my notes, and published by Dr. Hunter in his Commentaries, to which I have added some practical remarks." I take it for granted, therefore, that these observations were never, or, if they were at all, not in any season, read to the pupils during the dispute: but I am more apt to conclude, that they were drawn up for the purpose of being published in the Commentaries, after the return of John Hunter from the wars. They never appeared before the public till March 1764, and it could not be avowed, that they were ever read at any time in the form they are published, as the rudiments of them, till the Commentaries came forth, were only in notes belonging to John Hunter.

This performance stands as the first subject in John Hunter's book on the "Animal œconomy;" and, independant of his experiments on the absorption of veins, is to be considered as the first production of the author. As it is found in the Animal œconomy, it is very different from what may be seen in the brother's Commentaries; it has a new introduction, and contains much additional matter; there is a case annexed to it of the year 1767, only ten years after

ter Pott's cases, which were said to forestall them in the discovery.

Not being able, therefore, from any authority, to consider these observations as relative to the discovery of the *hernia congenita*, I shall look upon them, as explanatory observations on a subject already discovered: and although they would have fairly been higher in merit, if they had not arisen out of information already established; yet in examining them abstractedly, with the view of seeing what farther explanatory matter is offered upon a subject, which has been explained, they will be found to possess much independent merit: and I should not follow the bent of my inclination, nor comply with the command of my understanding, if I did not voluntarily own, that John Hunter, by these observations, has fairly obtained anatomical fame.

One cannot therefore refrain from lamenting more seriously, that they should have been produced in a turbulent storm of contention, in a paroxysm of envy, and in an act of claiming professional rights belonging to others. I should have received more infinite satisfaction, if John Hunter had given me the opportunity of applying to him, the lines which Denham wrote on
Fletcher,

Fletcher, and closing these disputes more honourably to his fame, by the following.

I need not raise
Trophies to thee from other men's dispraise;
Nor is thy fame on lesser ruins built,
Nor need thy juster title the foul guilt
Of eastern kings, who to secure their reign
Must have their brothers, sons, and kindred slain.

I find myself now approaching to that page of the life of John Hunter, which cannot afford me pleasure, if it were not derived from a conscious intention of supporting truth: and as I know from what will be told by me, that the credulity of his admirers will be first shocked, and from the natural transitions of the human passions, their anger will be next roused; it is for that reason, and that alone, I have determined to be as explicit as possible.

John Hunter has published on many subjects; and if the eye of criticism were to peruse the whole of them for this particular purpose, I do not know but the opinion would be, that the explanation, the language, and the style of his writing were at least ample to the expression of his ideas: and that, if there be any obscurities

scurities and any errors, as there most certainly are in very great abundance, these do not so much arise out of defect of language and style in his writing, as from a native obscurity in his ideas: they are most commonly, if not always, the consequence of a confusion in his mind.

If I were not to enquire into the truth of this question, however delicate it might appear to those who wish that it should not be enquired into,—that truth which is positively necessary to be made known, and which presses hard for the clearest explanation, would hereafter be doubted; although I am now justified in saying, that it cannot be contradicted. Posterity might otherwise have said from persuasion, especially from what appears upon the face of his publications, that John Hunter was a man of considerable knowledge in literature;—and posterity would prove it, in the plainest manner, by referring to the papers of experiments and observations now before me,—to the many papers published in the Philosophical Transactions of the Royal Society,—and to all his other works.

The truth is, that he only furnished the images, and that the writing part was always performed by another:—he prepared the skeleton, and another covered it with composition:—he

found the materials, and another made them up into dresses for the public :—he was incapable of putting six lines together grammatically into English ; and, at his lectures,—he was often found so far incapable of making out the sense of his own notes, as to pass over the subject they were meant to explain.

It was owing to want of education, that his notions of things were so very imperfect, and his conceptions so very contracted : instances arising from this original defect, are to be found throughout his images, and if they had been confined to them alone, they might have passed without observation : but they operated strongly in his conduct towards others ; and not only the profession, but those who follow it, have experienced in a very unpleasant degree his vulgarity from a want of the polish of education, as will be made hereafter apparent.

The three disputes which have passed already before me, were carried on in a monthly publication, called the Critical Review : and the system of that undertaking was in a great measure broken in upon, to be made subservient to the purposes of the Hunters, who had obtained that ascendancy over the Editor, as to command

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his services for the promotion of any cause, which their views or ambition suggested.

It is necessary for me, from the motive of truth therefore to say,—that all the attacks, and all the replies of the Hunters, which gained so willing an admittance into the Critical Review, during these disputes which have engaged my attention, were revised, corrected, and published under the immediate eye, influence and direction of Smollet——Smollet—the author of the History of England, Humphry Clinker, Peregrine Pickle, Roderick Random, and if I mistake not, the Tars of Old England,—and Smollet—the then editor, and, if I mistake not, one of the proprietors of the same Critical Review. Smollet was at their command for any services they wanted of him;—for whatever was calculated to raise their reputations, by the ruin of those who stood in their way, his praise and his abuse were equally at their obedience: And to say the truth they could not have selected, out of every circle of authorship upon the face of the earth, a more bitter or clever fellow, not only for consolidating their ideas, but also for conducting them forth to the public.

I only attribute the keen conduct of the disputes on the part of the Hunters, as they are to be

be found in the Critical Review, to the disinterested assistance of Smollet. I do not mean to be understood in saying that Smollet extended his services farther. So far as I have introduced Smollet, was purely to explain the zeal of that Review in the cause of the Hunters. This epifodical digression does not impair in the least the force of my general assertion,—that John Hunter never was the author of any production which has appeared under his name. But if I had not thus previously grounded my assertion,—if this fact had not been known, his admirers might have been astonished at my imputation of his want of literature; and more especially, when in the two productions I have already examined, they read the Greek anatomical derivatives, most accurately given;—and the Latin terms in anatomy most nicely correct;—so as there cannot even be found, what carelessness in science will frequently commit,—the mark of a *caret* (—) omitted. If I had not thus explained myself, I should have been told, that John Hunter's experiments on the absorption by veins, and his observations on the state of the *testis*, appeared not at the time when the disputes in the Review were carrying on, but so long after as in the year 1764. I acknowledge the fact, and by it mean to say, that I do not know, that Smollet wrote

wrote *them* for him, but I know that John Hunter could not have written them.

If I had tacitly acknowledged, that the disputes in the Critical Review, and the two publications under the name of John Hunter, had been written and conducted by himself, I should have proved too much for the belief of his admirers.—And as the sting of the critic, throughout the whole of the disputes, was acutely pointed,—as the argument was conducted with a poignancy that cannot be outdone,—charged with Greek and Latin quotations, which the lore of literature alone can furnish,—it might have been concluded, that John Hunter had once been senior wrangler at one of our universities, and that it was fabulous altogether that he ever came from the workshop of a carpenter directly into the dissecting room of an anatomist. Now this his admirers will hardly venture to admit, or that he ever pleaded his inexperience in literature, as an excuse for his errors,—or for his deriving assistance from others. If ever in all his publications, or in whatever his admirers have said of him, an apology for his imperfections had been offered, *that* might perhaps have silenced the observations of his opponents, or at least have softened their criticisms: but he always conducted himself, as if he really had fed
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on the choicest fruit of science, which the tree of learning can only produce.

I shall for a while turn away my attention, from the vain flights of him, who thus attempted to soar, like another Icarus, with dedalion wings, to contemplate the character of the immortal de Haller, and to shew a contrast that in itself will be fully descriptive of my intended motive.

In the three separate disputes which I have demonstrated, de Haller's share in the discoveries has been already distinguished: and curiosity would naturally presuppose, that those contentions positively arose out of the importance, and value of the three distinct anatomical rights;—that the just title to the discoveries would have equally awakened a jealousy for anatomical property in every one of the parties. But these objects were never seen by the eye of this philosopher in any such magnitude. De Haller pursued his anatomical studies without consulting his breast upon the impulse of fame, or without feeling suffocations of jealousy. What he was discovering was the result of his natural taste; and what he had discovered was considered by him, as dead game by the true sportsman; with a liberality of heart he gave it away, and went directly in pursuit of some other investigation.

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The following abstract has been made from the ingenious Henry's life of him.

De Haller was born in the year 1708. In his early infancy he manifested an activity of mind, a faculty for labour, and a strength of memory necessary for those who are desirous of comprehending many sciences and pursuing great operations. He was the first scholar of his time; and in his juvenal days delighted in poetical effusions, of which he has exhibited many valuable specimens. But he soon gave up the pursuit of poetry for the investigation of nature; and made choice of the only profession which would allow him to devote himself to that study without reserve, namely that of physick.

On entering upon this intense undertaking, he renounced wine for ever, that he might be certain to avoid the abuse of it; and in order to guard more infallibly from seduction, he thought himself obliged to observe a rigorous severity in his manners. He began his studies at Leyden, where he found an anatomical theatre well supplied with subjects; cabinets of natural history; a very extensive library, and every thing which could encourage and invite to study. There he found himself in company with Boerhaave, Albinus then young, and the famous Ruyfch,

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the great improver of anatomical injections and preparations, still living at Amsterdam, prosecuting his studies at the age of ninety. Here he took his degree; the thesis for which was on the salivary ducts, in which he displayed the knowledge he had acquired in anatomy, and proclaimed himself an observer capable of enriching that science with many important improvements.

In 1727 He visited England; was introduced to Sir Hans Sloane, who had at that time formed his collection of natural curiosities, and had the pleasure of becoming acquainted with Plumtree, Cheselden, and Douglas, men distinguished throughout Europe for their professional abilities. From England he went to France, and was in danger of prosecution for obtaining dead bodies. He then went to Basil and studied botany; and returned to his own country in the year 1730.

The first poetical productions of de Haller were by him committed to the flames. His taste for poetry now returning, he bridled it in such a manner that its charms should not be sufficiently alluring to detach him from the more severe and useful studies. He only cultivated the muses in his solitary walks, in those hours of the night when sleep forsook him, and during those recesses

recesses from labour with which his state of health sometimes forced him to comply.

His immense labour in anatomy employed the largest share of his time; and tho' separated from his masters, friends and competitors, his own private cabinet and select library supplied the place of academic aid. Here he laid the foundation of that vast extent of knowledge which comprehended every species of literature. The discoveries of every cultivated age and nation were extracted in the course of his reading, which he continued with unremitting attention during his whole life; without being diverted from it by the vicissitudes of fortune or embarrassments of affairs.

In 1736 he made botanical excursions, ascended the mountains of Jura and Alps, and descended to the marshes in Switzerland. The studies of mineralogy and zoology were equally extensive to his comprehension. The republic of Berne established for him an amphitheatre where he taught anatomy.

Soon after he was invited by George II. to promote the university of Gottingen; and there was established for him an anatomical, botanical, and surgical professorship. This he accepted,

accompanied by a young wife, whose personal qualities had captivated his heart, who had borne him children, and who, by the sweetness of manners with which she had adopted his taste and pursuits, formed the happiness of his life. But this undertaking proved fatal to his dear Mariamne, who died in consequence of an accident which befel her on the journey.

The regency of Hanover gave him every proof of their esteem for talents; and it was thus that he established that fame of Gottingen, so justly to this day celebrated over the world. He was so truly original in physiology, that he might be fairly said to have been the parent of it. To this end he investigated the study on exact anatomy of man and other animals. Nor was it till after thirty years of labour, that he thought himself justified in publishing his discoveries, and which was the æra of a revolution in anatomy principally owing to the powers of de Haller.

A review of new publications was undertaken by him in the whole circle of medical science, in natural history, physics, chemistry, metallurgy, and œconomics. He undertook the review of the different articles, besides histories, voyages, and descriptions of climates and soils. By the influence

influence he had with the princes of the empire, he formed the undertaking of Mylius to travel through America; and by the interest of George II. de Haller was made a baron of the empire. After an absence of 17 years, he returned home to Berne, where he was elected a member of the sovereign council: a title which enabled him to fill several places in government, and to one he succeeded by lot in 1753, when the administration of the salt works was brought by him to perfection.

His attention to the duties of a magistrate did not entirely take him off from his physical pursuits. His experiments on incubated eggs were made at Berne. He completed his physiology, arranged his library, and collected his works. He furnished the Supplement of the Encyclopædia with articles on the subjects of anatomy, medicine, and physiology. As perpetual president of the university of Gottingen, he remitted not, during his absence, his attention to its interest. He was offered by George II. the chancellorship of it: this produced a conflict in his mind, whether he should leave his native country the second time for Gottingen. But it was determined that he should remain at Berne: that republic, desirous of retaining and fixing him more firmly to the service of the state, assured him

him of their wish, and settled on him a pension for life.

He died in the year 1777: a memorable year for the loss of the modern age; in which departed besides de Haller, Voltaire, Linnæus, and Rouffeau. His valuable library, consisting of 13512 volumes, on the subjects of anatomy, surgery, the practice of physic, botany, and natural history including his diaries, *herbaria viva*, and about 150 manuscripts, mostly written in his own hand, was offered to the London booksellers, a number of whom agreed to unite in treating for it; but before they had taken any farther measures, the whole was purchased by the emperor of Germany.

De Haller was most agreeable in conversation. His elocution was free, strong, and concise. His immense reading, fertile and faithful memory, and sound judgement, enabled him to adapt himself to all dispositions. In his person he was tall and finely proportioned. His countenance, which had a serious cast, from being short sighted was full of expression. He was superior to the affectation of wit, and disdained to make a parade of the knowledge he possessed. His soul was gentle and his heart replete with sensibility.

PART II.

FROM HIS ENTRANCE INTO THE ARMY, WITH CONSEQUENT TRANSACTIONS, TO 1770.

THE study of anatomy is generally pursued from three distinct motives.—

1. To lay the necessary foundation for those who are hereafter to follow the practice of physic and surgery.

2. To enable those, who pursue it with more than common ardor, and with a longer attention to the subject, to obtain the qualifications,—not only by investigating the human subject but also the various classes of animals,—necessary for becoming hereafter teachers of the art of anatomy.

3. To furnish the philosopher of independant fortune, whose delight it may be to investigate the operations of nature, with the only true means of arriving to any degree of perfection for the accomplishment of that end.

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The first and second are the general motives, the third is particular and very rare indeed !

Of the first,—the motive is founded upon necessity, as without the knowledge of anatomy, the practice of physic and surgery would be disgraceful to those who pursue it, and detrimental to society who sought for relief from ignorance : the study of it therefore from this motive brings annually to London nearly three hundred pupils, who distribute themselves at the various hospitals, and when the hospital hours for seeing patients are over, fill up the remainder of the day by attending the courses of their favourite anatomical lecturers. Two courses are usually read in the year ; the first commencing in October and ending in December, the second in January and closing in April.

Of the second,—the motive arises from a laudable propensity ; as the lecturers in anatomy are characters of the highest importance to society, and ought to be regarded with a degree of veneration. For acquiring their knowledge, they must overcome toilsome difficulties, and escape the dangerous effects of unwholesome processes. Their intercourse with intellectual nature cannot be obtained but by severe attention, and under particular inconveniencies : they must pursue
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sue their enquiries in the coldest season, and from want of a better regulation, are reduced to keep up a connection with a necessary order of men for obtaining the materials, without which they could not, in due succession of seasons, display the result of their laborious and ingenious dissections to their pupils.

This valuable branch of science, which diffuses its influence into the practice of physic and surgery throughout the dominion, and from which every subject of it sooner or later receives his portion of good effect, is conducted by rarely more than four or five lecturers in London. Within these ten years, five are now fresh upon my memory, who have fallen the victims of putrid *myasma* in the prime of life, whilst they were fulfilling this important duty for the welfare of their fellow beings. The expences for procuring subjects and large rooms are very considerable; and it might have been hoped, that an undertaking so dangerous to health, so unpleasant in the process, so laborious in acquiring, and so hazardous in fortune, would procure in return an adequate emolument. But I have reason to believe, that the profits among all the anatomical teachers amount altogether to no more annually than three thousand pounds.

From the account which is given by William Hunter of his brother John in the commentaries, John had, during his dissections for his brother, experienced some severe indispositions, which retarded his progress in anatomical engagements: and this might have been one cause why he renounced the lecture-room, or at least relaxed somewhat of that severe application, which was demanded from him who was hereafter to become, what his brother then was, an anatomical teacher of some eminence. Perhaps the cause of ill health, together with his want of education, and without knowledge of the technical terms of the art, or ability to deliver, either orally or by writing, those necessary *formulae* found in prescriptions, deterred him, at that time, from pursuing anatomy with any intent of becoming hereafter a regular teacher. There must have been some such reason for his abruptly declining this pursuit for the army: as he who was wanted to assist his brother before, and when the fame of his lectures was annually increasing, must equally be desirable when that fame had crowded the theatre: moreover than all this, in the absence of John Hunter, another supplied his place, which proves at least that he was not then suited to the undertaking.

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This was the commencement of his being a surgeon. And in May 1756 he became the house-surgeon to St. George's Hospital, in which situation he only continued for about five months: he resigned the office in September following. He succeeded to Hopkins, and was succeeded by Gunning, the present worthy surgeon-general to the army.

Upon the strictest enquiry, I have reason to be persuaded that this was all the surgical education John Hunter ever received: and to my own knowledge I can speak it, that the period of five months duration at the hospital in the office of house-surgeon is the shortest which can be found in the unerring journals of hospitals. The usual time for the residence of a house-surgeon is generally twelve months, but sometimes it has been extended to two years: and the usual mode of selecting the house-surgeon to that office is, by his first having as a dresser discovered sufficient signs of disposition and attention to duty, as to merit the preference: for the office of house-surgeon is of great importance to the interests of the hospital: his duty consists in receiving and attending to all accidents, in sending to the surgeon upon an emergent occasion, in attending to the pressing calls of the surgical patients during the absence of the surgeons, in superintend-

ing the conduct of the pupils, in keeping the instruments under his care in order, in furnishing the preparatory dressings on days of operations, and in holding the key of the dead house.

John Hunter's education seems to have been upon an inverted ratio to all other surgeons. He to become a surgeon, served a long apprenticeship to anatomical pursuits, and only five months to surgical: whilst others, to become surgeons, serve their apprenticeships with surgeons; and for a year or two pursue their anatomical studies, and that at a period of life too, when their minds are in preparation, and their ages favour the reception of that important acquisition to practice.

Anatomical information is purely a mechanical study: whereas the art and practice of surgery consist in a general knowledge of established principles, and a desire for excellence resulting from observation: the mind is constantly in the exercise of improvement, and practice presents frequently a case that demands a fresh exercise of the judgment. Besides the requisite aid of anatomy, no one can be said to be a perfect surgeon, without having spent some time in observing the practice of the *materia medica*. His short residence at the hospital would not allow

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John Hunter to impress upon his mind the general outlines of surgery, and for want of which his taste for ever after appeared to be vitiated; and his being totally unacquainted with the *materia medica*, rendered all his prescriptions bald and informal.

But there is one saying by John Hunter which appears to be so very paradoxical, that from his admirers alone the explanation of it must come. He constantly asserted that he never read:—was it then possible for any man possessing, as he did, but barely the mechanical acquirements of anatomy, without education, without reading, with five months application to surgery only, and without knowing any thing about the *materia medica*, to establish the true excellence of the art, founded upon general principles, of a surgeon? But notwithstanding what he has been known to assert to the contrary, I believe that he did read; and I believe he was so much in admiration with the idea of being supposed to make rules in surgery for himself, that he hoped, by thus denying that he read, to avoid being detected, when he borrowed from another whatever was his design to be passed for his own.

Without there had been a fixed intention of his becoming hereafter a teacher in anatomy, it
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might be certainly considered as something too much for John Hunter to continue on the pursuit ; as the rewards of the art are only derivative either from acquiring the requisite knowledge for a teacher, or from assisting the education of a surgeon.

He therefore, to lay a foundation for becoming a practical surgeon, obtained an appointment, I believe, upon the staff in the army ; and in the year 1761 was with the army that took Bellisle ; and in the subsequent year, he accompanied the army to Portugal, returning to England in May.

The *Memorabilia* of a great man are ever of some account : and therefore, although it be not the leading province which I have assumed, I shall note a trifle or two of those occurrences, which have been handed down with unerring fidelity during his absence. It was at Bellisle that he first took to cramming the stomachs of lizards and worms, and first *discovered* a never failing method of dissolving every bubo without its coming to abscess. As I have treated this discovery after the manner it merits, in my observations on his treatise on the venereal disease, I shall spare any farther critical remark.—And it was at Portugal, that he made an experiment which

which, as he has described it, was for ascertaining the faculty of hearing in fishes.

These valuable portions of anecdote are drawn from what John Hunter has said of himself. But I think I need not have been so explicit upon the *discovery*, as no one upon the face of the earth would have ever ventured, or ever will, to assert for him, that he could do what he there says he did:—that he could dissolve every bubo without its coming to abscess, no one but John Hunter himself would dare to say it:—but what is very unfortunate after all, no one who knows any thing of the matter ever believed him, or did he believe it himself; for in his book on the venereal disease, there are eight instances of buboes coming to abscess under his own care.

Of the disputes which have taken up a great deal of my consideration, it might have been presumed, that John Hunter was rather a partaker than a principal; and from that, an inference might have been drawn, that when in a state of separation from his brother William and anatomical points, he would be found to stand thus alone, without his discovering that turbulence of temper which, as hath since appeared, he natively and radically possessed.

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He had scarcely arrived at Portugal, before he excited an uneasiness among the faculty, which their situations had never experienced before. He turned the common intercourses of social good humour into suspicious tauntings of jealousy: He created a faction and a consequent disgust. This brought on an explanation from one who was his senior in the army by ten years;—from one who had been a faithful follower of the fortune of the Duke of Cumberland, and had dressed his wounds in battle: He was roused to draw his sword upon John Hunter, which was sheathed without the quarrel being reconciled—for what reconciliation can remove suspicion!

The confirmation of this I am not disposed to doubt, but some there are who may: those I will assist as far as it is in my power by assuring them,—that the manly veteran Tomkins, of Park-Place, is very capable of explaining the fact.

On his return to England, and at the close of the war, he took a house in Golden-Square, and found himself in point of fortune, better than nothing by his half pay; that enabled him to pay his house rent, and some other necessaries, requisite ever for those who sit down in practice waiting for patients:—and here commences his first career of a London surgeon.

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What happened to John Hunter, happens to every surgeon in the beginning: there was not employment enough furnished by the practical art, to fill up the active hours of the day. But his resources were in an extraordinary degree advantageous over most young surgeons under the same predicament for want of patients: he experienced no lassitude: he had furnished his mind with the means of employment, and to say the truth, he was of an uncommon turn to industry.

He opened a room for dissections, and demonstrated subjects to his pupils: he began to make preparations upon his own account: he carried on those reflections with a new vigour, that had palled upon his mind before he went into the army, and he, or some one else for him, put into a condition those papers on *the Injection of the testis*, on *the Absorption by Veins*, and on *the State of the Testis in the Fœtus*,—and on *the Hernia Congenita*, which were not published until the year 1764, as I before have declared.

He had not at this time exacted those rigid severities of temperance to which he was observed to adhere at his latter part of life. John Hunter at this time, and for some time after,

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was a companionable man: he associated in company, drank his bottle, told his story, and laughed with others. And in all probability by that suppression of the solitary passions, which never could have befriended his ease,—by suspending their practice and their mischief, he found himself an extraordinarily happy man. But these halcyon days did not last long. He found also what is commonly true, that familiarity did not enhance professional estimation. He was soon sickened of the dissipation he had somewhat contracted by the society of officers from the army, and more readily yielded to his natural inclination for indulging his calls to speculative points of natural history and comparative anatomy.

In February 1767 he was elected a fellow of the Royal Society; and in December 1768 he was chosen surgeon to St. George's hospital in the room of Gataker.

In 1770 he removed from Golden-square to Jermyn-street, and took the house which his brother had left when his own was finished which he had built in Windmill-street.

During all this time he found himself at leisure for meditating plans of life, that plainly denote
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an intrepidity of mind and a vigour of application, which natural obstacles insurmountable by most, could not suppress in him; which few have ever attempted, although supplied by the completest aid of early academic learning.

The practice of surgery had at this time, or for long after, afforded no opening for him. Hawkins, Bromfield, Sharpe, and Pott, embraced almost the whole of family practice; whilst Adair and Tomkins carried from him the chief of the practice derived from connections formed in the army.

His sole dependance was on his anatomical power; and from that he drew the greatest advantage it could possibly admit. He had the art of giving a consequence to every thing that he did, by the dextrous use he made of his anatomical machinery. He aimed to be admired rather than useful—marvellous than true—difficult than important. Of this I speak from a thorough conviction, that if John Hunter had experienced a better education, he never would have gone the length which he did, nor succeeded so highly.

With the exception of what was published in his name by his brother William, in the year 1764, there does not appear to be any thing by

John on his return from the war, up to the year 1772. If there were any publications, they must have terminated like many more by others, they must have experienced the fate of abortions, or at least I know nothing about them. And here was an opportunity of ten years, which a man of true genius would have embraced. He would, though late in life, have laid that foundation of literature, which had escaped him in his earlier part; or he would have declined the vanity of public fame, for the private cultivation of useful knowledge. John Hunter did neither the one nor the other.

His professed delight was the study of the animal œconomy: but his ambition carried him far beyond the useful prosecution of that science, so flattering to the laudable disposition of those, who pursue it purely for information. He was never discovered in attempting to explore the occurrences most necessary to be accounted for, as the pride of his heart was only to select an obscure subject, which involved in it so much matter of wonder as to raise the public attention, or to make that his own which another had just taken up before him.

