

**Observations on the cancerous breast. Consisting chiefly of original correspondence between the author and Dr. Baillie, Mr. Cline, Dr. Babington, Mr. Abernethy, and Dr. Stokes / Published by permission of the writers. With an introductory letter to Mr. Pitcairn.**

### **Contributors**

Adams, Joseph, 1756-1818.  
Baillie, Dr.  
Cline, Mr.  
Babington, Dr.  
Abernethy, John, 1764-1831.  
Stokes, Dr.  
Pitcairn, Mr.

### **Publication/Creation**

London : T.N. Longman and O. Rees, 1801.

### **Persistent URL**

<https://wellcomecollection.org/works/unksdvqz>

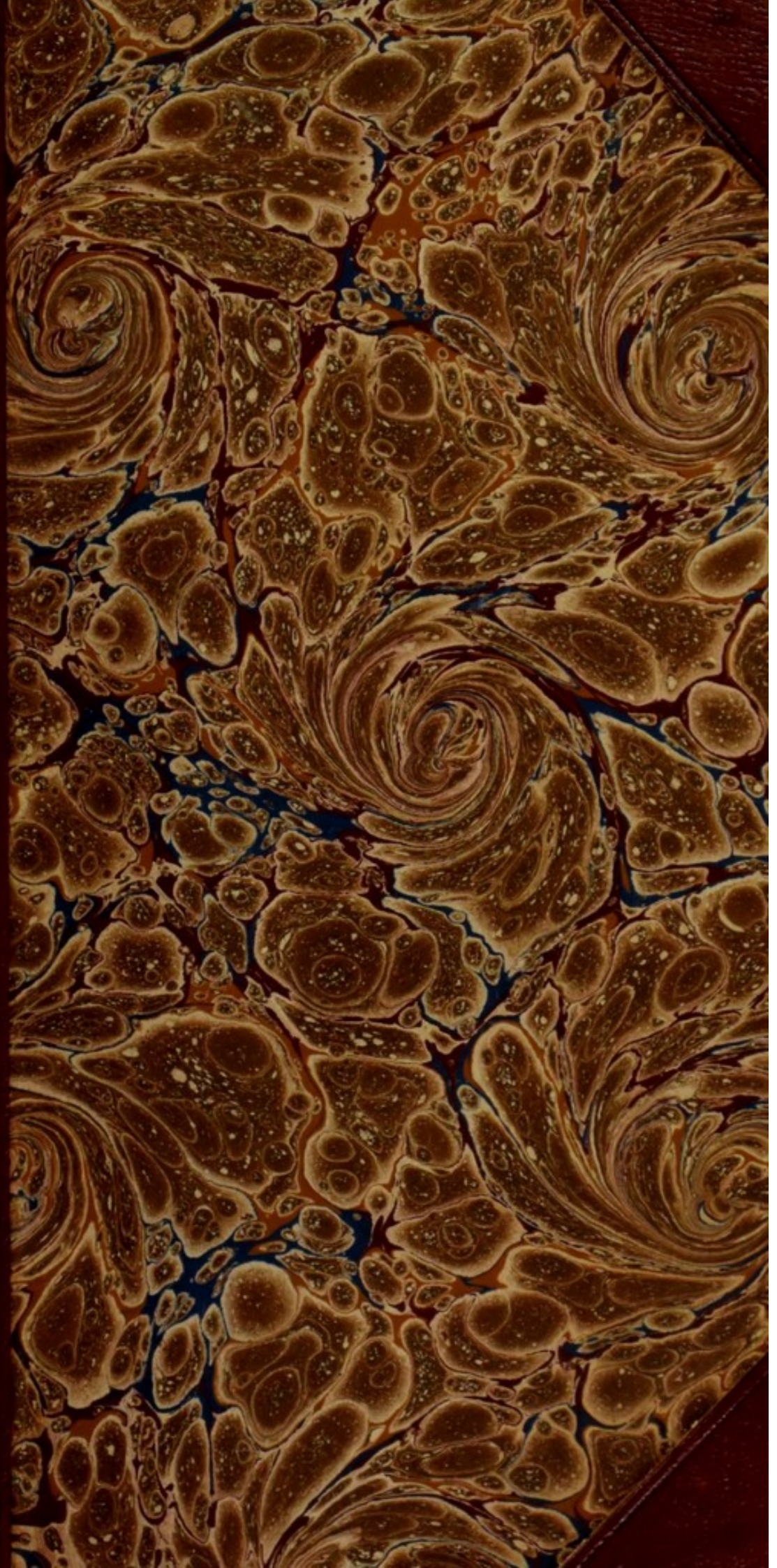
### **License and attribution**

This work has been identified as being free of known restrictions under copyright law, including all related and neighbouring rights and is being made available under the Creative Commons, Public Domain Mark.

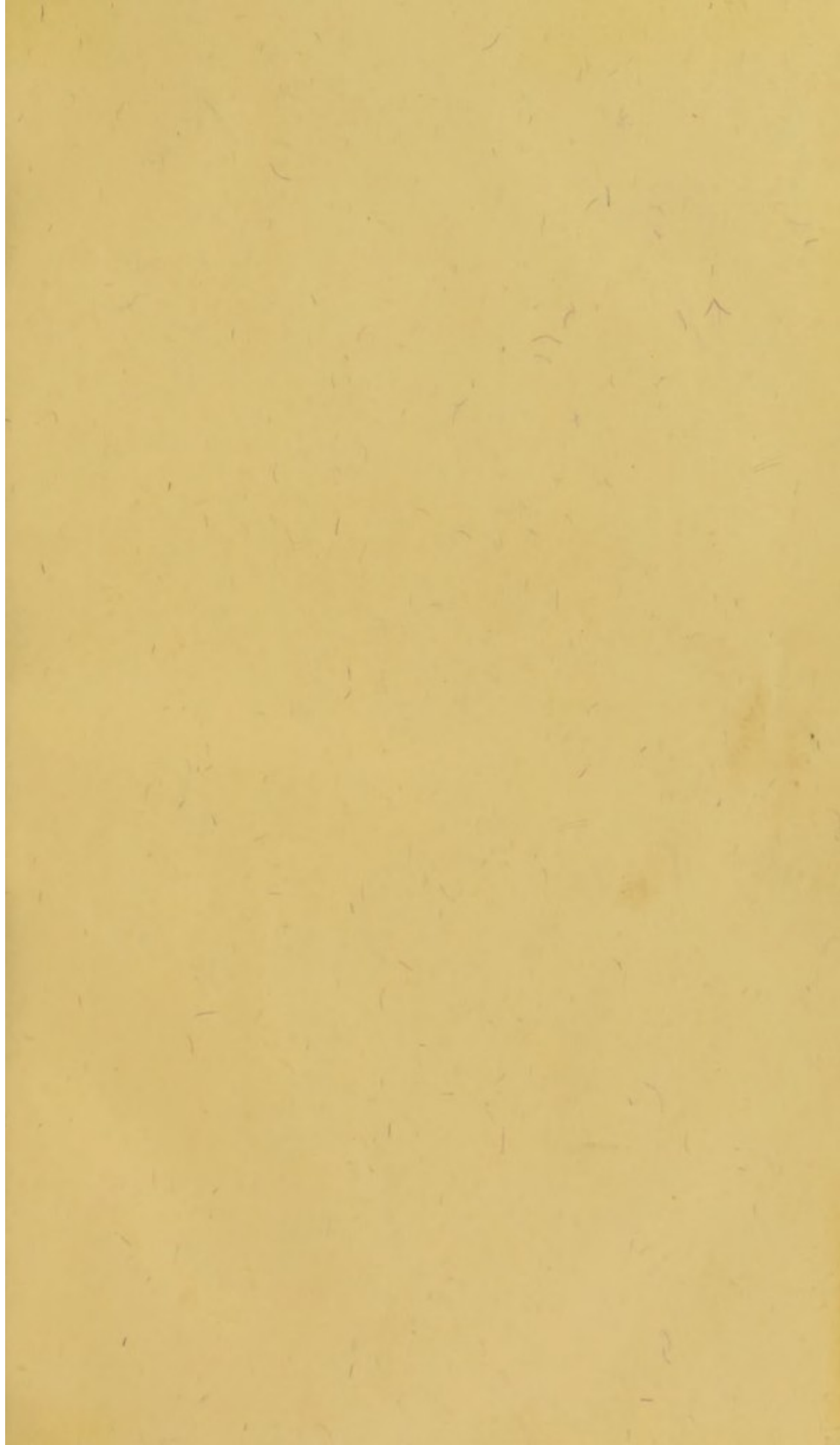
You can copy, modify, distribute and perform the work, even for commercial purposes, without asking permission.

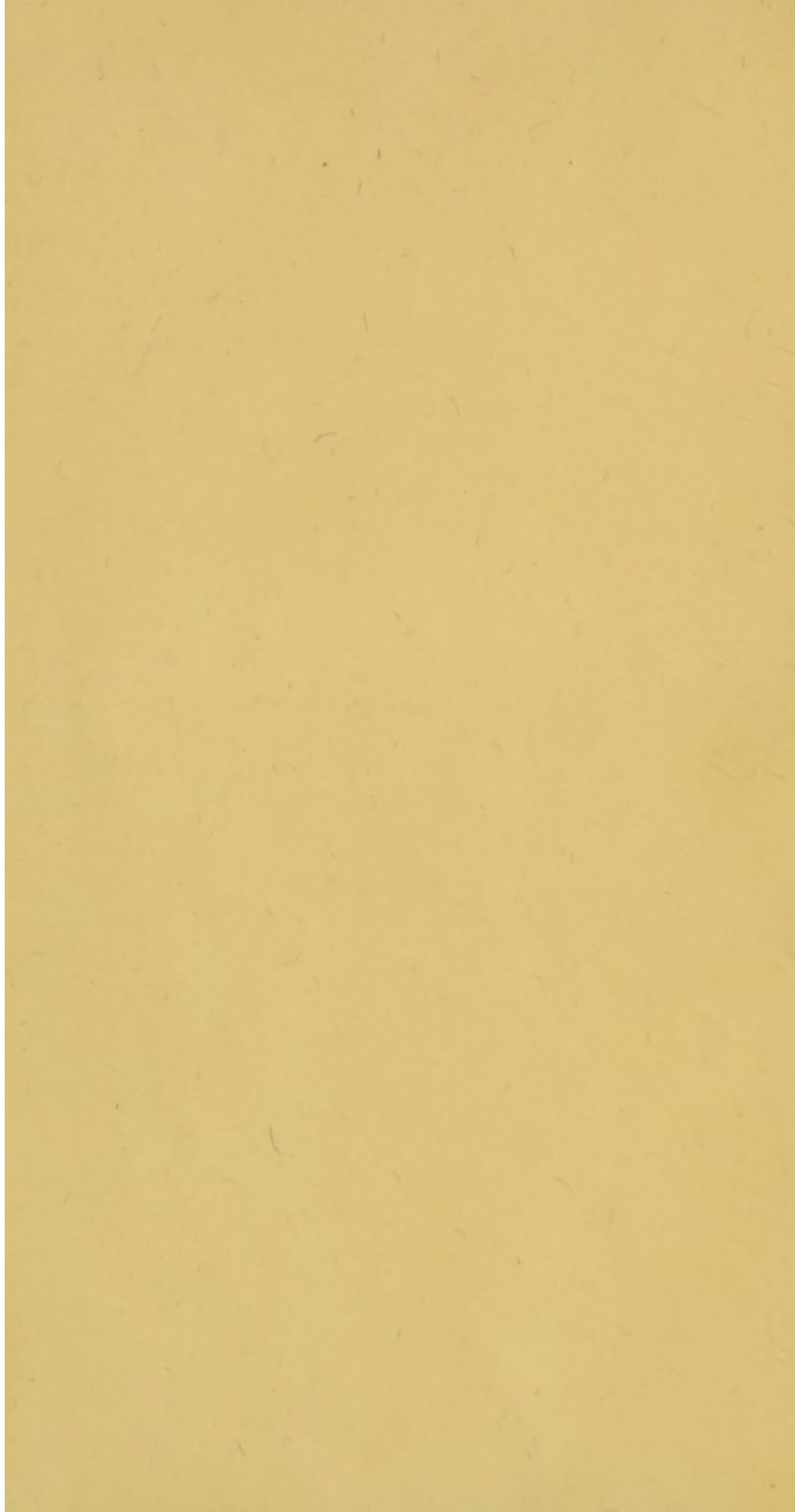
**wellcome  
collection**

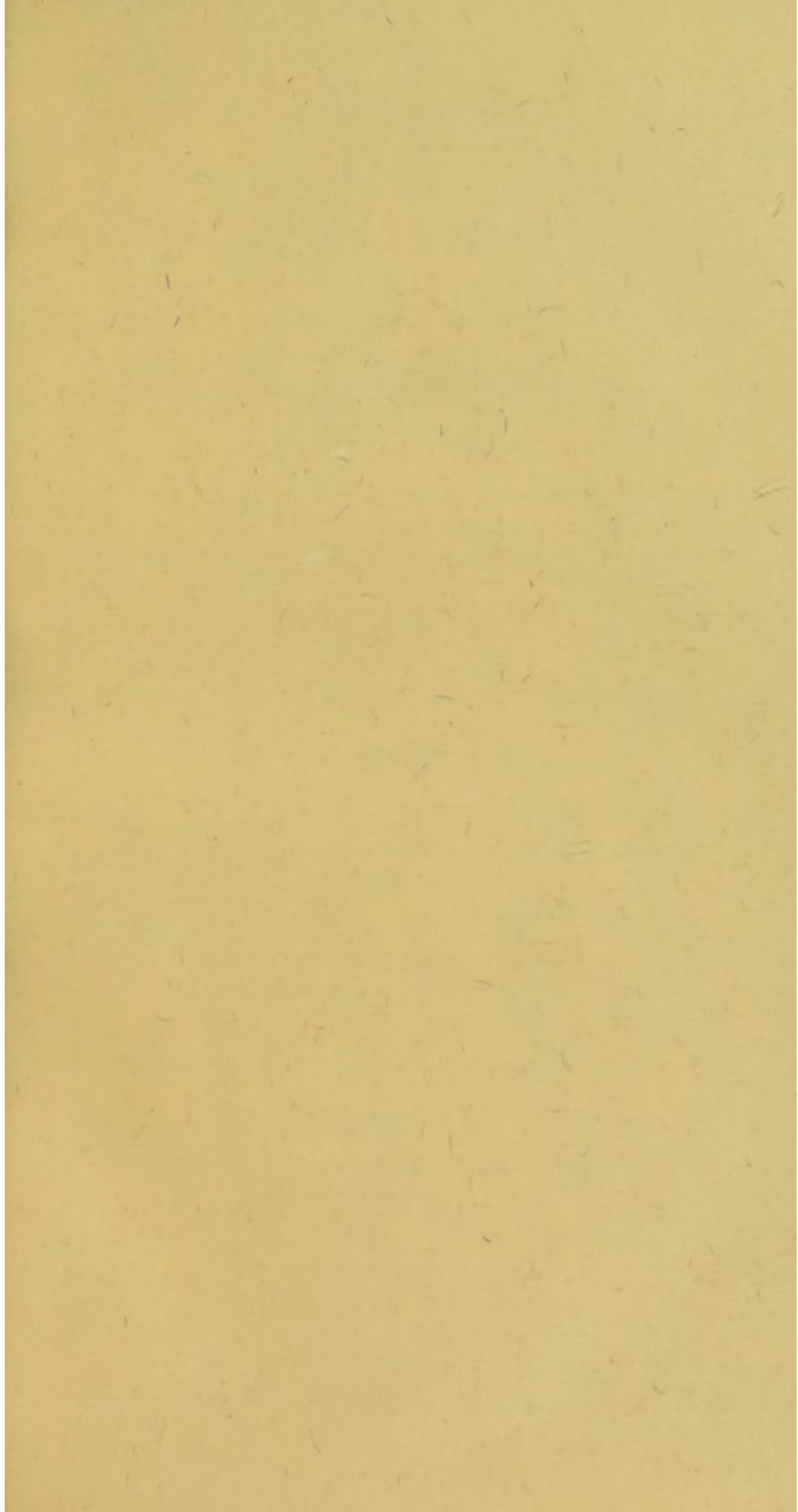
Wellcome Collection  
183 Euston Road  
London NW1 2BE UK  
T +44 (0)20 7611 8722  
E [library@wellcomecollection.org](mailto:library@wellcomecollection.org)  
<https://wellcomecollection.org>