He cared not about the truth, nor the use which might be made of any investigation in nature;

nature : and if he could give his subject the air of novelty, he cared neither from whom he took his information upon it : both his repute for understanding, and his hope for success, depended upon his passing the idea for his own.

Objects out of the sight, and beyond the reach of common observation,—objects in their nature singular, insignificant, and useless, were chiefly among his most favoured selections. From the year 1772 to the year of his death, he published a paper of such a description. Every revolving season came forward with a fresh supply of his bounty. He assumed the high office of Nature's storekeeper. He ramified, dilated, and expanded her hidden secrets,—to the prying eye of common curiosity,—or rather to the uninformed admiration of vulgar credulity. I am not to confine this sentiment to any particular rank which men hold in society, and which is the mere creature of fortune—not to measure it by a scale of personal or hereditary property, but to extend it to the condition of the mind as reflected by actions ; and then it will be seen to a demonstration, that the credulous vulgar may be equally tenants in squares and in alleys.

He had the wonderful art of hanging heavy weights to slender wires ; and by this he contrived

trived to have his papers presented to the Royal Society, and to obtain a reading of them. But lest the Philosophical Transactions, which are difficult to be got at, should not serve to promulgate what he had written, he has collated the papers, and published them, with other matter, under the title of "*Animal œconomy.*"

An observation frequently illudes the due impression, when for the evident truth it contains one is obliged for the information upon it to another. With this regard I refer every one who peruses the productions of John Hunter. Whenever he undertook to treat upon a subject already treated upon by another, his aim was either to pass the author over in silence, or in his way, to forestall the invention, by alluding to notes remotely said to be made upon the subject by himself, and excusing himself for not having published them before. Such has been his habit from the beginning of the disputes I have already considered; and it will too plainly appear, that he never was ashamed of the vice—that he never endeavoured to correct it, and—that it never forsook him.

If the immortal Newton had been his cotemporary, these notes would have been called forth

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as vouchers, for contending with him the right in discoveries.

This is a catalogue of his papers, published in the *Philosophical Transactions*, with their dates.

	vol.	page.
June 18, 1772. On the Digestion of the Stomach after Death. - -	62	447
July 1, 1773. Observations on the Torpedo.	63	481
February 27, 1774. Of certain Receptacles of Air in Birds. - -	64	205
March 17, 1774. On the Gillaroo Trout.	64	310
May 11, 1775. On the Gymnotus.	65	395
June 24, 1775. Experiments on Animals and Vegetables, with respect to their Power of producing Heat. -	65	446
March 21, 1776. Proposals for the Recovery of People apparently drowned.	66	412
June 19, 1777. Of the Heat of Animals and Vegetables. - -	68	7
February 25, 1779. Account of the Free Martin. - -	69	279
January 17, 1780. Account of a Woman who had the Small Pox during Pregnancy.	70	128
June 1, 1780. Account of an extraordinary Pheasant. - -	70	527
November 14, 1782. Account of the Organ of Hearing in Fishes. -	72	379
March 7, 1785. Anatomical Remarks on a new Marine Animal. -	75	333
March 22, 1787. An Experiment to determine the Effect of extirpating one Ovary upon the Number of Young produced.	77	233
April		

	<i>vol. page.</i>
April 26, 1787. Observations tending to shew that the Wolf, Jackall, and Dog, are of the same Species. -	77 253
June 28, 1787. Observations on the Structure and Oeconomy of Whales.	77 371
April 30, 1789. Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackall. -	79 160
February 23, 1792. Observations on Bees.	82 128
Six Krohnian Lectures on Muscular Motion, from 1776 to 1782.	

I have given an account of those papers, which were accepted by the Royal Society : and I have more to add of a similar description, which were not offered, or if offered, were not accepted.

Observations on the glands situated between the rectum and bladder, called Vesiculæ Seminales. -	<i>Animal Oeconomy.</i>
Of the Structure of the Placenta.	<i>idem.</i>
Some Observations on Digestion.	<i>idem.</i>
On a Secretion in the Crop of breeding Pigeons for the Nourishment of their Young.	<i>idem.</i>
On the Colour of the Pigmentum of the Eye in different Animals. - -	<i>idem.</i>
The Use of the Oblique Muscles.	<i>idem.</i>
A Description of the Nerves which supply the Organ of Smelling. -	<i>idem.</i>

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These might be fairly termed pleasurable amusements resulting from the intense pains of a student of nature: but to John Hunter they brought an estimation superior to any improvement in the art of surgery: he never made use of that art, but for the emolument he derived from it: he considered solely the profits of surgery, as a means for carrying on the expences of speculation. If he had been a man of independent fortune, he would have been a naturalist: and as he was a surgeon without fortune, he made that profession subservient to his favorite passion.

The following are his chirurgical productions:

1. The Natural History of the Teeth, in two Parts, containing 258 Pages, 4to. with Plates. Price 1l. 1s. 1778.
2. A Treatise on the Venereal Disease, containing 398 Pages, 4to. with Plates. Price 1l. 1s. 1786.
3. Observations on the Inflammation of the Internal Coats of Veins.—A Paper published in a Volume of Transactions for the Improvement of Medical and Chirurgical Knowledge. 1793.

These three are the only performances claimed by him within my knowledge; and I certainly shall not hunt about for others; as, if there were any more by him belonging to the art of surgery,

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I should imagine they would,—by founding their own importance, speak for themselves.

I shall proceed to make any cursory remarks which might offer on my perusal of those abstract portions of observations upon the animal œconomy, without dwelling with much obstinate tenacity of criticism upon them. Errors or singular opinions, which might be found among these, are as unimportant to the immediate and necessary purposes of man, as a misconception of the simplest product in the creation can possibly be :—whereas the works of John Hunter, which prescribe rules for the practice of surgery, refer to the most direct and nearest interest of society. Errors in surgery, once established by the authority of a great name, become mischiefs to which imagination can trace no boundary : they might be ramified to every district of the world, wherever the implicit pupil who has imbibed them resides ;—and when he has sucked in the poisonous notions directly from him who has created and dispensed them, the consequent effects will be strongly experienced by the public at large.

It is much more common for man to follow the instructions of those whose abilities he esteems, than to permit his understanding to act from
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from the result of its own reflection. Human talent will rarely allow of few who can correct opinions, which have been formed in the early part of education, by the after exercise of their own judgment.—And few, from among many, who follow with exactness the instructions they learnt in their youth, can boast of the active ability of amending or correcting prejudices become habitual from their early impressions.

I shall take the subjects according to their dates; and shall, by considering, in their turns, natural, anatomical, and surgical productions, procure that change upon the mind by which attention is more likely to be kept awake.

from the result of his own reflection. Human talent will rarely allow of two views of the same opinions, which have been formed in the early part of education, by the habit of their own judgment. And now, from among many, who follow with exactness the instructions they hear in their youth, can some of the active ability of anything or contrary prejudices become habitual from their early impressions. I shall take the latter's according to their cases and that of considering in their turn, patient, anatomical, and surgical professions, procure that change upon the mind by which a person is more likely to be kept awake.

PART III.

EXPLANATORY REMARKS ON ALL HIS VARIOUS PRODUCTIONS IN NATURAL HISTORY, ANATOMY AND SURGERY.

June 18, 1772.

ON THE DIGESTION OF THE STOMACH AFTER DEATH.
PHIL. TRANS.

JOHN Hunter gives three cafes to prove, that he found a diffolution of the ftomach after death: in confequence of which there is frequently a confiderable aperture made in that *viscus* at its great extremity. The three cafes were difcovered on fubjects who had experienced fudden death.

“ Being,” as he fays, “ employed upon this fubject, and therefore enabled to account more readily for appearances which had any connection with it; and obferving that the half diffolved parts of the ftomach were fimilar to the half digefted food, it ftruck me, that it was the procefs of digeftion going on after death; and that the ftomach, being dead, was no longer capable

pable of resisting the powers of that *menstruum*, which itself had formed for the digestion of food."

This paper has not appeared to attract much public attention. But considering its importance, it is rather singular that it should have remained unnoticed, until an enquiry into the truth of it was undertaken through a train of experiments by the Abbé Spallanzani, whose fifth dissertation is expressly for the purpose of demonstrating digestion in animals with membranous stomachs: the cat—the dog—man—whether digestion takes place after death.—

The Abbé seems to be a lively accomplished physiologist: he built his experiments upon the practice of Reaumur, and improved them by that natural addition in the application of the labours of two men of ingenious candour persevering on a subject beyond what one alone could accomplish. But this was not the sole merit of the Abbé: solidity in judgment appears to have been always consulted in his experiments: he never undertook them without being previously assured by the reflection of reason, that he was about to practise the most necessary experiment for obtaining the truth of a certain end.

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He addresses John Hunter in an easy manner: he styles him one of the best anatomists of the present age; who had frequently found, in the dead bodies which he opened, that the great curvature of the stomach was considerably eroded, and sometimes dissolved. In the latter case, the edges of the wound were as soft as half digested food, and the contents of the stomach had got into the cavity of the abdomen;—that such a wound could not have existed in life, as it had no connection with the disease, and more frequently appeared in persons who died violent deaths. In order to discover the cause of this phenomenon, he examined the stomachs of various animals, both immediately and some time after death. In several he observed the same appearance. Hence he thought he was enabled to assign the cause. He supposes the solution to be owing to a continuance of digestion after death, and that the gastric fluid is capable of dissolving the stomach when it has lost its vital principle.—From this discovery he infers, that digestion neither depends on the action of the stomach nor on heat, but on the gastric juices, which he considers as the true *menstruum* of the food.

When, (says the Abbé,) this short but sensible paper came to my hands, I was engaged in
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experiments on digestion ; the result of which were, that some subjects were opened sooner and others later after death ; *but among the numbers I inspected, not one had its great curvature of the stomach dissolved or much eroded.* I say much eroded, because I have often seen a little erosion, especially in different fishes, in which, when I had cleared the stomach of its contents, the internal coat was wanting. The injury was always confined to the inferior part of the stomach.—If these facts are favourable to Mr. Hunter, a great number are against him. And here the Abbé proceeds in a vein of neat sarcastic humour, most admirably calculated to let a man softly down.—It must have been melting when poured upon the hissing hot passions of John Hunter,—melting as the wanton flakes of snow descended into the glowing bosom of a rosy virgin.

These facts, (says the Abbé) do not however destroy the observations ; mine are only negative,—his are positive ; and we know that a thousand of the former do not destroy a single one of the latter, provided it is well ascertained. I have no reason to distrust Mr. Hunter, for his paper has the air of ingenuousness and candour, which usually accompanies truth.”

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The Abbé modestly proceeds to say, that the ill success of my experiments did not induce me to abandon the idea of digestion after death: it only led me to consider it in another point of view. The result of which follows:—

These facts I think decisively prove, that animals, at least the species just mentioned, continue to digest after death. If we consider the matter rigorously, it will be proper to obviate a difficulty that may be started. However careful we are to kill the animal immediately after it has swallowed food, it is certain, that there will be a short interval between the time the food gets into the stomach, and the death of the animal, *and that the gastric fluids act upon it during this interval: moreover after death they will act for some time just as in life, since the vital heat is not instantly extinguished.* The digestion therefore observed in dead animals may, if not entirely, at least in part, be produced by the gastric fluid acting during life, *and a short time after death.*

The Abbé pursued the investigation by farther experiments, which tended to confirm, and which did confirm, the *necessity of heat to digestion* in many animals. He then concluded this curious enquiry by resolving to see what change
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would take place upon flesh, when the stomach was taken out of the body. And he finishes the subject in the following manner:—

In these experiments, I did not perceive any erosion of the stomach, any more than in those made with the view of verifying Mr. Hunter's. I only saw, what I had seen before, a slight excoriation of the inferior part. We must therefore infer, that the coats of the stomach suffer less after death than flesh introduced into it.

Upon reviewing the experiments, and following paragraphs, it cannot, I am apt to think, be doubted, that digestion goes on for some time after death. I therefore entirely agree so far with the celebrated English anatomist, but I cannot with him suppose, *that this function is independant of heat*; numberless facts related in this work fully prove the contrary.—Thus says the Abbé.

These experiments and conclusions, drawn from them by the Abbé Spallanzani, produced from John Hunter a paper at some length, entitled *Observations upon Digestion*, throughout the whole of which he evidently discovers signs of disconcertion. He begins with moralizing;—he proceeds with *scolding like a very drab*;—and he

he finishes without producing a *fresh instance* of the coat of the stomach being found to be dissolved;—notwithstanding the idea was gone abroad—the observation was open to any one who chose to seek after it—and notwithstanding the number of years that had elapsed, from the time of John Hunter's first paper being published—the time of Spallanzani's ingenious observations upon it—to the time of John Hunter's last observations.

To what purpose was all he could say besides, —if he could not conjure up another case to back the three that were disputed?

Was it because the Abbé had said his first paper was short, that he made his second so long?

Why has he referred us to his usual resource in argument, by recalling attention away from the fact to what he did at Bellisle in the years 1761 and 1762;—and does his cramming the stomachs of lizards and worms substantiate the appearances of stomachs in man after death—so strongly as examining human stomachs?

Does his abuse of priests,—by saying that it is presumption in them to affect to reason upon a science in which they have but a superficial

knowledge,—confirm the existence of a phenomenon which he is contending for?

If I can take upon me to assert, that since the propagation of these three cases given by John Hunter so long back as in the year 1772, there has not been *another* discovered, where the stomach has been digested after death,—I think I substantiate a ground for true criticism. And if I can take upon me to assert, that there is *no instance* to be found of any such case, *before* the time of John Hunter's propagation of the idea by three cases,—I think I am authorized in saying, that I do not believe him—that I doubt both cause and effect.

I cannot point out any thing more obvious, than that the digestion of the stomach would appear to the eye of *every observer*, if in reality such a fact existed. But as it will ever be found difficult to see what never happened, I must assign *that* as a reason why the digestion of the stomach after death has proved, from the observations of all men besides John Hunter, to be a *Nonentity*. The cause of its not being seen cannot be from ignorance in anatomy or physiology: to see this phenomenon only requires the use of the eyes—only such a use of them as might be applied to the confirmation of an object already established.

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If the gastric fluid, independant of animal heat, after the departure of the vital principle, possess in its nature a power of dissolving the stomachs of those who die suddenly—if the gastric fluid can thus dissolve a stomach solely by its power, when that fluid was not vitiated, and—when the component parts of that stomach were not impaired by long disease—how much more frequently would the cause and effect, be discovered on the stomachs of those, who lingered till death—and where there was a vitiating power acting upon a part feeble in resistance?

If Spallanzani has proved by experiments that digestion after death cannot go on *beyond the time of departure of vital heat*—if observations tell us, that excepting to John Hunter, the case has not occurred—if reason points out that the gastric fluid upon a cold lifeless subject is perfectly inactive—then it follows that the cause of those cases given by John Hunter has not been truly defined: and the ingenuity of physiologists might be at rest with respect to any cause they might seek to assign for it, until the case happen again.—Curiosity need not be awakened at least, until the desultory evolution of this phenomenon—this eccentric and blazing comet with a fiery tail—be again discovered in the anatomical hemisphere.

July 1, 1773

OBSERVATIONS ON THE TORPEDO.

PHIL. TRANS.

THE torpedo or cramp fish, or electrical ray, the raja torpedo of Linnæus, is a flat fish much of the figure of a thornback. Its measurement in those of a moderate size is about four feet in length, and two feet and a half in breadth. A species of them has been found in the Mediterranean, on the coast of France, at Torbay, and at Waterford.

This fish was early known to the Greeks, who, from the name they gave it, discover that they had a knowledge of its torporific qualities: calling it, *ναρκη*. Pliny and Plutarch both note it.

Among the moderns,—Reaumur has made experiments upon it, and has communicated them to the Royal Academy of Paris in the year 1714. He described the muscles by which the electrical power is conveyed, and was followed by Redi, Steno, and Lorenzini.

Since these researches, experiments to ascertain—whether the shocks given by the torpedo corresponded with the theory of electricity,—were for
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some time the favoured pursuits of John Walsh, who published his paper about the time that John Hunter's anatomical description came forth, and which was read to the Royal Society, at the instance of John Walsh. Two plates were annexed to this paper. The experiments of Walsh were continued on by others. Cavendish published also the result of his experiments in the Philosophical Transactions.

As John Hunter followed Reaumur, Redi, Steno and Lorenzini, in dissecting the torpedo, *but without noticing their names*—I do not doubt nevertheless by his anatomical knowledge he might have left the subject improved.

Feb. 27, 1774.

OF CERTAIN RECEPTACLES OF AIR CELLS IN BIRDS.

PHIL. TRANS.

I Have constantly endeavoured to make a distinction between the merit of him who makes a discovery and of him who prosecutes that for farther discoveries—which has been already established. The first must be considered as an active result of original ingenuity—the second might be merely the mechanical progress of anatomical observation.

John Hunter presented this paper to the Royal Society as containing original information—as the pure result of what he himself had discovered.

The subject is truly ingenious and highly curious : and considering it both in a natural and anatomical view, it imparts useful information.

The cells in the bodies of birds which receive air from the lungs are to be found (in some only,) both in the bones and in the soft parts ; and have no communication with the cavity of the common cellular membrane.

The bones which receive the air are lighter, less vascular, containing little oil without marrow, and weaker than other bones. The Turkey cock, the pouting pigeon, the pelican, the owl, and goose, with many more, possess those air cells,—some in the soft parts, and—others in the bones, of which I shall not go into an explanation. The swelling of the Turkey cock,—the pouting of the pigeon,—the hissing of the goose and of the owl are all explained by a knowledge of this discovery.

When John Hunter published this paper, he, as far as his reasoning or his prudence suggested, assigned to the lungs of those birds the power and channel of conveying the air into the cells : and what was not done by the channel of the lungs, he said he did not know by what means it was done.

In this paper, he makes it out to be understood,—that it should, if it were not really entitled to originality, have all the appearance of it. And in order that this prudent suggestion might be the better received, he with much plausibility and seeming ingenuousness aims to shew that he could demonstrate some parts, but was at a loss in accounting for others : and thus has he

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said,

said, "I do not know by what means the air is diffused universally to parts."

This *candid* confession of not being perfect in the knowledge of a subject newly discovered, *seemingly* carries with it an air of truth, which would throw the shallow misgiving sceptic off his guard : and his not quoting a single authority would naturally save the trouble of any reference. But John Hunter I know had no delight in comparative anatomy, when it was to be connected with comparative ingenuity ; and the darker he made the room of information, the more he was at liberty to do within it whatever he chose without being detected.

Next to the importance of this subject, as really useful in accounting for what would be phenomenon without it, is the question that arises upon the originality of the paper published by John Hunter without a reference to another name. And knowing as I do—that his natural propensity was for making every thing his own,—that every discovery was first to be brought within his own vortex,—and afterwards to be diffused to the public for their good, without his caring for the real author,—I might perhaps be supposed to decide against judgment and right,—if I were not to assign my facts.

When

When the Profeffor Camper had read this paper in the following year 1775, he instantly refers to a paper published by himfelf, upon the fame fubject, and printed at Amfterdam. It was joined to a volume of other physiological fubjects; one of which was, “a fhort Expofition of the Anatomy of a young Elephant,” &c. Thefe are his words, extracted from a catalogue of all his works, under the article of the year 1775.

In eodem diario extat epiftola, in qua, me diu ante clariff. Job. Hunterum aëris ingreffum intra cava offa volucrum detexiffe, evidentiffime demonfiro; etiam rationem, qua aër per tubas Eufiachianas in mandibulas avium et intra duplicaturam tabularum cranii ingreditur: quam fe ignorare adhucdum in vol. lxiv. Phil. Transf. ann. 1774. edit. faffus eft. pag. 211.

Roterodami 1774. vol. i. p. 235.—Difertatio de Offium majorum in avibus ftruttura. Ubi aëris ingreffum in cava offa per refpirationem demonfiro, et inventionis hujus diem 10 Feb. 1771 indico.

In a fecond paper, written to explain more fully his firft, John Hunter has thus answered the charge of Camper: and it will appear that according to circumftances he can fuit the tone

of his voice—the professor having come over to England on purpose to clear up the plagiarism.

“ When I wrote this account to send it to the Royal Society, I did not then know by what means this was done : for in that I said ‘ but by what means I do not know ;’ that is, I did not know whether it was conveyed by the trachea, where it passes along the neck, or the Eustachian tube. Professor Camper, when he did me the honour to call upon me, was so obliging as to take some pains to shew me, in the lower jaw of the hawk, the hole where the air entered ; which makes me suspect he did not understand what I had written. For after having given the marks by which such openings were particularly distinguished, it will be hardly supposed I could say that I did not know the hole where the air entered.”

March 17, 1774.

ON THE GILLAROO TROUT.

PHIL. TRANS.

THE stomachs of trouts are uncommonly thick and muscular : they feed on the shell fish of lakes and rivers, as well as on small fish ; and take into their stomachs gravel or small stones, to assist in comminuting the testaceous parts of their food.

The trouts of certain lakes in Ireland, such as those of the province of *Galway*, are remarkable for the great thickness of their stomachs ; which, from slight resemblance to the organs of digestion in birds, are called gizzards ; and the species that have them are called *gillaroo* or *gizzard* trouts.

Daines Barrington suggests, that *gillaroo* may be either a corruption of *Killaloe*, the name of a town near to where those trouts are caught ; or formed from a Welsh word signifying stomach and an Irish word signifying strong, so that *gillaroo* might be the same as *strong* stomach.

Watson had written on the subject before John Hunter, and proved that the digestive power

power in this fish was carried on in reality by a *stomach*; and not, as erroneously conceived, by a *gizzard*. John Hunter has only said the same at second hand. But in saying this, *I beg to be understood as not asserting that John Hunter has borrowed any of his ideas from Watson*; or if he did, he has not acknowledged them,—not having mentioned the name of any one throughout his paper.

May 11, 1775.

ON THE GYMNOTUS.

PHIL. TRANS.

THIS fish is known more commonly by the name of the electrical eel. It is a fresh water fish, found in the river of *Surinam*. The common size is from three to four feet in length, and from ten to fourteen inches in circumference about the thickest part of the body. It resembles the conger eel.

This fish has been lately much the object of examination. Bancroft, in his essay on the natural history of *Guiana*, has described it; and Garden had given an anatomical description of the component parts of it in a paper in the Philosophical Transactions before John Hunter; but whose name or whose progress in the enquiry is not regarded by him. Garden's paper is dated from Charles Town, America, Aug. 14, 1774, and contains the best description of this wonderful fish.

The astonishing property of this fish is its power, exerted at its own pleasure, of giving an electrical shock to any person, or number of persons joining hands, provided the first touched the

the fish, and the last put his hand into the water in which it swam.

The shock is conveyed either by the immediate touch of the fish with the hand, or through the same metallic or other conductors which convey the electric fluid; and it is intercepted by the common non-conductors of that fluid. These electric qualities depend upon uncommon large muscles.

Walfh was prosecuting his enquiries into the nature of this power in the gymnotus at the same time that he was in the torpedo, and procured from John Hunter an anatomical description of this also.

John Hunter has purely obeyed the invitation of his friend, and has given a perfect anatomical description of the gymnotus with an illustration by three admirable plates; with no further comments than what tended to explain the subject.

*June 24, 1775.*EXPERIMENTS ON ANIMALS AND VEGETABLES WITH
RESPECT TO THEIR POWER OF PRODUCING HEAT.

PHIL. TRANS.

JOHN Hunter is most commonly induced to make choice of his subject, from some one having, a little previously to the time which he gives it his consideration, treated upon the same. And whenever that is the case, he always endeavours to induce the public to believe—that he was in possession of the knowledge he has to impart independent of that which others have suggested. It is from that cause, he always refers his readers to notes made at some distant date, and for the same reason, he is ever cautious in mentioning names or quoting authors.

These are his motives for beginning this paper after the following manner:—

“ Some late ingenious experiments and observations, published in the Philosophical Transactions, upon a power which animals seem to possess of generating cold, induced me to look over my notes, containing some which I had made in the year 1766, indicating an opposite power in animals, whereby they are capable of resisting any external cold while alive, by generating

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rating within themselves a degree of heat sufficient to counteract it."

The whole of this paper consists in experiments, and the notes of experiments must be a detail of them. The names of Fordyce and Erwin are appealed to as witnesses to some of the experiments: the former was the author of some late ingenious experiments and observations alluded to by John Hunter; but he, forgetting what he has before asserted with respect to the dates of his own notes, has confessed inadvertently, in a part of his paper, that some of his experiments were made many years after, as it can be proved from their dates. I am induced to believe, from the internal evidence of my own understanding, that he had never thought of the subject until it were thus brought forward with great philosophical eclat by Cullen, Fordyce, and others.

The experiments made by Fordyce in a heated chamber, on the effects of heat upon the human constitution, of which a report was given in the Philosophical Transactions prior to this paper by John Hunter—and the experiments on the power that certain animals possess of producing cold, made by the modest and ingenious Crawford, a report of which was given in the Philosophical

sophical Transactions subsequent, contain perhaps the fullest and most satisfactory arguments on the accommodating temperament of living animals to climates, which have ever been adduced.

But both these valuable papers in their purposes, go much further in explanation of many phenomena in the animal œconomy, than the limits of my present intention will permit me to demonstrate.

The mechanical philosophers, particularly Bacon, Boyle, and Newton, considered heat as producible in any body: and they were certainly right as to the cause of heat. Men with minds less comprehensive have contradicted them from not tracing effect up to cause. When de Luc evinced, by a variety of experiments, that the expansions of mercury between the freezing and boiling points of water correspond precisely to the quantity of absolute heat applied, and that its contractions are proportionable to the diminution of this element within those limits, he cannot be said to have shewn a cause of heat.

In the application of the cause of heat perceptible in animal bodies, mechanism, upon the principle of vitality, appears to be the cause, and

not density, as supposed by Boerhaave. Subsequent philosophy has suggested—that heat is a distinct substance, or an element *sui generis*: and Crawford has observed, that by viewing the phenomena in that light, they will be found to admit of a simple and obvious interpretation, and to be perfectly agreeable to the analogy of nature. This may be very fair, as the perception strikes our senses; but still there is beyond that argument an occult cause, which has not in my opinion been more satisfactorily assigned or explained, than by the mechanical philosophers whom I first alluded to.

Whether the subject be discussed under the head of cold or heat, as affecting animals, it is immaterial, both the terms being relative: and therefore John Hunter cannot be said to be more original, because he has varied his title from those who pursued the same question, any more than thus,—that—when he treats of heat he means freezing.—He freezes animals to determine, how far the principle of vital heat bears up against the influence of cold. His experiments, if they be true, carry with them no manner of information:—if they be true, no effect for the benefit of man can possibly be derived from them:—as they were made on reptiles of the lowest order, and whose anatomical construction

struction is *sui generis*. They amount in number to 44, and were chiefly conducted on dormice and on worms. They will never be read but from curiosity, and will be directly rejected from their trifling insignificance.

March 21, 1776.

PROPOSALS FOR THE RECOVERY OF PEOPLE APPARENTLY
DROWNED.

PHIL. TRANS.

JOHN Hunter observing that the endeavour to recover persons apparently drowned was a new practice, and had furnished as yet but few important clear facts, takes upon himself to issue out his instructions, somewhat after a manner similar to a proclamation.

“ Having been requested,” (he says,) “ by a principal number of the society established for the recovery of persons apparently drowned, to commit my thoughts on that subject to paper, I readily complied, hoping, that although I have had *no opportunities* of making actual experiments upon drowned persons, it might be in my power to throw some light on a subject so closely connected with the inquiries which for many years have been my business and favourite amusement.”

After having perused this self approving paper with the utmost attention, and remarked its leading instructions, I am able to say, that in comparing it with what has been advanced by Cogan, Letfom, Kite, and Colman, it falls very
short

short of their practical reasoning ;—and further, that in some of his remedies there is cause for seriously suspecting practical mischief. His double *bellows* does not accord at all with my opinion :—it is not suited to the case. For if the recovery of drowned persons consisted in procuring the action of the lungs upon the principle of mechanism, previous to the action of the heart, and independant of muscular and every other vital power, yet observations made by the maturest opinions are against the propriety and possibility of any such promising prospect :—the natural effect from drowning forbids all hopes in it.

This *bellows* of his was said to be made according to his notes in the year 1755, for the purpose of trying some experiments upon a living dog. It was constructed in such a manner, as by one action to throw fresh air into the lungs, and by another to suck out again the air which had been thrown in by the former action, without mixing the two airs together.* The muzzle of this *bellows* was fixed into the trachea of a dog, and by working it he was kept alive. I shall not contend the facts of the invention of the *bellows* and of the experiments on the *dog* :—of the former being invented and tried upon the latter in the year 1755, and by the record of

* He took the idea most probably from the construction of the bagpipes.

of notes then made,—of these being ten years afterwards brought forward to answer the purposes here required.—This is not worthy my time. But the probable inference from this application of the *bellows* ought to be remarked.

Is the case of a live dog, as stated by him, applicable to the case of a drowned man—where the vital powers, the actions of the lungs, the heart, and the muscles assisting respiration, have ceased, are suspended, or possibly never can be restored? and what from observation has been the state which the lungs have been found in of a drowned man? An old opinion prevailed that the water by its force and weight rushed into the lungs, filled them, excluded the air, and produced death almost in the same manner, as a collection of water sometimes does from a disease of the lungs. But this opinion was confuted by Becker, and in which he was joined by the illustrious de Haller.

However de Haller has said, that upon opening a woman drowned in the river *Leine*, and who had remained several hours under water, he had an opportunity of observing what he had previously discovered by experiments—viz. that all the parts of the lungs and breast remaining entire, upon pressing the lungs the
water

water which had made its way into them, evidently regurgitated by the wind pipe; so likewise, upon pressing the stomach, the water which the woman had swallowed returned by the gullet. The lungs were entirely black, and the heart void of blood.

But we ought not to conclude, (de Haller says) from this instance, that Becker's hypothesis is not founded on truth: for a very probable cause may be assigned for it, and both observations, of the water being found or not in the lungs, may be reconciled thus:—

If the body, for example, should be opened immediately or very soon after the person was drowned, there might be perhaps no water found in the bowels, the constriction of the *glottis* excluding it.—But if it be not opened till several hours or perhaps days after death, the tone of the muscles in that time being relaxed, the *larynx* and *glottis* will both be opened, and the water, especially if the person was drowned where it is deep, will, by its proper weight, gradually make its way through the relaxed passages into the lungs and stomach.

De Haller made experiments for ascertaining what hopes there may be of recovering persons
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drowned, some of whom *have been said* to be brought back to life after lying a considerable time in water. And these in my opinion are more satisfactory to demonstrate what happens, what is true, and what is proper and possible to be done, than any hypothetical conjectures that might be formed by the success of a *bellows*.

In the year 1753, two drowned dogs died within twenty five minutes, so as not to be recovered by all the arts that could be used. There was water found both in the stomach and lungs, and by compression it run out by the wind pipe mixed with a great deal of foam. The lungs were red but swam in water.

A cat was plunged suddenly into water, and died irrecoverably in the space of two minutes. In the stomach there was no water, but it had got into the lungs, and run out foaming, mixed with the air of the *trachea*.

In another dog which was irrecoverably drowned, there was a great quantity of water both in the stomach and lungs; in the *cava* and pulmonary veins there was a deal of black thick blood.

In

In a fourth dog, which was taken out after remaining *seven minutes* under water, and was quite dead, there was a quantity of water found in the stomach, and likewise in the *trachea* and lungs, mixed with foam: the vessels belonging to the right auricle and ventricle were full of blood, and those belonging to the left, empty.

From other experiments it appeared, that the *glottis*, in animals that have been drowned, remains open; that the water found in them has been swallowed voluntarily, but it does not make its way into the lungs of the dead body by gravity; for in those animals that are plunged into water after they are dead, there is no water found neither in the stomach nor lungs.

In all the experiments which the ingenious Evers published in his *thesis* at Gottingen in 1753, the event was almost the same as abovementioned.

The cause of death, in animals that are drowned, seems to be chiefly by the water drawn into the lungs, and by the last strugglings of the animal conquassated into foam with the air contained in the *trachea* and lungs, which foam cannot be expanded by any dilatation of the *thorax*.

These experiments leave little hopes of recovering persons who have been drowned, seeing that the obstructing foam cannot be expelled from the *aspera arteria* and lungs. How far the *bellows* of John Hunter is calculated to remove this obstructing foam—to disengage it from the lungs and *aspera arteria*—to restore the suspended circulation of air, blood, and vital heat—must be very obviously known to those who consider that a double *bellows* made for the purpose of exhausting or filling a substance with air, cannot exhaust from that substance a *viscid foam*, nor restore vital heat suspended at least if not annihilated.

It might be considered as out of season, when reason even interferes in obstructing the enthusiasm of humanity; and therefore the delusive hope, founded upon the most sanguine expectation of the recovery of drowned persons shall not be damped by any opinion from me,—where the prospect is ever so distant.

Having finished the serious part of this subject without relieving my mind of a burthensome reflection which must ever attend it—I shall only remark farther upon a note of John Hunter's:—

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He has said—"I shall consider the situation of a person drowned to be similar to that of a person in a trance. In both, the action of life is suspended, without the power being destroyed: but I am inclined to believe that a greater proportion of persons recover from trances than from drowning, because a trance is the natural effect of a disposition in the person to have the actions of life suspended for a time; but drowning being produced by violence, the suspension will more frequently last for ever, unless the power of life is roused to action by some applications of art."

That which he has produced as similar, has been destroyed by him for want of similarity.

What is, a Trance?—Comparisons for the elucidation of a truth are generally drawn from familiar subjects; at any rate the subject for illustration by comparison, should have been defined, by somebody, in order to be known. Medically speaking, I have never read of, a trance—historically, I have heard of it: but it was when I was a child, from the gossip of old women, as something told at a late hour—as something too affrighting for the house maid in a family ever to go to bed without the footman. What author has defined it? Where, I ask his admirers, am I to look for the information which he was in possession of?—

of?—But such were the strong and abstract powers of the illustrious John Hunter!

A Trance has been ever defined as an, Ecstasy. In this sense, poetically, we can read it in Spencer, Milton, and Thomson;—but I have never read a medical case which authenticates, a Trance, and know not where to find one:—neither Motherby, nor Wallis after him, have given the word a place, even in their *medical* dictionaries.

Locke has asked—whether, what we call Ecstasy—be not dreaming with our eyes open? I answer—that it is; and refer for an example of my assertion—to John Hunter!

1778.

THE NATURAL HISTORY OF THE HUMAN TEETH, AND
A PRACTICAL TREATISE ON THE DISEASES OF THE
TEETH. IN TWO PARTS. PRICE 1l. 1s.

THIS is a work of public importance, and comes fairly before the criticism of a surgeon, the second part of it being altogether a performance purely surgical.

It is the first ripe fruit which has dropped from the tree of his anatomical and surgical cultivation. It proclaims an intention of being presumed to be all his own—of being independant of any borrowed aid—of being the only publication upon the subject :—there is not throughout the whole of it a single author alluded to,—there is not a thought suggested of any one having treated upon it before : it is to be deemed a solitary tree of surgical knowledge, planted, raised, and perfected by John Hunter alone.

In the following treatise he also professes, that the observations were made before the year 1755. He gives a consequence to the subject. He says, “ that the importance of the teeth is such, that they deserve our utmost attention, as well with respect to the preservation of them, when in a healthy

healthy state, as to the methods of curing them when diseased : that the diseases which may arise in consequence of those of the teeth are various ; such as abscesses, carious bones, &c. many of which, although proceeding originally from the teeth, are more the object of the surgeon than the dentist ; who will find himself at a loss in such cases, as if the abscess or carious bones were in the leg or any other distant part."

He proceeds to draw a strong and interesting or interested line of duty between the offices of surgeon and dentist.

" All the diseases of the teeth, which are common to them with the other parts of the body, should be put under the management of the physician or surgeon ; but those which are peculiar to the teeth and their connections, belong properly to the dentist."

He proceeds to state the separate duties of those distinct professional offices.—

" It is not my present purpose to enumerate every disease capable of producing such symptoms as may lead us to suspect the teeth ; for the jaws may be affected by almost every kind of disorder. I shall therefore confine myself to
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the diseases of the teeth, gums, and alveolar processes; which parts having a peculiar connection, their diseases fall properly within the province of the dentist. I shall also purposely avoid entering into common surgery; not to lead the dentist beyond his depth, and to matters of which it is to be supposed he has not acquired a competent knowledge."

Notwithstanding what he has asserted in the above paragraph, he has been found to treat every diseased case according to his best ability as a surgeon.

He proceeds to explain some further intentions.—

"In order that the reader may perfectly understand what follows, it will be necessary for him previously to consider and comprehend the anatomy and uses of every part of a tooth, as explained in my natural history of the human teeth, to which I shall be obliged frequently to refer. Without such previous study, the dentist will often be at a loss to account for many of the diseases and symptoms mentioned here, and will retain many vulgar errors imbibed by conversing with ignorant people, or by reading books in which the anatomy and physiology of the teeth

are treated without a sufficient knowledge of the subject."

This last paragraph is in the highest style of a great and learned author, and it flatters my consequence inasmuch as he has flattered his own: for if his work be of that importance, it reflects an importance upon him who criticises it: it is —*dignus vindice nodus*—and it shall have the fairest justice.

I shall begin by avowing an axiom which I doubt if it can be fairly controverted:—that no man can sit down to write the life of another without being capable of tracing his motives. For if the general motives be not marked which lead on to the great variety of undertakings the opportunity of life is constantly offering,—if the motives be not truly ascertained,—they might be seen indeed in many views, but it is very rarely, without that clue which leads directly to the knowledge of the heart, be in the possession of the biographer, that he ever can be capable of developing their origin. A laudable motive not being well understood,—by considering it abstractedly,—by looking at it askance, might be shamefully perverted. I therefore shall first of all, trace this motive in John Hunter for writing this book.

John

John Hunter, at the time he published this book, had but very little practice; the whole circle being then filled up by names to which I have before alluded. Independant of his half pay as an army surgeon, his whole support depended upon professional emolument. It is not to be wondered at therefore, that when the common field of practice was already stocked, and its pasturage short, a man of his mental resource and active application, should be directed in search after fresh ground, where he could by himself range at large.

In this motive, he followed in some measure the practice of the French surgeons, but it was directly against the habits of the English. Hawkins, Bromfield, Sharpe, and Pott, were proud and unaccommodating professional men. They were above submitting to consultations with dentists. Their patients who wanted advice for relative complaints of the teeth, sent for or went to them, and from them took the instructions which the dentists were to obey. Such was the rude and gothic state of practice, when this motive offered to John Hunter.

He laudably condescended to accommodate himself to the necessity of the case:—and to fill up this chasm in practice, thus proudly kept

open till now, he placidly attended on fixed days and hours at the house of a dentist,—to aid him by consultation for the benefit of his patients. But few are amongst the happy favourites of fortune ! John Hunter was not found to bestow his smiles upon every dentist ;—his sincerity in friendship confined him alone to the family of the Spences. This instance of friendly generosity and partial attachment, is too well known to be rendered doubtful ; as sooner or later there will scarcely be found a family in England, that cannot, from meeting him there,—but what is more impressive,—from sharing in his surgical bounty, attest its truth,

The following note therefore, taken from the present work, p. 90, cannot be considered as corroborating a fact already established upon the memory of the public ; but it will tend to prove what might be otherwise doubted by some, viz. John Hunter's sincerity in friendship for the family of the Spences,—as he has here publicly avowed it. After dealing out his cautions to the dentists at large “ not to extract the teeth too quick,” somewhat after a similar manner which *Hamlet* delivers his caution to the *Players*, he thus is found to distinguish the family of the Spences. “ I must do Mr. Spence the justice to say, that this method appears to be peculiar to him, and that

that he is the only operator I ever knew, who would submit to be instructed, or even allow an equal in knowledge; and I must do the same justice to both his sons."

In recollecting the name of Spence the elder, an incident comes over my memory which I cannot refrain from explaining. I have scarcely introduced a story in this life, and have therefore a greater right of claiming the attention;—especially as this is not altogether episodical, but leading directly to the point I am to establish,—and more especially as it shews, that talent will always make its way, if he who possesses it looks strait forward to obtain that reward which distinction will always pre-eminently exact.

When I was an apprentice in Hatton-street, in the year 1762, a painful tooth provoked me to have it drawn; and there was no one so high in fame for extracting teeth, as the elder Spence. To him I went into *Grays-Inn-lane*, and although he was situated in the vilest neighbourhood, I found not only the neatest appointments in his professional line, but also an operator which commanded the highest respect from his figure and his conduct,—from his personal demeanour and the appearances of things in general.

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His shop was exquisitely neat. The barbers blocks were as white as soap-suds could make them, and the blood basons were as shining, as if they had been directly brought home from the scowerers. The teeth exhibited as specimens in the shop, were as white and polished as ivory: they were developed of every perishable attachment; and the only wonder was—how they came to lose their destined homes, and how they were found where I saw them.

There was a painted hand in the window with ruffles pendant over the wrist—the lace of brussels imitation—holding a tooth betwixt the forefinger and thumb:—the figure of the hand—the graceful air of the finger and thumb—the pattern of the tooth—and the point of brussels lace, were so highly finished by the artist, that Denner even would have adjudged himself to be outdone if not distanced both in the design and execution. The civility of Spence was beyond all expression.—

After I had gone through the dreaded process of having my tooth extracted, and after that sort of explanation which ever takes place between the professional man and patient had been fully discussed, I was most kindly introduced into a back room:—there I was surprized at finding that
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a connection betwixt the philosopher and the man of an ingenious profession was only separated by a partition of board. The floor was covered by the freshest baize, green as the carpet of nature in the month of May; and upon a shining mahogany table, there was placed what I had never feasted upon with my eyes before,—an electrical machine. At that time its rarity enhanced its estimation.—Franklin and Ingenhouzen had not made their improvements notorious by publicity: the progress of electrical invention was just upon the dawn: from what I have since recollected of this machine, it was made upon the principle of the Abbé Nollet's—the conductor being suspended to the ceiling by silken chords. He excited my wonder, by the dancing of figures,—ringing of bells,—attraction of hair and feathers—and firing gun-powder placed out in the yard. Upon taking my leave, he shook me warmly by the hand, and said, young gentleman, we professional men never take any thing from one another! When I came home I found a Hare just sent to me by my father,—and from the impression of gratitude and the high entertainment I had received, was instantly induced to carry the Hare myself to my generous benefactor.

A man need not have been endowed with the power of prophecy, to foretell that the region of
Grays-

Grays-Inn-lane would not long be honoured with an inhabitant of Spence's promise:—in a few years after, the best house in *Soho-square* was ornamented by him; and before the date of this work of John Hunter,—a professional coalition betwixt them was adjusted.

Spence was a diffident man: and by the change which he made of situation—by the eminence which reputed merit had conducted him to—he experienced a sort of revolution in practice, which his modesty disposed him to shrink at. He found an alteration in the nature of his practice:—he was not only to dispossess the rotten tenants of their mansions, but was now engaged in adorning the sound ones—in arranging those which intruded upon each other—in filing, cleaning, polishing—and at length in transplanting. He did not therefore reject the proffered aid of John Hunter. The coalition was formed without difficulty betwixt two men, who laudably aspired in their distinct provinces;—who were born in the same country,—and who were, much upon a par in their education; and the mutual interest it promised to create, gave vigour to that scheme, in which John Hunter more than Spence was to be exalted.

John

John Hunter was to produce this work, which would authorize Spence to introduce him as a chosen surgeon best acquainted with diseases of the teeth and jaws, because he had written a *learned treatise* upon them, and had made them his favoured study. Spence soon found his house crowded by all the fashion of the age: the stock of beauty even to this day has experienced an encrease by their consultations: and had it not been for a few unfortunate cases, where by transplanting of teeth the venereal disease was conveyed, and where the patients thereby fell victims to that poison, in all probability neither of them would have been disgraced by the connection.

Almost all anatomists have written on the teeth, but there are few who have treated it from their own observations: they have all servilely copied merely each other, excepting Eustachius, Fallopius, Columbus, André Dulaurens, Rolpinkius, Duverney, Bertin, Albinus, and some professed dentists. John Hunter, I repeat it again, has not alluded to a single name throughout his book—but that of Spence. This work by him contains little originality, as the following

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remarks

remarks will fully demonstrate, and that it was not written from practical observation. He very properly begins the subject with a description of the maxillary or jaw bones, and first of the upper. But he must certainly have supposed his reader to be already acquainted with anatomy, from the very curtailed description he has given of those bones, and the omissions of importance he has committed. It is highly requisite for a dentist to know in what manner the teeth are supplied with blood and nerves, and how these are conducted to the teeth, as they certainly are as much connected with the teeth, as their *periosteum* or gum; for by their means some very considerable changes are brought about.

He has taken no notice, in his description of the upper jaw, of those cavities first mentioned by Fallopius—of those maxillary sinuses or *antra* by Highmor;—although in the second part of his work he treats on one of the diseases to which the membrane lining those cavities is liable, namely suppuration or abscess. Of these the uninformed dentist must therefore be at a loss to know where their situation is, and of course their suppuration or abscess. He has however been particularly full in his description of the alveolar processes

puration, and a gum boil, when a part of the socket has been destroyed, will appear externally : or this membrane may be preternaturally thickened, and a fleshy kind of substance be produced, which may in the end degenerate into bone, and which is confirmed by the following facts.

The *periosteum* covering the stumps of teeth, and those of decayed teeth that have been painful from their vessels being exposed by *caries*, is invariably found considerably thickened. In stumps especially which have remained for some time in their sockets, after the crown or body of the teeth to which they belonged has crumbled away, a bony thickening or swelling of their extremities is most commonly observed. The *exostosis* of teeth arises therefore from an ossification of the *periosteum*, which is sometimes so extensive, that all the fangs of a tooth will be united together by this process, and which is readily distinguished from an original union of the fangs, by its rough and unequal appearance, and then a new *periosteum* is formed.

A remarkable case of *exostosis* of teeth, was seen in a man who had not a single tooth in his head, whose fang or fangs were not thus affected. Dying of a fever, his teeth were drawn by
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a surgeon,—not with any view to disease, for he was unacquainted with the man's former sufferings in his jaws,—but observing this swelling of the fangs of all the teeth extracted, he was induced to enquire,—whether he had not had a painful affection of his face; and his case being remarkably obstinate, was well recollected. Even the fangs of the *incisors* of the under jaw were thus affected, although it is very rare to see them so; and which arose perhaps from the vessels supplying them being so very minute, and from the *foramina* through which they must pass being also so minute,—that whatever irritation there may be, it seems to have but very little effect in impelling a more than usual determination of blood to the teeth.

That an *anchylosis* may take place between the tooth and its socket, either at its formation or afterwards, I cannot pretend to deny, but it is a very rare occurrence indeed. It has been enumerated among one of the circumstances rendering the extraction of teeth difficult; but as far as I can learn from persons who have paid very particular attention to the teeth, they have never seen any thing of the kind. They have known a tooth so intimately blended with the socket, by means of the *periosteum*, as to be difficult to determine, whether it was not by a bony union;
but

but by macerating some time in water, it has been separated, when the membrane has been by putrefaction found to be destroyed.

In p. 39 and p. 40, John Hunter has made an assertion which seems to be not founded in truth—"That affections of the whole body have less influence on the teeth, than on any other part of the body: thus in children affected with the rickets, the teeth grow equally well as in health, although other bones are much affected; and hence their teeth being of a larger size in proportion to the other parts their mouths are protuberant."

It has been fully and generally known, that rickety children are considerably longer in getting their first teeth than others, and that they have their second smaller than common. As this is the fact, and as the teeth appear above the gum in proportion to their progress to perfection, so this observation by John Hunter must be a conjecture in his theory. He was led to it, perhaps, from remarking that weak and puny children generally get their teeth sooner, and have their second teeth larger and stronger in proportion to their other bones,—a fact which cannot be satisfactorily explained: it has been plausibly conjectured, that from the arteries
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of the body being more relaxed in such children, and consequently those likewise supplying the teeth, a greater quantity of the matter which forms the teeth is permitted to permeate them. But if this were the case, other bones would likewise develop themselves in the same proportion, which does not appear to be the truth.

In p. 41, where the cavity of teeth is treated on and its contents, he has not noticed the membrane which lines the cavity, upon which blood-vessels have been seen ramifying in teeth, that have been minutely injected; nor has he mentioned it under the article of *periosteum*. In p. 47, he says—"I chuse to divide the teeth into the four following classes. *Incisores*, commonly called the fore teeth; *cuspidati*, vulgarly called canine; *bicuspidates*, or the two first grinders; and *molares*, or the three last teeth."

Ancient anatomists named all the teeth, the canine only excepted, from their use: hence the Greeks termed the *incisores*, *τομῆοι*, and the Romans *canini*, from the supposed resemblance they have to dog's large lateral teeth; but they should have been named as all the others have been, so as to give some idea of their use; then they would have been termed *laceratores*: as when we want to tear any thing, we instinctively place it between

tween those teeth; and the reason is obvious, because they have longer fangs than any of the rest, consequently are more firmly fixed in the jaw-bones, and are more adapted to be opposed to any force.

Molares, from their action on the food, are properly named:—is there then sufficient reason for altering the names of these teeth to those of *cuspidati* and *bicuspidates*, because when the former are first formed they have one point, and the latter two? Surely there is as much necessity for changing the names of the others from their having a certain number of points, as for changing the names of these. Of late years a spirit of innovation seems to have prevailed, for no better reason, than to render the knowledge and study of science more intricate and difficult of attainment. If terms long established and universally received are to be exposed to capricious changes, science must be perpetually involved in perplexity and confusion.

It has been confidently asserted, that no work contained a particular description of the teeth but this by John Hunter. It is very true that he has named no author, and therefore ignorance might be misled. Eustachius has been particularly full upon this subject; and whoever will be at the pains

pains of comparing the one with the other, will soon be convinced that the difference consists only in the collocation of the sentences. Some French authors have also been very explanatory upon teeth, and have not failed to inculcate,—that to be a good dentist, it was necessary to know the teeth singly and separately when they were out of the mouth.

In p. 60 he mentions the maxillary *sinus*,—that the first and second grinders of the upper jaw are placed immediately under it; and in p. 63 he speaks of the *antrum highmorianum* without noticing, that these cavities are known by both those names;—so that the uninstructed reader must consequently conclude the latter to be some other cavity in the jaw.

Of the formation of the jaws and teeth in the *fœtus*, a very accurate account is given in *libro secundo Annotationum Academicarum Albini*; to this I refer the reader for full information. In p. 79, treating on the milk teeth, John Hunter has said,—“These twenty are the only teeth that are of use to the child, from the seventh, eighth, or ninth month, to the twelfth or fourteenth year.”

If these were the only teeth useful, why should others appear before that time? The first adult

molars appear at the seventh year,—five years before the earliest period he has mentioned. And as soon as the fourteenth,—nay even at the thirteenth,—the second *molars* make their appearance. These facts may easily be proved beyond the defence of his *admirers*, by watching the mouths of children at these ages.