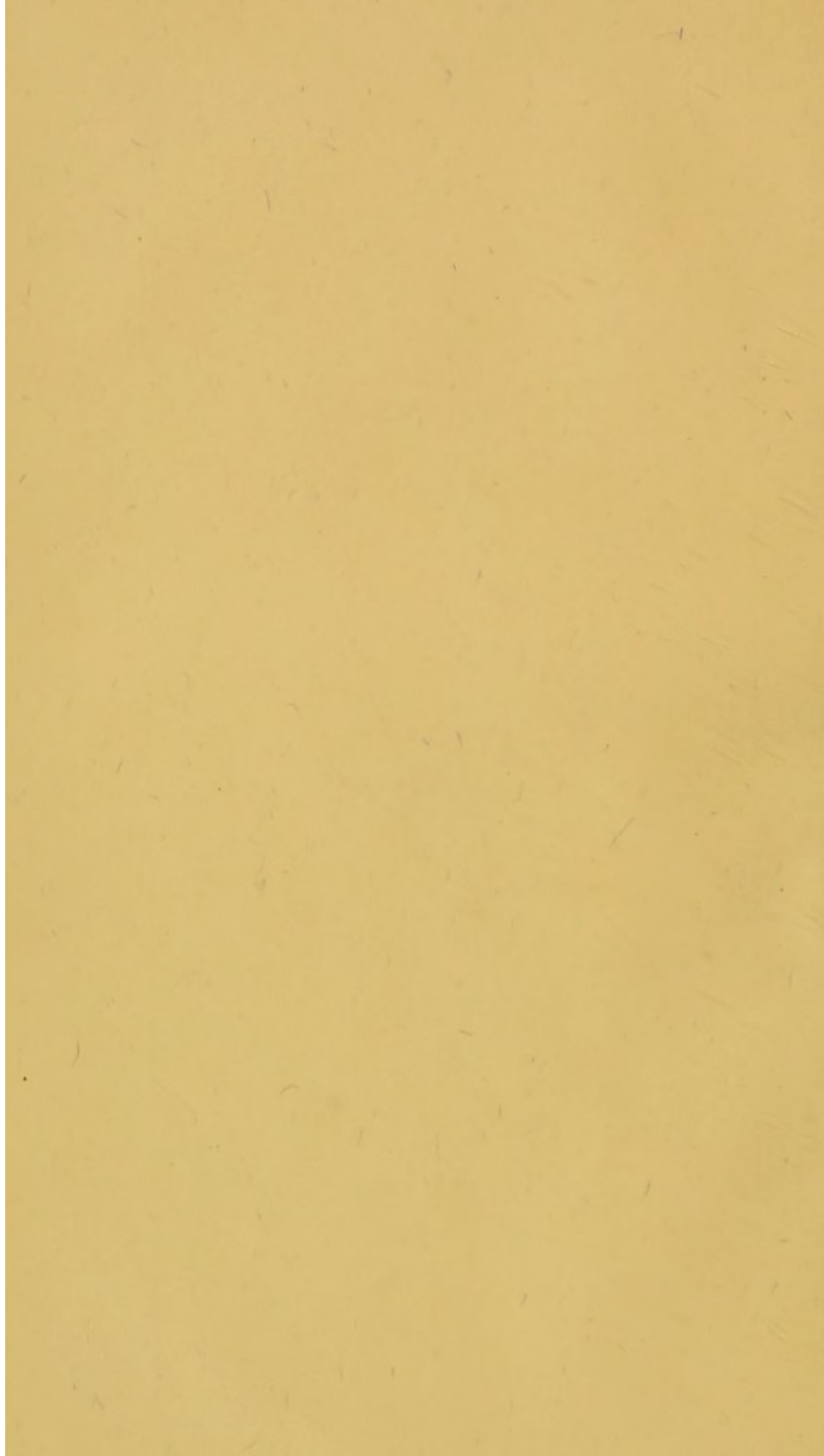


10282/B









XIV. C 54500

OBSERVATIONS  
ON THE  
CANCEROUS BREAST.

CONSISTING CHIEFLY OF  
*ORIGINAL CORRESPONDENCE*

BETWEEN THE AUTHOR AND

DR. BAILLIE,		MR. ABERNETHY,
MR. CLINE,		AND
DR. BABINGTON,		DR. STOKES.

PUBLISHED BY PERMISSION OF THE WRITERS.

WITH AN

INTRODUCTORY LETTER TO MR. PITCAIRN.

---

BY JOSEPH ADAMS, M. D.

OF THE ROYAL COLLEGE OF PHYSICIANS, AND PHYSICIAN IN THE ISLAND OF  
MADEIRA.

---

Παιησον δ' αἶθρην, δος δ' οφθαλμοισιν ιδεσθαι·  
Εν δε φρεσι και ὀλεσσον, επει νυ τοι εἶαδεν ἕτως.

HOM. IL. I. XVII. v. 646.

---

London:

PRINTED FOR T. N. LONGMAN AND O. REES,  
Paternoster-row.

1801.

By T. Gillet, Salisbury-square.



ORIGINATIONS

1860

CINCINNATI BRISTOL

THE COLLECTOR

OFFICE OF THE COLLECTOR

OF THE CITY OF CINCINNATI

AND OF THE COUNTY OF CLAY

AND OF THE STATE OF OHIO

IN THE YEAR 1860

PRINTED BY J. W. MASON

J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

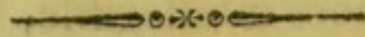
PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

PRINTED BY J. W. MASON

## ADVERTISEMENT.



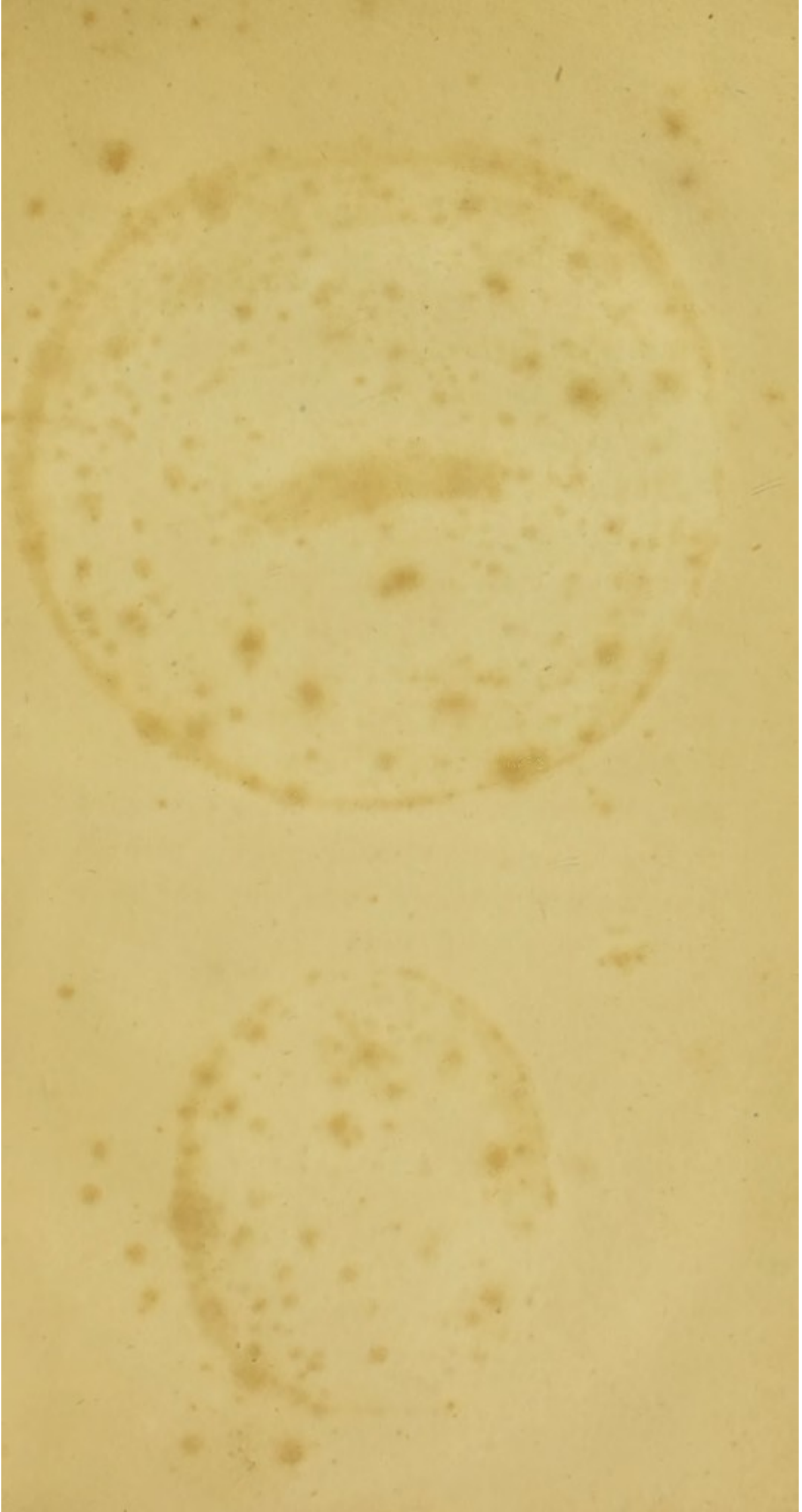
MORE than six years ago it was the Author's wish to call the attention of medical writers to a greater accuracy of language than has been in use amongst most if not all of them. It was generally admitted that the "Observations on Morbid Poisons" were not compiled without industry, and that attempt at accuracy which the public has a right to expect; but the severity of the language has been as generally disapproved. As there cannot be a doubt of the justice of a sentence which even the partiality of friendship reluctantly pronounced, it was not less the Author's wish than his duty in the present publication to avoid falling into the same error.

With this view he has taken pains to collect the opinions and answer the objections of men whose abilities are universally respected, and whose friendship it will ever be  
his

his wish to perpetuate. Besides the check this might prove to his language, it has given him an opportunity of placing a new set of opinions in such a variety of lights as must render them more intellegible, whether they be deemed conclusive or not.

Any apology to his correspondents would be superfluous, because no one can suspect it was the Author's intention to offer any thing more than hints to such men. The whole must be considered as written for the public; though for the reasons above mentioned the epistolary style is preserved.

All the arguments in favour of the separate existence of the hydatid being contained in Dr. J. Hunter's valuable paper, permission has been obtained to reprint and prefix it to the present work.

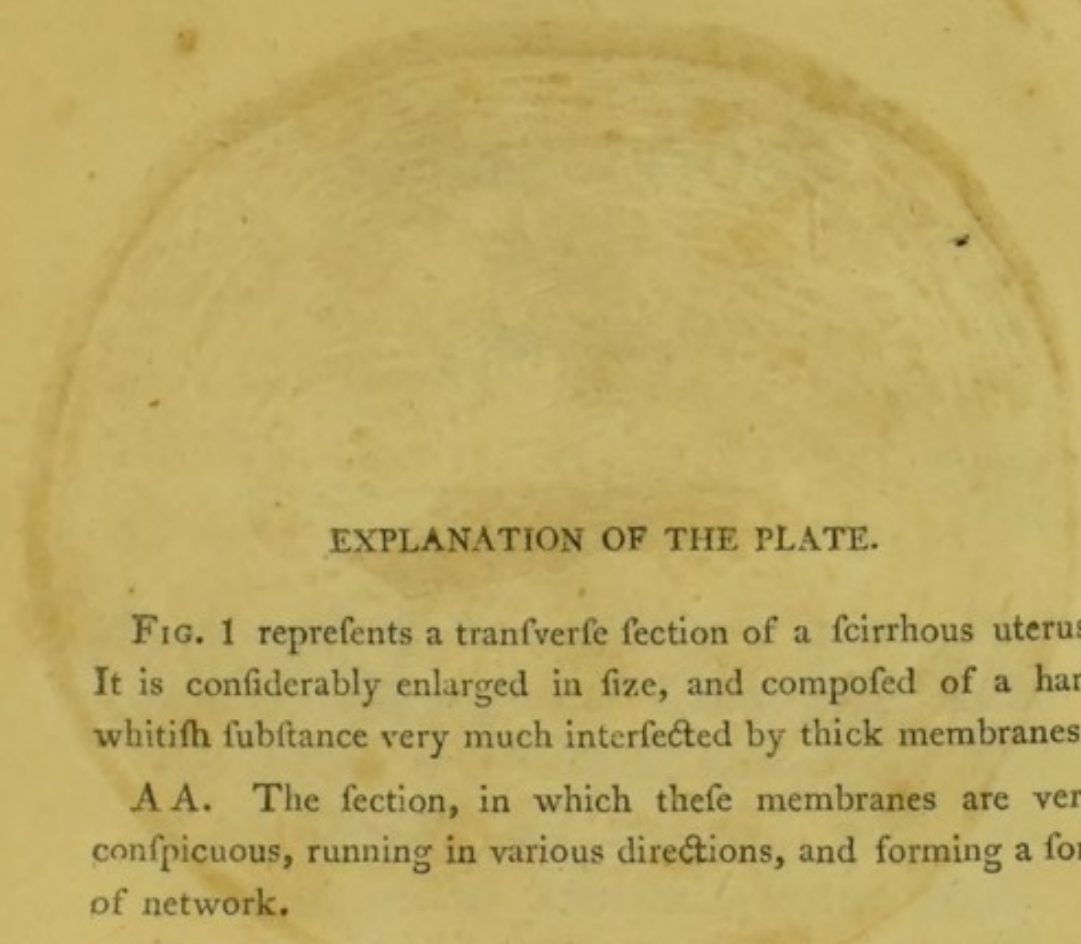


*Fig. 1.*



*Fig. 2.*





EXPLANATION OF THE PLATE.

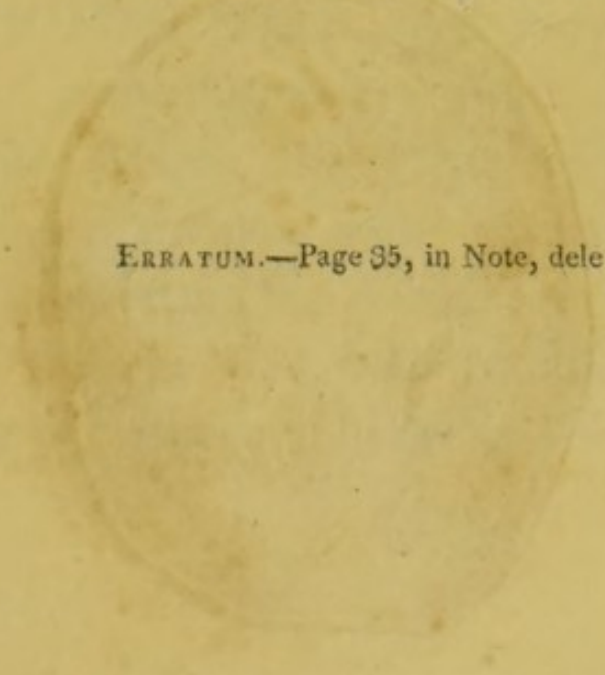
FIG. 1 represents a transverse section of a scirrhus uterus. It is considerably enlarged in size, and composed of a hard whitish substance very much intersected by thick membranes.

A A. The section, in which these membranes are very conspicuous, running in various directions, and forming a sort of network.

BBB. Three small oval tumours growing in this uterus.

C. The cavity of the uterus, a little rugged or irregular:

FIG. 2 represents a lateral section of a scirrhus prostate gland. In this too, similar thick membranes are observable intersecting the substance. They are probably nothing else than cellular membrane thickened by disease.



ERRATUM.—Page 35, in Note, dele H.

Faint, illegible text, possibly bleed-through from the reverse side of the page.

Second block of faint, illegible text, also appearing to be bleed-through.

## OBSERVATIONS,

Æc. Æc.

---

*An Account of the Dissection of a man, that died of a Suppression of Urine, produced by a Collection of Hydatids, between the Neck of the Bladder and Rectum; with Observations on the Manner in which Hydatids grow and multiply in the human Body. By JOHN HUNTER, M. D. F. R. S. and Physician to the Army. Read April, 17, 1787.*

**T**HOMAS BELL, aged forty-six years, a carpenter, and a stout man, died suddenly, March the 17th, 1786. The following particulars were all that could be learned respecting his disease previous to his death: that he had been complaining for four or five weeks of more or less pain and  
B difficulty



difficulty in making water, which he himself supposed to proceed from gravel, but by those about him was suspected to arise from some venereal complaint. In consequence of this suspicion he was visited by a surgeon, who examined the penis, and found a natural *phimosis*, but no venereal complaint. When questioned respecting his difficulty of making water, he said, that he had been in much pain, but was now easier, and that some urine had come away involuntarily. He was able to sit up in bed at this examination, but in an hour after, in attempting to turn himself, he expired.

The body was examined thirty hours after death.

*Head.*—The brain was in all respects natural; it was not firm, however, for so short a time after death. The water in the ventricles was in the usual quantity. The blood in the vessels was fluid both in the head and other parts of the body.

*Thorax.* The viscera were all found.

*Abdomen*

*Abdomen.*—The belly was very tumid, which, on removing the common integuments, was found to proceed from the immense size of the bladder. It was distended enormously, and reached fully eight inches above the *pubis*; its fundus was within two inches of the arch of the *colon*. Upon letting out the water, which amounted to five or six pints, it appeared that there was a large tumour between the neck of the bladder and the *rectum*, which completely filled the pelvis, and thrust the bladder forwards and upwards. On cutting into the tumour much water rushed out, and along with it many hydatids of various sizes; the largest was about an inch and an half in diameter, and the smallest not larger than a pin's head. The tumour was intirely filled with hydatids and the water that surounded them, and both together they were more in quantity than a pint and an half. There were besides two or three smaller tumours near the neck of the bladder, also containing hydatids; and there were two bodies, not larger than common beans, adhering to the bladder, containing a soft cheefe-like substance.