In p. 84 he has said, “that the first adult *molaris* comes to perfection and cuts the gum about the twelfth year.” I have already said that it perforates the gum long before this period. It is not completely formed at this time, but is much sooner than the twelfth year,—the time limited by him. This shews how little his attention was to the shedding of the teeth, or he could not have avoided knowing that the first adult *molars* make their appearance in both jaws with the permanent *incisores*,—about the seventh year, when children usually begin to shed their teeth. The second *molars*, which he says cut the gum about the eighteenth year, generally appear through the gum the thirteenth or fourteenth; rarely later than the latter period.

I have known the *dentes sapientiæ*, particularly in females, appear as soon as the seventeenth year; but more commonly not till the twenty first or twenty third year. He makes them appear

pear from the twentieth to the thirtieth. With regard to their coming to perfection, it is some time after they have cut the gum, that their fangs are perfectly formed, though they are not so late before that happens, as he has observed them to be. It very frequently occurs that a tooth will be completely formed, which does not appear in the cavity of the mouth: this is often the case with the *dentes sapientiæ*, and where there has been sufficient room for them. When this happens, the socket fills up, and in proportion as it does so, pushes the tooth into the mouth: and this explains—how teeth in one or other of the jaws, by their opponents being extracted, become longer,—and how stumps are protruded.

In p. 93 he has said,—“How the earthy and animal substance of the tooth is deposited is not perhaps to be explained.” If we may reason from analogy, it is explicable by saying, that the arteries have that power.

In p. 96 he has said, when treating on the enamel of teeth,—“It is a calcareous earth, probably dissolved in the juices of our body and thrown out from those parts which act here as a gland.” It will be almost needless to observe upon this, that earth is not soluble in any *menstruum*, and therefore this cannot be the truth of

the case. But that calcareous earth may be carried into the blood, in such small particles as to be capable of permeating the most minute vessels, and by them deposited on particular parts,—it cannot be questioned.

In p. 99 he has said,—“ I have seen two or three jaws where the second temporary grinders were shedding in the common way without any tooth underneath, and in one jaw, where, in both the grinders, I met with the same circumstance.” He afterwards gives the case of a lady, who desired him to look at a loose tooth not yet shed, which he directed to be drawn out, as another might come in its place, which did not so turn out. And in p. 100 he has said,—“ These cases prove evidently that in shedding, the first teeth are not pushed out by the second set, but that they grow loose and fall out of their own accord:” and then he says directly “ That the succeeding teeth have some influence on the shedding of the temporary teeth is proved by those very cases; since in one of the first mentioned, the person was about twenty years of age, and the other lady was thirty, and it is reasonable to believe that the shedding of the teeth was so late in those instances, from the want of the influence, whatever it is, of the new tooth,”

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The shedding teeth do not grow loose, and fall out of the mouth of their own accord; but are always influenced in this respect by the shooting up of the permanent teeth, and which the following case will tend to evince.—A young gentleman had the lateral *incisor* of the shedding teeth placed between the first and lateral one of the second set, on the right side of the upperjaw at the age of sixteen; which being drawn to give the adjacent teeth an opportunity of approaching each other, as the *dentes sapientiæ* made their appearance, there was not the least sign of any wasting of the fang visible, which doubtless there would have been, if the milk teeth did grow loose and fall out spontaneously. But there are many other instances of this fact, which it may be needless to enumerate. In the instances which he has mentioned, and which he says prove that the succeeding teeth have some influence,—or those teeth would not have remained so long after the usual period,—there was no opportunity of knowing whether there were teeth under those that became loose; it is most probable that there were, or the shedding ones would not have been loose.

It every now and then occurs, from some unknown cause, and it seems to have been so in the cases he has mentioned, that a tooth will be
arrested

arrested in its growth and remain stationary for a length of time, when from some incidental stimulus, it will become completely formed and make its appearance.—The following case is an instance of this:—a robust gentleman of the university of Oxford, at the age of forty three, had not shed the *cuspidatus* and the two *molares* on the right side of the upper jaw; the *cuspidatus* and first *molaris* were knocked out accidentally by the oar of a boat, and the second so loosened, that he removed it with his finger and thumb. In the space of two years from that time the permanent *cuspidatus* and *bicuspides* made their appearance.

In p. 113, treating on the sensibility of nerves, John Hunter has said—“Nerves of the teeth would seem to be more sensible than nerves are in common, as we do not observe the same violent effects from any other nerve in the body being exposed either by a wound or fore, as we do from the exposure of the nerve of a tooth.”

All nerves possess sensibility alike, but communicate to the *Sensorium commune* a greater or lesser degree of pain when morbidly affected, according to the resisting power of the parts on which they are distributed. Hence we do not feel that intense degree of pain in inflammatory affect-

affections of the *viscera*, the liver and lungs for example, as we do in those of the most solid parts; and hence the very sharp pain in the tooth-ach is caused by the stimulus of cold determining a preternatural quantity of blood to the cavity of the teeth;—for the bone not giving way, that excruciating pain is felt from its resistance. Pregnant women from the same cause are tormented with pain in almost all their teeth, without having them at all affected by a *caries*, and which usually ceases upon their loosing blood.

In p. 116, treating on the irregularity of teeth, he has said,—“ This happens only in the adult set of teeth, and is owing to there not being room in the jaw for the second set, the jaw bone being formed with the first set of teeth and never encreasing afterwards; so that if the adult set does not pass further back they must overlap each other and give the appearance of a second row.”

To suppose that the jaws do not encrease in those cases after the first set of teeth are formed, is highly ridiculous, and contrary to truth. If it were so, the teeth of the second set would invariably be irregular; for their magnitude makes them always in every jaw so very different from those of the first set, that there never could have been

been an instance of the sort. Permit me to ask, —to where the teeth are to pass further back, if there be no room for them in the jaws?

In p. 121, treating on the decay of teeth, he has said,—"From what cause this proceeds is hitherto unknown."

It is not for me here to treat on the causes of decay of teeth, which are certainly known,—which are obvious,—and which neither call for my discernment nor reflection.

In p. 122 he has said,—"It is best to draw a tooth on that side where the alveolar process is weakest."

To a person who knows how to fix his instrument, it is immaterial on which side he does it. But John Hunter's advice encourages fracturing of the alveolar process, which should be always avoided, because the gum, when it does happen, is generally lacerated and sometimes torn from the necks of the adjacent teeth, and their alveolar processes; in consequence of which, an exfoliation of the denuded bone will take place, in about six weeks or sometimes much longer from the operation. Surely an injury of such a description ought if possible, to be avoided.

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ON TRANSPLANTING TEETH.

It will be necessary before I make any remarks on what John Hunter has said respecting the transplanting of teeth, as the world in general has been persuaded to believe it to be a new operation, to acquaint it that the learned and faithful observer in surgery, Ambrose Paré, who wrote on the latter part of the fifteenth century, has made mention of the transplanting of teeth, lib. 16. cap. 26.

First of all he gives a case where he returned three teeth into their sockets which had been knocked out, and which fastened successfully: and then he follows with a report of a successful case of the transplanting of a tooth. But he did not of himself perform the operation, nor was he present when it was performed. He says of it,—*Auditum habeo ab homine fide digno.* But as seeming to have his doubts respecting it, he closes the case with adding,—*sed ut jam dixi de hac re, nihil præter auditum habeo.*

From amongst several more authors, Mauquet de la Motte, *Traité de la Chirurgie, t. i. obs. 2.* has related, in the reflections he was making upon what had been his remarks on cases of teeth,

which were drawn and replaced in their sockets, by observing first,—that it is prejudicial to draw a tooth that is not decayed, and the pain of which depends only on the irritation of the membrane enveloping the root of the tooth. He advises, if a tooth under such circumstances be removed, that it be restored speedily to its place, *puisque reprend aisément*. He has given a case to prove, that a tooth thus extracted and again restored, will, if it fastens, partake of all the sensibility of a tooth untouched.—It was of a gentleman who had a tooth drawn, and upon finding it sound, had it returned to the socket directly, and it united perfectly well. He hoped that by the nerve at the bottom of the socket being broken, he should have been hereafter free from pain of this tooth, but was mistaken, as in some years after, he was cruelly tormented with pain in it, insomuch as to have the same tooth again drawn. This was with difficulty effected after many tugs, and in extracting it a portion of the lower jaw bone came away with it.

Fouchard, p. 342, has given a case of the sensibility of a tooth, after it was transplanted from the mouth of a soldier. It was in the year 1715: the name of his patient was Romalet, captain in the second battalion of Bourbonnois. It remained in, for six years, and was drawn for being
caries.

caries. During the disease it frequently gave pain, particularly when the stump was filled up with lead.

These instances one would imagine are sufficient for proving the antiquity of the practice of transplanting teeth. And as all of them are dated prior to the *nativity* of John Hunter, he therefore could not possibly refer back to his *notes*, as was his practice in almost all other cases, for the purpose of being supposed the original author of transplanting of teeth. But if it be true, as he has said, that he never did read, probably it might notwithstanding have been his own original suggestion.

In p. 127 and p. 128, he is found to assert what is contradicted in the second part of his work. Treating on the transplanting of teeth,—“In like manner a fresh tooth, when transplanted from one socket to another, becomes to all appearance a part of the body to which it is now attached, as much as it was of the one from which it was taken; while a tooth which has been extracted for some time, so as to lose the whole of its life, will never become fixed. The sockets will also in this case require the disposition to fill up, which they do not in the case of the insertion of a fresh tooth.” Whereas in p.

105 he has said,—“ The insertion of dead teeth has been recommended, and I have known them continue for years without any alteration.”

This is an operation which has been likewise formerly practised, and many credible instances are recorded of its process. The practice however of both seems now deservedly to be discontinued : that of transplanting the teeth, on account of the disease which may be communicated by it : and without a boast of any other pretence than duty, I am to tell, that in its *bannishment out of practice*, I have been professionally instrumental. In the thirty last pages of my observations upon John Hunter's treatise on the venereal disease, I have established a case which no authority can destroy, of a lady who fell a victim to the venereal disease in consequence of a tooth being transplanted from the mouth of an *infected* girl into her own.

That of the insertion of a dead tooth seems to be discontinued from want of success : and as the method of fitting in natural teeth is arrived to such perfection so as to remain firm, when there are fangs for them to be inserted upon, ten years or more, where can be the necessity of transplanting teeth ;—an operation, independant of infection, the success of which is uncertain ?

Diseases

Diseases of the teeth have engaged the attention of almost every writer on surgery both ancient and modern : and as far as my observations have gone, they have been treated on in a more concise, satisfactory, and erudite manner, than in the work now before me. Of this I can speak with unbounded confidence, that the modern system of surgery would arraign many parts of John Hunter's practical treatment of disease as found in this treatise; and that he has not reached by many degrees the present standard as universally adopted.

Much remains to be corrected, and much to be yet done, in this part of surgery, which has been too much neglected, and which it is reasonable to suppose may be in some measure attributed to the work now before me; as competition is naturally arduous, but becomes more so in the opinions of young writers, when they think they are engaging in a contest with a *giant in fame*.

OBSERVATION ON P. III.

John Hunter has said,—that he transplanted a tooth from a persons head and fastened it with threads on the comb of a cock. The cock was
killed

killed some months after, when he injected the head. The comb was then taken off and put into a weak acid, and then he flitted the comb and tooth into halves. He found the vessels of the tooth well injected. But in a *note* to this he adds,—“ I may just remark that this experiment is not generally attended with success. I succeeded but once out of a great number of trials.”

This *note* was extremely necessary, and it enhances the value of the preparation. And I may just remark also, that if this preparation is to be found in his *museum*, it is that which I should have been the proudest of,—it being inestimable: and as one can hardly suppose that he would have thrown such a *pearl* away, it must of course be now seen in a conspicuous part of the *museum*. But if it be not, I am afraid that the whole will be suspected as a fabrication; especially as he has said, in p. 36,—“ We cannot by injection prove that the bony part of a tooth is vascular:” therefore instead of this experiment being seen in p. 36, it is sent out of the way from the true assertion there, to p. 111.

The plates to this work are most excellent.

Feb. 25, 1779.

AN ACCOUNT OF THE FREE MARTIN.

PHIL. TRANS.

FREE Martin is a name given in this country to a cow calf, cast at the same time with a bull calf, which has been conceived to be a kind of hermaphrodite,—that is, never known to breed, nor to discover the least inclination for the bull;—nor does the bull ever take the least notice of this animal. It has all the external marks of a cow calf, namely the teats and the external female parts, called by farmers, the *Bearing*. When these animals are preserved,—it is not for propagation, but for all the purposes of an ox or spayed heifer,—to yoke with the oxen and to fatten for the table. They are much larger than either the bull or the cow; and the horns grow larger, being very similar to the horns of an ox. The bellow of the free martin is like that of an ox; and the meat resembles that of the ox or spayed heifer, being generally, as it is said, finer than that of the bull or cow, and is more susceptible of growing fat with good food.

John Hunter has given an anatomical description of three of those animals; and according to his account there does not appear to be any system

system observed by nature in their formation; for if there were, they would all of them have corresponded with each other; whereas on the contrary, they all of them have differed most materially from each other, at least according to the relation which he has given of the appearances of parts of generation which offered from his dissections.

If these dissections have been made by him, and if he has honestly reported them, his conclusions from them are very lame. For I am apt rather to presume, that the same uniformity in appearances of every free martin externally, is an indication of their internal construction being uniform also. Natural history has correctly stated the external appearance of every free martin to be uniform; and John Hunter has not ventured to contradict it. Whereas he alone, as far as I know, has given an anatomical description of the parts of generation; and in that description is seen a jumble of the sexes confounded together; and that confusion in each is seen to be varying from the other. Now if this be the fact, and as he has stated it, a free martin cannot be strictly termed an hermaphrodite, but a *lusus naturæ*. Every one, according to him, is a *lusus naturæ*.

I be-

I believe the time will come, when it will be made apparent—that an imperfection in the organs of generation, either of a male or female animal in general, will no longer be said to be owing to both the sexes being confounded together. In all those singular cases which have come before my observation, and which have been described as hermaphrodites,—they have proved to be nothing more than an imperfect formation in the parts of generation of one or other of the sexes: for I never yet have been able to trace a solitary instance of both being confounded together. And in this instance of the free martin, I shall only add, that if upon dissection there be a variation in anatomical appearances, the inference is fair in presuming, that the external figure of the animal would correspond with it—since the figure it is known by is the only indication of its true nature: but a variation in its external figure has not been insisted upon.

There is evidently a contradiction either in the nature of the case, or in John Hunter's report of it. If the organs of generation in the free martin be dissimilar, the figure of every free martin will vary according to the relative predominance of either sex:—one would have a *penis*, another a *scrotum*, with or without testicles, &c.

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whereas

whereas externally this is not the fact. I shall not pretend to draw any conclusions against John Hunter's statement of the appearances from dissection, because he alone has dissected them; but I shall leave them to be filled up hereafter, when some other anatomist has made his report upon the appearances of the parts of generation in three free martins which likewise *He* has dissected.

Jan. 17, 1786.

ACCOUNT OF A WOMAN WHO HAD THE SMALL POX
DURING PREGNANCY, AND WHO SEEMED TO HAVE
COMMUNICATED THE SAME DISEASE TO THE FÆTUS.

PHIL. TRANS.

IF this paper had not been printed by the Royal Society when it was, there would have been no trace of it; as John Hunter did not think proper to reprint it with the rest of his papers that were published in the Philosophical Transactions, in his book entitled “Animal œconomy.”

Prior to the date of this paper, John Hunter had made up his mind upon theory, and had expressed it to his pupils in every course of his lectures from their commencement. And when he had made up thus his mind upon theory, no fact arising out of practice, be it ever so stubborn, or positive, or true, would induce him to alter his theory:—and this was the reason why he has said, that the pregnant woman *seemed* only to have communicated the small pox to the fœtus, —and why he has thus obstinately contended against the fact.

His intention was to suppress this fact as long as he could, from a variety of motives;—for as it was a fact, it accounted for other possibili-

ties of infection denied also by his theory and his habit of practice :—it accounted for the possibility of a fœtus contracting the venereal disease from the infected mother—it accounted for the possibility of a child contracting it from sucking an infected mother,—and moreover than all this,—it accounted for what has been proved by cases also, the possibility of a person being infected with the venereal disease by a tooth being transplanted from an infected subject into the mouth of a sound one.

Many of these facts having been brought forward, since this paper has *seemed* to dispute them, has been the cause of his suppression of it in his Animal œconomy, and also of his dropping the practice of transplanting teeth.

I shall not have occasion to argue this question.—I shall produce facts against his theory; and that which would not satisfy his belief, will be proof to all the world besides,—not excepting his admirers.—As he has contended the cases given by Van Swieten, and the case by Grant, of Mrs. Ford, in *Phil. Transf.* vol. 70,—I shall refer to one more given in the *Phil. Transf.* vol. 71,—and produce the copy of another, the strongest in point of fact that can be adduced, and which no contrary theory can hereafter destroy.

stroy. This case, because it militated against his theory, John Hunter refused to present to the Royal Society. However it obtained a reading in 1786.

CASE BY WILLIAM LYNN, SENIOR
SURGEON TO THE WESTMINSTER INFIRMARY.

In November, 1785, the wife of Mr. Eve, a coachmaker in Oxford-street, being then in the eighth month of her pregnancy, was seized with rigors, pain in the back, and other febrile symptoms. In two days time, the disease shewed itself to be the small-pox; and though the pustules were of the distinct sort, yet they were uncommonly numerous. On the eleventh day they began to turn; and on the twenty-second day her labour took place, which, according to her reckoning, was a fortnight before the regular period; that is, when she was advanced in her pregnancy eight months and two weeks.

The child, at the time of its birth, was covered with distinct pustules all over its body: they did not appear to be full of matter till three days after; at which time I took some of the pus upon a lancet, from one of the pustules on the face. With this lancet I afterwards inoculated, on the 2d of December, 1785, a child of
Mr.

Mr. Chaters, in Church-street, Soho, in both arms. On the 7th, the inflammation began to appear in each arm, and continued daily encreasing till the 11th of December, when the child sickened, and was affected with all the symptoms which usually precede the eruption. On the 12th the sickness and fever abated, the pustules of the distinct sort of small-pox made their appearance, and the child having regularly gone through the several stages of the distemper, was perfectly well in three weeks.

It may be proper to observe, that Mr. Findlay, surgeon, in Sackville-street, and Mr. Holladay, late surgeon to Sir Edward Hughes, in the East-Indies, were present, both at the taking of the matter, and at the subsequent inoculation of the child.

As no circumstance can prove the identity of the small-pox more indisputably, than its being communicated, with the usual symptoms and progression of the disease, from one subject to another; so it appears to be ascertained from the above facts, that a child can receive the varicellous infection from its mother, *in utero*.

June 1, 1780.

ACCOUNT OF AN EXTRAORDINARY PHEASANT.

PHIL. TRANS.

PITCAIRN, having received as a present of game, a hen pheasant, whose *feathers* were *variegated* in an extraordinary manner, from a Baronet, exhibited it as a curiosity to Banks and Solander; and John Hunter, happening to be present, was desired to examine the bird, and it proved to be a hen.

Lady T—— had also a favorite pyed peahen, which had produced chickens eight several times!!!

Have I not been sufficiently full upon this subject?

Nov. 14, 1782.

ACCOUNT OF THE ORGAN OF HEARING IN FISHES.

PHIL. TRANS.

NOTHING can more incontestably prove the necessity there is for every one who undertakes the writing of a life to enquire into *Motives*, than the evidence which this paper illustrates. John Hunter begins it, dated in the year 1782, with asserting, that the *organ of hearing in fishes* is still a subject of dispute, whether they possess the sense or not. And to prove that he was the discoverer of this faculty in fishes—he refers back, not to any publication in the year 1760, but to an observation made by himself at that time, when he was with the army at *Bellisle*.—There, says he, I had discovered this organ in fishes, and had the parts exposed and preserved in spirits. But lest his anatomical proof should have deceived him, he tried an experiment inferior in its nature, and more liable to fallacy, than his anatomical discovery.—Observing in a pond full of fish, that they were playing upon the water, when he was at *Lisbon* with the army in the year 1762, he fired off a gun:—the moment the report was made, the fish seemed to be all of one mind, for they vanished instantly, and raised a cloud of mud

mud from the bottom. This was not so certain and infallible an experiment as that made at *Bellisle*; as if fishes possessed no organs of hearing, concussion of air upon water, produced by the explosion of a gun, might have operated upon their sensations.

I shall not make any remark upon the probability of these observations by John Hunter: at any rate they were but observations only; and as such, he has, it is true, been so far uniform, as to be found in this instance, as well as almost in every other,—where a contention for making any discovery has rendered such a resource necessary for him,—to *antidate* his observations, and to *postpone* his publications.—He made his discoveries in the year 1760, and published them in the year 1782.

But as the best reply to his claim of originality, I shall proceed to the demonstration of facts tending to prove, that he pursued this subject not in common even with others, but much later, and that the discovery of the faculty of hearing in fishes was made prior to any part which he could lay claim to.

I shall not lay any stress of proof from what Willoughby had discovered in his natural his-

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tory of fishes, published at *Oxford* in the year 1686, as whatever John Hunter had or might have taken from him, he could find in Geoffroy.

I shall first take notice,—that the Abbé Nollet first of all evinced, by diving under water for this purpose, as appears in *Memoires of Academy of Sciences* 1743,—that water was capable of receiving and transmitting to the animals contained in it those-particular impressions that constitute sound.

Curious figures of the auditory ducts, and of the great variety of little bones or *lapilli*, are exhibited in Klein's history of fish, *Philos. Transact. abr.* vol. 9. p. 114.

I shall next say, that Professor Camper, the great rival of John Hunter, has shewn that fishes are really endowed with the faculty of hearing, and has described the anatomical organs adapted to this purpose in a dissertation published by him in November 17, 1761, and again in 1774. The following are two of the titles which may be produced from a *syllabus* of all the works of this accomplished scholar and ingenious philosopher,—besides *three* more, which I have not noticed.

Harlem: 1762, Tom. 7. pars 1. *Dissertatio de Organo Auditus Piscium Squammigerorum*, p. 79, quod 17 Nov. 1761, primus detexi.

Parisiis

Parisiis, 1767, in actis, &c. *Memoires de Mathematique, et de Physique, presentés a l'Acad. R. des Sciences, &c.* 1764. Tom. 6, p. 177, reperitur dissertatio, seu *memoire sur l'origine de l'ouïe des poissons*. De organo auditus piscium, gallicé: descripsi in eâ organum auditus, et cerebrum *Lophii*, seu *Ranae Piscatricis, Esocis, et Rajae*.

And lastly, I shall say,—that the commissaries of the Royal Society of Physicians at *Paris*, appointed to examine the work of M. Geoffroy, entitled *Dissertations sur l'Organe de l'ouïe de l'Homme, des Reptiles, des Poissons*, published in 1778, infer,—that the human species, quadrupeds, and cetaceous fishes, form the, *first*, class of animals whose organ of hearing is, the most subtle, and of the most perfect construction;—that birds are to be placed, in the *second*;—reptiles, who have the external mark of the *tympanum*, in the *third*;—cartilaginous fishes, in the *fourth*;—spinous or prickly fishes, in the *fifth*;—fishes of the eel kind, which seem to have only two semicircular canals completely formed, in the *sixth*;—and serpents, who appear to have no semicircular canals, in the *seventh*.

Do not these facts which I have adduced, demonstrate the natural propensity, or imbecility

rather, in John Hunter, stronger than any words of mine can prove it? What of originality had he to boast in the discovery, of hearing in fishes, by a publication so late as the year 1782, when he had only, as it plainly appears, the gleanings from others to pick up?

1785.

ACCOUNT OF A NEW MARINE ANIMAL.

PHIL. TRANS.

I AM at a loss to account for this animal, or rather species of worm, being described as *new*. Whatever novelty there is annexed to its nature is to be considered as that peculiar fort, which is naturally a result of the first impressi^on made upon the senses, and which is the effect of an individual never having observed the same object before. But if this animal is to be called a *new* one from no one having ever discovered or previously described it, I must beg leave to oppose those who might have ever entertained such an idea.

Amongst the rocks between the islands of *Nevis* and *St. Christopher's*, the greatest varieties are to be found, lodging themselves in little cavities of those rocks and of the larger sea plants of the stony kind. They are discovered by the branching forth of their *Tentacula*, which exhibit a beautiful display of various colours, resembling a fine double flower.

A gentleman who had a place in the custom house at the island of *Nevis*, and who had a
taste

taste for natural curiosities, exhibited an extraordinary collection of American *Aetiniæ*; and in this collection no less than five of this species, here termed a *new* animal, were to be seen: their appearance in the body, when drawn out, and their size, resembling a leech all but in colour. But after all, it might perhaps have been never seen before at Barbadoes, from whence this description was sent; and the revolution of the coast produced by the *Hurricane*, might have given a new creation to this species of *Aetinia*, never found by any one at *Barbadoes* till then. Why had it not occurred to the correspondent who sent the account to John Hunter,—that the *Hurricane*, which he has described to have been previous to this new birth in the creation, might have been the *groanings of nature* in the very act of parturition?

In the Linnæan system, this is of the *mollusca* order of worms, including five species. Of this, in the Philosophical Transactions, John Hunter has given two sorry plates, and out of proportion:—one describing the animal in its stony shell, with its double *Tentacula* spread out of it—and the other the body of the animal itself out of its stony shell.

March 1786.

A TREATISE ON THE VENEREAL DISEASE.

4to. 1l. 1s.

THIS work of the celebrated author has already engaged my attention, and undergone my criticism through three publications following each other, from June 1786 to their completion in May 1787—comprising in the whole 465 pages 8vo.