Between the stomach and the spleen, and over one end of the pancreas, there was a large tumour, to which the three above parts adhered; the stomach and pancreas slightly by cellular membrane; the spleen more intimately, so as to make a part of the tumour: with the spleen it was about ten inches in diameter. It was irregularly shaped, and made up of several smaller tumours. There was considerable variety in the contents of those tumours; in one there were hydatids of various sizes, like those mentioned above; in another there was a substance like isinglass, a little softened in water; in a third there was clear water in a considerable quantity, with very minute particles, like small grains, adhering slightly to the sides; and in a fourth there were hydatids, some full, others burst, and with their coats compressed together, and forming the isinglass-like substance. The tumours or sacs had all thick coats, endowed with a strong contractile power, that forcibly protruded their contents through any opening made into them. They had two coats; an outer, which was strongest and thickest, and an inner, which was tender, soft, and pulpy.

As to the structure of the hydatids, it was the same in large and small; a transparent bag, uniformly round and smooth, filled with clear water. The bag appeared to consist of two coats, or layers; for on handling them, the outer coat would get rumpled, and occasion a degree of opacity, but by wiping the hydatid it became again clear and transparent. They appeared to be completely spherical, except that the large ones were a little flattened by their own weight when laid on a plate. They adhered no where to the sides of the sac, nor to one another. When they were opened, their coats possessed a strong contractile force, so as to roll themselves up in part. On examining a number of hydatids, some of them appeared of an amber colour, and with thicker coats than the rest; and when opened, their inner surface was found covered with small hydatids, which were not so large as the heads of pins, and looked like minute pearls or studs set in the inner coat.

Some of the water containing the small grains mentioned above, was examined with a microscope, and found to have floating in it numerous minute hydatids, of which the largest were the little grains visible to the naked eye and a two hundredth part of an inch in diameter; the smallest were less than a red globule of blood, and they were of all intermediate sizes. The coats of the largest were a little rough with numerous filaments, or *villi*; and on using a deeper magnifier they had somewhat of a mulberry appearance.

When the young ones growing in the coats of the larger were examined with the microscope, they were found not to be set in the coats, like pearls, but to be covered by a thin transparent membrane, so as to lie between two layers. It is not improbable that the small globules attach themselves by the *villi* to the side of the hydatid and to each other, and thereby give the appearance of being covered by a thin membrane. However that may be, the globules being found

of various sizes floating in the liquor, seems to prove that they are originally formed there, and not in the coats of the hydatid, upon which they are afterwards deposited. The number of those that had young ones in them, was few in proportion to the others.

The hydatids in their growth and decay appear to pass through various stages; they are first found floating in the fluid that fills the hydatid, and afterwards attached to its coats. The hydatid thus pregnant with young, if the expression may be allowed, adheres to the neighbouring parts, increases in size, and becomes itself a sac, containing numerous small hydatids. These after a certain time decay, and the skins or empty bags are squeezed together into a substance like isinglass. It is probable they still undergo a further change; two small bodies of the size of the common bean, of a cheese-like consistence, and covered with a skin, were taken notice of adhering to the bladder near its neck; it may be a question whether those were not the remains of hydatids? but that must be determined by future observations.

It is to be observed, that the young hydatids are found in two very different stages ; in the one they are attached to the coats of an hydatid, that float loose in the parent bag or sac ; in the other, extremely small globules adhere slightly to the inner surface of a bag or sac, which is firmly attached to the neighbouring parts, and covered with a strong outer coat. It is obvious that the progress of growth is very unequal in those two, and indeed inverted ; for in the first the young ones are as large as the heads of pins, while the parent bag is not larger than a walnut, and floats unattached ; but on the contrary, in the second there is a large sac with a strong outer coat, and more tender inner one, adhering strongly to the surrounding parts, while the young ones that are very slightly attached to its sides, are not of a larger diameter than an hundredth part of an inch. Whether those are merely accidental differences in the growth, or depend upon some more essential distinction must remain to be determined by future observations.

The

The term *hydatid* is used in different senses; thus the hydatids of the *placenta*, of the *ovaria*, and of the kidneys, which may be considered as morbid changes in the substances of those bodies, are totally different from the hydatids of the present case, which produce their like, and multiply in great abundance, without any further connection with the human body, except in so far as it affords them a nidus. The observations I am going to make are confined intirely to this last mentioned kind. They have been found in various parts of the human body; they have been discharged both by vomit and by stool \*; they have been brought up from the lungs by cough †; they have been voided by urine; and they have been discharged from tumours in different parts of the body ‡. Le Cat has taken notice of their inner coat being covered with small *mamillæ*, an appearance produced by the young ones, and supposes them to be the

\* Philosoph. Transact. Vol. xxii. p. 1797,

† Med. Transact. Vol. ii. p. 486.

‡ Philosoph. Transact. Vol. xxv. p. 2344.



glandular grains of the spleen distended with lymph. They have also been considered as diseased lymphatics; but it must be obvious, that the young ones growing within the larger, at the same time that these last are floating in a liquid without connection with the sides of the sac, cannot proceed from any disease in the lymphatic system, if by that be understood the absorbent vessels. It is unnecessary to animadvert upon Le Cat's opinion, as anatomy has not hitherto been able to demonstrate the existence of any glandular grains in the spleen, which appears to be a congeries of blood-vessels. It may be a question whether the hydatids are not of an animal nature, and possessing a life peculiar to themselves? We are not yet in possession of a sufficient number of facts to ascertain this point; but the discoveries that have been made respecting the nature of hydatids in other animals, promise to throw some light upon the present subject.

The ingenious naturalist Pallas, in his *Miscellanea Zoologica*,\* has given an account

\* Hagæ Comitum, 1766, p. 157.

of the observations made by others upon hydatids, and added discoveries of his own. There are only two before him, who knew the hydatids in animals to be alive, and to possess a peculiar structure and power of motion. Those were Dr. Tyson and Phil. Jac. Hartmannus. Pallas considers Tyson as the first discoverer, but seemingly without reason, for Hartmannus published his account in the year 1685;\* and Tyson in 1691.† Neither of them appears to be acquainted with the other's discovery. Tyson observed the neck and mouth of the animal, and saw them in motion; Hartmann further saw the whole body in motion by putting them in warm water. Pallas has examined them very minutely, and gives them the name of *tænia hydatigena*, from finding their heads of the same structure as that of the *tænia*. But in all that Pallas saw he never found any pregnant, so that he was unacquainted with the manner in which they multiply. Goetze, a German writer, has given figures of the young ones adhering to the parent bag; and

\* Miscell. Na. Cur. Dec. 2. An. 4to.

† Phil. Transf. for that year.

Mr. Hunter has preparations of that kind in his collection. The Abbé Fontana\* has observed them in sheep, seen the motion of the young ones adhering to the sides, and also with a microscope examined the heads of them, and found them resembling those of *tæniæ*. So far the observations of authors go with respect to the hydatids of animals; in regard to the hydatids of the human species, Tyson has remarked that they are different from the former; that they are uniformly round, without any appearance of a neck or head; but he declines giving any opinion of their nature. Pallas † likewise mentions a kind of hydatids found in the human and other bodies, without any neck or mouth, but leaves their nature to be decided by future observations. He says their coats are very elastic, and when cut retract with so much force as to roll themselves up, turning the inside out.

It is not altogether allowable to conclude, from the resemblance of the human hyda-

\* Opuscoli Scelti, tom. 6. † P. 172.

tids to those of quadrupeds, that the first as well as the last form a part of the animal kingdom: yet they agree in so many circumstances, that such a conclusion appears very probable. In quadrupeds they have their seat generally in the *abdomen*, and most commonly in the liver or spleen; the same is true of the human hydatids. In the human they multiply by the young growing on the inside of the hydatids; and the same has been observed in quadrupeds. The analogy fails when we look for a mouth and neck in the human; nor has any peristaltic, or undulating motion been observed in their coats; but this last appearance has not been looked for in a proper way, that is, by putting them in tepid water as soon as they come from the human body.

With regard to the time requisite for the growth and decay of the different sacs containing hydatids, it is difficult to form a conjecture. Our patient was able to follow his trade as a carpenter till five weeks before he died; it is probable therefore that in that time or a little more, the sac between the neck of  
the

the bladder and rectum had grown to that size, which produced a suppression of urine and death. The growth and decay of the hydatids serve to explain the increase and diminution, which have been observed in tumours of the abdomen proceeding from this cause, as in the case mentioned in the Medical Transactions.\* It will not be deemed out of place to mention some particulars of a dissection that have come to my knowledge, that in a great measure explain the appearances in the case above alluded to, and confirm the truth of the conjecture subjoined to it.

A patient died in the Infirmary at Edinburgh, with the symptoms of the encysted dropfy. His abdomen was swelled with many irregular protuberances. On examining the body, there were found numerous encysted tumours full of hydatids. They had their basis in the liver, from which they proceeded not only downwards into the abdomen, but upwards into the thorax, and

\* Vol. II.

making their way through the diaphragm, came in contact with the lungs. Had the patient lived a little longer, it is not improbable that a communication between the bags containing the hydatids and some of the branches of the *trachea arteria* would have taken place, which must have produced the same symptoms as in the case abovementioned.

To return to the dissection. The hydatids did not appear to have done any injury to the parts, except by mechanical pressure, for the viscera were all uncommonly found. Of the numerous cases of hydatids related by writers, it may be remarked that hardly any of them proved fatal whenever they found an outlet. In the present case, the pressure upon the neck of the bladder, and consequent suppression of urine, are evidently the causes of death. The manner of death from such a cause is not unusual; after a time, the pain produced by the distention of the bladder ceases, there are no more efforts to make water, and yet the patient dies suddenly. This sudden death

has

has been imputed to a tranflation of the urine, as it has been expreffed, to the brain. If any idea is to be annexed to thefe expreffions, it muft be, that the urine is effufed either in the ventricles, or upon the furface of the brain; but no appearances of that kind were obfervable in the prefent cafe.

When the prefence of hydatids is fufpected, it would appear to be of great confequence to procure them an outlet; but it muft be obvious, that being generally feated in the abdomen, that bufinefs muft be left intirely to nature. Upon the fuppoftion that they belong to the animal kingdom, no internal medicine promifes to be of fo much ufe as mercury, which appears to have been given in one cafe with advantage.\* But this is a fubject there is no entering upon without a greater number of facts.

To this account I will fubjoin a query refpecting the manner in which the hydatids came to be lodged between the rectum and

\*Med. Tranf. Vol. II.

bladder. It has been observed that they are most commonly found in the liver and spleen, and in the present case their original seat would appear to have been in the last of these viscera; may not, however, one of the sacs or bags in the spleen have burst, by which the contents would be spread all over the abdomen, and from their own gravity would naturally fall into the pelvis; and may they not have adhered to the neighbouring parts, and so multiplied there?

I have been more particular in relating the circumstances of this case, because such seldom occur even to persons in extensive practice; in consequence of which it often happens in rare cases, that the views and experiments suggested by one case, are of no avail, for want of another opportunity to verify them. It will therefore be understood, that what is advanced respecting their being endowed with a life of their own, and also their mode of breeding or multiplying, is given merely as probabilities, and well deserving of future investigation, whenever opportunities may offer.

*Charles-Street,*  
17th April, 1787.

C



## SUPPLEMENT.

IN the year 1788, I had an opportunity of examining some hydatids that were found in the abdomen of a sheep. Before I saw them the bowels had been taken out, and they were adhering to the fat about the kidneys, and also to the liver, and to the fat near the urinary bladder, in considerable number. They would appear to differ in some essential particulars from the human hydatids, yet they resemble one another in so many things, that there is reason to infer therefrom, that both belong to the animal kingdom. It appeared to me, that a short account of them would form a proper supplement to the case of human hydatids, read some time ago to the society.

The hydatids in the sheep were exactly the same with those described by Tyson.\* They consist of a mouth, neck, and oblong

\* Phil. Transf. 1692.

spherical

spherical body. The mouth had nothing of the cruciform appearance, if I may be allowed the expression, that late writers have made the characteristic mark of *tænia*, and which they say is to be found in all hydatids. The mouth examined with some care with the microscope, appeared to be a simple longitudinal aperture. The neck was composed of rings, and there appeared very fine circles surrounding the body. They varied in size, from that of a chestnut to the dimensions of a turkey's egg. When put in warm water, though it must have been twelve or fourteen hours after the sheep had been killed, they moved briskly, with a kind of peristaltic motion all over the body. Each hydatid was lodged in a separate sac, which was little more than sufficient to hold it, for the neck was reflected upon the body. The sides of the sac were lubricated with a mucous fluid.

The human hydatids have no neck or mouth, and they are in great numbers in the same sac, and of various sizes. In these particulars they differ from those of sheep;

nor could I find any young ones attached to the inside of the hydatid, as in the human. In their mode of decay, however, they resembled each other completely. The sac became thicker and stronger, and at the same time diminished in size, and compressed the coat of the hydatid into a substance like isinglass. In the last stage this substance became like a mixture of chalk and water, and the side of the sac hardened, and appeared in some degree petrified; for the hardness was more like that of a stone, than of a bone.

I had, in the summer of 1791, an opportunity of examining the hydatids which are found in the brain of sheep, producing the disease called, in some parts of the country, the *staggers*. The hydatid is lodged in the substance of the brain; in one sheep there were two hydatids, one in each hemisphere of the brain; they were of an irregular oval shape; they had no mouth; their coats had the same appearance as in the hydatids found in the abdomen; and when put in warm water, they had a strong peristaltic motion.

In

In some there were clusters of young ones adhering to their inner coats. These were somewhat oval in their shape, and adhered by one end; but on detaching them carefully, and examining them with good magnifiers, I could never find the cruciform mouth described by some writers.

We may observe, that the mouth is not essential to the hydatid in sheep, which renders it still more probable, if not altogether certain, that the human hydatid is an animal.\*

\* There being great reason to believe that hydatids form a distinct genus in the animal kingdom, and being so essentially different from the watery vesicles which are found in the *ovarium*, kidney, and *placenta*, it might be proper, in order to avoid ambiguity and confusion, to distinguish them by a name peculiar to themselves. Linnæus (a) calls the hydatid in sheep *Hydra Hydatula*; but neither his generic character, nor specific differences, which are chiefly taken from the mouth, apply to the animals in question, in some of which there is no mouth. The Greek language, that inexhaustible source of derivation, readily supplies us with a name. The word ὕδρια, *Hydria*, is very apposite. If that be adopted, we shall have *Hydria Humana*, *Hydria Ovilla*, &c.

(a) *Systema Naturæ*, Vermes, Zoophyta, Hydra.

# LETTERS,

Éc. Éc.

---

## LETTER I.

TO JAMES PITCAIRN, ESQ.

DEAR SIR,

Madeira, 1 Nov. 1796.

**T**HUS far we have been attending to the subject of digestion, only as it concerns man and such other animals, as prepare their own food. To these it seems necessary that their food should be dead before their powers can produce any change in it. Your attention must now be directed to a set of beings in whom neither gastric juice, nor any other digestive organs can be discovered. Since, therefore, they must derive their whole support from absorption, it is obvious they can only exist in living animal matter.

Intestinal worms, though incapable of living after the death of the animal that supported them, are in many respects more complicated

complicated than those we are now to consider. The most simple idea of animal life we can well form is that of the hydatid, consisting only of a membranous bag containing a transparent fluid. It might be doubted, whether such should be called animals, were it not that those found near the kidneys of sheep have been seen to move. They are also described with a head and neck. Those in the human body give no other proofs of life, than a contractile power.

Such a conformation is more simple than that of plants, most of which have not only a power of converting air and water into their own substance, but parts differently formed for various functions; and many of them even generative organs, with a distinction of sexes. But in the human hydatid nothing is found, excepting a hollow membranous sphere, filled with a coagulable fluid, which, for its support, has no other power than that of absorption, and which multiplies without any generative organ that can be discovered.

Dr. John Hunter, in a paper inserted among the memoirs of a society for the improvement of medical and chirurgical knowledge, has given the history of this animal from its birth to its death. Of its origin we shall probably for ever remain ignorant. While it continues to advance in size, both membrane and contents are almost transparent. In its decline the membrane loses something of its transparency, and its contents become of an opaque appearance and an amber colour. After this the whole is converted into a substance resembling isinglass. Sometimes the containing sacs become calculous, and the hydatids resemble chalk and water.

There is much variety in their appearance, even before any change has taken place, that should indicate an approach towards decline. Sometimes the substance of the liver is almost wholly occupied by a single large hydatid, the growth of which has been gradual, and its life long. At others, we find numbers of them floating in a fluid contained in one common sac, many of the  
 same,

same, and others of different sizes, and apparently of different degrees of maturity.

It should check the pride of the professors of our art, that however successful the wits may call us in killing, we are unable to destroy the life of this destructive animal.

This letter, though very long, is much shorter than it would have been, had you not kept such good company. For that reason I say nothing of the *living*, waiting to be instructed by you on all that is going on at home. But while I wish you not to overlook the *dead* [referring to some remarks on Sydenham on the Small Pox, and Hunter on the same and on Digestion] the few I have offered may not have occurred to those who are so much better employed. *Optumus quisque facere quam dicere mavult.\** I trust, however, that by this time you are convinced some advantage is to be derived from reading; and also, that such advantage must be purchased by the closest attention on your own part.

\* Sallust. Bell. Catil.



As the gentlemen, to whom the rest of the packet is addressed, have given me leave to publish this part of our correspondence, you will have an opportunity of perusing it at your leisure, and of favouring me with your remarks. In these treat me with as much severity as you please; but make yourself master of the whole question, and examine all the references before you state your objections. Do not, however, let this prevent your writing; assuring yourself, that besides the medical intelligence I expect from you, &c. &c.

JOSEPH ADAMS.

LETTER

LETTER II.

TO DOCTOR BAILLIE.

August 20, 1796.

LET me first thank you, my dear Sir, for the terms in which you permit me to print our correspondence. At a distance from science, from hospitals, from dissecting rooms, and often from books, to be thus remembered by those who luxuriate in all these delights, may for a time renew those scenes we have reluctantly left, and make us anxious to protract the charming illusion.

I know not whether I should most admire the readiness with which you forgive an unprovoked harshness of language, or the frankness with which you admit the truth of a remark.\* It would, at least, have be-

\* "Were my book to come to a second edition, I should make a distinction between these two diseases, and should mention, at the same time, that you have very properly taken notice that they ought to be distinguished." *Extract from a private letter from Dr. Baillie dated July 30, 1796.*

See also *Morbid Anatomy*, 2d edition, 1798, p. 366.

come me to observe, that in you only have I discovered what appears to me a true description of carcinoma—masses of various complexions, intersected with strong membranous fibres, with a cartilaginous structure—sometimes cells, containing a sanious fluid, with fungus shooting out after ulceration.\* As your only object was to describe appearances, I know not how it could have been done with greater perspicuity or brevity. The task remains for me to trace the progress, and if possible the laws, by which these changes are produced.

Perhaps I am bolder from having seen less than yourself, or, as I would willingly suppose, from having examined carcinomatous tumours immediately after their removal in the living subject, while you have been principally conversant with the dead.

The object of your enquiries too was chiefly the viscera: hence the uterus has

\* Morbid Anatomy, first edition, chapters on the stomach, rectum, intestines, testicles, uterus, &c. &c.

more commonly fallen under your observation than the breast or testicle, which may be removed during life. It is from the frequent observation of these immediately after their removal by an operation, that I conceive I have discovered what has been overlooked by others, or drawn conclusions so satisfactory to my own mind, as to authorize their publicity.

On the cancer uteri I have little to add, but the double satisfaction of finding, that I have neither overvalued my own accuracy, nor your candour. It was far from being my intention to hint that the sloughing phagedænic ulcer of the uterus is the only cancer of that viscus.

You say, you cannot help thinking, "that the term hydatid should be confined to a semiopaque white bag containing a fluid capable of being coagulated, and often smaller hydatids; all other cysts you think should have a name expressive of their contents."\*

\* Extract from the abovementioned letter.

The only apology I can make for myself is, that cancer was no part of the original design of my former publication, nor did it seem to come within the pale of morbid poisons. But as some morbid poisons had been called cancerous, and as every cancer was generally considered a morbid poison, it became necessary to devote a chapter to the subject, and desirable to compress an extraneous article into as few words as possible. This is certainly not a sufficient apology for obscurity and terms ill defined. Be assured, Sir, I have suffered enough for it. Those few pages have cost me more trouble to explain, than to defend all the rest of the volume.

It is this, and the importance of the subject, that induce me again to appear in print; and no better method occurred of making myself intelligible, than by collecting the objections of such of my correspondents as have the best means of information, and publishing them with my answers. After having for years examined a subject in every possible light, and fancied ourselves master  
of

of every part, it is extremely difficult to bring the mind back to that state in which only it is possible to be aware of the difficulties of explaining to others; or in fewer words, it is extremely difficult to treat that as new to others, which has been long familiar to our own minds. I ought to add, that what we conceive a discovery of our own is carested with such fondness, that scarcely the most pointed remarks of others are sufficient to convince us of any defect.

Thus, you see, I challenge your most unreserved objections, and, if I know any thing of myself, shall respect you the more in proportion as you are the means of rendering me the more fit to meet the public eye.

I remain, Dear Sir,

Yours most thankfully,

JOSEPH ADAMS.

LET-

## LETTER III.

FROM DOCTOR BAILLIE TO THE AUTHOR.

DEAR SIR,

I THANK you for the very candid and friendly letter, which I received from you a few days ago. I will state to you very shortly what has hitherto fallen under my observation relative to scirrhus structures, and you may make any use of it you think proper in your publication.

In parts which have become scirrhus, I have commonly observed the structure to consist of a very firm light brown substance, intersected by membranous or ligamentous septa, which run in various directions. The membranous septa are more numerous, and of greater thickness in some cases than in others. There is occasionally mixed with  
this

this structure a cartilaginous substance. The whole structure I have sometimes known to be cartilaginous, resembling very much a piece of common cartilage which had been previously rendered soft by being steeped for some time in a dissolving fluid.

Ulcers are often formed in scirrhous structures, and fungous excrescencies occasionally grow from them. Cyfts containing a kind of serous fluid are sometimes found in scirrhous structure; but they seem to me frequently wanting. They occur, I believe, most commonly in the breast and testicle, and these glands in a scirrhous state I have had few opportunities of examining. From what I have observed, I should be inclined to believe, that cyfts are only sometimes formed in a scirrhous structure, but are not essential to it. In this, however, I may be mistaken; and it may be found by a more minute observation, that the formation of cyfts always constitutes a part of a scirrhous structure. If you should be able

D

vation



vation about the nature of scirrhus, it will give me very real satisfaction.

I have known a substance which possessed the common characters of scirrhous structure to be converted into a kind of bony matter. In this, I believe, that the earthy part will be generally found to be in a larger proportion to the animal part than in common bone. Muscular and membranous parts I have known to be affected with scirrhus, as well as those which are strictly glandular. A fatty membrane I have seen affected with the same disease. The fat was almost as hard as a piece of gristle.

Structures which appear to the eye to be of the same nature, and which would commonly be called scirrhous, differ very much from each other in their disposition to run into ulceration. These ought perhaps to be distinguished from each other, and to be arranged in separate classes. This is a very short account of what it has occurred to me to observe about the nature of scirrhus and cancer.

Respecting

Respecting the propriety of the term cancerous hydatid, I have only to remark, that if you can prove the cysts in a scirrhous structure to possess a life peculiar or proper to themselves, like the hydatids found in the liver, there would seem to be no impropriety in the term. But if they possess merely a living principle similar to that of the ordinary diseased structures in an animal body, I should think it better for you to adhere to the name of cyst. This, however, is merely an opinion; and the name of any thing is of little consequence, if its real nature be fully explained.

I have enclosed with this letter a drawing which represents the surface of a transverse slice of a scirrhous uterus.\* It will point out the general appearance of the membranous septa which are so frequent in this sort of structure. There is also a drawing of a slice from a scirrhous prostate

\* See Plate II.

gland, and which shows the same circumstances. These, I thought, would convey a more distinct idea than any verbal description.

I remain, dear Sir,

Yours faithfully,

M. BAILLIE.

*London, November 10, 1796.*

## LETTER IV.

TO DOCTOR BAILLIE.

MANY thanks, my dear Sir, for your ready answer to my last ; and still more for the full account of your observations, which at the busy period of their date must have been inconvenient to you. They are such as I could not but expect from your accuracy ; and it gives me pleasure to find, that allowing for the difference between the dead and the living subject, there is nothing in them that contradicts what it is my wish to offer.

As you admit the term hydatid, I shall proceed to my definition. By this term, I mean an animal, consisting only of a cyst and its contents, incapable of existing but in living animal matter, having powers of se-

creting or absorbing from its nidus the food which fills its whole cavity, and of producing an offspring similar to itself by no generative organs that can be traced. If it should be objected that the etymology of the word would confine it to those cysts which contain a watery fluid, I would remark, that such is not the case with the common hydatid, the contents of which are coagulable, and sometimes tinged with red particles of blood. For this and other reasons, I should divide these into *hydatis lymphatica*, and *hydatis cruenta*.

That which is the subject of our present enquiry, I should call *hydatis carcinomatosa*, which, besides the difference of its contents, has also the property of stimulating the part in which it lives to form a kind of fungus, for purposes I shall endeavour hereafter to point out: my first business is to prove, if possible, the animalcular existence of carcinoma: for this fungus, though in the cancerous breast it is usually considered as the whole of the scirrhus, appears to me only an appendage to the carcinomatous hydatid.

As you have been less conversant with the cancer in the breast than the viscera, you may, perhaps, not have examined with your usual accuracy a quantity of apparently diseased fat; which, when the part is very much enlarged, is found inclosed in different portions of the scirrhous, and of which the whole posterior part of the tumour, as well as the space from that to the axilla is usually made up. This diseased fat, as it is sometimes called from its appearing thinner, more transparent, and of a greenish yellow hue, I have found to have all the properties ascribed to the hydatid in the human subject.

Let me first transcribe your arguments\* in favor of the separate existence of the common hydatid. "Life, you say, may be attached to the most simple form of organization. In proof of this, hydatids have been found in the brains of sheep, exactly resembling those in the human liver, and which have been seen to move; and there-

\* Morbid Anatomy, 2d edition, p. 225.

fore are certainly known to be animalcules. The hydatids, indeed, of the human liver have not, as far as I know, been found to move, when taken out of the body and put into warm water; were this to have happened no difficulty would remain." The reason why this cannot often happen, you very justly observe, is because "the subject is rarely examined till a considerable time after death." The man, whose case is related by Dr. J. Hunter, was opened thirty hours after death. As he died somewhat suddenly, it is probable, that by this time absolute universal death had taken place.\* If so, it is no inconsiderable proof of the separate existence of the hydatid; for though it does not appear that Dr. Hunter made the experiment in warm water, yet he found that "when the hydatids were opened, they contained a strong contractile force, so as to roll themselves up in part." Now, if this

\* Memoirs of a Society, &c. vol. i. p. 38. The author remarks that the blood was every where fluid. This would be a further proof that absolute universal death had taken place, if we could be sure that the blood did not afterwards coagulate.

contractile

contractile force was not the effect of elasticity, it is a sufficient proof of the life of these hydatids. You will therefore require no more of me than to prove the same properties unconnected with elasticity in the fatty part of the cancerous breast. At the same time, I cannot require your entire assent, till you have proved to your own conviction a thing so easily reduced to demonstration.

Immediately after the operation, take the amputated part, and cut it in a transverse, or indeed in any direction, and wherever you discover this fatty appearance, you will see the surface at first smooth under your knife. In an instant after, you will find a papillary appearance all over the yellow-green surface.\* Each of these papillæ you will find to be part of the contents of a capsule, the contraction of which has produced this conical figure. If the amputated part has been exposed long to the cold, or thrown into water and left there for several hours, a section of it no longer exhibits this ap-

\* See Dr. Babington's Letter, p. 63 of this work.

pearance,



pearance, nor is it possible in this stage to distinguish at first sight what I call carcinomatous hydatids from common fat. By this it appears that the yellow-green transparency of the fat, and the contractile power of the inclosing tunics could only arise from life, and the degree of heat with which that life was attended.

But though the colour and papillary appearance no longer remain, it will not be difficult to distinguish this fat from the common adipose cellular substance. By an accurate examination of the surface you will detect the cells or loculamenta in which the fat is deposited; and with a blunt instrument you may easily dig out the fat free from all fibres. The containing cell is usually so strong that there is little danger of breaking it, and when thus emptied retains its figure, being supported all round by other cells or by the fungus.

Notwithstanding the general resemblance between cancer in the stomach, uterus, and breast, I suspect there is some difference. I

am

am not now speaking of the malignant ulcer; but where a structure having the general properties of the carcinomatous mamma is found in those organs, I am inclined to think that the contents of the hydatid part of the disease are different. Few opportunities have occurred to me of examining them in a recent state: of course none of exploring their contents immediately after an amputation from the living subject. But from what I recollect, and still more from your remark, that these membranous or ligamentous septa contain a firm light brown substance, it would seem that the cells are filled with a substance that cannot be mistaken for common fat; unless we suppose from the very slow progress of the cancerous stomach that an alteration takes place in the contents of the cells, as we find was the case of a melicerous tumour mentioned by Hill.\* I hope I do not misunderstand you in considering the "substance intersected by septa," as the same as if inclosed in cells. Though you consider these septa as alto-

\* Cases in Surgery, edition 1772, page 47.

gether

gether membranous or ligamentous, and I have given them a muscular power, yet this difference will easily be admitted, when it is recollected that in the examination of the dead carcinomatous hydatids the whole support of the cells is derived from the neighbouring parts. In this state, therefore, it would be impossible to distinguish them from membrane or ligament, and in this state only can you have seen internal scirrhi.

The cartilaginous substance you speak of, and which I call the fungus generated before ulceration, most commonly makes a portion of what is called the scirrhous part of the cancerous breast; and if ulceration has taken place, a fungus of a softer kind usually springs either round the ulcer or forming the basis of it. I shall now offer my conjecture on the use of this fungus.

I am not to remind you that among the many important discoveries of our departed friend we are to reckon the following: that though extraneous bodies near the surface usually stimulate the neighbouring parts to suppuration,

suppuration, for the purpose of dislodging them, yet that living animals or living animal matter may be so situated without producing the same effect. But when these animals die they produce the same effect as other extraneous substances; that is, suppuration takes place to dislodge them.\* This is well known to those who have been in tropical climates, where the guinea-worm penetrates the human skin. While the animal remains alive, it never stimulates the parts to suppuration. Whenever, therefore, an attempt is made to extract it, great care is taken not to break it: for if that happens, the animal dies, and suppuration takes place under the skin through the whole length of what is left of the worm.

Now, if carcinomata pass through the same stages as Dr. J. Hunter has remarked of the common or lymphatic hydatid, is it not probable that on the death of any of them suppuration will follow, and that

\* Hunter on the Blood, &c. page 208. Also compare Edin. Medical Ess. abridged, vol. ii. page 507, & vol. ii. page 136; and Hill's Cases, page 52.

• this suppuration may expose the living hydatids in such a manner that many of them may die from not being surrounded by living animal matter? To prevent this, I conceive a fungus is formed, which incloses individuals or clusters of them in separate compartments, so that the death of one set produces no effect on the rest.

As far as my observation extends, this fungus grows in every direction where it is necessary to preserve the hydatids. When a cluster of hydatids dies, the fungus between it and the surface ulcerates, or sloughs slowly, till the compartment containing them is exposed. By this time, if the progress has been very slow, all the tunics of the hydatids are detached, and the surface being clean will make an attempt at healing. If no dead hydatids are in the neighbourhood, it will often, for a time, scab, or even skin over. But if, when the cavity is exposed, some of the tunics of the hydatids retain their attachments, the attempt at healing will only produce an exuberance of fungus with retorted edges. This will  
continue

continue till all the tunics or fragments of them are detached; after which, if no new impediment arises, the edges will take a different direction, and the part heal for a time.

I shall now add a few words on the cysts containing a serous fluid, which are sometimes found in a scirrhous structure. These appear to me common hydatids, or as I call them, hydatides lymphaticæ, formed in the fungus, which was destined to support or rather preserve the carcinomatous hydatid.

Thus you see we perfectly agree that *cysts containing a lymphatic fluid* are not essential to the carcinomatous breast, but only occasionally found in them. I have suspected, when the fungus containing these cysts has been particularly hard, it has arisen from the lymphatic hydatids formed in it acquiring those cartilaginous tunics which you have remarked in other parts of the body. I recollect one woman, who, giving me an account of what was amputated from her breast, not inaptly described it as like a gizzard.

I have

I have seen the bony [or rather calculous] appearance you speak of constitute the whole of what was supposed to be a cancer in the breast. It was gradually separated by suppuration, and the patient recovered. May not this have been a change, not of the scirrhus as you suspect, or of the fungus, as I call it, but of a lymphatic hydatid, or a large cyst full of them? We know that the breast is sometimes the nidus of that description of hydatid only, and Dr. J. Hunter has traced the progress of this animal through its various stages, until in some he found the sac hardened compressing the coats of the hydatid into a substance like isinglass. In this last stage the substance he says is become like chalk and water, the side of the sac appearing more like stone than bone. I would further remark, that these bony or calculous appearances are to be traced in every place in which *hydatis lymphatica* has been discovered. You have traced them in the liver and in several other parts; they are not uncommon in the testicle; Baron Haller describes them in the ovaria, the thuroid gland, and the eye. They are com-

mon

mon in the eye-lid, where the watery encysted tumour is often found. I mean not to suppose by this, that *all* the bony or calculous appearances found in these parts arise from such a cause; but as it is well known that an extraneous body too deeply seated to stimulate the parts to suppuration will sometimes be found imbedded with calculous matter, and even cysts have been seen filled with a similar substance;\* the subject is at least worth further enquiry.

Your large opportunities will furnish the means, and your candour will not be backward in communicating your future observations on these interesting subjects. I have, therefore, only to request a patient perusal, and to renew my assurances, &c. &c. &c.

JOSEPH ADAMS.

\* See Dr. Austin's Treatise on the Stone.



LETTER V.