If in these I have not said enough upon the subject, and if what has been said by me be not well said, nothing which I now could advance will avail me any thing. But I have not the smallest ambition—for reprinting or circulating any opinions which I have laboured to inculcate, beyond that becoming point of a fair publication;—for fanning the breath of fame by sending forth old publications under new titles,—and for making up a new book on “animal œconomy” out of old papers in the Philosophical Transactions. Nor should I have noted this now of myself, or ventured to have spoken at all of myself; but by way of apology for a chasm here apparent,—a chasm of no less magnitude in the professional life of John Hunter, than will be naturally

rally seen from his great work on the *Venereal Disease* being thus omitted.

But there is an observation which readily offers to some:—that I, who attempted to think for myself in his life time, and who then examined some of his productions, am more justified on those I am now engaged in, than another would be, who waited when John Hunter could not defend himself. But even that argument, in my opinion, is only applicable to the weaker species of men; and they should be told—that it is his works which I am considering;—that these refer to life beyond the limit and the power of him who produced them;—that these are bequests from him of the art of surgery and of physiology to posterity—and are at any future time liable to be examined, although with less means for the obtainment of truth;—as the further any one is removed from an object, the more obscurely will its reality be discerned.

Independant of what the opinions of intelligent enquirers after knowledge have done for us both in this country, I shall, without a doubt upon the question, produce a translation of the *Review* which the University of *Göttingen* has given between us, on the subject of the Venereal Disease, with only remarking, that it has been
sent

sent to me since it was known I was thus engaged.

(*Translation.*) *Göttingen* 1787, vol. 3. p. 1922.

“ J. Hunter’s Dissertation on the Venereal Disease, from the English, with three plates, 1787. Pages 688, in great 8vo. exclusive the register.

“ There are not so many annotations and most necessary informations as in page 339 of the knowing and learned translator, as would have been necessary for making this book by a translation only, universally useful, which in his original contains so many singularities and numberless errors. It is to be wished that the translator had, as in page 140, used through the whole book the admonitions which *J. Foot* has made against it. In this case the translation would have had a very great preference of the original itself.”

Such was too proud a testimony when coming forth from that university, nursed into eminence by the illustrious de Haller—by him who first filled the chair of president, and which he vacated only with his breath—from that university where modern literature looks for meritorious patronage. Let not the admirers of John Hunter assume a sottish scorn and frown indignantly

on *Truth* because her smiles have been thus bestowed upon me :—she will but the more from that cause care for me. And if she come delegated to me by a *German* university before she has been sent to me by my countrymen, I am content :—I am but in that predicament of a greater man, and whom I only can imitate in my sincerity for the obtainment of knowledge.—Lord Bacon has said, “ that the present justification of my name I leave to foreign nations, and the future to my countrymen after some time has passed over.” I do assure John Hunter’s admirers that I never was but once in Hanover, and not then at Gottingen, and that I have no influence personally there ;—but if I ever visit Switzerland, my zeal would direct me to Berne, where de Haller lies entombed, and would lead me to follow the example of Tully, who sought through brambles the tomb of Archimedes.

March 22, 1787.

AN EXPERIMENT TO DETERMINE THE EFFECT OF EX-
TIRPATING ONE OVARIUM UPON THE NUMBER OF
YOUNG PRODUCED.

PHIL. TRANS.

THE practice of spaying or spading animals, is vulgarly known to the lowest order of men, and it is generally followed by the idlest man in every parish.

When this operation is performed effectually, the two *ovaria* which are appointed by nature to every animal, are taken out. The usual way is to make the incision in the middle of the flank, rather aslope, two inches and a half long, that the fore finger may reach the *ovaria*, which are two substances in a female dog about the size of an *acorn*, on both sides of the *uterus*; one of which is first drawn out of the wound and separated from its connection,—and the other then taken out also.

The purpose of John Hunter's experiment was to take away from a Sow *one ovarium*, and leave the other,—in order to ascertain how far this partial privation would influence the animal respecting the quantity of young which she might hereafter produce, in proportion to ano-

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ther Sow that had not been thus partially spayed;—both Sows being in every other sense as nearly similar as possible.

I shall not make any comment upon his introduction to this ceremonious piece of curiosity; as it would be endless to follow up with remarks the observations of him who thinks and writes at random,—who never seriously weighs what he has to offer, and never stands in awe of an absurdity. He has heard of there being such a thing in reality as a *third* in its nature dissimilar to the *two* which has produced it, and this he has applied to the young of a female. However this *Tertium* of our philosopher is generally of a similar nature to *one* or *other* of its parents.

The result of this experiment was, that the spayed sow ceased to breed two years before the other.—That the spayed sow produced in all her farrows 76 young in four years—and that the perfect sow produced 162 in six years.

John Hunter infers from this experiment—that the *ovaria* are from the beginning destined to produce a fixed number, beyond which they cannot go, although circumstances may tend to diminish that number. He has obviated every possible objection to this experiment by a note, where he says,—“It might be thought by some, that

that I should have repeated this experiment; but an annual expence of twenty pounds for ten years, and the necessary attention to make the experiment complete, will be a sufficient reason for my not having done it."

I do not know that such is the destination of the *ovaria*; for if it were, the general consequence would be, that with few exceptions, the number of young from every sow would be nearly equal:—but whether this be the fact or not—whether every sow nearly produces any equal quantity of young or not—this experiment wanders from ascertaining *that*, much more than common observation would, upon the quantity of young by each produced, when made by a sow-breeder. If he had neither time nor disposition for the expence attending the whole which was necessary for the perfecting of his experiment, that was his own concern: the public has nothing to do with it:—the public, if they wanted any part of this experiment, wanted what was satisfactory for ascertaining the presumed fact; but being disappointed, it was not for him, nor was it the part of a physiologist, to set the minds of men a gadding after a *Chimera*.

John Hunter should have been told,—that nature does nothing in vain, and that supererogation

gation is not to be found in her œconomy. Either the *ovaria* like the *testes* are amongst her securities for making procreation more certain, when one or other of them might be rendered from disease imperfect,—or both are necessary for the complete purposes of procreation. I am apt to conclude that the two *ovaria* are for the purposes of securing procreation, and not as he supposes for encreasing it. But whether the truth lies with his opinion or mine, nobody will think of searching for it; as whoever keeps breeding fows, or any other animals for breeding, is generally desirous that they should be prolific, and therefore any means which may be devised for restraining procreation *partially*, will be considered as an officious and thankless kind of information.

April, 1787.

OBSERVATIONS TENDING TO SHEW THAT THE WOLF,
JACKAL, AND DOG, ARE ALL OF THE SAME SPECIES.

PHIL. TRANS.

IT will be necessary first of all to state—that two different animals may breed, but that the animals produced by them, may be incapable of going on with further procreation. Thus from a mare and a jack ass, a mule is gotten, but the mule is not known to breed—or at least the fact is so rare, as to be disputed. The race therefore of mules would be extinct, if it depended upon any power in them for propagating a species.

But the facts adduced in this paper tend to prove,—that the wolf, jackal, and dog, not only copulate with each other and produce young,—but that their young can also copulate and produce young, to succeeding generations. It has been proved thus far in part, but not altogether.—It has been proved that a dog and a wolf will produce young; and which young will go on with propagation;—and it has been thus proved by a dog and a jackal, but not by a wolf and a jackal—therefore John Hunter's title is erroneous.

Lord

Lord Pembroke had a bitch-wolf half bred, from a dog lining a bitch-wolf—she produced puppies by being lined by a dog :—and a bitch from this litter produced four litters of puppies, by four distinct dogs. This bitch lived twelve years, was buried in his garden—and over the place of her grave was written the following inscription :

Here lies *Lupa*,
whose grand-mother was a wolf, whose
father and grand-father were dogs,
and whose mother was half wolf and half dog.
She died the 16th of October, M.DCC.LXXXII,
aged 12 years.

John Hunter procured a bitch jackal, half bred from a dog lining a bitch jackal, and this bitch was lined by a dog and had puppies. Such is the summary of this paper.

There is yet a palpable imperfection which ought to have been obviated. It does only appear, that the breed was carried on by dogs copulating with the half bred bitch wolf, and bitch jackal : whereas it was necessary for perfecting the system, that the litters from them should have interlined and produced their breed;—or
that

that males of their own litter, should have lined the bitch half bred wolf, and jackal.

John Hunter has said,—that the fox seems to be farther removed from the dog, than either the jackal or the wolf,—and that it is confidently *asserted only*, that the dog and the fox will breed, but that this has not been ascertained. Such is the refined taste of this physiologist,—that he doubts obvious facts, and establishes difficult ones. There is not a fact more generally received, more obviously to be ascertained by every enquirer into natural history,—than the known power of propagation between the dog and the fox,—and also, than the known power in their offspring for *continuing on* that propagation. To those who knew him as well as I did, the cause of this feigned scepticism will be easily traced. He wanted to pluck a feather from the cap of Buffon, who has expressed himself with delicacy upon this subject: but yet at length, from the fact of a dog and wolf having copulated and produced young, he is free to admit,—that they not only are of one *genus*, but nearly of the same *species*:—and this in my opinion is all that ought to be granted—even from the fresh facts which have been adduced in the paper which I am now considering.

John Hunter could not prove—that the dog and fox were not only of the same *genus*, but *species* also, by a spontaneous unrestrained copulation of two sorts of animals existing in common in the same country; and therefore he denied their alliance. But I will go farther,—I will defy any man to prove—that herds of wolves and jackals would forsake their *proper* kind, and *spontaneously* copulate with dogs. The question taken in a liberal sense is not—what is seen to be done by a compulsion in the calls of nature,—but what would be done, by the general free accordance of it;—and in this view, Buffon has neither declared too much nor too little; but has confined his observation strictly within the limits of propriety.

It appears from Buffon, that he did not succeed after many attempts, to get his bitch wolf to copulate with a dog—but that, although the experiment failed with his, it succeeded with another's.—It appears that the bitch wolf, which was the origin of the Produce belonging to Lord Pembroke, was solitary and confined by Brooks of the *New Road*, and therefore, when she was in *heat*, thus *submitted* to copulation with a dog.—And it appears, that the bitch jackal was on board of a ship, when she was lined by a dog, of which

which the whelp that John Hunter had, was the Produce.

Do these facts give nature the fair play to found new reasoning upon? And is the natural boundary of her laws, by which she is governed, to be thus sophistically misconstrued?

1787.

OBSERVATIONS ON THE STRUCTURE AND OECONOMY OF
WHALES.

PHIL. TRANS.

FROM the very nature of this subject,—from the magnitude of the animal, and the interest it bears to society by the commerce of oil, spermaceti, and whalebone,—from the tons of shipping and the body of sailors, that it keeps in employment,—and from the enquiries in consequence, which go forth, more especially amongst those who have the administration of public affairs, and commercial regulations—men of education, activity and business,—it is morally impossible, but that something should have been known about Whales before John Hunter presented this paper:—if all intercourse in natural history had been excluded from the inhabitants of Great Britain, of what was passing in other parts of Europe, the very spirit of trade in order to find out—how much more could have been made of a whale—would have commanded an attention to the subject—if any information had been thought defective, *even* by a *premium* for obtaining it.

I have perused this paper with more than common attention. I was induced to it, from
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the vast promise I had heard of it,—from what the newspapers of the different times had announced—as he never did any thing of this sort without having the kindness to acquaint the public—and from what his admirers had also said about it.

But—it is to be presumed at any rate,—that John Hunter could never have possibly given the Linnæan description of the *genera* and *species* of whales without having read; and having read upon the subject,—it was to have been presumed—that he would have had the goodness to have told us in this paper, who were the authors he had read, and from which of them he had taken his *classic Nomenclature*;—as in all probability where he found *that*, he found more. But it has not pleased him to quote a single name excepting that of Dale, the author of *Antiquities of Harwich*.

Such has been the constant uniform practice of this modern philosopher; and he would not have given the classic names, but for the pomp which accompanies his introduction; as after he has once copied them, he drops them. He leaves the *Phocæna*, for Porpoise, and the *Delphinus Delphis*, for Bottle nose. He never after is found to use any other than the vulgar terms, in which
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he was at home at his ease; and with these he proceeds in his enquiries.

A Whale in the Linnæan system of Zoology, is the seventh order of the class of *Mammalia*; the characters of which are,—that the animals of this order, have breathing apertures on the head,—pectoral fins,—the tail placed horizontally,—and no claws. The order includes four *genera*, viz. *Monodon* or sea Unicorn, *Balæna* or whale, *Physeter*; and *Delphinus*, comprehending the Dolphin, Porpoise and *Grampus*. From these four genera, the various species are classed by this accurate physiologist.

And although the enquiry cannot be pursued into the structure and œconomy of whales, which any other author has made, by referring to this paper of John Hunter,—although he has followed his natural propensity, by shutting out every piece of relative information upon a subject thus curious, valuable and important,—yet it will be found to have engaged the attention of naturalists, and anatomists from very remote times, down to the present. Thus far I can assert, that the *genera* are described by terms of Greek,—and that Aristotle and Pliny have both described the whale:—that the subject has been treated on in Pantopidan's Natural History of Norway,
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—in Crantz's History of Greenland,—in Pennant's Zoology,—and that there is not a single modern German anatomist, who has omitted to dissect and describe the *Balæna*.

John Hunter in this paper says, that he examined the *Delphinus Phocæna* or Porpoise, both male and female, several of them.—The *Grampus*, two of them.—The *Delphinus Delphis*, or Bottle nose, a young and an older one.—The *Balæna Rostrata* of Fabricius, one, which was seventeen feet long.—The *Balæna Mysticetus*, or large whalebone whale, the *Physeter Macrocephalus*, or spermaceti whale, and the *Monodon Monoceros*, or Norwhale, have also fallen under his inspection. Some of these were too long kept before he procured them, to admit of more than a very superficial inspection.

By this account it appears,—that he had examined several of the most trifling species, such as the Porpoise and Grampus,—that the largest was the *Balæna Rostrata* of Fabricius, only seventeen feet,—and that the most important and valuable species, viz. the *Balæna Mysticetus*, or large whalebone whale, the *Physeter Macrocephalus*, or spermaceti whale, and the *Monodon*, or Norwhale, had only “fallen under his inspection.” I have every reason to believe, that he never saw
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in his life time, a *Balæna Mysticetus* and a *Phyfeter* awhole, either dead or alive :—as these are so very important in point of size, and of whalebone, oil, and spermaceti;—as these are found so remote from England;—as these never have been, nor can be brought home awhole;—as the value of these, consists in what is taken from them;—the world would have wrung with his report of himself upon such an occasion,—if he had really dissected such whales,—instead of its being but barely told in the faintest tones of their “having fallen under his inspection.” It is for those reasons, I am almost persuaded that he neither had dissected nor seen them.

In this paper, He is particularly diffuse, and opens his subject upon general observations—that the anatomical appearances of various whales are not uniform,—that notwithstanding whales have urinary bladders, there is no apparent reason why they should have them. He goes into a chemical disquisition upon Spermaceti, a science with which he was totally unacquainted; and he has asserted, of which I trust merchants will reap the advantage, a novelty,—that Spermaceti is not confined to the head only of a whale, but is to be found every where. He discusses his subject under different heads; and in treating on the mouth, the following observation will shew what was

was the radical knowledge he possessed of the species of whales.

“ There is a very great variety in the formation of the mouths of this tribe of animals, which we have many opportunities of knowing, from the head being often brought home, when the other parts of the animal are rejected ; a circumstance which frequently leaves us *ignorant* of the particular species to which they belonged.”

Perhaps the history of criticism never furnished a stronger instance of its necessary utility, than is exemplified by the above paragraph : as every definition of the varieties in the species of whales, is principally if not wholly dependant on the forms of the head,—of the teeth—of their having some or none, few or many—of their tongues, their nostrils, their spermaceti, their whalebone—with a wonderful variety of other certain characteristics, which cannot escape a conversant physiologist.

He proceeds to describe the whale bone in the jaws, and proves it to be an animal substance not bony, but similar to hoofs, hair, nails, and feathers. He says that the mouth and *oesophagus* are wider than in other animals, but not in proportion to the size of the fish ;—that the stomachs in each are in number from five to seven ;—that

their food is fish ; that their intestines are uniform,—and that they have kidneys, ureters and bladder—that their kidneys are made up of substances put together like a pavement.

The blood of whales is in great profusion, the heart large, and the arteries resemble those of other animals. He describes the *larynx* and the lungs—that they consist of two oblong bodies, are very elastic, and have very small cells;—He also demonstrates a diaphragm.

In his description of what he calls the blow hole, or passage for the air, he says, none but the whale-bone whales have the organs of smelling : the Porpoise, Grampus, Bottle nose, and Spermaceti whales have but one orifice externally : whale-bone whales have a double. The *glottis* and *epiglottis* are united with the posterior nostril, so as to shut water out from the lungs. They have both *cerebrum* and *cerebellum*.

Whales, he says, possess the sense of touch, which is seated in their cuticle ; they have tongues which vary in the different species, and these are endowed with the sense of taste. He is extremely diffuse upon the organ of hearing, and has displayed a wonderful degree of accuracy upon that subject. This led me to suspect that he had some

some design by being thus particular, more than on any other property of the whale: I attended through the whole of his description, but could not find a single authority quoted by him: but on turning to the *syllabus* of Petrus Camper's works, I found—that the Professor,—John Hunter's formidable rival,—had treated on this very subject in the year 1765 and 1776.

Harlemi. Aët. Harlem. Tom. 11. part. 3. De Organo Auditus Ceti. page 193. anno 1765.

Ib. Tom. 17. part 1. De Sede Organi Auditus, ejusque præcipua Parte Ossea in Balænis Mysticetis, egi 1776.

Are not John Hunter's admirers obliged to me for demonstrating to them these facts;—and am not I more sincerely their friend, than he was, who would not impart to them, even in confidence, what I so readily do?

The future part of this paper is huddled up in a very summary manner; and where much was expected—where curiosity was most excited, the least information is obtained. This paper is of some length in the Philosophical Transactions—is swelled by his abstract reasonings,—and deficient in matter of fact. It is that very sort of

paper, that a man would write upon a subject which he does not understand. When Johnson had read Cibber's History of the Stage, he gave that performance the best of all characters by the following observation upon it—"See how easy it is for a man to write upon a subject which he well understands!!"

He has described the parts of generation—the testicles of the male situated within the *abdomen*: he has not mentioned any thing of their size. He has also described the *penis* under the same cloud of obscurity. Of the female parts of generation, he says, that the external opening is a longitudinal slit—that the rest consist of *vagina*, two horns of the *uterus*, fallopian tubes, *fimbriae* and *ovaria*,—and that these throughout are found to be uniform.

There are annexed to this performance no less than eight plates of whales—and parts of whales.

He says, he does not know, when they copulate, whether they do it in an erect posture, or otherwise; nor any thing of their time of gestation. The female has two nipples on the posterior part of the *abdomen*, and he thinks she brings forth but one young at her time of parturition.

OBSE R-

OBSERVATION.

The article of April 30, 1789—Phil. Trans. “*A Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackal,*” being annexed to that of April 26, 1787, being printed with it in his Animal œconomy, and making there, but a continuation of the same paper,—has been already reviewed with that paper. *The six Kroynian lectures* on Muscular Motion, from 1776 to 1782, are not published in the Philosophical Transactions, but they have been printed and partially circulated among the Fellows,—and I have perused them. That which does not face the public eye, I have no desire publicly to investigate,—and therefore I decline it, from that cause alone.

1792.

OBSERVATIONS ON BEES.

PHIL. TRANS. PART I. P. 128.

I SHALL first of all, give a short abstract of the contents of this paper, and then my comment shall follow. John Hunter opens his subject with the history of the bee, saying that the bee has been rather considered as a fit subject for the curious at large; whence more has been conceived than observed:—that Swammerdam indeed has erred on the other side, having with *great industry* been *very minute* in the particular structure of the bee; and again he says, that Swammerdam often attempted *too much accuracy* in his description of minute things. He finds bees to be the inhabitants of Europe, Asia, Africa, and America; and he proceeds to describe the general properties of the female or working bees, telling us that accidents happen to bees, and that his bees have been subject to them. He ascertains the heat of bees, and the heat which eggs require. He has found out that bees never evacuate their excrement in the hive: but he has not told us, what they do in the winter, when during several months, they never quit the hive. He talks about their society, and says, when the queen is lost, their attachment

ment is broke ; they give up industry ; probably die ; or we may suppose join some other hive.

The swarm he makes to consist of three classes—a female or females, males, and those commonly called, mules, which he says are supposed to be of no sex, and are labourers. A hive sends off two or three swarms in the summer : the swarm sometimes goes back, he believes from the loss of their queen : he killed several of those that came away, and found their crops full, while those that remained in the hive had theirs not so full. Having thus as he styles it, set the bees in motion, he proceeds to the subject of

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This is the material of their dwelling or comb : and he fully affirms—*that it is their wax* : and his next consideration is the mode of forming, preparing, or disposing of it, and in giving—a *totally new account of the wax*.

I shall first shew, says he, it can hardly be, what it has been supposed to be. I have observed, that when the weather has either been so cold, or so wet, as in June, as to hinder a young swarm from going abroad, they have yet
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in that time formed as much new comb, as they did in the same time, when the weather was such as allowed them to go abroad. *The wax is formed by the bees themselves. It may be called an external secretion of oil, and I have found, says he, that it is formed between each scale of the under side of the belly.* In the bottom of the hive he shews us a good many of the scales lying loose, and some pretty perfect, others in pieces. I have endeavoured, says he, to catch them either taking this matter out of themselves from between the scales of the abdomen, or from one another, but *never could satisfy myself.* It is, he also adds, with *these scales that they form the cells called the comb*; but perhaps not entirely, for I believe they *mix farina with it*; however this only occasionally, when probably the secretion is not in great plenty. The bees, he says, who gather the *farina*, also form the wax, for I found it between their scales.

After explaining this discovery, he proceeds next to describe the comb and its cells,—then the laying of the eggs—from the eggs to the maggots with their food—and from the maggots to the chrysales, with their coats—making the egg in hatching five days, the age of the maggot four, and the chrysalis thirteen: but he adds how far it is accurate, he cannot say.

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He treats of the seasons, when the different operations of the bees take place. He says—in the month of August we may suppose the queen impregnated by the males; and as the males do not provide for themselves, they become burdensome to the workers, and are therefore teased to death much sooner than they would otherwise die. And when the bees set about this business of providing the winter store, every operation is over, except the collecting of honey and bee bread. The whole of the males are now destroyed; and indeed it would have been useless to have saved any to impregnate *the queen in the spring*. In the winter months they live on the produce of the summer, and get as close together as the comb will let them. In this manner they appear to live through the winter. He states the consumption of something within the hive during the winter, by the difference of its weight being seventy two ounces and some drams less,—losing from month to month more or less of that balance in the end.

He next proceeds to the process of the *Queen Bee* for incubation, in consequence of her having been *impregnated by the males in August*, that is, *six months to March following*, when he says the eggs in the oviducts are beginning to swell:—and he farther says,—I believe in the month

of March she is ready to lay them, for the young bees are to swarm in June, which constitutes the queen bee to be the earliest breeder of any insect we know. He says,—he found, in April, young bees in all stages.

He proceeds upon the queen bee, and tells us—that she has excited more curiosity than all the others, although much more belongs to the labourers. After having consumed six pages in criticisms upon the late discoveries of Schirach, and the opinion of Wilhelmi, which will be explained by me in proper time, he says,—that the queen—in whatever way produced—is a true female, and different both from the labourers and the male. He describes her, and adds, it is most probable that the queen which goes off with the swarm is the young one; for the males go off with the swarm to impregnate her, as *she must be impregnated the same year because she breeds the same year*. The queen, he adds, has a sting similar to the working bee,

He believes a hive or swarm has but one queen, at least he never found more than one in a swarm. Supernumerary queens are mentioned, he says, by Riem, who asserts he has seen them killed by the labourers, as well as the males. Riem, he says, also asserts *he has seen the copulation*

tion between the male and the female, but does not say at what season. He doubts this.

He proceeds next to say,—that Schirach supposes the queen impregnated without copulation. He knows not whether Schirach means by this that she is not impregnated at all, and supposes, like Debraw, that the eggs are impregnated after they are laid, by a set of small drones, who pass over the cells and thrust their tails down into the cells, so as to besmear the egg. He then adds a note to this, saying that Debraw, knowing the drones *died in the latter end of summer or the autumn*, was obliged to suppose a small set of males that lived through the winter for the purpose.

He then says—that, the circumstances relative to the impregnating the queen not being known, great room has been given for conjecture, which if authors had presented as conjectures only, it would have shewn a candour; but they have given what in them were probably conceits, as facts.

He then describes the male bee, which is known by the name of drone. He tells us the males are hatched the latest, but also that the maggots are too young for the *investigation* of them,—they being all very much of the same

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size. In the month of August, *probably* about the latter end or beginning of September, they are dying, but *seem* to be hastened to their end by the labourers.

He proceeds to describe the Labouring Bee; of which he says,—this class, for we cannot call it either sex or species, is the largest number of the whole community. There are thousands of them to one queen, and probably some hundreds to one male; as, says he, we shall see by and by. It is supposed they are the only bees which construct the whole hive, and that the queen has no other business but to lay eggs. They are the only bees that bring in materials; the only ones we observe being abroad; and indeed the idea of any other is ridiculous, when we consider the disproportion in numbers, as well as the employment of the others, while the working bee has nothing to take off its attention to the business of the family. They are smaller than either the queen or the males: not all of an equal size, although the difference is not very great. The queen and the working bees are much alike: they are all females in construction, having the female parts, which are extremely small, and would be easily overlooked by a person not very well acquainted with the parts in the queen. This has been observed by Riem.