FROM MR. CLINE TO THE AUTHOR.

MY DEAR SIR,

London, 21 July, 1796.

I RECEIVED your letters of the 20th and 22d of May, in which you mention a wish to address a letter to me that you intend to publish. Whatever you write will be interesting, and I cannot have the least objection to your using my name. I have not lately met with a case from which a drawing could be made that would answer your purpose. I have lately taken out a small tumour from the breast which was without the least appearance of cavity; and I assisted at an operation performed by Mr. Blicke, where there were several cavities containing a greenish-yellow gelatinous substance in distinct cells. I have often met with such cells in these tumours, containing  
different

different fluids, sometimes like serum, and in others of a dark bloody appearance. But so far as I have hitherto observed, they did not give me the idea of being living hydatids; that is, a perfectly circumscribed membrane without any communicating vessels from the surrounding parts. Hydatids are not a part of the animal in which they are found, any more than the worms in the intestinal canal. In some diseased testicles there are many hydatids: but this is not the testicle that becomes cancerous. The true scirrhus is not an hydatid testicle.

You are of opinion, that steatomatous tumours are nourished by venous blood. From the shortness of a letter, I probably do not conceive your meaning. Every part that is supplied with blood must receive it from the arteries; the veins alone return it; unless you suppose a distribution of vessels like the *vena portarum*, which answers the double purpose of vein and artery.

The tumour which I removed from the back, and which I suppose is the one that you allude to, weighed sixteen pounds, and was entirely adipose. I left skin enough to cover the whole surface of the wound, and secured the vessels by ligatures as I divided them: for although none of them were large, yet the loss of blood must have been very great, if I had completely removed the tumour before any of them were tied. The operation was near an hour; the cicatrix was a line. I first raised the integuments from the tumour, and then dissected the tumour from the fascia covering the muscles of the back.

When an incysted tumour is small, it may be cured by an opening that will discharge its contents; and then by applying lapis septicus to the internal surface, so as to destroy the cyst. But when they are very large, this practice must be dangerous, because the suppurating surface might be greater than the constitution is able to support. The removal or destruction of the cyst is generally necessary, for it rarely has  
any

any disposition to heal. Even the leaving a very small part of the cyst will be sufficient to prevent the complete healing of the wound.

\* \* \* \* \*  
\* \* \* \* \*

A small pamphlet on the climate of Madeira, naming the diseases it would remedy, might be useful.

I am yours most truly,

H. CLINE.

LETTER VI.

TO MR. CLINE.

MY DEAR SIR,

IT is most probable that in an opinion so new I have not expressed myself with sufficient clearness, but when speaking of cysts and cavities I mean two distinct things. The true carcinomatous cysts are not without great care to be distinguished from fat, especially if the cut surface is covered with blood. But in a large old cancer this is rarely universally the case, because no blood vessels will be found in any of the carcinomatous cysts. The substance, however, in which they are inclosed, and which is usually considered on account of its hardness as constituting the whole of the cancer, is often extremely sanguiferous. When all this is removed, you will frequently

quently observe a surface much resembling a greenish yellow fat. On a closer examination, which may be best made on the amputated part, you will find that this fat is not contained in that cellular substance which forms the common cellular membrane, but in distinct cells having no communication with each other, and also having no blood vessels ramifying through any of them.

Unless we were together, it would be difficult exactly to comprehend each other's meaning; but as you seem to confound cavities with cysts, there must have been some inaccuracy in my language.

The more obvious cavities are of three kinds. The first is, I conceive, the common hydatid; and, as the amputated part has usually been soaked in water before it is examined, when a section is made through this cyst, its contents escape almost unobserved, the cyst still retaining its figure on account of the cartilaginous nature either of its tunic or the fungus in which it is imbed-

ded. This therefore has the appearance of an empty cavity; but is, as your greater accuracy describes, "cells filled with serum."

Another kind of cavity is often filled with a gelatinous substance of different consistence in different cavities, and often in the same. These appear to me carcinomatous hydatids that have gone through their different stages of birth, growth, and decay, and are retained in the inclosing fungus, till either an operation or the gradual ulceration or sloughing of the fungus exposes them.

The third kind of cavity, which shows itself without a very close examination, consists of cells filled with a dark bloody fluid, and which I take the liberty of calling *hydatid cruenta*. They are usually inclosed in a much looser and more sanguiferous fungus than either the lymphatic or carcinomatous hydatid.

Whether I am right in calling these cells Hydatids or not must be determined by future  
 ture

ture observation; but it has never yet been disputed that hydatids may be filled with a bloody fluid, as well as with lymph. The case communicated by Watson in the Philosophical Transactions was a cluster of hydatids, some of which were filled with lymph, and others with a bloody fluid. They were attached to “ a spongy substance, answering the purpose of a placenta.” Now if these were hydatids, I cannot see in what respect they differ from those cysts, which are often found in cancerous breasts filled with the same fluids, and attached to a spongy substance which I call the fungus.

I perfectly agree with you that the true cancerous testicle is not the same as the hydatid testicle; but you must allow with me, that the testicle as well as the breast is a nidus for hydatids, as well as the seat of carcinoma. I shall go a step further, and taking the liberty of using my own language, assert that I have seen the same breast prove a nidus for carcinomatous and common or lymphatic hydatids; and if those collections  
of



of lymph mentioned by Dr. Monro and Mr. Gooch\* were not hydatids, or if the cysts filled with serum, which you have observed, were not hydatids, they seem at least to approach nearer to them than any diseased appearance hitherto described.

I might further add, that not only the testicle and breast, but the lip, the uterus and the eye, all of which are known to be the seat of carcinoma, are also found niduses suitable for common hydatids.

I am however ready to allow that there is a scirrhus testicle different from either the carcinomatous or lymphatic hydatid; but this differs no less from the scirrhus breast; nor have I ever seen it in any other part subject to carcinoma, excepting the lip. It exhibits, when opened, a kind of granulated appearance, tolerably regular, and in the center there is usually a fluid like a beginning unkindly suppuration. The testicle thus af-

\* See "Morbid Poisons," chap. vii. "On diseases called cancerous."

fected is always much more regular in its external figure than either the lymphatic or carcinomatous hydatid testicle.

My suggestion that steatoma is nourished only by venous blood can hardly be called an opinion; it was rather a matter of enquiry, or a hint which I wished you to pursue. It arose from having frequently observed a great number of vessels over the surface of some of these tumours, which protruded beyond the common integuments, and never perceiving any pulsation in the largest of them. I have also remarked that no dangerous hæmorrhage has ever followed the extirpation of these tumours, and if they are fed, as must be the case, entirely by absorption or secretion, probably arterial blood may not be necessary for them, as we find in the secretory part of the liver. Whether this is really the case or not, I suspect there is some peculiarity in the distribution of the blood vessels, even on the surface of these tumours, which has not yet been detected.

I cannot

I cannot dispute the justice of your remarks on the inconvenience of leaving any part of the cyst of these tumours. Without doubt, nothing can excuse such a practice but a determination on the part of the patient not to submit to the knife. But if these tumours have no blood-vessels ramifying through them; if, when a part of them is left, they produce no effect like the common process of ulceration or sloughing, nor any of the restorative processes we meet with after violence or disease from any other cause, permit me to enquire to what we are to impute actions governed by no analogical laws we can trace in any other part of the œconomy?

This difference is not less remarkable than the manner in which their life is supported. We know of no other parts, however distinct they may seem in their functions from the neighbouring parts, but have vessels which may be traced into their substance, and which communicate with the surrounding ones. Nothing of this kind is the case with the true encysted or steatomatous

tous

tous tumour. Yet they appear to have life and strong powers of preserving it; for the remaining part of the cyst retains its attachment, and not only like other foreign bodies, prevents the healing process, but is able for a long while to resist those actions of the system, by which other foreign bodies are dislodged.

I have seen a portion of the tunic of an encysted tumour in the back, which was too deep for caustics to reach with safety, kept a sore open for two years. It was attended with little or no inconvenience to the patient, and covered with a thickness of spongy granulations before it was properly treated. When first opened, its contents were a turbid kind of lymph resembling thin pus, and the tunic contracting, the case was treated as a common abscess. I suspect it was this necessity of extirpating every part of the watery encysted tumour, (or, as I should call it, the lymphatic hydatid in a solid part) that produced the old practice of cutting out the tunica vaginalis in the operation for hydrocele. Experience has taught us, that  
 this

this is unnecessary, and that the sides of the cavity may be united so as to obliterate it altogether. We also find that the cyst containing the matter of an abscess soon becomes the basis of suppuration and future granulation, or in cases of very large abscesses it sometimes sloughs. In either case it is no lasting impediment to the healing of the part. I leave you to decide what is the cause of this difference, at the same time urging in my own behalf, that my conclusions have not been hastily drawn.

I remain,

Dear Sir,

Yours most truly,

JOSEPH ADAMS.

LET-

LETTER VII.

FROM DR. BABINGTON.

DEAR SIR,

I HAVE been honoured with your letters of the 25th of May, and 5th of July, for which, and for the specimens of minerals sent with them, but which I have not yet received, I beg you will accept my best thanks, and believe that I shall always have great pleasure in attending to any thing which you may wish to make an object of inquiry.

With regard to the question you put to me respecting the operation at which you and I assisted Mr. Fearon, the best answer I can give is, that, though I cannot at this distance of time charge my memory with the result of your examination of the amputated breast,

breast, so as to state it with sufficient accuracy, I perfectly well recollect your pointing out certain papillary appearances, such as I had never before attended to, and which I considered at the time as tending to strengthen the opinion you had then adopted respecting the nature of cancer.

Your friends here, so far as I know, continue well, amongst whom you may reckon

Your's very sincerely,

W. BABINGTON.

*Basinghall-street,*  
Sept. 9, 1800.

LET-

## LETTER VIII.

TO MR. ABERNETHY.

DEAR SIR,

GREATLY as I feel obliged by your last favor, it would have been much heightened had your leisure permitted you to finish it for the press. But while your flattery almost reconciles me to your injunctions, it induces me in my own justification to quote those few words, by which I am precluded from quoting any more.\* It is, indeed, allowing me a large liberty, more than I am willing to use, as it is more than

\* " I know you will take no unfair advantage of any one . . . . To write otherwise [than incorrectly] would take more time than I can spare. You have my free leave to write to me and of me in public, to state any thing I have written as mine or your own, only if you ascribe any thing to me, put the thought into your own language." *Extract of a private Letter.*



I would allow to any other person. Without the least intention of doing wrong, there is a natural bias in the mind of man, which gives to every thing a turn towards those objects on which it is most inclined to dwell. Hence the same expressions being understood in different lights would of course be paraphrased differently; under your injunctions I can only keep as close as possible to what appears to me your meaning; and if at the same time all inverted commas are omitted, the public and yourself must acquit me.

In stating your objections, you wish me to inform the world how these animalcules [carcinomatous hydatids] multiply. Whether like some hydatids from their external surface; and if so, how are the young removed to another part of the adipose substance; so that a portion of the scirrhus or fungus may be formed between them?

In answer to this let me observe, that you very fairly allow me the analogy of other hydatids known to be such, and whose  
generation

generation is admitted to be from their external surface. The manner in which they are divided into different compartments by the intervening fungus, I conceive to be as follows.

In examining a carcinomatous breast amputated in an early period, we meet with little or no fungus. By an early stage I mean, Before the disease, how long soever it may have existed, has made any considerable progress. If the progress has been sufficient to exhibit any superficial marks, by a circumscribed puckering of the skin, we find the fungus usually confined to the space between the carcinomatous hydatids and the surface; but if the disease has made considerable progress, so that the whole breast is much enlarged, it is then that we find various compartments in the fungus filled with hydatids in different states of their progress towards maturity and death.

Hence it seems as if the hydatids had a period of existence short in proportion as their powers of multiplication are greater.

Till they multiply (supposing them in a situation that affords them a nidus for it) they appear more or less in a torpid state, occasionally growing, and at other times stationary. But the death, or perhaps even the approach towards death, of any individual or number of carcinomatous hydatids instantly becomes a stimulus to the surrounding parts to generate this fungus, which, by separating the dead from the living, produces in different parts of the same breast two different actions at the same time. One is a kind of ulceration, or more commonly continual sloughing of the fungus which incloses the dead hydatids; the other is the formation of new fungus to protect the living hydatid, and in many instances, if not in all, the fungus becomes itself a nidus for the generation as well as protection of future hydatids. That it does so for *hydatis cruenta*, when such are the contents of a cancerous breast, we have every proof that our senses can furnish. For in these cases the fungus is always much softer and spreads faster, if the integuments are removed by the knife, caustic, or ulceration,

ation, and the whole appearance when removed is similar to the description of those hydatids which have escaped from the uterus, adhering to a spongy substance resembling or serving as a placenta.

Now if the fungus is altogether formed for the protection of the hydatids, its growth will be generally according to the necessity for it. This will vary like all the other secondary operations of nature, many of which, however salutary, will sometimes be productive of disease. Thus the growth of the fungus being posterior to the generation of the hydatids which it encloses, will consequently separate the latter into different clusters or families. This is most probably only one of the causes of the distance at which they are found: for you will admit that if one part of a breast is become a fit nidus, another will be as likely to be so. You will also recollect that lymphatic hydatids in the liver are sometimes at a distance from each other, though more commonly connected; and that in the cavity of the

same abdomen different facts are found without any apparent connection.

I am very much in hopes that your next objection will bring us to understand one another better: I could almost flatter myself it might bring us to agree. You admit the propriety of my remark, that the fungus divides the fat into distinct portions; but you object to my addition, that the fat thus enclosed should be free from *common* cellular substance. Thus then we agree that there is a fungus, or if you prefer the term, a scirrhus, which divides different portions of fat from each other.

Before I explain the peculiarity of this enclosed fat, let me in my turn beg you to account for the great quantity of fat you find in a truly carcinomatous breast. Compare the size of the two breasts in the same subject, and examine how much of the increased size in the diseased one is made up of fat. Is it probable that such an increased quantity should be a healthy secretion, or that it should even be deposited by any process

process similar to what takes place in a state of health? I admit with you that it has the same appearance, and also that it has a cellular apparatus. But this appearance is only the same when the amputated part has been kept long enough for the fat to consolidate, and the cellular apparatus is altogether different. Instead of the apparently irregular net-work, which forms the common adipose cellular membrane, and which is so entirely intermixed with fat, that without cutting into small pieces and dissolving by heat it is impossible to separate fat from membrane, the cells of true carcinoma are so many small and firm capsules, each containing its distinct portion of fat, without connection with each other but by lateral adhesion. If this will not be sufficient to explain my meaning, I must impose on you the reading of the letters to my other friends.

You next enquire whether I conceive steatoma to be animalcular, and if so, what is the difference between that and carcinoma?

F 4

I can

I can make no objection to this question from one who will never take advantage of having extended the field of our controversy and thus engaged me in defending points, which, if weaker than those I have chosen, are not necessary for their support.

You will recollect that as the separate existence of the common hydatid was not my discovery, so I have not presumed to say any thing in its defence: and as Dr. Baillie has given his definition of life attached to the most simple form of organization, I have thought it sufficient to show that all the properties he requires, including motion, are discoverable in the fatty cells of the carcinomatous breast. Hence, though there is certainly a strong analogy between carcinoma, as I have described it, and steatoma, yet there is also a most important difference. Muscular contraction may be traced in the tunics of carcinomata by the elevation of their contents into a papillary form. This is not the case with steatoma. There are other differences to be stated hereafter; but as motion is considered the strongest proof of life, this is enough

enough to show, that as the proofs of the vitality of carcinoma are not supported by, so they are not to fall on account of any analogy, however strong, between that and steatoma.

But while I am throwing the weight of the first argument on such abler hands, and availing myself of authorities so well established, it is not my wish to fly from the digression you propose. On the contrary I am glad of this opportunity of engaging in it, because a consideration of encysted tumours in general will very much illustrate what I conceive the properties of carcinoma. And though no contraction can be perceived in the tunics of most others, yet this may arise from the contents of some being too solid, and of others too fluid to form the papillary appearance I have described in carcinoma.\*

Thus

\* When what is called watery encysted tumour, or as I should term it the lymphatic hydatid in a solid part, enlarges itself so as to appear on the surface, I have observed that, on being punctured before its death, the fluid contained in it, though somewhat turbid, escapes  
with



Thus without further preface I am free to acknowledge that not only steatoma, but atheroma and meliceris, as they have been called since the days of the Greek physicians, that is all encysted tumours, whose cyst and contents have no communicating branches with the surrounding blood vessels appear to me animalcular, or at least to have the same œconomy as has been admitted in hydatis lymphatica.

with considerable force, like water from a compressed bladder, or urine when the muscular coat of the bladder and urethra are in a healthy state. This is different from what happens in an abscess of any kind, the contents of which, when the containing cyst is punctured, how full soever it may be, always dribble over the integuments, at least as soon as the effect of the overstretching of the sac has ceased. I leave you to determine whether this is to be imputed to muscular force in the tunic of the lymphatic hydatis. This hint, however, is only offered for your own observation, as I need not repeat, that the proof of the life of this encysted tumour is not necessary to the support of my theory of carcinoma. Do you recollect, whether, on puncturing the tumours in the man's neck whose case is contained in your last letter, any such peculiarity took place in the escape of the fluid?

That

That this is the case I conceive :

First, Because they are all found in the same parts of the body, and often in the same individual tumour.

Secondly, Because they are all free from any communicating branches in the surrounding blood-vessels.

Thirdly, Because they all appear to have a power of growth, after which they die without otherwise affecting the body in which they existed, but by their local stimulus.

Fourthly, Because the cyst containing either of them is incapable of suppuration, and subject to none of those laws, by which capsules formed to prevent the diffusion of matter in absæsi, or suppuration, or original tunics when preternaturally distended with fluid, are governed.

Fifthly,

Fifthly, Because a similar mode of multiplication may be traced in each.

That they are often found in the same parts, and even in the same tumour, or as *I* should say, that the same parts prove a nidus to all of them, will be hardly questioned. It is well known that the breast is often the nidus of hydatis lymphatica. That it is also the seat of steatoma is not less certain: but this has not been so frequently observed, because most hard indissoluble tumours in that part are considered as cancerous or scirrhous. There is, however, a case related by Wiseman,\* which appears to have been steatoma, as he terms it gangrenous, or as it ought rather to have been (if my opinion is just) hydatis steatoma, which having passed through its period of existence, and being now no longer living animal matter, stimulated the neighbouring parts to throw it off, like other extraneous matter. That this was not cancerous, Wiseman himself was sensible; and also that it

\* Vol. i. page 185.

was what is called an encysted tumour which is enough for my purpose. Justamond describes two cases of steatoma in the breast, which he accurately distinguishes from true carcinoma: I have no doubt but you have met with several. I much suspect that many supposed cancerous cases which have been cured are of this kind, or some other species of hydatid different from the carcinomatous.

In the abdomen and other cavities the instances are numerous of steatoma meliceris or atheroma found in the same cyst with the lymphatic hydatid. Haller mentions such cases in his *Opuscula Pathologica*; others occur in the *Edin. Medical Essays*, and in most other collections of that kind.

The proper nidus for hydatids of all kinds in the human body appears to be in those parts which retain their life, and are not absolutely necessary for the support of the machine. I need hardly observe, that besides this there must exist a peculiar aptitude in the constitution for the generation of these

these animals, as we find is the case with all the others that exist in the human body. Hence we are not to be surprized if some of the causes do not produce the effects in similar parts, and in parts apparently similarly situated in different people; or if in some the aptitude should be so great as to produce the disease without those previous changes which seem necessary in others.

It is universally admitted that the mammæ of women after the period of gestation is passed, and consequently after they become useless for the purposes they might once have served, are peculiarly liable to this disease. If before this an injury occasions an obliteration of any of the tubuli lactiferi, that part of the breast becomes useless, and the cancerous period may be anticipated. Whenever the breast becomes carcinomatous at an early age, and without a previous injury, it has in all the cases I have seen been either in single women or women who have had no children. But to this rule I have no doubt there are many exceptions, nor would those exceptions prove any thing more than  
a peculiarly

a peculiarly strong aptitude in the constitution for the support of these animals.

The uterus, about the same period though in a much less degree, is subject to carcinomatous and other hydatids; but the ovaries in a peculiar manner to all, excepting the carcinomatous, or if these occur, as I suspect they do, it is without that fungus which always attends them in external parts. The cavity of the uterus has often been the seat of lymphatic, cruentous, and steatomous hydatids, probably from hæmorrhages in which the blood has remained in a coagulated state and retained the living principle. When the placenta or part of it remains without losing its life and becoming putrid, it will sometimes prove the nidus of a peculiarly formed hydatid lymphatica. The testicle has also become the seat of different species of hydatids at an early age; but this has most commonly been the consequence of previous disease or injury.

Violence of any kind in almost any part of a body possessing the aptitude for hydatids

tids will produce them. This probably arises, as in the uterus, from extravasated blood or lymph coagulating and retaining its life. You cannot forget the hydatid testicle which occurred in Bartholomew's, a short time before my departure, and the history of which was traced to the kick of a horse received a few months before. Dr. Stokes furnishes me with an instance of hydatids forming the substance of a tumour which arose from a blow near the eye. I have myself known a violent blow on the abdomen produce hydatids in that cavity. Dr. Letson traces a similar case to a fall from a horse.\* Hill gives two instances of the same kind, and one of these was connected with atheroma.

But I have said enough to show that the same causes will produce any of these encysted tumours, and that they are often found in the same parts and sometimes in the same facts.

My second position, That all these kinds of encysted tumours are free from any commu-

\* Mem. of Med. Society, vol. ii. page 32.

nicating branches with the surrounding blood-vessels is universally admitted.

But you must have patience with me, whilst I mark the difference between the cyst of the watery encysted tumour or *hydatis lymphatica* in a solid part, and the capsule of a common abscess. To form the latter we find, as Mr. Hunter taught us, an effusion of coagulated lymph, which connects the cellular membrane in such manner, as to prevent the diffusion of pus into that membrane. If any blood-vessels in the neighbourhood are easily traced, we find them at those extremities which approach the abscesses contracted and plugged with coagulated lymph, to prevent hæmorrhage as in cases of mortification. This has been very well remarked by Dr. Stark\* in his account of tubercles in the lungs, and by Dr. Baillie in his *Morbid Anatomy*.† But the tunic of encysted tumours we find composed of different strata, which may be all easily dis-

\* *Medical Communications*, vol. i. page 361.

† *Second Edition*, page 66.



sected from each other. I have separated not less than ten from a watery encysted tumour, all of them disposed with a degree of regularity never met with in common abscess. Nor is the difference less in the arrangement of the blood-vessels. Instead of finding the arteries obliterated or shortened with coagulated lymph at their extremities, we find each series of vessels continued to its remotest ramification, and often the number considerably increased: but whether the contents of the tumour are solid or fluid none of the surrounding vessels enter it. I ought to add, that in puncturing through the remaining cysts the force with which the contents escaped was so surprisngly great, that it could only be accounted for by elasticity or strong muscular power: but as this is not necessary to the support of my opinions of carcinoma, I shall not insist upon it any further.

That they grow and die in the manner that has been traced in *hydatis lymphatica* will, I am aware, be doubted by most; especially as their longevity depends on so many

many causes with which we are totally unacquainted. As, however, we find them sometimes stationary for years, we may suppose, that during that torpid state no progress is made towards their growth or consequent dissolution. This is somewhat similar to the state of an egg before incubation, and also to some of the less complicated animals. But this uncertain progress may be traced in the lymphatic, as well as any other hydatid. What is called suppuration in meliceris, or gangrene in steatoma, appears to me only the death of those animals, in consequence of which the integuments give way as in other cases of extraneous matter near the surface, and the tunic of the hydatids no longer retaining life, its contents escape if pulpy or fluid, and if harder crack with all the appearance of mortification. But this mortification, though it sometimes gives considerable alarm, never extends beyond the tumour, which is thrown out with more or less ease in proportion as its situation is more or less superficial. Of this kind the case already referred to in Wiseman is an instance, and your letter furnishes

me with another in the progress of those tumours we examined together in the hospital previous to my leaving London. You tell me that one of them afterwards inflamed and sloughed, the portion thus affected fell out, a hardness remained below, the increase of which in some measure filled up the chasm. What was this but the death of one steatoma and the increase of another?

When I before offered my opinion to the world, it was short, because I was unwilling to broach so much new matter at once. You will pardon me, if on the present occasion I run into the other extreme, and am somewhat too prolix.

Let us then view the difference between opening these tumours or imperfectly cutting them out; and suffering them to die, or to use the common language suffering the soft ones to suppurate and the hard ones to gangrene. But this properly belongs to my next proof, viz. that the cysts are subject to none of those laws, by which capsules formed to prevent the diffusion of matter and

original

original tunics preternaturally distended with fluids are governed.

When the matter of abscess is dislodged, whether by art or the process of ulceration, if the opening does not reunite, the capsule becomes the basis of suppuration and granulation. When an original tunic is preternaturally distended with fluid, as we find the tunica vaginalis testis in hydrocele and the bursæ mucosæ, so as to form what are called ganglions, the consequence of opening them and preventing the divided edges from re-uniting is, that the sides collapsing on each other grow together. Hill, describing one of these bursæ mucosæ as an hydatid, remarks that it was all absorbed into the constitution. This is now known to be the most convenient way of curing that disease, by inflicting a blow which may break the sac without injuring the skin, after which the fluid escapes into the cellular membrane, and is taken up by the absorbents.

When all these encysted tumours were confounded together, the directions were in all

G 3 to

to remove the sac. Hence the barbarous practice of cutting away the tunica vaginalis in hydrocele, till Mr. Pott introduced, or which is the same thing revived the practice of the featon. Even while the featon and caustic were in use, the general opinion was that the tunica vaginalis was made to slough away by the caustic, and to this the advocates for that operation imputed their more certain success. Mr. Hunter taught us, that this appearance of slough was nothing more than coagula of the contained fluid, and every improvement of the operation shows that the sloughing of the sac is altogether unnecessary.

But the true hydatid, whether its contents are lymph, adeps, or whatever else, can never be cured but by the removal or sloughing, whether by art or nature, of it sacs. It is true that some of them have been cured by featon, but I need not remind you how long the process is in this case to what we have always found it in hydrocele. To what do you impute this difference but to the cyst of the hydatid retaining its life and its own economy, and as long as it does retain them

them preventing every restorative process in the neighbouring parts?

Lastly, a similar mode of multiplication has been traced in all of them.

I have before reminded you that the mode of growth and multiplication is various in *hydatis lymphatica* as well as its size, figure, and longevity. When seated in the cavity of the abdomen, they seem in the most favorable situation, and multiply the most readily. The liver also proves a favourable nidus, though apparently less so than the open cavity. When found in the breast or any other external solid part, their multiplication is very slow, and unless contained in a cyst, they often do not multiply at all, a single *hydatid* being all that can be discovered. The proper nidus for *hydatis carcinomatosa* seems the female breast, and when found in other parts, they multiply very slowly. *Steatoma* and *meliceris* are always slower than either of the former in their progress, which however depends on the favourableness of their situation. If found within the cavity

of the abdomen or thorax, and in a situation that admits the patient to live long enough, they multiply considerably though slowly. If pendulous, as in some cases of wens from the neck, their situation is not unfavourable for growth and multiplication, and either a single one or a few grow to a great size, or a number is found contained in the same sac. But if lodged in any solid part, particularly if between muscles, their multiplication or growth is usually very slow.

That steatoma, however, does multiply, has been tacitly admitted by most authors. Almost every case related with sufficient accuracy speaks of several found in the same sac, and some authors describe what appears to me a process of multiplication. Dr. Akenfide's \* patient remarked that when he cut off his tumours, *suckers* always arose from them. Hill † found the remains of an old meliceris buried by another that had grown over the surface of it; and what is

\* Medical Transactions, vol. i. page 64.

† Page 47.

not a little curious, the power, which the new hydatid possessed of preserving its life, prevented the dead one from producing suppuration by removing it from the surface. Justamond remarks, that when steatomatous tumours are seated on the skin of the scalp, they are very seldom solitary. We must therefore either suppose they are all formed at the same time, or that they multiply; for besides these instances, to which may perhaps be added that with which you have furnished me, I can see no reason to suppose, that numbers of these contained in the same cyst should not be the effect of the same mode of multiplication as is admitted in *hydatid lymphatica*.

This I hope will be sufficient to show the analogy between these tumours: I now proceed to the difference between carcinoma and all the rest.

If carcinomatous hydatids were to be confined to a cyst, as I suspect they often are; and even, that they often form part of what are called steatomatous tumours; there  
 would



would, I conceive, be no difference between them, steatoma and meliceris; excepting that their contents would be of a consistence between the two latter; for I never could find any difference between the contents of carcinomatous cells and common fat after it is cured or separated from its cellular substance.

The principal peculiarity of carcinomata is the power they possess, when not inclosed in a common sac, of stimulating the part in which they grow to the formation of that fungus, which is usually denominated scirrhus. When free from an inclosing sac too, and placed in a favourable situation, they multiply much faster than any other hydatid, excepting the lymphatic or cruentous. But in tracing the history of all of them, we have found much less difference than at first appears. Recollect your opinion of the man's case from whom the lump sloughed and fell out. When we were together, you observed that you thought the lumps in the groin were lymphatic glands excited to disease by the testicle, and though not strictly cancerous,

ous, that they spread like that complaint. This man had collections of such lumps in different parts of his body. Hence we may conclude (if, as I suspect, they were steatomatous) that he had a peculiar aptitude for steatoma,\* which spread like carcinoma, and probably should the same aptitude occur in a female, and the seat prove the breast, and the steatomata be confined by no general sac, the succession, though slower, would be as universal to the axilla as we find it in carcinoma. But for this aptitude I conceive the patient in Bartholemew's might have recovered when the first lump died. Such was the case of a lymphatic hydatid in the breast mentioned in the Ed. Med. Essays.† The sac lost its elasticity probably by death, after which it burst and fell out. The wound healed without any trouble.

\* Such was the case with Dr. Akenfide's patient related in the Medical Transactions. In this man it was hereditary. I know two families in one of which a brother and sister have wens; in the other the mother and three or four children.

† Vol. i. Art. 17.

Carcinomatous

Carcinomatous hydatids then are principally formidable on account of the rapidity with which they run through their stages, when seated in a part favourable for their multiplication and attended with a strong constitutional aptitude. The power of stimulating the part to form a fungus very much encreases the misery of the disease, because after the death of the hydatid it is not thrown out like steatoma, but a tedious process follows of sloughing or ulceration of the containing fungus which is continually repeated. The property of stimulating to the production of a fungus is not entirely confined to this species of hydatid. But this and *hydatid cruenta* seem to be the only two that possess that property before the skin is broken.

When either the lymphatic steatomatous or melicerous hydatid is imperfectly extirpated, if the remaining part of the cyst retains its life, a fungus is sometimes generated round it. The same happens when the carcinomatous breast is imperfectly extirpated. A fungus resembling very florid granulations

granulations grows very rapidly and sometimes skins over. If the hydatids were cruentous, imperfectly amputated, and the wound does not unite by the first intent, the fungus never skins over, is much more spongy and sanguiferous, spreads to an enormous bulk, and becomes the nidus of other hydatids.

○ This property of skinning in the fungus inclosing imperfect carcinomatous cysts seems to arise from these hydatids having in some instances a power of restoring themselves after a part is cut off, in consequence of which they not only retain their life, but their secretion is confined within their cysts, whilst the remaining portion of other hydatids not being able to recover themselves; as long as they retain their life produce a secretion, which keeps the sore constantly open.

Such, Sir, are my reasons for believing that carcinoma, steatoma, meliceris, and atheroma possess a life similar to what is admitted in hydatids, or as I distinguish them in *hydatis lymphatica* and *hydatis cruenta*.

If,

If, on a subject so new, I am still imperfectly understood, my only excuse must be, that it is possible to write too much as well as too little. All matters of fact must be ultimately determined by demonstration, and even when you have met with every appearance I have described, it must remain for yourself to draw your own inductions.

Let me now offer a few words on two other cases contained in that last favour, again regretting that your injunctions prevent my stating them in your own words.

The first is the case of a man who had diseased lymphatic glands in his neck. The account he gave of himself was, that a part of the neck inflamed and suppurated, that it was punctured by a lancet, that the surrounding parts became hard, that the wound enlarged by ulceration, that its edges became roundish and everted, that a chasm appeared below from which a watery fetid ichor distilled, that again another gathering took place, which being treated in the same manner, terminated in the same way. This happened

pened repeatedly, and when you saw him the tumour was large, hard, and containing cells such as before described and irremovable. Beneath it was a red point of the skin under which matter fluctuated such as he described to have existed in the beginning of the disease. This, by his desire you punctured, and as he foretold, the skin, which was formerly soft, hardened like the rest of the tumour. After this he suddenly left the hospital.

When the mind has dwelt long on the same subject, there is always danger lest it should associate with every new object those ideas which are so frequently presenting themselves. This makes me almost afraid of, but it must be my apology for urging, how much this case favours my opinions. These little abscesses as they might seem, but which (as you think them worth so much notice) must have materially differed from the common appearance of suppurating glands, and the remaining cells, (which you afterwards add exactly resembled cancerous cells) I conceive to be a set of lymphatic

phatic hydatids not inclosed in any cyst. Their growth and multiplication were slow, and their decline in succession. The inflammation in the skin and the fluctuation underneath I conceive arose from a progress towards decline in the hydatid, in consequence of which the muscular power of its tunic was lessened. They were probably punctured before they were dead, so that their tunics did not stimulate the neighbouring parts to any process towards dislodging them, and what remained in this diseased or imperfect state discharged a watery fetid ichor. Such is the usual discharge of carcinomatous and all other hydatids, when fragments of their tunics form the surface of an open sore, and as long as they retain their life. Wiseman had probably this kind of hydatid in view, when speaking of some cancers he says \* “They will be soft underneath the inflammation, so may be thought full of matter; but if you open them they will only gleet.” In another place he speaks of a *stinking* gleet. The only diffi-

\* Vol. i. p. 169.

culty is the *apparently* good pus that was discharged by the puncture. As by your guarded expression it does not appear that the fluid was subjected to any particular examination, you will not be offended, if I suspect that it was what I have seen in other hydatids in the stage I conceive yours were. Perhaps you can scarcely recollect whether the fluid, though opaque, was not thinner than the best pus, or whether it approached that water-gruel appearance which you describe in your next case.