Indeed

Indeed one might suppose that they were only young queens, and that they became queens after a certain age; but this is not the case. They have stings, which is another thing that makes them similar to the queen. He gives an account of the stings—and concludes this article with saying that nine thousand bees will thereabouts fill two quarts.

He then proceeds to the parts concerned in the nourishment of the bees. He describes the tongue *very fully*; and next the *œsophagus*, at the end of which there is a fine transparent bag, which is the immediate receiver of whatever is swallowed. In this bag the bees deposit their honey, part of which is regurgitated, and the rest goes into the stomach for digestion and nutrition: whatever remains to be regurgitated is never found to be altered;—it is pure honey. He proceeds to describe the stomach; and when he has finished this subject, he begins with the senses of the bees,—then their voice,—then their female parts, and the oviducts of all the common female bees:—and this, with every other anatomical part, is done in a Swammerdam-like manner. He proceeds in the same Swammerdam-like manner to describe the male parts, as if he had really the power—had bestowed the same attention—and had as much time for it—as Swammerdam had.

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He then recurs back to a former ground, and with a misgiving scepticism debates his *first asserted belief of the death of all the males in August*, especially as those bees he had termed in his first outset to be *mules*, have been so lately described by him, or rather Swammerdam, as all to possess female parts of generation. He speaks again of *the queen breeding in April by the impregnation of the males who died in the preceding August*, and says,—what is very true,—that this must *puzzle* any one not acquainted,—(or acquainted I say,)—with the mode of impregnation of the females of most insects. He flies to his usual resource of proving his facts relative to one insect, by experiments made not on that species, but on another, as if he doubted his own hypothesis. He makes those experiments on silk worms, and endeavours to make the case of the bee bend to the case of the silk worm: and as he flies to experiment for this,—it must be presupposed,—that before he began those experiments, he knew *less of the silk worm* than he did of the bee—as what he has said of the bee, he was told by former authors over and over again:—and as the facts depended upon experiment, it is to be presumed—that he did not know what he was to learn from the silk worm—until his experiment was gone through.

After

After having exemplified to his own satisfaction, that he had ascertained with precision, the truth of the *autumnal impregnation* for the *spring propagation*, not by an experiment made on bees but on silk worms,—he proceeds to describe the sting of bees anatomically, after the manner of Swammerdam also; and from that, he sports his opinion on the life of bees. He has observed—that the life of the male, is only one summer, or rather a month or two; and this we know, says he, from there being none in the winter; otherwise their age could not be ascertained, as it is impossible to learn the age of either the queen, or the labourers. Some suppose, he says, that it is the young bees which swarm; and most probably it is so; but, he adds, I think it is probable also,—that a certain number of young ones may be detained to keep up the stock. There must, he adds, be a period for a bee to live, and if I were to judge from analogy, I should say—that a bee's natural life is limited to a certain number of years. Pray can his admirers tell us what is the animal whose period is not limited? One bee, he says, does not live one year, another two, and another three:—but has not John Hunter already said, what nobody disputes,—that the drones live but one summer, nay not more than two or three months?

After

After this—he proceeds to be more extravagantly opiniative, than in what I have thus lately detailed; and at length reverts to the comb of the hive, which he figuratively calls the bee's *furniture*, and which is wearing out and in time unfit for use. He observes that the bees did not clean out the excrement of the maggots which croud the cells and fill them up, making them clumsy in comparison with original ones.

This subject is not similar, in its nature, to many of those chosen by John Hunter; it is of universal notoriety, and perhaps has excited the enquiries of naturalists beyond the reach of our historical knowledge. Among the antients who have treated on it, are Aristomachus, Aristotle, and Pliny; and among the moderns who have most claimed attention, are Swammerdam, Meraldi, Reaumur, Thorley, White, Wildman, Riem, Schirach, Wilhelmi, Bonnet, and De-brow. Every article which refers to bees, has been fully discussed by one or other of those authors.

For

For ascertaining their minutest anatomy, and other investigations into bees, Meraldi, Reaumur, and Swammerdam take the lead; for the improvements of the hives, and statement of their swarms, Thorley and White are to be consulted; and for late discoveries in the propagation of bees, Schirach, Wilhelmi, Bonnet, and Debraw, are to be preferred. Whoever will be at the pains to refer to those authors, particularly Reaumur's History of Insects, besides the common place books, such as the Academy of Sciences, Philosophical Transactions, and Universal Dictionaries, will find,—that the whole account of bees, as given by John Hunter, is collated and made up, from the information of others,—from what has been already registered. If this could not have been proved, reason would assure us of the fact; as whoever is desirous of obtaining the truth, relative to bees, and of improving upon the observations of others, must employ, as most of those authors I have alluded to, have declared they did,—a patient attention to the subject for a series of years. Whether such an exercise of his time, could have been actually prosecuted by John Hunter, I leave those to say, who knew how all his hours were devoted. I do not doubt but he might have had hives at *Earl's Court*, and that he sometimes slept there. This paper was published in the year 1792, and

I am certain, that most of its contents were made up very lately, as he could not have known—what Riem and Schirach's books had said upon the subject, before they were translated;—and I know *the German*, who translated the papers for him, and the time when he did it. Besides Debrow, whom he quotes, obtained the reading of his paper from the Royal Society, in November 1776: from this I infer,—that if John Hunter studied the subject at all, he did it at a time of his life, when he had the least leisure for it.

He himself has borne testimony to the minute anatomical description on bees, by Swammerdam: I therefore shall confine my observations to two points. First,—to what he presumes to be his discovery of the formation of wax; and second,—to the discoveries made by Schirach and Debrow, which he has severely reproached;—discoveries of the utmost importance,—and which, for the better ascertaining the œconomy of bees, and the colonizing of them by art, must ultimately prove of the highest value.

I. Of the wax, he has said,—that it is a secretion of the bees,—that it is their wax,—and that his is—*a totally new account of the wax*;—and that it is *formed by the bees themselves*. It may be called

called an external secretion of oil ; and I have found, says he,—that it is *formed between each scale of the under side of the belly*. But he adds,—that he has never caught them at taking the matter out of these scales. He makes an inference, which is opposed by the observation of other authors,—that they can, being provided with this wax by nature, work up their combs as well in wet weather, when they do not go abroad, as in dry. The best authors have said otherwise: they have told us,—that the bees in about ten days, if the weather be fair, are first employed in forming the comb and completing it;—and wax has been seen by Thorley especially, within those rings of the belly on bees, which have returned home laden.

To suppose—that a young swarm of bees, who have the whole of their comb first of all to provide, can secrete in ten days or a fortnight, when they are by bad weather confined to their hive, more wax in weight—than the whole swarm will weigh,—is, in my opinion,—to suppose an impossibility. And moreover, if the bees were thus confined at home by weather, how could a young swarm be furnished with *farina*, to work it up with their secretion? As after he has so roundly asserted,—that this secretion, is the wax,—he softens down that by adding,—that *farina*

mixed with this secretion, forms the wax. To suppose that,—if his theory were true, wax would not be always found in the act of secretion, considering the quantity which must be secreted,—is also to suppose an impossibility. And to suppose,—that he, who has made this discovery, could not see the bees applying it, in the way he would wish to persuade us—that it is applied,—is also to suppose another impossibility. But this is not all: I shall contravert John Hunter's being the discoverer of the idea,—prove that Riem has previously sported it, and what is fortunate for me,—prove also that he had seen it. Riem's idea is quoted in Schirach's book on bees,—and neither John Hunter nor his translator could have overlooked it, as it directly follows a Paragraph quoted by John Hunter. In page 240 of Schirach's French edition, and in a chapter under the title of

RESULTATS DES OBSERVATIONS 1764,

Article 3me. *Riem a observé l'accouplement
—de la Reine, avec les Feaux-Bourçons,*

John Hunter has literally had this translated for him,—as may be seen in my abstract; and the subsequent article is that—which I have alluded to,—and shall here give the copy and translation.—

Art. 4me.

Art. 4me. *Le Naturaliste de Lauter affirme, qu'il a vu sortir d'entre les anneaux des ouvrières de la matière à cere : que cette matière sembloit transuder de l'intérieur, et que c'est avec cette cere transpirée, qu'elles forment les commencements des cellules.*

The naturalist of Lauter affirms,—that he has seen flowing out from between the rings of the working bees, the matter for wax:—that the matter seemed to transude from within,—and it is with this wax thus transuded—that the bees form the commencement of their cells.

So far from this being a new discovery, it is mentioned also by Pliny; and it having been so long exposed to common observation, and by time gaining no credit, is a strong reason for supposing it not to be true. It is very clear, that John Hunter was not the author of the discovery—and as clear—that he assumed to be the author of it.

II. I shall now revert to the second point,—to the discoveries made by Schirach and Debraw, and which John Hunter has, without argument or proof, with so much asperity, and indignity scouted. The outlines of the opinions adopted by former naturalists, were exactly like John Hunter's. They asserted—that the queen bee is
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the only female in the hive, and the mother of the next generation;—that the drones are the males by which she is fecundated;—and that the working bees, or bees that collect wax in the flowers,—that knead it, and form from it the combs and the cells which they afterwards fill with honey—are of neither sex,—and which John Hunter calls mules.

But of late, Schirach has given a very different view of the classes which constitute the Republic of bees. He affirms, that all the common bees are females in disguise;—that every one of those bees, in the earliest period of its existence, is capable of becoming a queen bee;—that the queen bee lays only two kinds of eggs, those that are to produce drones, and those from which the working bees are to proceed. Debraw has said,—that the trials made by Schirach, seem to evince the truth, and adds—that he himself by trials also, is able to pronounce on their *reality*.

Debraw's subsequent discovery has most undoubtedly befriended Schirach's. As Debraw has shewn, how that which Schirach could not have accounted for, came to pass. Debraw is to be commended for his candour, in allowing to Meraldi and Reaumur, the originality of the observa-

observation, which he so usefully has applied; and although this had escaped Schirach, yet it could not annihilate the common occurrence observed by him, as nature will proceed uniformly in her operations, whether we can account for them or not—but we must first observe them, before we can think of accounting for them.

Debraw's discovery is founded, upon what has been said before, by Meraldi and Reaumur:—the former said, we have found a great quantity of drones much smaller than those we had formerly observed, and which do not exceed in size the common bees, &c. &c.* And Reaumur has said, we likewise have found drones that were no bigger than the common bees.† *These* have been proved by Debraw to remain *during the winter* in the hive, and by *these* the eggs of the queen are *fecundated*,—instead of the queen being impregnated by the large drones,—as John Hunter has said,—even after he was informed of this plain fact,—*before* the winter in the month of August,—and *six months* at least before the subsequent spring,—which is the time that the queen is to lay her eggs thus impregnated.

* Royal Academy of Sciences, 1712, p. 333.

† Natural History of Insects, p. 591.

But

But what renders John Hunter's theory an absurdity is,—that the old queen bee should go thus impregnated *six months* during the winter, —whereas after the winter, neither the old queen, nor the young one, swarmed in the spring, go more than a *month*.

I must beg permission to explain the trials of Debraw.

In order to ascertain the fact, that the eggs are fecundated by the males,—he took a swarm of bees, and having separated the drones, by shaking all the bees into a tub of water, and leaving them in it, till they were quite senseless, replaced the working bees and their queen, as soon as they were recovered, by spreading them on a brown paper in the sun, in a glass hive. The queen laid eggs, some of which, at the end of twenty days were hatched into bees, others withered away, and several of them were covered with honey. Suspecting that some of the males, having escaped his notice, had impregnated only part of the eggs, he was anxious to ascertain the fact; and with this view, he removed all the brood comb that was in the hive, and determined to watch the motions of the bees, after new eggs were deposited in their cells.

On

On the second day, he perceived the operation related in a former case by him, and to which I refer my readers;* and on taking out a piece of the comb containing two of the bees, which had thrust the posterior part of their bodies, into the cell, he examined them, and found, that they had no sting; and upon dissection, he discovered in them, by the help of a microscope, the four cylindrical bodies, containing a whitish liquor, which Meraldi had observed in the large drones.

In a subsequent trial, Debraw separated from the same parcel of bees, all that had no stings, and he found no less than fifty-seven of the number exactly the size of common bees, which on being pressed between the fingers, yielded a small quantity of whitish liquor. Having killed all these, the remainder of the swarm was restored to the hive. On the fourth or fifth day, the queen bee deposited her eggs in the cells, but no part of the process of impregnation could be discovered; the eggs, after the fourth day, instead of changing in the manner of caterpillars, remained in the same state they were in, the first day, except that some of them were covered with honey: all the bees left their hive, and attempted to get into a neighbouring hive, proba-

* Vide Debraw's paper.

bly in search of males; but the queen was found dead, having been killed in the engagement.

Debraw made another trial, which must be considered as absolutely decisive on this subject. He took a part of the unimpregnated brood comb, and placed it under a glass-bell, in which he confined a queen, and some common bees without any *drones*; the other part of the same brood comb, he put under another glass-bell with a few drones, a queen, and a number of common bees. In the former glass, the eggs remained in the same state; there was no impregnation; and when the bees were released in the seventh day, they all flew away. The drones, in the other glass, were observed to impregnate the eggs in every cell on the day after they were put in; the bees remained in the hive, and in the course of twenty days, every egg underwent the necessary transformations, and a numerous young colony was thus produced.

It is with pleasure I can say,—that these discoveries by Schirach and Debraw have made their way all over the *Continent*. John Hunter was never seen to worse advantage, than through this paper. It consists of forty pages in *Quarto*; and being upon a subject more exposed to criticism than most of his others, his errors and crooked

crooked intentions are seen more glaringly by the world at large. He appears throughout the whole of the paper to be flat, wavering and equivocal;—constantly floundering like one who has found himself beyond his depth,—like a *fish* out of his Element—or rather a *man* not in his.—

THE FOLLOWING PAPERS ARE TO BE FOUND IN JOHN
HUNTER'S BOOK "ON ANIMAL ŒCONOMY."

I. OBSERVATIONS ON THE GLANDS SITUATED BETWEEN
THE RECTUM AND BLADDER CALLED, VESICULÆ SEMI-
NALES.

JOHN Hunter has said, that the *vesiculæ seminales* have been considered as *reservoirs* of the semen, secreted by the testicles,—in the same manner as the gall-bladder is supposed to be a reservoir of the bile;—but his analogy is not a just one. And although the *vesiculæ seminales* have been supposed to be for the reception of semen—previous to ejaculation by former authors,—yet those who have said so, have not justified themselves, by having resource, to such an incomplete analogy.

For my own part I have my doubts upon the question,—whether the *vesiculæ seminales* be for the purpose of a receptacle for the semen, or of secreting glands. But of this I am confident,—that the change he has made in their destination does not improve the truth,—does not clear up the fact—nor convince the understanding. For he has said,—that the bags called *vesiculæ seminales* are

are not the feminal *reservoirs*, but glands secreting a peculiar mucus, and that the *bulb* of the *urethra* is, properly speaking, the receptacle in which the semen is accumulated previous to ejection. This is much more improbable than the former, and I cannot bring myself to give the smallest credit to it.

It may be presumed—whenever a man has been worked up to the almost immediate and direct act of emission, and has from any cause baulked the intention, so as not then to emit,—that the semen has been circulated through the *vasa deferentia* from the *testes*, and found a receptacle *somewhere*; and as we know, that *sometimes*, such is the state of the case, it is most probable—that the receptacle is in the *vesiculæ seminales*.

His plates annexed to this subject are very bad and unnatural.

2. ON THE STRUCTURE OF THE PLACENTA.

THIS offspring has been claimed by two fathers,—by both William and John Hunter. It has been published by William in the year 1764, and is to be found with the former disputes annexed to it, in his *Commentaries*. But in consequence of their having disagreed, the right in it was disputed, and contested *even* by the brothers,—who had joined in contesting it with others,—about the year 1779. John sent in his claim to the Royal Society; but as the fact had been given before, to the public, which the paper contains,—it was refused a place in the *Philosophical Transactions*.

It consists of an investigation into the anatomical connection between the mother and *fœtus in utero*. As this subject has been so repeatedly bandied about, and so long demonstrated by anatomists, in their successive courses of lectures, for a series of years, not less than forty,—from 1754 to the present time,—I shall decline dwelling upon it.

The plate annexed to it, is a miserable one:—It gives just as good an idea of the country in the *Moon*, as it does of that which it is intended to explain:—it will serve for either.

3. ON A SECRETION IN THE CROP OF BREEDING PIGEONS FOR THE NOURISHMENT OF THEIR YOUNG.

JOHN Hunter has not told us, why this paper did not go into the Royal Society: I am sure that the subject is more worthy, than that of the *new marine animal*, or the *curious pheasant*. But he has told us in this paper,—that during incubation, the *coats* in the *crop* of the pidgeon are gradually enlarged and thickened, like the *udders* in female animals;—that the whole, except what lies on the *trachea*, becomes thicker, and takes on a glandular appearance, having its internal surface very irregular. It is likewise more vascular than in its former state,—that it may convey a quantity of blood, sufficient for the secretion of a curdy substance, which is to nourish the *brood* for some days, after they are hatched.

John Hunter was right to except the encrease of size in the crop about the *trachea*, for fear his theory should *choke* the pidgeon; and he judged also wisely in giving an encreased substance an encrease of blood vessels, for the purpose of an encreased secretion. What nature in her ignorance does not do, his wisdom supplies.

—She

—She can admire in him that which she cannot accomplish—but he does.

I do not believe this theory in the utmost extent to which he has carried it, and for the following reasons. First, that the cock pidgeon feeds the hen, and the hen feeds the cock: and second, that sometimes the cock not only incubates the eggs, but feeds the two young brood. Although John Hunter has gone very close in this paper to say,—that the male and female both in their turns incubate the eggs and feed the young,—although he has not either positively denied or asserted these facts,—yet he has not ventured to assert—that the crop of the cock pidgeon undergoes the same anatomical change during incubation as he has said—that the hen's does:—whereas for the confirmation of his theory, as cock and hen both alternately perform these same offices, *both* should enjoy the same natural endowments. The two plates, which he has annexed to this paper, might be fairly deemed to be a design in the artist, for caricaturing his theory.

4. ON THE COLOUR OF THE PIGMENTUM OF THE EYE
IN DIFFERENT ANIMALS AND THE USE OF THE OBLIQUE
MUSCLES.

THIS subject is purely theoretical, and particularly adapted to the mind of its author,—furnishing him with an opportunity, without comparative contradiction from others, of displaying the full energy of his genius,—and containing the best specimen of his perspicuity, with which he discusses different objects, and by that illustrates their truth.—As purely a piece of theory, without any practical purpose whatever, I will not attempt to search after a cause for criticism: but I will politely and patiently wait until hereafter,—when the primeſt among all his admirers, shall be pleased to fix upon any points in this desultory effusion, where the truth of the theory, and the practical advantages resulting from it, is to be found—challenging our admiration,

John Hunter has so rapidly, in this paper, shifted his arguments, if they may be so called,—has gone into the theory of vision, so unintelligibly to common comprehension,—has so turned aside the purposes of investigation, by never leaving one single point well explained—and

has so constantly brought forward successions of ineffectual conjectures,—that the reader is left in one continued state of disappointment from the beginning to the end. His ideas are no sooner hoped to be found out, than they vanish away in tangents,—retiring to their several and sacred recesses of the brain which gave them their creation. He marks out no direct path, but imitates a swallow in the air, when he is seen in the pursuit of insects.

5. A DESCRIPTION OF THE NERVES WHICH SUPPLY THE
ORGAN OF SMELLING.

BEING ignorant of the time when he published this paper—I cannot so well pursue his motive for doing it, as I could certainly have done, if I had known its date. The strong presumption is,—that he published it after *Scarpa*, the professor of anatomy in *Pavia*, had published on the same subject, and which was in the year 1782:—and by John Hunter's recurring to his usual resource of *notes* made so far back as the year 1754, I am apt to think,—that my opinion is right,—and that this paper was not published until since the year 1782.

After having gone into his accustomed proofs of originality, and entertained himself in the most liberal manner, with the *notes* which were made, and the demonstrations which were shewn, in the many courses of successive lectures given by the brother William, from the year 1754,—and after having amply gratified his natural desire for proving an insufficiency in the knowledge of Willis, Winslow, and de Haller, upon this subject, he proceeds to his demonstrations from the result of dissections.

I can only say of this paper,—that he has not satisfactorily determined the question relative to the distribution of the *first* pair of nerves, nor the purpose of that branch of the *fifth* pair which he has alluded to: if this assertion be doubted,—let any candid person refer to his paper, which must speak in this case for itself.

There are two plates annexed to this paper, most elegantly executed: but I do not pledge my opinion,—that they convey a perfect representation of the thing in nature, for which he has thus expensively displayed them.

•OBSERVATIONS ON THE INFLAMMATION OF THE INTERNAL COATS OF VEINS, READ IN FEB. 1784, AND PUBLISHED IN A VOLUME ENTITLED TRANSACTIONS FOR THE IMPROVEMENT OF MEDICAL AND CHIRURGICAL KNOWLEDGE, 1793.

THE purpose of this paper is to announce, that a vein in the arm from the operation of bleeding, sometimes becomes inflamed; and that tis possible, the inflammation may proceed along the internal coat of the vein through its whole direction, extend to the heart, and thereby kill the patient. The indicative cure recommended for this alarming case, is suggested in consequence of its having been *once* practised by John Hunter, and, as he supposes, with success. "When inflammation takes place beyond the orifice, so as to alarm the surgeon, he should immediately make a comprefs upon the vein at the *inflamed* part to make the two sides *adhere* together, &c."

He says nothing about the common means for reducing inflammation.

This paper is of no other importance, than that it relatively is capable of being, from example, fraught with mischief. If it had, in a general way, described the nature of inflammations,

tions, which sometimes follow in consequence of opening a vein in the arm by a lancet, and pointed out—how far the treatment already established upon the soundest principles of surgery, for the cure of these common cases, was injurious to this uncommon case;—if it had nicely and critically marked out a distinction, between the mode of treatment universally adopted in common cases,—and if it had drawn a necessary line by which inflammation on one anatomical part, differed from that on another, and for which, a different practice was necessary in the treatment of each,—I should have then been enabled with more facility to distinguish, what at present I find myself under some difficulty in accomplishing to my satisfaction, namely—that essential difference which this piece of novelty aims at.

I can, in perusing this paper, find no instructions authorizing the surgeon who reads it, with any intent to obey it, how to discriminate betwixt an inflammation in the vein, and an inflammation on the other parts of the arm. It must necessarily follow therefore,—when this paper is perused by a practitioner who is not already established in his own opinion, but is guided in his practice—from what is told to him by others,—that if he believes what John Hunter

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ter in this paper would feign to inculcate,—he will be implicitly and indiscriminately applying a tight bandage to compress the inflamed vein, whenever an inflammation follow the operation of bleeding by a lancet,

If this paper had announced—that the inflammation in the vein from bleeding, required a different treatment from other inflammations, which arise from bleeding,—and had also explained to us—that the treatment to be adopted in this case, was not intended to interfere with the more general mode of treatment, as adopted in all other instances of inflammation,—such as relaxing the arm, bleeding, fomenting, poulticing, and exhibiting medicines calculated for the abatement of fever, consequent to local inflammation,—I should then have been enabled to have met the question in its purest abstracted sense,—whereas I have now to combat it, in one of a more compound and intricate nature.

I have to appeal to the most established and ablest surgeons,—whether John Hunter has not, by thus directing a compress to be laid on the arm, for the avowed purpose of stopping an inflammation in the vein, held out to practitioners in general, a system tending to produce the most alarming consequences;—or whether he
has,

has, throughout this paper, explained any difference, so that it can be decided with safety and certainty, when this compress on the vein is proper, and when it is not:—Innovation should be clear, and when it is not, it is dangerous in surgery.

The worst cases of inflamed arms are found to arise from a long neglect of the inflammation; and such have been adduced in his late publication by Abernethy of St. Bartholomew's,—a young surgeon whose genius, though yet in its blossom, promises hereafter the choicest fruits of science cultivated on a mind, richly endowed by nature. The treatment of those cases, some of which were bad—as adopted by Pott, and—as recited by Abernethy,* was exactly that sort of treatment, which will be ever ample, for the cure of every aggravated state of inflammation of the arm, independant of that brought on from virus, whenever assistance be had in time: and when such a mode of treatment does not succeed, it is not because it is not the best,—but because the case had been too long neglected: the danger, there, arises from procrastination, and not from want of remedy already established.

* Vide Surgical and Physiological Essays.

Having thus far explained myself,—I shall revert to this anatomical and newly discovered disease, examine into its possible and probable consequences, and enquire—whether it possesses in its true nature, such fatal distinguishing marks,—as to warrant an innovation in practice—and as to demand a mode of treatment, which, in my opinion, would be productive of a general mischief, more fatal, than if the distinction had never been made, or, the innovation suggested.

In the cases of inflammation from bleeding, described by Abernethy, or rather sketched out by him, for he has not gone into the particulars of their treatment, they *all did well* without a compress; and it does not appear that he did any thing for them, out of the common way in surgery. But notwithstanding he has found,—that he could have dispensed with this novelty, yet he has said,—that “the application of compress, at some distance from the punctured part, in order to unite the inflamed sides, appears to be perfectly judicious.” He has also said,—that “the inflammation of the venal tube is extensive; and it is indeed very probable, that much sympathetic fever will ensue; not merely from the excitement which inflammation usually produces, but also, because irritation will be continued along the membranous lining of the vein

to the heart." And to do him justice, he has said—what John Hunter has not—"that the nature of the disease being known, the treatment is commonly evident. The diminution of inflammation in a vein, is to be attempted by the same general means, as in other parts." I am afraid,—that he has described such an inflammation, as will not admit with safety any compression, or as cannot be borne with any possibility of good effect. It is apparent,—that the motive in John Hunter and in Abernethy is, to compress the inflamed vein—that the inflammation of the internal surface of it, may not extend to the heart, and by that kill the patient;—but can it with propriety or safety be done?