This was a carcinomatous tumour, lately amputated in the hospital, containing several cells all inclosed in one capsule and filled with dissimilar substances, some with a fluid like water-gruel, and some hard like cheese. In the surrounding fat there was neither scirrhus nor the appearance of disease. Two or three of the axillary glands were affected in the same manner, and contained in a similar capsule.

Still the same idea presents itself: hydatids of different descriptions contained in a



common fat. 'Tis true the cheesy appearance is generally considered as characteristic of scrofula: but your silence on that head leads me to suppose, that you had very good reason for believing that such was not the nature of these cysts, nor need I remind you that this cheesy substance has been remarked as forming the contents of cysts contained in a common sac with almost every species of hydatid. That the surrounding cellular substance should be free from scirrhoty (or as I call it fungus, and diseased fat, is precisely what might be expected when the hydatids were contained in a cyst. The hydatids themselves were not carcinomatous, the contents of whose cells, as I have before remarked, differ only in their recent appearance from common fat: they were besides inclosed in a common sac, which rendered the protection of a fungus unnecessary, unless in a very advanced state of the disease, and then only between the capsule and the integuments.\*

\* See Morbid Poisons, Chapter on Cancers.

I know your goodness will excuse the liberty I have taken with your cases and with the rest of your letter. If wrong, you will not impute it to design, but a dulness on my part, or the causes above assigned. But before I conclude, let me thank you for the most friendly part of your letter, and give you my reasons for not strictly adhering to your advice. You could almost wish me not to publish until my opinions are ascertained, as I have already said enough to claim the discovery, and more could not be done without opportunities of personally examining specimens of the disease.

As your letter is dated 1796, and we have now arrived at 1800; and as in the meantime I have solicited the objections of most of those who have the largest opportunities of information, you will allow me to use the language of an orator you are well acquainted with *Εἰ μὲν περὶ καὶ τίνος, &c.* But I trust you will not suspect me of offering such new opinions without repeated personal examinations, or what appeared to me positive proofs. If these were not publicly demon-

H 2

strated,

strated, you need not be informed, that no diligence was wanting on my part in seeking for an opportunity after having made up my mind to leave London. Even since my arrival, and as long as my continuance was doubtful, I was contented with observing what was going forward at home: and had any thing occurred which might contradict these opinions or render their publication unnecessary, I should gladly have remained silent. But as the subject is not unimportant, and as I am likely to be fixed some time in this delightful spot, which allows me no opportunities of correcting any inaccuracies, this must be my apology, if I am still thought premature.

Farewel, Dear Sir, and believe me,

Your sincere Friend and Servant,

JOSEPH ADAMS.

LETTER IX.

TO DOCTOR STOKES.

DEAR STOKES,

THIS interruption to our correspondence surprises me the more, as my last was favoured by Mr. Earfoth, to most of the letters by whose conveyance I have received answers.

I have further to regret your objection against furnishing a letter for insertion with my own. At the same time let me thank you for this mark of friendship, in the freedom with which you warn me against a failing I am too apt to indulge, or perhaps to fall into without being aware of it myself.

self.\* To you however it is unnecessary to urge, that on this occasion I have no departed friend whose ashes are to be watched, no master whose opinions are to be defended. What is now offered is so entirely my own, that the consciousness of differing from every one else will I trust make me careful in my language, as it ought to make me doubtful of my opinion. But I seem to forget that I am writing for the public as well as for yourself.

Whatever De Haen's opinion of this theory of *hydatis lymphatica* may be, I could at least be glad, if those who doubt whether

\* "I am glad you go on writing, and shall be glad to appear in the same book with you; but cannot approve the prize-fighting form, as it will inevitably lead to that sarcastic acrimony which is the great drawback from your *Morbid Poisons*. . . . I would have you publish the letters of all the correspondents, or at least all the remarks contained in them. . . . But do not give a long verbiage. . . . If you will take the trouble carefully to peruse my letters, and throw them in the best order, I shall be happy to be made one among you." *Extract from Dr. Stokes's Letter.*

they

they are animals, would tell us what they suppose them to be. To me the question is of less consequence than it may at first seem, for whether they are distinct animals or not, one thing is certain, that they have all the properties of life, and go through all those changes which I contend for in carcinoma. That lymphatic hydatids grow and multiply is universally admitted, that after a time their cysts lose their contractile power and transparency and burst, is not less certain; and all this without the intervention of blood-vessels, and often without any communication with the body in which they exist, but through the medium of a sac filled with a fluid in which they float. However I am obliged to De Haen for another instance, if any were wanted, of lymphatic hydatids forming part of a carcinomatous tumour, and to you for a case in which they were the consequence of a blow.

In order to direct my enquiries in the last part of this subject I shall transcribe your opinions of the disease and the mode of treatment. A cancer you suppose is a dis-

eased growth, but not originating in an hydatid, and not propagating itself by the absorbents, not always accompanied with fungus in its ulceration ; that the best mode of operation is to cut out the tumour, cutting away no more than what is actually diseased, and healing up the wound as soon as possible ; that change of climate from a cold to a warmer is the most likely preventive. In many of these particulars I agree with you, but as your opinions were probably not taken from a recent subject, I shall make no apology for differing in other points.

If your opinion was not formed from recent subjects, it is not surprising that you offer no theory of the nature of this *growth*. However, for the reasons abovementioned I am ready to admit your term. I will also agree with you that it is not always attended with fungus in its ulceration. But in all cases of true carcinoma, I have never seen one in which the fungating process did not precede ulceration and continue in different forms till all the carcinomata were thrown out under the ulcer, after which it will  
often

often heal. To explain myself better, let us trace the disease from its beginning.

At first we find a lump in the breast, which continues stationary or encreases slowly or fast, till at last we perceive a somewhat circumscribed hardness, corrugation, and redness immediately under the skin. It rarely happens that the part is amputated till it arrives at this stage. It is therefore not easy to say whether the fungus is formed long before it discovers itself under the skin. It is however certain, that this redness and corrugation are always a prelude to ulceration. I shall stop here to correct two generally received errors, as they appear to me.

The first is, that this corrugated redness arises from varicous veins; the second, that the disease has derived its name from this appearance. That varicous veins may be sometimes discovered in the fungus it is not my intention to dispute, but it is not less demonstrable, that the appearance above-mentioned arises only from the thinness of  
the



the integuments previous to ulceration. As to the term cancer, it appears to me that the ancients had a much better reason for the use of it. I mean its property of proceeding *backwards*, or contrary to the progress of abscesses which is towards the surface. Whilst the fungus is extending to the skin, the carcinomatous hydatids are multiplying internally, and seem to avoid the skin as much as possible. When ulceration has begun, it is not as in common abscesses, because matter has approached the surface with a previous elongation and as we call it pointing of the skin. For the fluid, which consists only of hydatids altered in their form by the loss of life, makes no progress towards the surface, but remains till the gradual sloughing and ulceration of the fungus exposes the cavity in which they are contained. When by these means the fragments of the cysts and turbid fluid into which the hydatids are converted by death escape, the sides of the cavity do not collapse like that of common abscesses, but expose a ghastly cavern, from which, instead of pus, a watery discharge is secreted, attended with a very peculiar

peculiar smell. Round the mouth of this cavern fungus sometimes grows, and instead of any attempt to unite the skin curls in a contrary direction, which encreases the depth of the cavity. This continues as long as any fragments of hydatid-tunics retain their life. As they die, ulceration takes place to detach them, like other extraneous bodies, and this ulceration being unattended with fungus renders the cavity for a time still deeper. After all the fragments of tunics are detached, the wound will perhaps continue stationary or heal, or more commonly another set of hydatids will be so far advanced as to produce a fresh sloughing or ulceration.

If this progress of the disease from the surface inwards was sufficient to admit the allusion to the supposed motion of a crab, the more rapid cases might with still more justice; for in these the patient will sometimes be destroyed before the skin is broken.

Thus you see we agree that the ulceration is not always attended with fungus. I need hardly

hardly add how perfectly our opinion coincides, that the disease is not propagated by the lymphatics.

On the mode of operating I may take greater liberties with you as a physician, than I could with most of my other correspondents.

If we could exactly ascertain how far the disease has spread, there might be some propriety in cutting out only the diseased part. But in a distemper that arises spontaneously, what reason can we have to suppose, that the whole of an organ similar in all respects to the part which took on the disease will not fall into the same. This you seem aware of, by proposing change of climate as a preventive. In this last proposition I cannot but agree with you, because the disease seems, if not peculiar to, at least only terrible in a cold climate. Celsus remarks, that though incurable, yet when left to itself it rarely shortens life. This was in Italy where the winters are often cold. In this country, though most of those cases have  
 been

been exhibited to me as a stranger, I have seen only two in an occult, and one in an ulcerated state. They were all of above twenty years standing and rarely attended with pain. In tropical climates the disease is scarcely known. But however mild it may be here when left to itself, it is said not to be less fatal after an operation than in England. The most experienced portugueze surgeon mentions the tragical consequences of amputation. I have been informed of two instances in Barbadoes, in which the event proved equally calamitous. I believe in both the disease had been brought from Europe.

It is not difficult to see the reason why carcinoma should spread faster after an operation than before. When a part is taken away, the process of nature is to restore it by granulation. This new substance continues for a time incapable of the functions of originally formed parts, and never acquires the same strength. It is therefore much less able to resist the growth and multiplication of hydatids, and forms a much better  
nidus

nidus for them. The modern method of bringing the cut surfaces together, and if possible uniting them by covering the whole with sound skin, so as to supersede the necessity of granulations, is therefore an important improvement. It is to this I suppose you allude in proposing to heal the part as speedily as possible.

But before we enter on the operation, it is at least decent to discuss one important question. Shall it be performed?

The late Dr. Alexander Monro, a name I never think of but with a kind of veneration, observes, that of sixty cancers, at the extirpation of which he had been present, only four patients remained free from the disease for three years: of these three were cases of occult cancers, and the fourth a cancerous lip.

Mr. Hill says that he has extirpated no less than eighty-eight genuine cancers, eighty-four of which were ulcerated, and all excepting two *recovered of the operation.*

Your

Your remark is very just, that a greater part of Hill's cases were probably of the lip, the cancer of which you suspect is different from that of the mamma; Monro's only successful ulcerated case was a lip.

The cancerous lip is often different from the carcinomatous hydatid, consisting only of a reddish granulated substance different from any thing I have ever seen, excepting in the testicle, nor can I form any satisfactory conjectures concerning the nature of it. It is however much more easily relieved by an operation, because in all the cases I have seen the disease has never been unconnected like carcinoma. By cutting therefore with the freedom which Mr. Hill recommends, and by uniting the edges without granulation, which has ever been the custom in this operation, a permanent cure is very likely to follow.

Even when the disease is true carcinoma, which I have frequently known the case, the lip does not appear so favourable a nidus as the breast. The hydatids are most  
of

of them smaller, not from their nature being different, but because few of them have arrived at their full growth. The quantity of fungus is for the same reason inconsiderable. After the operation the parts are much more easily united, and it has been the invariable practice, even of those who object to leaving skin, to heal these by the first intent, the parts being so particularly favourable for it. It seems also as if the lip in its new state, which supersedes its former looseness of texture and its consequent greater exposure, will prove a still less favorable nidus for carcinoma than before.

Besides these two cancers of the lip there are many others, some of which as I have remarked are very improperly so called,\* being the effect of morbid poisons and even curable by remedies. These frequently yield to caustics, which is never the case with the two I have particularized. Another reason, why Dr. Monro's patients were so generally unfortunate, might be the high reputation

\* See Morbid Poisons, Chapter on Cancers.

he had so justly acquired, and his residence in the metropolis. To such men, and to such places the worst of cases are generally brought as the last resource. Mr. Hill remarks, that in proportion as his reputation increased, his success was comparatively less. It appears too that most of his successful cases were not of the breast, for in page 9 (edit. 72) he seems to enumerate them all. "Of five cancered breasts only one was ulcerated, which with one of the occult kind did not heal; a third relapsed, probably from the whole not being fully cut away; and the other two are still firm and sound in 1772, though one of them was cut in 1761, and the other in April 1766."

If I am correct in my idea of this passage, of these eighty-eight cases only five were of the breast, four of which were in a fit state for the operation, and of these five cases only two succeeded. Mr. Hill calls the whole eighty-eight *genuine* cancers. It is neither my intention to doubt his veracity nor the propriety of the expression. But without questioning either, there is no reason to suppose

I

pose



pose that they were all carcinomatous. Most incurable ulcers we are in the habit of calling cancers: a reference to the "History of some remarkable cases that succeeded" will convince you that I am not assuming any great latitude in suspecting that most of them were not carcinomatous.

If I were to give the result of my own observation, I know not how to account for it, but where every thing has appeared to succeed, the patients have in many instances died within a year or two after the operation. Where they have lived longer the disease has sometimes re-appeared in a few months; sometimes at a period so remote, that all apprehensions concerning it were at an end. It is however much more satisfactory to give the result of other men's practice than our own, especially when their cases are related without any bias towards our own opinion.

The two successful cases of Hill remained found till the time of his publication, which made the period after the operation eleven  
years

years in one instance, and six in the other. Of the contents of these tumours we have no description; but in their previous history we are informed, that the first originated in a blow, as soon as the pain from which had subsided, the breast remained free from uneasiness for twenty years. The second had existed three years before the disease was pronounced cancerous. From that time a further space intervened, but uncertain how long, before the operation; the only unsuccessful case whose history is related was rapid.

In a review of Mr. Fearon's cases I shall also confine myself to the breast. In these there is less attention to dates; probably because, many of the patients being of the Dispensary, it might be difficult to learn their previous history, and still more so to trace the subsequent event.

The successful cases which continued well till the publication of his third edition in 1790 are nine.

Five slow cases : cases ix. and v. [the latter confirmed by the history of case ix.] cases xii. xiii. and xv.

Two more rapid cases : one (case xxi.) in a young woman, from a blow, ulcerated in two years ; continued well after the operation sometime in 86 to 90. The other (xxii.) successful for the same period, had existed three years before the operation.

Two cases : the previous history of which is not traced.

The successful cases, without any mention of the period to which they continued found, are

Three rapid : (cases v. xiv. very rapid, x. rapid after Mr. F. saw it.)

Two without any previous history.

The fatal cases are three : Two died whilst the patients were making up their mind about the operation ; (case iii. 18 months from the commencement of the disease, and  
case

case iv. of the commencement of which we have no account, but its progress was rapid during the period that Mr. Fearon traced it.)

Case viii. very rapid; died after a second operation. No exact dates. But from the commencement, which was without previous injury, to the close could not exceed six months; and the first operation appears to have been performed less than a month after the commencement of the disease.

One case communicated by Dr. Cheston; the event uncertain, but the prognosis unfavourable; the disease rapid; as far as can be collected from the case (xix.)

#### GENERAL SUMMARY.

Cases which continued well till the publication of the 3d edition in 1790 :

Slow . . . . .	5
More rapid . . . . .	2
Previous history unknown	2

—9

Cases in which the operation succeeded, but without any account how long they continued found :

Rapid . . . . . 3

Without any previous history 2

—————5

FATAL CASES.

Rapid . . . . . 3

Undecided, but the prognosis unfavourable :

Rapid . . . . . 1

From these and Mr. Hill's cases it appears, that the probability of success is to be estimated before an operation by the previous progress. If that progress has been slow, the aptitude must be proportionably small, and vice versa.

If a lump, of whatever description, gives no pain, there is at least a probability that it may prove steatomatous, melicerous, or lymphatic hydatids, or perhaps scrofulous.

If

If it should prove carcinomatous, or cruentous hydatids, as long as it is unattended with pain the progress must be slow, and whilst it is slow a short delay is unimportant. I should therefore under such circumstances state every part of the question as fairly as possible to the patient, leaving the decision to herself. But in all such cases consideration is to be made of her general character for temper, firmness of mind, and capacity of reasoning herself, or comprehending our propositions. If after all she is determined to suffer every thing in order to know the worst of her situation, (as will often be the case) the operation may be performed in order to ascertain the contents of the lump ; or if the tumour has arisen to an inconvenient size, though without pain or alteration in the teguments, it may be justifiable to remove it ; and should it prove steatomatous, as is highly probable, the operation will not be so painful as is generally apprehended. But when pain comes on, and the skin is corrugated and discoloured, the substance below continuing hard, circumscribed, and without fluctua-

ation, there can be no doubt that the disease is carcinomatous or cruentous hydatids, and that a fungus is formed between it and the skin. Even in this case I should not be forward in urging an operation, unless it was to extend greatly beyond the hardness, and unless the parts could be afterwards united by the first intention. But if every diseased part is removed, and the parts heal without granulation, whatever the future event may be, it is certain that the patient will suffer less by an operation in which so much diseased matter is removed, than by the progress of so painful a distemper.