When the vein is thus inflamed, it appears from the cases recited,—that the vein is not singly inflamed, but other parts in vicinity to it are so also;—and that the whole of the symptoms are worse than from any other part, being the original seat of inflammation: this is attested by my quotation from Abernethy, and also, —that the irritation produces fever and tumour of the arm, in an exacerbated degree, to inflammations not peculiar to veins. I think, it was very fortunate for those cases described by Abernethy,—that they got well, *before* the idea of a compress had been imparted —and

—and that they only left behind them, the slight imperfection of a vein collapsed, by what he terms, adhesive inflammation, which is a *Barbarism*; for it is, adhesion in consequence of a degree of inflammation.

Hence it appears,—that the inflammation is in the same degree, if not in a worse, on arms, where veins are said to be affected in particular, than on arms, where veins only share the common inflammation. For as the cause originates in vein, it cannot at any time and in any case be said to be exempted. And it appears also,—from the whole which I can collect from the descriptions of cases by both these authors—that they are more calculated to display anatomical distinctions, and the variety of inflammations arising out of them, than to establish, upon a more improved foundation, the practice of surgery,—although the latter seems to be the avowed intention of both for writing their Essays, or if it be not, there can be no other motive so good. Therefore I say, from what I can collect out of these descriptions, without assenting to or denying,—that an inflammation within the vein as is described by them, does really or not exist,—I am confident in the opinion,—that if such a case be attended to in time, it will always do as well, as a case of a more common de-

scription,—and that, *ceteris paribus*, there is no more danger to be dreaded from *this*, than from any other.

I shall now examine, when this compress is to be put on the vein; and as John Hunter has not directly told us, when, the season must necessarily be collected from inference. It surely cannot be put on, before inflammation has appeared, as then, every one who is bled should have one;—and to put it on, after inflammation has subsided, would be an act of supererogation. Of course the time then for putting it on is—when there is danger of the inflammation spreading,—when the arm is swollen,—when the fever is raging,—and during the acts of relaxing the limb by fomentation, poultice, bleeding, medicine, and indulging to its utmost extent, freedom to the parts, and ease. And further, the compress must be put, on a part in the actual state of inflammation,—or otherwise, adhesive inflammation as it is called, cannot take place.

I think this is *a sort* of a dangerous kind of practice,—that it is an innovation unwarranted by reason, or the true principles of surgery;—and that such a treatment is more calculated to produce fatal effects, than any prospect of inflammation

flammation reaching the heart of a patient, when he is under the care of a plain surgeon.

Nor am I convinced—that there is a possibility of the matter formed within the vein being driven by the laws of circulation into the heart :—if it be true, that the vein does thus in some cases suppurate internally, for some length, surely a vein thus conditioned, must be rendered incapable of carrying on the circulation either of blood or matter.—Its valves are likely to be destroyed, and its capacity to be closed up.

Of the adhesion of veins, after an inflammation by bleeding, there has been a profusion of instances produced : but yet I think these instances do not so frequently offer to common observation, as from so much having been pledged about adhesion, there would be reason to infer. I have particularly noted, whenever there has been, after bleeding, a slight abscess of the orifice, so as to cause a loss of the cellular substance under the skin, and to leave an indentation in the arm, so as to admit a substance as large as a pea to lodge within it, and fill up the hollow,—that although the vein did not rise, when tied up for another bleeding, at another time,—yet the vein is always to be felt, and has not

not at least hitherto, ever yet been found *by me*,
—not to yield blood after the lancet.

Upon the whole, there seems to have been a display of anatomical and theoretical conjectures gone forth, and which has not only attracted attention but also excited curiosity, from the rare instance of its author having written on any part of surgery, or on any subject applicable to surgery so immediate as the present. To those who never will apply the compress, the distinction will remain without a difference in practice; and to those who will apply it, who will at all events worship the Golden Calf of Leicester-square, I have only to say,—that, in this instance, I should fear their temerity might be compensated by the loss of reputation. They might, it is true, by a compress thus conditionally applied to a vein, stop the progress of inflammation to the heart, and bring on thereby mortification in the arm.

Here my explanatory remarks are brought to a conclusion. What Baudius says of Erasmus seems applicable to John Hunter :—*Magis habuit quod fugeret, quam quod sequeretur.*

PART IV.

SERIES OF TRANSACTIONS FROM 1770 TO THE FINAL
CLOSE;—WITH AN ACCOUNT OF THE PROGRESS AND
ARRANGEMENT OF HIS MUSEUM.

WHERE a life has been totally engaged, and every hour of it, even, has been absorbed, in the variations of natural history, anatomy, and surgery, if these points had been excepted, there would have remained nothing to have been written upon. When John Hunter was abroad, he was constantly thoughtful about that he had left to be done, on his return home; and perhaps, there cannot be found his equal, who so completely filled up time, in active industry. As his works are in such high estimation with his admirers,—as these constitute him, in their opinion, the ablest physiologist and surgeon in the world,—as his admirers are persuaded that his works will be read, as long as the arts of the press, the type, paper, and ink, hold together,—surely then a comment upon them, must have been exacted from somebody.

In 1770, John Hunter had the honour conferred upon him, of Surgeon Extraordinary to
his

his Majesty. In the following year, he was married to Miss Home, the daughter of a surgeon: she has borne him a son and daughter; the former is entered in the Temple, and the latter, at present, is at home with her mother. To her he was directed, not only by personal attractions, but also mental endowments, which she possesses in a very eminent degree. She has exhibited specimens of poetry in sonnets, which for beautiful fancy, and pleasing harmony, are excellent in their style: and from the blandishments of her natural disposition, he found the cares and asperities of his life soothed to the end,—as long as his heart continued to vibrate. She was to John Hunter, what his Mariamne was to de Haller; but the abrupt stroke of death deprived de Haller too soon of that bosom comfort, by which life is endeared, which he mourned in accents of the most plaintive and melodious poetry, and which, for a long time, saddened all his pursuits,

To unbend the mind from that Tedium which, during the summer months, comes over every man of care, stationary in this Metropolis,—to refresh the animal functions, half poisoned and debased, by anatomical miasma,—and to be as little as possible out of the way of the sudden calls of a surgeon, John Hunter chose a cottage
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at *Earl's Court*, about a mile in the midst of fields, beyond *Brompton*. There he sometimes retreated for fresh air, and took his hobby horse along with him. Nobody of common curiosity could have ever passed this original cottage, without being obliged to enquire, to whom it belonged. By observing the back of the house, a lawn was found stocked with fowls and animals, of the strangest selection in nature,—as if it had been, another repository belonging to Brooks;—and in the front, there were to be seen four figures in lead or stone, representing Lions,—two in a form *passant* placed upon the parapet;—and on the ground, two more *couchant*, guarding the double flight of steps, leading to the vestibule. On the sides of the area, were seen, two pyramidal collections of shells, of a very contracted base, and mean height,—each of them, seeming to conceal a subterraneous entrance to a Golgotha. Over the front door was presented the mouth of a Crocodile, gaping tremendously wide,—

—To gorge with blood his barbarous appetite.*

And to prove, that there lived a Philosopher within this humble retreat, and that a flash of Lightning will equally dart, on the roof of a cot-

* Dryden.

tage of a surgeon, as on the turrets of a Palace of a Prince—there were placed erect, high above each gable wall, electrical conductors, daring its temerity. Here it was that John Hunter dreamed over many of his projects,—realized experiments on animals,—and laid the foundation of his *Fable of the Bees*;—and here was his country residence during his life time.

Here it was, that he pastured those Buffaloes which he so lately, as in 1792, put into harness, and trotted through the streets of London,—not judging, that he might have been fairly out-rivalled, by a showman's Dromedary,—especially, if there were, and probably there would be, the additional effect of a Monkey mounted on his back,—playing its little antic tricks. Savage beasts, said to have been snared, on the lofty and arborous mountains of Thibet, or on the dreary wilds of Boutan, and imported here, for autumnal exhibition, on carnival days at Smithfield, held in honour of St. Bartholomew, were sure to be first shewn to John Hunter;—their cunning parasitical keeper—prodigal of his illustrious name,—thus enhancing the estimation of his rare Asiatic curiosities.—We are also told, that Giants and Dwarfs, were certainly retained by him for dissection,—whenever the fates should so determine it—whenever the sisters shears

shears of destiny, should cut the threads, on which their lives suspended.

From 1770 to 1780, John Hunter's professional profits did not keep pace with his expences; and these ten years were particularly preparatory for obtaining information, and acquiring fame, that were hereafter to raise him to eminence, and reward him for all the toilsome labour, which every hour was engaged in. This might be deemed his probationary æra. His printed productions were during this period, prosecuted with uncommon sedulity, and his Museum was constantly gaining accessions. Besides these engagements, he at last found out—that it was necessary for him, in order to be great, to become a Lecturer; and that which he shrunk from, when he absconded the lecture-room, and entered into the army, he found himself now bold enough to undertake. In the autumn of 1773, he advertised,—not a course of anatomical lectures, but a sort of a skirmishing course—something new, and which could not be compared,—consisting of surgical, physiological, and comparative anatomical branches,—and so mixing them together, as either to confound or illustrate each other. There were two unusual circumstances attending this annunciation:—his terms were high, and his introductory lecture was not open.

I recollect having called for his syllabus, and thus finding, that the design was not liberal, from that cause only, declined being his pupil; what I gained, or what I lost, can be only decided by those opinions, which he has published, and of which I have disapproved.

These lectures were continued at his house in Jermyn-street, with very unequal success; and his dissecting room was opened also, under the same capricious visitations. To some of his courses, I have been told, he had nearly fifty Attendants, and I have been also told,—that in the autumn of 1786, after the publication of his work on the venereal disease, he had, but twelve. To imagine even—that this undertaking was carried on with equal facility by John Hunter, that it could have been, had he been properly educated,—would be romantic; more especially as nature had been very sparing to him, in the gift of elocution. It was from this cause, or a much worse,—that he lectured at home, and not at the hospital: instead of lecturing at the hospital, free of expence to its pupils, as was done by Pott, and of openly imparting his system to those, who were desirous of encreasing the stock of surgical knowledge,—he could, by thus lecturing at home, shut out every one capable of comparing his dogmas with
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established doctrines,—infuse without contradiction his principles into the minds of his pupils,—and take their money into the bargain. Purchased science is of more value than that obtained, as a gratuitous boon, from a surgeon of an hospital by his pupils :—thus thought John Hunter, but not so—Percival Pott.

In the beginning, these lectures were written on detached pieces of paper :—and such was the natural confusion of his mind,—that he would be frequently found incapable of explaining his own opinions, from his notes :—and after having in vain tried to recall the transitory ideas, now no longer floating in the mind, nor obedient to the will,—after having in vain rubbed up his face, and shut his eyes, to invite disobedient recollection,—he would throw the subject by, and take up another. Although the greatest part of the contents of his lectures were afterwards copied fair, by another hand, yet—upon every new opinion, in every fresh course, which his imagination had suggested,—scraps of papers were thus constantly produced, and consequent embarrassments, as constantly experienced in the explanation of them. It was in these predicaments—that he desired his pupils not to take notes, or if they had, to burn them, for that probably

bly, in the next season, he should find cause for changing his opinions.

The pursuit of his lectures, and the illustrations he brought to his aid, from the lore of comparative anatomy, made him, with more eagerness encrease his stock of preparations;—a stock adapted to answer two purposes,—to demonstrate out of it, to his pupils,—and to show it to those who admired most, what they least understood. His first floor, and back apartments, were filling apace,—inasmuch that he was not able to find room, for the *Camela Perda* given him by Lady S——, the tallest animal known, and which browses upon the branches of trees: he therefore,—that it might be in sight,—cut off its legs, and fixed it, in the passage.

By John Hunter being well grounded in anatomical practice,—by his constantly producing fruits of investigations, in successions of papers sent into the Royal Society,—by the fair reputation he had of an anatomist,—by the foul application he made of that, in taking every possible method to insinuate to the world, in the language of an egotist, that there was not his equal,—by his never stirring about any branch of the sciences, without apprizing the public of its importance,—he thus depressed the modest merit

merit of others, and exalted his own. It is for this reason, I am ashamed, for the honour of human nature, to own and repeat,—that, had his education been more liberal, or his heart more polished,—he never would have gone the length which he did, nor succeeded so highly. If a body were to be embalmed, John Hunter was sent for,—if a virtuoso solicited a dissection or preparation, to him he applied,—if any thing strange in nature occurred, the explanation of it came from him. In these articles, whether the object arose from curiosity, or admiration for knowledge—he found himself equally fought after: and as his vanity was flattered,—he willingly dissipated this waste of time;—those then who thus employed him, not recurring to him for the opinions of a surgeon,—as that was the last and latest reputation he could establish, or in the obtainment of which, he succeeded.

During the practice of Hawkins, Bromfield, Sharpe, and Pott,—the surgical engagements of John Hunter, were limited within the recommendations of those pupils, who had known him in his brother's dissecting room, and who were now scattered to various places, at home and abroad. The chief operations performed by him, at this period, out of the hospital, consisted in such undertakings, as the judgment of able surgeons had

had induced them to decline. Such desperate cases, John Hunter was never found to hesitate in embracing : he sought for the opportunity rather than rejected it, whenever it offered. A half pay Officer, resident in Westminster, had a species of Rupture, for which he had consulted both Pott and Watson, and both declined performing the operation,—knowing how strong the chances were, against the success. The patient was too eager for relief, to permit himself to rest satisfied, with the judgment of these surgeons ;—John Hunter performed the operation, and he died the next day. This was his reasoning,—if I do not succeed, I cannot be blamed, as opinion was,—that the case would not admit of success ;—but if I should succeed, I snatch a leaf of laurel, beyond the reach of those who despaired of the possibility.—If I fail, it may not be generally known, but if otherwise,—it shall be.

At this period I know—that his constant habit was, to receive his patients in the morning, —without any decent preparations for their visits. Sometimes he was found, with his hands besmeared in the act of dissection, and sometimes,—after having washed his hands,—in putting on those coverings of linen, over his wrists, —which are commonly called keep-cleans, or cover-

cover-fluts. All those scenes had their effect : the supposition that a man, thus engaged, was most capable of giving the best opinion, in a case of surgery, was generally believed. I have frequently smiled, on hearing the opinions, that men of liberal educations even sported, upon the engagements which they found John Hunter employed in. They could never have been brought to conceive, from their own judgment, but that he must have been, the first, in his profession ;—and it would have been foolish to have attempted to correct that judgment,—by asserting, that decency and knowledge were not incompatible ;—and that the surgeon, who had cultivated the science of anatomy, in its proper time and place,—who had the understanding, to throw the foulest appearances of the anatomical processes, into the back ground,—and to impart by practice, the result of them, only,—was a wiser man, and consequently capable of giving a better opinion: As one occurrence will bring to the recollection, another in similarity,—I have thus frequently assimilated this trait in John Hunter, to that suggested by Richard the Third,—when he contrives to be seen by the Lord Mayor, and Court of Aldermen, with a prayer book in his hand, and in an apparent fervor of devotion.—The study of Pott had all the appearance of, one, belonging to a private gentleman ; and his per-

son was neatly fitting, for the reception of his patients. He needed not the borrowed aid, of visible signs of anatomy, for proving,—that he had stored his mind with all that was necessary, for perfecting his professional knowledge. But such are the tricks which lead the minds of the ignorant into captivity,

I am now to tell of a very unpleasant misunderstanding, which took place, between the two brothers—William Hunter and John,—some time before the close of the year 1780. In the minds of many—the cause will be deemed of too trifling a consequence, to ruffle the spirits for a moment:—but trifles, in little minds, are always viewed through a magnifying medium. If I understand the cause of quarrel aright,—it arose, from John Hunter having invited William, to the sight of a diseased part of a soldier, who had died in consequence of it;—and William having found,—that this diseased anatomical property, would prove a valuable preparation for his museum,—caused it to be taken to his house, and refused to give it up, to the claim made by John. This was resented by John, and this proved to be so serious a foundation, for the separation of friendship and affection between two brothers, as never afterwards found any abatement. John enlarged the wound, thus made,
by

by demanding from William, his claim to an anatomical right, on the structure of the placenta, which William had published in his Commentaries in 1764, and John afterwards in his Animal œconomy. To this claim made by John, William replied,—and to that, John gave in a rejoinder. The Royal Society received their papers, but proceeded no farther into the merits of the question betwixt them. William Hunter dying, about three years afterwards, left his property away from John, and placed the superintendence over his Museum, into other hands.

An aggressor in a quarrel, is rarely the first prone to forgiveness. Whether John Hunter discovered much worldly prudence, in his conduct on this affair, I shall not say; he was not rich, and his brother was: but this I will assert, that in this quarrel, the whole of the manly deportment, was on the part of John. It is not, because one man is dependant upon another, to be reconciled by reason, or morality,—that an inferiority of fortune, should force him to compound with an insult founded in an act of injustice. Nor is it the value of the right, which constitutes the degree of injury, done by one to another, but the overbearing audacity of the act. William, palpably, aimed to take advantage

tage of John's dependance; and John's state of mind was not accommodating enough, to submit to it. But, notwithstanding all this, John was found assisting his brother in chirurgical offices, during his last illness; and perhaps he was then seen in the discharge of a practical duty,—in a situation to be envied—by the best man, that ever was born!

William had a querulous and hypocritical way of uttering his complaints; and he would describe the imaginary injuries, done to him by others, with all the insinuations of the worst of crimes. He had the art of making out a plausible story, against his enemies, suggested by his native jealousy. He strongly resembled in this part of his character, the little Bard of Twickenham. John could never talk of his injuries; but would get rid of the passions they provoked in him,—by the bitterest utterings of swearing—Sterne's Dr. Slop could not have been a match for him.

In the spring of the year 1781, the theory of John Hunter's paper—on the Digestion of the Stomach after Death—and the advantageous knowledge resulting, from the thousands of experiments, which he boasted of having made,—were brought into practical realization. He
was

was retained, by a gentleman of the name of Nolan,—the friend of the late Captain Donellan, who suffered execution for the murder of Sir Theodosius Boughton—to attend as a witness, at his trial, at Warwick assizes,—and he accordingly went. He was expected to be considered, as a second Daniel—to save this arch-criminal,—and to bring him purified,—out of the fiery furnace of justice. I am afraid to express my sentiments, upon this act of John Hunter, by going into particulars. I very well recollect, how his conduct was considered, in the opinion of the day,—and I know how I viewed it then—and what I said upon it. But as I have made up my mind, not of myself, to discuss this transaction, by recurring to any comments I might have then formed,—I shall, in their stead, produce the summary of his evidence by the judge,—from the trial, that was published.

“ For the prisoner you have had one gentleman called, an able man, and who is likewise of the faculty.

“ I can hardly say what his opinion is, for he does not seem to have formed any opinion at all of the matter. He at first said he could not form an opinion, whether the death was or was not occasioned by the poison, because he could
conceive

conceive that it might be ascribed to other causes. I wished very much to have got a direct answer from Mr. Hunter, if I could, what, upon the whole, was now the result of his attention and application to the subject; and what was his present opinions; but he says, he can say nothing decisive. So that upon this point, if you are to determine upon the evidence of the gentlemen who are skilled in the faculty only, you have the very positive opinion of four or five gentlemen of the faculty, that the deceased did die of the poison. On the other side, you have what I really cannot myself call more than the doubt of another; for it is agreed, by Mr. Hunter, that the laurel water would produce the symptoms which are described."

After mentioning the names of the faculty, whose opinions were positive—it will be needless for me to speak of their reputations: Ash, of Birmingham, Parsons, of Oxford, Rattray, and Wilmer, of Coventry:—three of whom, had made experiments on laurel water, and were thereby enabled to speak, from its effect:—but I will have done of the subject!

The great accession of articles, pouring into the Museum,—the death of William Hunter, excluding all collateral prospects—the resignation

tion of practice by Hawkins,—and the threatened decline of it by Sharpe—opened to John Hunter, plans, and prospects, more extensive than any he had hitherto formed, or conceived. He was resolved not to be outdone by his brother, in the estimation of his Museum; and not to be depressed, but exhilarated rather, from the unnatural delinquency he had lately experienced. He was determined to keep up the appearances of professional eminence, by anticipation; and to be a candidate for professional preference, whenever vacancies gave him, the pretension. He therefore found—that the house in Jermyn-street contained not dimensions, sufficiently capacious, for the plans which his active mind suggested; and in 1783 he took a house, upon a much larger scale, in Leicester-square, about the middle of the eastern side, which extended through, into Castle-street. This was fitted up in a very expensive manner;—and here he established an expansive room for his Museum,—another, for a public medical levee, on every Sunday evening,—another, for a lyceum for medical disputation,—another, for his course of lectures,—another, for dissection,—another, for a printing warehouse and a press,—and another, for vending his medical works,

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This undertaking could have been, alone, attempted but by a man of enterprize; and it could, alone, have had a prospect of being prosperous, but by his natively possessing more intrepidity, more industry, and consequently more credit,—than perhaps any professional man of his own, or any former time. I do not wish to go into John Hunter's private affairs, beyond what is necessary to justify my declarations on his public, and therefore I shall shortly say,—it was not, because his profits from practice authorized him to engage in this undertaking, that he embarked in it, for as yet his practice was far from being the greatest,—but it was, because this was the most probable chance, which offered amongst others less desperate, and consequently less likely to succeed;—as desperation was the stake of John Hunter, at that period of time.

His whole reliance was, upon the opinions of men; and these were to be obtained, by the appearance of things. The popularity of a surgeon, had long engaged his attention. To have retired from among the foremost, when vacancies were offering,—would have been in some measure,—to have declined his own pursuits,—to have renounced his first object,—and to have been out of the sight of that public, after he had thrown himself so much in their way. It would have

have appeared, as though he were not to be found, when he was called upon, by the clamorous voice of popularity, and when he had given in his preferable claims;—claims which have promised so much, and have proved so little.

The new situation he had chosen, was convenient and central: and from this time, fortune seemed highly disposed to favour all his projects, and implicitly to surrender her froward controul, over any of his adventurous engagements. Every thing that John Hunter now did, was considered by the public in general, as being the best possible method, in which every thing could be done. This is very strange, yet it is very true, that miscarriages—which fairly ought to have been attributed to an inferiority in knowledge, and not to an inevitable consequence in the nature of the thing,—where the best means which were known, had failed from an impossibility or inadequacy in their power,—never affected the reputation of John Hunter. He could do such deeds, without impeachment of character, as would have destroyed the reputation of any other surgeon. Whatever has been done by him, and which has, by its notoriety, allowed of an open investigation from those, most competent to compare it, with what could have been done by others,—has uniformly and

constantly convinced me,—that John Hunter possessed not the common talents, for common practical surgery. This assertion can only be proved, by the result of his practice; and I do not hesitate to stake my reputation, by saying,—that from the numerous instances of rashness or insufficiency which I could adduce,—I am authorized to pronounce him, to have been,—a very inferior, dangerous, and irregular practical surgeon.

In the month of December 1784, two cases offered to John Hunter, from the bites of the same mad dog: one—of a young gentleman, Master Rowley; and another—of a French woman. Both of them were bitten, by the same dog, and at the same time. The accident happening in Jermyn-street, the young gentleman was immediately sent to John Hunter. Reputation had directed where he was to go. The consequence of this bite was,—that he died in January 1785, of hydrophobia. The woman, whom John Hunter likewise treated, died also. I do assert—that John Hunter did not do, for these patients, the best that could have been done, by the art of surgery—and that the miscarriage was not owing to the inevitable nature of the cases. The bite was inflicted upon Master Rowley's lip; and in the application of
caustic

caustic to it, he offended against the principles in surgery, two ways.—First, against the established principles, by using the caustic, instead of the knife; and second, because caustic was not, when applied to a wound of that description, adequate to the certain extinction of the poison.

If another had done thus,—or if another had done many other things, which he did—his reputation would have not only fallen into contempt, but a severe reprobation would have gone along with it. But this was not all,—he had the unfeeling effrontery, to enter into a public correspondence, with a physician in Suffolk, about the symptoms of Master Rowley's death; as if there had been success in the event of the case. This physician had not attended Master Rowley, but had obtained his information, without assigning his motive, from Tufon, a surgeon of good reputation, and who attended the family. Tufon was never given to understand—that the information was to go forth to the public—and therefore, when this indiscreet correspondence, painful to the feelings of the father, Admiral Rowley, was shewn to him,—Tufon was obliged to say, that the correspondence had not his approbation. The Admiral replied,—“ had they succeeded in saving my

child, it would be justified, but as it was, such a transaction made public, could only be productive of fresh pain to me, and of disgrace to them.” —And this is the light, in which the catastrophe must be viewed, by every honest man of feeling, all over the world. The standard of truth, is neither so imaginary, difficult, or liable to be variously construed, as ignorance or venality might hope to inculcate: upon surgical points well ascertained, and upon those fully explained, —it can be realized, to every well designing common capacity.

I am now to tell,—how John Hunter, when embarked in this desperate adventure, steered his course,—piloted his vessel thus deeply laden,—and how he brought the several commodities it contained, to general account.—How he conducted himself, under all his plans, from the æra of 1784, after going into this new house, to the close of 1788, when the death of Pott, opened before him new resources, from increased practice.

The loss of a public and able man, is ever to be deplored; because the world can never spare him, without feeling a convulsive shock, whenever it be deprived of abilities,—such as belonged to Percival Pott—and such as were carried by him

him into honourable practice. But yet there was a consolation which soothed the reflection, at the moment that it contemplated his sudden departure.—That he had not died,—before he had been granted, many and prosperous days,—before he had stamped the true principles of surgery, on the minds of practitioners of the present age,—nor before he had, by the works which he had published, conveyed their inestimable value to posterity. It is curious to remark, that such a man enjoyed no lucrative, nor honourable professional office, which was in the power of the Court to bestow;—that as he sought not honours, they were not bestowed upon him;—that as he solicited not that which alone is honourable, by its spontaneously following merit,—so did he pass through a life, with unblushing neglect, from every administration, to its 74th year, without it.

THE MUSEUM.

I shall first proceed, to give an account of the arrangement of the Museum, in this new house,—a valuable collection,—and of a nature rare and extensive. If John Hunter did not form it altogether,

together, from a love for science,—if it were not formed, out of an overplus of wealth,—if the lucrative posts which he enjoyed from government, and the emoluments redundant out of practice, did not, as might have been expected, enable him to leave this valuable Museum, entire to posterity, and in the manner which a pure philosopher would have hoped,—perhaps, it might be owing to his not having calculated upon the chances of life,—as such belong to wisdom alone, as such are the uncertainties of every hour of existence,—and that he might be overtaken, sooner than the plans he had arranged could have been half perfected, much less have been brought to conclusion. The Museum, as fitted up in this new house, was to be seen in all its glory,—the lustre of which, has captivated the attention of every true philosopher, and dazzled the eyes, and excited wonder in the minds, of the uninformed vulgar,

It does not belong to the province of any man, to interfere in this part of the appropriations of John Hunter's time, and fortune. Every person has the right of enjoyment in his hobby horse, when that does not intrude nor trespass upon the felicities of society. And it must not be forgotten,—that the nature of the profession of a surgeon, exacts from him who is destined

to follow it, to become constantly stationary. A surgeon cannot absent himself for months, nor days, and again return to his occupation; and he must be in waiting even, when he is not, in actual employment. If John Hunter then, thus turned his attention to the collection of a Museum,—instead of building a new country mansion,—of changing pasturage, and arable into lawn,—paths, into gravel walks,—rivulets, into sheets of water,—and down, into plantations,—his amusement, though not of so general a nature, was surely as innocent;—and the talent, which brought the natural history of the world, into a focus, was moved to it, by suggestions of the mind, as exalted, if more rare, than that which is more commonly employed, in changing the surface, necessary, for the useful product of nature, into pleasurable spectacle.

In my observations upon this Museum, I shall only contend against one material inference, which not only John Hunter has strongly dwelt upon, but the vulgar at large have drawn from it—that it positively constituted him a surgeon of greater ability, than if he had not collected it, and than other surgeons, who had not such a Museum. Nothing can be more incompatible with reason. If he made this, his leading object, other pursuits, from the nature of man, must
confe-

consequently have been the more neglected by him. But collections of this nature, or of any other, similar in point of passion to them, have nothing to do with the principles of surgery. The art is in no measure advanced from them. For if it were so—and if it necessarily follow,—that whoever makes a collection of natural productions, must consequently be constituted a greater surgeon,—the Dukes of Portland and Sir Ashton Lever have been deprived of a valuable part of their reputations. The physician,—the painter,—and the statuary, must have studied anatomy,—but that could not have constituted, in either of them the art of surgery, without their having studied, and practised that also.

In an early part of John Hunter's life, he took up the idea,—that the structure and physiology of the human body, would never be made out clearly, but by attending to the structure of animals in general. On this principle his Museum was formed: and it consists of preparations of every part of the human body, both wet and dry; with corresponding preparations of the same parts, in all other animals who possess them. If an animal has a greater, or a lesser number of parts, than the human subject, it is preserved by him, on these accounts. The arrangement of the Museum is this: it begins with specimens of
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the most simple, or component parts of the human body, and of the same parts in other animals, where they differ in structure; such as a muscle, bone, tendon, ligament, cartilage, &c. To mention one instance; the bone from a human body, and the bone of a fish are very different, the one is opaque and heavy, the other transparent and light:—these circumstances will give rise to a variety of physiological reasonings.

It goes on to the more compound parts: as the heart from the human subject, and the hearts from all those animals from which they could be procured; shewing the different variations. The human stomach and the stomachs of other animals: the intestines, the parts of generation, the liver, spleen, kidney, lungs, brain, in short, every part of the body, the arteries, veins, nerves, and lymphatics, are shewn in preparations from the human subject, and from a variety of other animals. The bones too, of every animal that could be procured, are formed into skeletons.

In the arrangement of undissected animals, or parts of animals, of which there are a great number in the Museum, John Hunter has begun with what he called, the most simple animal, a polype, or a leach for example, and going on

to the more compound, ends with man. The deviations from nature called, monsters, are also in large numbers. There is a collection of extraneous fossils; these, consisting of the remains of petrified animals, have some connection with comparative anatomy: and lastly, a good collection of calculi. He also possessed a fine collection of diseases; but these do not properly belong to the Museum, but were used by him, in his surgical lectures.

I know of no Museum similar to this; it may be said to be, unique, or *sui generis*; nor do I think that the aggregation or consolidation of any former Museums, would have produced any thing like this: and I believe that the idea of forming such a collection, originated with John Hunter.

There are some preparations of vegetables in the Museum; but I believe they are only considered, in an analogical way. No one, who is at all acquainted with the nature of this collection, but will readily allow, that it was the production of time, expence, anatomical excellence, and intense application. It is very certain, that John Hunter laid out all the money he ever got, in this and pursuits connected with it. I imagine, that besides, what was sent home to him, from every quarter of the globe, and which was constantly announced

nounced in the prints of the day, every pupil and apprentice John Hunter had, contributed to this Museum, more or less, in proportion as he was zealous, or idle.

To the formation of the Museum I have known, but three, whose services are worth recording, namely, his Brother in Law, Bell, and André.—The first living with John Hunter, nearly the whole of the time, since he left school,—the second fourteen years,—and the third a shorter time.—The second was, besides, John Hunter's draughtsman, and has a short time since, embarked for India.—The third came to John Hunter, an anatomist already in a style, the most perfected, perhaps, of the modern age.—He was bred up, in the school of Watson, and seemed to have pursued dissections, and to have made preparations, purely, from the admiration of the arts. His delight in excellence was such,—that he would not permit any undertaking to go forth from his hands, with the possibility of any superiority in perfection. His neatness in dissections,—his knowledge of the arts of injections and preparations,—his deep acquaintance with natural history,—and above all—his modest merit, and humble deportment—sent him to do that, for John Hunter, no one, besides, could have been found, to have done for him, and which

he could never be brought, in any large scale, to do for himself. Besides these excellencies,—he was found to be necessary to John Hunter, in all points; and that upon stipulated terms, of a very inferior degree. He was his amanuensis: and this native philosopher was seen—when I went to John Hunter's warehouse, to purchase his book, on the Venereal Disease,—folding up the sheets of it, for the women to stitch them together:—such are the ludicrous sports of fortune. It is with infinite pleasure,—I am able to tell—that he has found a retreat, secure from the perilous peltings of adversity, as domestic librarian to a nobleman, whose highest characteristic is—to venerate virtue, and to protect merit.

Both, the departure of Bell, and André, proclaimed—that this was the æra, when John Hunter had brought his Museum, almost, to its acmé. And, as the subsequent part of his life was thronged with other avocations, it is to be presumed—that the time, I have chosen, to describe it, was the time, when it was most advancing to that pitch of perfection, which it is now to be seen in, as it is five years, since André left him.

There is an observation, which must be taken into this account, and cannot escape being noticed,

ticed, because it leads to ascertaining the value, in the present age, of wet and dry preparations,—and to place this Museum in a different estimation,—to that which was formerly annexed to those Museums of Tradescant, Hans Sloane, Mead and others. The articles, both wet and dry, are found to be of a perishable nature: and the wet cannot be preserved, by the most strict attention, and constant renewal of spirits,—beyond a very circumscribed duration of time. The finer, and more minute, and delicate parts of preparations, are the most perishable; the beautiful display annexed to these, are soon evanescent; the very medium in which they are preserved, tends to corrupt them; and that medium is also, very expensive. I saw the preparations, belonging to Ruysch, which are deposited in the Museum at Petersburg, going apace into decay.

As John Hunter's Museum derives all its value, from the system, or arrangement of it,—so when parts of that are gone, the value of the remaining, will be lessened, by the chasm produced in it. Since the art of engraving, has arrived to such a degree of excellence, and its artists multiplied,—these considerations are not to be so much lamented,—especially, when it is considered—that by engraving, every single article

article can be thus multiplied, and every physiologist can indulge his favourite taste, by such a circulation of the prepared system of the animal œconomy. Whereas one alone, can possess the original,—and that original, from its perishable nature, cannot last long. If this Museum be of that transcendant value, which the age is so highly disposed to attribute to it,—I should imagine, that it would be obtaining every point, by the whole being engraved; and this ought to have been gone about, during his life time. The subscription for it, would amount to a very considerable sum of money,—and the fame of John Hunter, would be thus perpetuated, upon, one, only, solid foundation—by his having produced a work of public utility, which the art of engraving, can make as durable as copper.—If, this, had been done in his life time, he might then have said—

Exegi monumentum, ære perennius—

Soon as he was settled in this new house, he sent out cards of invitation to those of the faculty, his selection suggested,—to attend on Sunday evenings, during the winter months, at
his

his levee; and they were regaled, with tea and coffee, and treated with medical occurrences. It would have been highly indelicate, for those to have gone there, who were not invited; and it might be presumed, as indelicate for those who did go, to talk upon matters, which did not favour John Hunter's theory. Hence, every case, that was produced, tended to confirm, one or other of his new opinions. That gonorrhœa cured itself,—that specific distance of virus was, exactly, an inch and a half up the urethra,—that every bubo could be reduced, without coming to abscess,—that caustic was the only remedy, for the cure of obstructions, and the prevention of hydrophobia,—that adhesive inflammation—suppurative inflammation,—gangrenous inflammation,—irritable inflammation—and sympathetic inflammation, were no barbarisms in expression,—that the blood which was the vehicle of infection, was not infected,—that transplanted teeth could not convey the venereal disease,—that bandages, on inflamed parts, did good,—that stomachs complimented death, by going directly into dissolution, turning their contents into the abdomen,—that self pollution was a very innocent diversion—and many more opinions of high value—such as that, mucus was pus, and of course, pus was mucus—and such was the jargon of

of this modern school—and such was the spelling book, for his brats in surgery.

————— He leads them, like a thing,
Made by some other deity, than nature,
That shapes man better; and they follow him,
————— With no less confidence,
Than boys pursuing summer butterflies.

SHAKESPEARE,

But lest, the general diffusion of these blessings for mankind—and lest, the benefit arising from them, should not, by this scheme of circulation, be fully adequate,—Another, was formed in January 1785, upon a larger scale of aggrandization, under the same roof. A society for disputation, where all were of the same opinion, and which consisted of the same members who visited his levee, was established. The room was called,—Lyceum Medicum,—galleries were erected around it,—the president wore his hat on,—and John Hunter was the patron. Here, his new opinions received, the finishing plaudits of approbation—and from thence, were disseminated fully into practice.

In this new house also,—a press was erected—and here, a synod was held, to correct his written documents: and here, he printed his book, on the Venereal Disease in 1786, the sale of which was

was rapid, at first, from curiosity being artificially raised,—as the papers of the day had announced—that it was to throw all former productions, at an humble distance:—but, after it had been once perused, that sale fell short of sanguine expectation. Why he printed at home—why he deprived a valuable profession of its profit—I am not to tell;—it can only be considered, as one of those steps in the ladder, that assisted him in climbing, to the summit of his prospect of ambition.

As he frequently had been heard to declare—that he never read books, on surgery and medicine—his library was barren of these subjects; it was but small, and consisted chiefly of books, on natural history. And this was the man, who was to lay London, under contribution, for his surgical art.—Let those consider this, who look upon it, as a piece of art, and the masterpiece of action,—to deceive, and make a prey of a credulous, and well meaning honesty.—Let those, whose foresight allows them no other clue to direct them, suspect the disinterested professions of him, who is only seen in every action, selfishly proceeding in his way of ambition, careless of consequences.—Let young men his admirers, feel the impulse of rising into eminence, by the modest path of genuine knowledge;—let them

adopt, for their model, the transcendant life of de Haller, and learn to forget, that of John Hunter;—let them shun the system of popularity, acquired, by courting the little vanity of vulgar opinion; and embrace the more intrinsic sentiments of philosophy, which the wisest amongst men, must ever, irresistably, venerate.

It will be found—that the advancement of John Hunter to that professional height, at which he was, at length, seen to soar,—was owing to the decline of those, who stood in his way. He did not supersede, but succeeded to them. The death of Pott, on the close of the year 1788, placed him upon a footing, equal, if not superior in point of practical calls, to any living competitor. I think I may affirm—that his consultations were more in fashion, than any other surgeon's—and that his range of practice was more extensive:—that we heard more of the name of John Hunter, than of any other surgeon.

There is no novelty, in remarking—that from the limited knowledge possessed by man—he is ever incapable of predicting certain events, belonging to futurity. No one can foresee, however he may merit it, both by talent and industry,—what will be the scale of estimation, which he
will

will be held in, by the public opinion, ten years before the time which he looks for, arrives. John Hunter had wisdom enough—so far to mistrust the idolatry, he worshipped, and therefore—a long while, before the death of Robert Adair,—he had obtained the promise in reversion, of as many of the lucrative appointments, held by that gentleman, as he possibly could. In the year 1789—vacancies of these, and many more, offered by the death of Adair—and the offices of Surgeon General to the army, and Inspector, were then succeeded to, by John Hunter:—but the influence of Keate kept from him Chelsea Hospital; and this was for ever after, seen by him, with an eye of discontent.

If this arrangement had been accommodated, according to the separate talents of the candidates,—John Hunter ought never to have been Surgeon General to the army.—In time of peace, a man, like him, might have drudged on through the business, without much difficulty, or embarrassment:—but in time of war, the duty requires qualities, which he never possessed;—a power of dispatching business,—writing letters with address, and discriminating into merit without partiality;—a just conception of the dignity, and honour of the office; and the persuasive masterpiece, of so convincing any one, who asked for

what he was not entitled, to expect,—as to induce him, by the force of well timed argument, to decline his pretensions, without the murmurs of dissatisfaction.

John Hunter was industrious, but he was slow; and letter writing was not in the scale of his education, or ability. He was biased, too powerfully, to his pupils, to be publicly just;—as he would know no merit, from the report of any one, where he was unacquainted with, or had not educated its possessor. He sunk the dignity, and tarnished the honour of the office, by the selections he made, and the establishment he formed, in the hospitals on the Continent. He affected to be too proud to explain, where he did not mean to serve; and the affectation arose from his incapacity,—from his want of the power, of placidly giving, a decent refusal. He arrogated a right of creating physicians out of apothecaries; and defied the interference, and the power of the college. He estranged himself from all intercourse, with the corporation of surgeons—he was never inclined to receive their recommendation of merit; and though chosen one of the Court of Assistants, in the year 1789, he never, but once, attended in his place. He hated his equals in the profession; and who can esteem him, who hates them!!

But—

But—although the court possessed the power—yet it did not enforce it; or for his contempt, he might have been reduced to answer the law, invested by their charter. He—who would not attend his duty at the hall, nor associate in the annual festival of harmony, established by the company,—could advertise his name, as patron, and chairman, at the feast of the members of his little senate, the Lyceum. He was not found, to be even decent, when it interfered with his pride; and in consultations, where he was the, last, called in,—if he did not like the, first,—he was certain to get him, discharged, on the second or third visit,—by saying, that there needed not the attendance of, two. This I am told was a favourite piece of practical revenge with him.

I am now arrived, to that period in the life of John Hunter, so much within the recollection of almost every reader,—that I should be wanting in common prudence, if I were found to explain any circumstance of his transactions, beyond what truth would justify:—and on the other hand, I never should have been fitting, for this undertaking, if I withheld from the eye of the public, such relations of facts, as are, by their being recent, more notoriously obvious. I will neither do the one, nor the other. I will
neither

neither screen, nor exaggerate his conduct. Let the relations of facts speak for themselves.

In reflecting upon human nature, and watching with common attention the general operation of the passions,—the best of men are frequently seen,—when bowed down by oppression,—disappointed in events,—or left destitute of all prospect, even, in the false flattery from hope—to betray a discontent of mind, and a jealousy for the prosperity of others.—

Ipse suum cor edens, hominum vestigia vitans.—

In such instances—the passions will flow irregularly—their ebb and their tide, will be experienced to the fullest extent,—and the power of reason will be in vain invoked, for keeping them under any degree of controul. In such instances—the nature of their situation, by the operation of adversity, upon susceptible minds, is ample to explain the effect: and in such instances—when by a happy reverse of fortune, the contention of the passions has subsided, the bosom is again restored to a state of repose, and its “ Lord sits lightly on his throne,”—when reason is again, in possession of its dominion,—every lively action will proclaim, both the cause, and the effect.

But

But this, was not the case of John Hunter:—nor can I find out his apology, by this mode of adaptation. He,—from a retrospect into the chances which were against him,—from his want of fortune originally,—and above all, also,—from his want of education,—must be deemed, by the highest expectation which might be formed of his merit, to have been in a situation—far beyond all prospect of reasonable suggestion. He had held for thirty four years, the half pay of surgeon upon the staff, and which had amounted upon the whole, to three thousand pounds;—he had so balanced his interest with his influence, as to suppress the alarming din, of serving any more;—he had found out, that another summer trip to Bellisle, or Portugal, could not create, another half pay, and that the honour of the service was nothing, in competition with the interest of him, who serves;—he had assurances, that by staying at home,—he could create a new interest, for the obtainment of higher emoluments, and in which, he at length succeeded. In time of war,—I speak not from knowledge,—the joint offices of Surgeon General and Inspector are set down, at twelve hundred pounds per annum. These lucrative posts, besides his, purely honourable, appointments, and such as were mixed with profit, as that of his being surgeon for eighteen years to St. George's hospital, were

were enough, one would have thought, to have gratified the proudest mind,—especially as these were obtained, by one, who in his habit of life, was strongly disposed to solitude—by one, who has been said, to find a retreat most congenial to his nature, in the recluse study of natural history.

Thus conditioned,—it might have been reasonably supposed,—that there could no longer have existed, an individual in the profession, whom John Hunter would have viewed with a jealous scorn; or whose prosperity, he would have hoped to obstruct. And thus conditioned—it might have been expected—that the turbulence of passion, the ambition for power, and the avarice for profit, would have been lulled to rest, would have, at least vanished, on the approach of those days of prosperity, which he had experienced, in so eminent a degree. But strange to be told,—such was not the consequent effect. He—instead of possessing the placid, and tranquil countenance of a man favoured, by the world,—was found to be waging a continual war, with the surgeons of the hospital, to which he had, for so great a length of time, belonged. He disputed their unanimous choice, in every fresh election. He preferred candidates, out of their regular course. He tried contest upon
contest,

contest, and found his influence fail. Under the same intolerance of temper, he went so far, as to keep back, the receipts of money, paid to him, by pupils of the hospital, for their public attendance, at the hospital ;—and contrary to custom, and the rule of the hospital, he claimed that money as his right :—for this, he submitted to the indignity of being summoned to the board, where his plea was heard, and he was forced to refund.

He was not likely, from his native disposition, to yield up points hereafter, in consequence of the loss of influence he had already sustained ; and which, the experience of contests had already discovered. And therefore—I have every reason to be persuaded—that, for the three last years of his life,—he never approached within the district of the hospital, without its affecting his mind, and disordering his whole frame. In one of those disputes, at the hospital—where the most trifling address might have easily turned aside, the heat of altercation,—his powers were seen, in the very act, to give way, he fainted—and instantly expired—

Frigidus obstitit circum præcordia sanguis—

O o

This

This happened at the hospital, on Wednesday, the 16th of October, 1793, and when, he was in the 64th year of his age.—He was carried to his house, in Leicester square—in a close chair belonging to the hospital—and was interred, on the Wednesday following,—in the public vault belonging to St. Martin's, a few select friends attending, at his funeral. On being told of this event, on the same day, I recollected having seen the bay stone horses returning, through Piccadilly, home, without their master,—and this circumstance introduced to my reflection—the sympathy which Virgil has attributed, to the war horse of young Pallas, in his funeral procession—

*Post Bellator Equus, positus insignibus, Æthon
It lacrymans—*

John Hunter could never separate the loss of Chelsea hospital, from the person of Keate,—nor, would he permit himself to reflect,—that if he had not the influence, for obtaining what Keate had,—and what, if Keate had not obtained,—another might;—and that, whoever had succeeded to it, would have been equally obnoxious to him, as long as, such a malignant impression remained, upon his distempered mind.—With Gunning, he had been acquainted, more than thirty years; and notwithstanding he had made, man, the study of his life, that book of nature,

ture, must have been blotted on the page, which could have informed him,—that Gunning would never tamely submit, to a personal insult, from any one. To Walker, he had been well known only a few years, less; and he must have evinced a superiority, in the art of tormenting, above all competition,—when he urged his good nature to enmity. He certainly misjudged his own power,—when he broke a lance with Keate—in trying to keep him out, of St. George's hospital. Besides the mortification he experienced, from finding, that Keate's personal influence was superior to his own; he brought another proof fatal to his ambition, to the test;—that, in the contest, almost the whole of the respectable part of the faculty, in Westminster, were against him.

By turning ones attention away, from these effects,—and seeking for a cause, to account for them, through the aid of reflection,—I think it not improbable, but that the whole of John Hunter's irritable conduct, and particularly within these last four years, might be attributed to the nature of that disease, which had been progressively increasing—and which, at length, was found, thus abruptly, to have been the cause of his death. He had long complained of a palpitation, about the region of his heart; and inspection has since confirmed, that—as it was

suspected—the vessels were gone into a degree of ossification. Some years since, he was induced to go to Bath, and try the effect of the water, there,—from thence he returned, somewhat refreshed indeed by purer air, but without the cause of complaint being removed. Such an interruption in the circulation of the blood, which constitutes the first principle of vital power,—must have physically operated—by producing in an exacerbated degree, very irritable symptoms on a mind—ever too prone to that indecorous propensity.

When it is considered that—before his appointment to the office of Surgeon General,—every minute of the day had from him, the stated allotment—and that in some degree, regulated by his choice, the state of his health, and natural powers ; and when it is also considered—that he was, at an age beyond sixty—thus harraffed by disease—reduced to forego some of his most favourite pursuits, in exchange for the toilsome task of new measures, in an office of bustle,—one can easily be brought to think—that the palpable diseased irritation of his mind, exacts all the apology—which humanity founded upon reason, can bestow.

I believe

I believe John Hunter to have been, one of the most industrious of men. The way in which his time was devoted,—before he obtained the public appointments,—was, as follows:—He rose very early in the morning, and went immediately into the dissecting room,—where he sometimes dissected, and gave directions concerning, what he would have done, in the course of the day. After breakfast, he attended to those patients who came to his house. At eleven he went abroad; and was employed in visiting patients,—attending at the hospital,—and when the occasion called for it, in opening dead bodies. He eat very hearty at his dinner,—and rarely drank more, than a glass of wine, and sometimes not that. In the evening, he was engaged in reading his lectures, and writing down observations, which he had made through the day,—or preparing, for the next coming publication. He seldom retired to rest till twelve, or one o'clock.

His person was about the middle stature: he was rather robust, but not corpulent: his shoulders were broad and high, and his neck remarkably short: by the exertions—which he constantly made, after the manner of something like a cough,—he seemed as if he solicited,—to set the circulation of blood a going. His
features

features were hard,—cheeks high,—eyes small and light,—eye lashes yellow, and the bony arch protruded. His mouth was somewhat underhung. He wore his hair curled behind. His dress was plain, and none of the neatest. He was frequently seen to smile in conversation—but it was generally provoked, from a ridiculous, or a satirical motive.

I am to confess—that in my account of him—his virtues have appeared somewhat thin, and shadowy;—but throughout his transactions, as well as his papers, I cannot accuse myself, of having passed a single virtue by,—but have given the scatterings I have found, the strongest impression they could bare.—It is for his admirers, to bring forth—with all their force—fruits praiseworthy of their venerated Patron;—especially, as the gentle spirit of philosophy was never seen, to pervade his public conduct. It was not in my power, to produce more instances of the philanthropy of John Hunter—than were to be found, in the public relations of his life;—but although I have no authority to reason upon, but facts before me,—I am not from that cause disposed to conclude—that he was more deficient in all the social virtues, than another; and will readily suspend my judgment, until the documents of them have been produced by his admirers,

admirers. Men frequently play characters abroad, and represent themselves at home. Virtue and vice are both engrafted, intimately, with human nature:—whichever of them is most predominant, will compress the other, and conceal it, from common observation.—It is, the busy zeal of a prying eye, which can alone discern, more than will be apparent,—upon the surface of every public character—and, that, of either extreme: David is known, to have been a man after God's own heart, yet, this pattern of excellence,—this paragon of virtue—has been discovered in a situation—tripping into vice: and Walpole has brought forward—anecdotes of virtue—and combined them with the history of Richard, the Third.

In many of the criticisms, perhaps, I may be told, that I have been over-nice: but my authority is strong. “What,” says Johnson, “is borrowed, is not to be enjoyed as our own, and it is the business of critical justice to give every bird his proper feather.”

Let fiction cease with life, and let us be serious over the grave.

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