If the disease has been of very long standing, and has even proceeded to ulceration without much pain or inconvenience, I should not urge the operation. But if the disease is rapid in its progress, or painful, or corrugation appears early on the integuments, the operation may be urged in the first instance, as a means of mitigating the patient's sufferings, whatever the event may prove; and in both instances with hopes that the patient may recover, or rather with

a cer-

a certainty that she cannot live long without it. The next question is, in what manner should the operation be performed?

The first step appears to me too often neglected, which should be to compare the diseased with the sound breast; and if we find a greater fulness in the former towards the neck and axilla, whether attended with stony hardness or not, we may depend on it that the disease extends beyond the indurated part. If a hardness in the axilla is attended with this fulness in the parts above-mentioned, we have every reason to believe that the multiplication of hydatids extends regularly from one perceptible induration to the other, though no fungus can be felt except in those parts where the hydatids from the greater rapidity of their growth and multiplication, or from their approaching nearer to the skin, have stimulated the parts to the production of this fungus. In this case, to remove all the disease would be to remove the whole breast and neighbouring cellular substance, extending superficially from the sternum to the axilla, and  
in



in depth to the pectoral muscle under the clavicle and still more inaccessible parts.

If at the same time the skin should be so much diseased, that it would not be easy to leave enough to cover such a surface, nothing could excuse such an operation but the importunity of the patient, or a confidence in the operator, that he has courage and perseverance equal to such an undertaking.

If the lump seems circumscribed and the axilla clear, the next enquiry is, whether every other part of the breast appears similar to the found one. In this examination some latitude must be admitted, the left breast being usually somewhat larger than the right. But this must be referred to the recollection of the patient at a period before the disease could have been suspected. We must also make allowance for any occasional exacerbations of inflammation, which extending much beyond the tumour by producing an encrease in the size and number  
of

of vessels, will for a time enlarge the whole breast.

Even after we have formed our conclusion, should the breast appear sound in every part but the lump, it will remain to be questioned whether the whole should not be removed. If the disease consists of any kind of encysted hydatids, it is probably unnecessary; but if of either carcinomatous or cruentous hydatids not inclosed in a cyst, whether they have arisen from an injury to the parts or not, I know not how we are to determine that no others have begun to multiply in the same breast. For though we may trace the disease to an injury, we never can ascertain the exact extent of that injury, how much of the breast may have been rendered useless by it, or how much of the coagulated lymph may have remained unabsorbed. Add to this, the pain of the operation is not greatly increased, and we shall be more certain of retaining skin enough to cover the crude surface.

But

But after the cutting part of the operation seems over, before the crude lips are brought together, it is absolutely necessary to see, if possible, whether the whole disease is removed. This is generally attempted by an examination of the surface of what is left and by feeling for any remaining hardness. Nothing can be more fallacious than such an attempt. The surface is often so much covered with blood as to render an accurate inspection impracticable, and carcinomatous hydatids may exist without a fungus, and consequently without a perceptible induration. Our attention therefore should be directed to the cut surface of the amputated part. This we can examine in the most favourable points of view without incommoding our patient, or without being interrupted by any bleeding vessels. If we find on its surface either an appearance of fungus or of that greenish yellow fat which I call the carcinomatous hydatids, or any thing different from the natural healthy state, we must apply to the corresponding surface of what is left, and renew our operation till what we cut off has a natural healthy

healthy appearance. “ Ut aliquid faciamus.”\* When this is duly considered, I leave you to determine, if by the previous appearance we have no reason to doubt that the disease consists of carcinomatous or cruentous hydatids, whether it would not be better at once to take off the whole breast.

If the cut surface of the amputated lump is found and the surgeon thinks it unnecessary to take off more, as soon as the patient is settled in bed, the next business should be to examine carefully the contents of the amputated part. If it is found free from carcinomatous or cruentous hydatids, the most favourable prognosis may be formed, and the patient should instantly be made acquainted with her consolatory prospects. If the operator is as well acquainted with his profession, and feels for his patient as he ought, no false shame will ever induce him to conceal that the operation might have been dispensed with. This could not with certainty be known but by the event, and the satisfaction he will diffuse by his

\* Celsus.

communication must be *his exceeding great reward*. Besides which, should the tumour prove steatomatous, melicerous, or lymphatic hydatids, it is highly probably they might have grown to such a size as to render a future operation desirable, were it only on account of their bulk.

If the contents prove cruentous hydatids in a cyst, we may form the same favourable omen; even if only attached to a loose fungus the whole of which with the hydatids have been removed, the patient may be encouraged, and her friends informed that the probability of a future operation is very remote. This prognosis will be very much strengthened if the parts heal by the first intent. If we find carcinomatous hydatids, how excusable soever it may be to make the best of things to the patient, we ought to describe the case to her friends as consisting of the worst kind of cancer, and the probability of relapse as uncertain, though the operation was attended with the most favourable circumstances. If the cut edges show carcinomatous hydatids, how free so-

ever

ever they may be from fungus, and from the nature of the parts the operator is unwilling to proceed further, we can only calculate the life and sufferings of the patient by the history of the previous progress of the disease.

After what I have said of the advantage of union by the first intent, it might appear unnecessary to repeat my objections against the caustic, were it not that some successful cases are so well authenticated, as still to preserve the reputation of a family receipt. I have never seen the whole progress of any case that has succeeded; on the contrary, all that I have witnessed have been greatly exasperated. I have however seen a female, who exhibited herself to such ladies as were disposed to try the remedy. Her breast had remained sound for several years, and her assertion was, that the whole cancer was drawn out by the caustic in a single lump, after which the parts healed readily.

We have happily a case most accurately described by Mr. Justamond, the whole  
having

having been under his own management. Having previously separated the cuticle by means of lunar caustic, he applied a mixture of antimony and arsenic in the form of a pledget cut to the size of the gland. The pain was intense for twenty-four hours. Several days after, on removing the pledget, the skin under it was found cracking all round and the tumour beginning to separate; to facilitate the separation, scarifications were made on the destroyed surface, and the crevices filled with arsenic and antimony reduced to powder. Contrary to the author's expectation, this application produced no pain; he therefore conceived it to be ineffectual, and, as soon as he perceived a separation at the edges of the tumour, applied the powder all round the separated edges, as low as it could be insinuated between the diseased gland and the sound skin. This produced more pain than the first application. By continuing the powder in this manner at intervals for little more than two months, the gland came out entire as a nut from its shell, or as if it had been clean dissected with a knife. The wound healed readily. Such

Such is the account of this valuable case from a man who relates his various failures with as much candour, as his success in this instance. The only omissions are a more minute description of the integuments before the operation, and of the contents of the tumour afterwards. Of the first however we may form a tolerable conjecture. "A very hard stubborn scirrhus in the right breast just above the nipple and an indurated gland under the axilla." It appears that the skin was entire, and as we have no account to the contrary, it was probably of its natural complexion.

Let me ask you, if you had seen such a tumour as this in the neck or the scalp, if you would not have called it steatomatous? and is not this presumption strengthened by every part of the history, by its insensibility when scarified and causticated, by the clean manner in which it was turned out, to use the expression of Hill and other writers of that date, to say nothing of the tumour in the axilla, which continued indolent for a year and a half afterwards, as long as we



have any account of the case. But it is well known that tumours in the axilla do not always partake of the nature of others in the breast.

I have never conversed with the proprietor of the Guy or Plunket remedy, but have known one and heard of other instances of truly carcinomatous breasts, which they have refused to touch, urging that they were not curable cancers. This makes me suspect that by long habit they have learned to distinguish in some instances steatoma meliceris and perhaps scrofulous tumours from true carcinomatous or cruentous hydatids. If so, we need not wonder at their success whenever they form a true prognosis; and although we cannot but regret that any unfortunate patient should suffer the tortures of these applications for months, to remove what might be taken away in a few minutes with comparative ease, yet it at least becomes us to do justice to their remedy as far as it deserves. There is however a still more important objection to this mode of treatment.

If

If we should be mistaken in our prognosis, how dreadfully must a true carcinoma be exasperated by the loss of health with which such painful applications must be attended! How great must be the loss of skin, and how large the cavity to be filled by spongy granulations! If the hydatids prove cruentous and are not contained in a sac, the fungus will rise infinitely faster than any future caustic can destroy it. That these events do frequently happen from the application of the above caustics appears, not only by the result after the tumour is removed, but also by the manner in which the tumour is described as separating. Mr. Juf-tamond remarks, that what he calls the gland came out clean, as if dissected by a knife. This is what we should expect if the tunic of a steatoma was separated from the surrounding parts. But when we are told of the roots of the cancer hanging in every direction, we cannot doubt either that the tumour was not steatoma, or else that there was some irregularity in the manner of applying the caustic. Unhappily the

event too often proves that the case was carcinomatous.

Having thus given you my opinion concerning the operation, I shall say a few words on remedies. You may smile when you find me once more an advocate for Storck. But if a man's moral character for truth stands fair, and he does all he can to improve the art, he surely deserves the thanks of mankind. In this light Storck has ever appeared to me, though I am persuaded he never cured a cancer: but he thought he did, and the honest Vanfweten was of the same opinion. He cured what was deemed a cancer, and where was he to learn whether it was really so or not? What is still more to the purpose, he described his cases accurately. Not satisfied with saying this was a cancer, as too many writers have been, we find every minute particular described, and whoever studies the cases accurately will be convinced, that few of them were such as might not yield to cicuta aided by the attention, rest, and diet of a well

well conducted hospital. The only two which were decidedly carcinomatous are the eighth and eleventh. The first of these was of six years standing, and the woman forty-five years old. The date of the other is not specified, but as the subject was sixty-seven years old, and gained a living by selling fruit in the streets, the carcinomatous aptitude could not be very strong, or the disease must have shewn itself earlier.

To Justamond we are indebted for a very accurate trial of cicuta arsenic and fixed air, in the very candid account he gives of his failure in each. If we take a view of such of his cases as appeared successful for a time, we shall find, whichever of the remedies was applied, it only promised success where the disease had been uniformly slow in its previous progress. The two women that lived longest under the use of his hemlock baths had very old cancers. The case which led him to hope he had found a remedy in arsenic had existed thirteen years before he saw it. Dr. Ewart's promising

case from fixed air was still older. That related by Mr. Simmons\*, though more rapid, continued more than a twelvemonth without pain; after that it seems to have been exasperated by ill health, poverty, and low diet. It was two years and a half from the commencement of the disease, when arsenic, rest, and a generous diet, produced in the course of five or six weeks a considerable amendment. I have known many other equally flattering instances, in which much more sanguine hopes were encouraged, than Mr. Simmons's prudence permitted him to indulge. Both patient and surgeon are apt to be too warm in their expectations, and the progress of most carcinomatous ulcers, excepting in very rapid cases, has often something flattering in it. Before ulceration exposes a dead hydatid or a family of them, increased inflammation is often excited, which as the cavity is opened, subsides, and the patient feels a temporary relief. If the progress of ulceration has been slow, and the hydatids solitary or few in

\* Reflections on the Cæsarian Operation, &c.

each compartment, they may be completely separated from the foveolus before it is exposed. In this case it may readily granulate and skin over, or cicatrize, according to the nature of the parts underneath. If the cavity remains lined in some parts with fragments of tunics, as these cast off, the surface appears cleaner, and gives a temporary hope ; and if the process has been expedited by the application of any caustic, we are willing to give our remedy its full share of credit. Doubtless the sooner these fragments are cast off, the greater probability there will be of the part healing before other hydatids are in a situation to keep it open ; or if ulceration has extended down to the muscles, a firm cicatrix may be formed.

From these cases it seems to follow, that all the success practitioners have hitherto flattered themselves with depends on the slowness of the progress of the disease. Thus we find that though Justamond's and Dr. Ewart's cases both healed for a time, yet

Storck's and Mr. Simmons's, though mild, were too rapid to be ever completely skinned over after they were once ulcerated. Wifeman\* gives three cases of this kind, which he calls scirrhus cancers. The first healed for a time; the others continued for many years without much inconvenience to the patients. Thus it generally happens that the cases which promise the greatest success are old cancers, or cancers in old people, which though held out as the most formidable, are in reality the most innocuous.—Formidable they may be truly called inasmuch as they place the disease beyond a doubt; but their former progress, or the late period of their commencement, evinces a comparatively less aptitude in the constitution; so that unless a change takes place in the general health or external circumstances of the patient, we have reason to hope that life will not be much shortened, and only occasionally embittered by this incurable malady.

\* Vol. i. pages 165 and 166.

After this I might plead my excuse for not offering any new mode of treatment or fresh remedy ; but as every thing is attended to that has hitherto been attempted in an incurable disease, you will indulge me with reading my conjectures. Such is all I have to propose, and in these I am not extremely sanguine : It is therefore the more necessary to collect all that is known of preventives.

Such is the well known sympathy between the uterus and mammæ, that the latter are usually somewhat turgid before menstruation : this is now and then attended with partial lumps. If these have been in the habit of returning and dispersing again for a series of years, we have every reason to hope such a constitution has no aptitude for the support of carcinomatous hydatids. But no lumps in such a part, from whatever cause, should ever be neglected. When they continue beyond the usual period, every attempt should be made to relieve them. In plethoric habits, whether of strong constitutions or not, evacuants, especially topical bleeding, are generally found successful.—

These



These should be particularly attended to at the time of life when the menstrual discharge has lately, or is expected to cease. In more delicate habits, particularly if attended with any chlorotic symptoms, and in which there may be danger of too free evacuations, chalybeate medicines are well calculated to encrease the menstrual discharge, if that should be necessary, and to excite so much action in the system as to produce the absorption of indolent tumours. At the same time it appears extremely important to support, by calico or flannel next the skin, a genial warmth over the whole body.

By these means, though our constant want of permanent success does not authorize us to expect a cure of true carcinoma, yet we have reason to hope that extravasated blood or lymph has been absorbed, which might otherwise have become a nidus for some species of hydatid.

As long as we have any hopes of dispersing a lump, we ought to continue our endeavours;

deavours ; because whatever its nature may be, should it be absorbed, every part of the breast may resume its original healthy actions : but after an operation this is scarcely to be expected. Even when we have reason to believe that carcinoma exists, I am not satisfied of the necessity of urging a hasty operation. Should the disease prove rapid, it would I fear outrun all our operations ; and if slow, there can be no danger in a short delay. It should however be constantly attended to, particularly if the period for the cessation of menstruation has approached. In all operations the great art is to lessen the terror of them ; and this is not likely to be accomplished by frightening the patient with threats of the knife or a lingering death. All this would be much better managed by a confidential friend well acquainted with her temper. Even a preference to the caustic, which it is certainly the duty of every surgeon to oppose, she should rather be reasoned out of than frightened. I have known many women, when treated as reasonable beings and left to reflect by themselves, after a just description

of

of the two methods, easily reconcile themselves to the knife, and bear the operation with a fortitude, which, from their former apprehension, was little expected.

Thus you see me without being aware of it running from the subject of remedies. What I have proposed are merely preventives, and have not even novelty to recommend them. You will readily suppose this is because I have none of my own to offer. None practically; nor such as should have been offered to the public without experiment, had I continued in London. It remains therefore for such of my correspondents as approve of my labours to give candid trials to those means, in the use of which I can only wish you success.

If my opinion of the disease is just, the indications of cure are, to supersede the multiplication of hydatids by the destruction of their life, or, if that cannot be accomplished, to lessen as much as possible the constitutional aptitude for their support and multiplication.

We

We well know there are certain diseases which lessen, or for a time destroy the aptitude of the juices for the support of other animals. On the access of the small-pox fever, it is no uncommon thing to see worms leave the human body, even without the common efforts of the subject in which they before existed.

I know not whether any patient with carcinoma has been seized with the small-pox; or if she has, what have been the consequences. The cow-pox, which is a much less dangerous disease, though often attended with a more violent fever, may with safety be used as an experiment. Should it succeed, we ought afterwards to expect less of the *darting*, or as other women call it the *growing kind* of pain, but that ulceration would soon commence or encrease, if it had already begun. Should the growing or darting pains return, we might suppose that some hydatids had recovered themselves, or that others, which before were in a stage too early to be affected by the change which was produced in the constitution, are now become

become active. In this case the cow-pox infection might be repeated.

That this is analogous to the mode of destroying other parasite animals is certain. Lice may be destroyed by a mercurial application to the hair; but the nits are not. A second application is therefore necessary after a certain interval. Those animals which in this country produce a disease very much resembling the itch, are readily destroyed by sulphurous or mercurial applications; but repeated applications are often necessary to destroy others as they arise in different parts. If the cure is effected in the autumn, it is often necessary to repeat it in the spring, notwithstanding a continuance of the remedy however long persisted in during winter. But of this subject, when I have collected a sufficient number of facts, I have promised our mutual friend Willan a fuller account.

I was preparing to beg you would not smile at my proposed remedy; but, if you judge by your own feelings, there is no danger

ger of your imputing what I have offered to a sportive fancy. I am the bolder in suggesting this remedy, because, whatever difference may exist in our opinions of the nature of the disease, still the mode is perfectly consistent with what we were both taught, and what seems daily growing the general opinion, namely, that diseased actions, which the original powers of the constitution are unequal to alter, must be changed by some counter-irritation, which though more violent is not permanent like the first disease.

In the other indication of lessening as much as possible the constitutional aptitude, we have every phenomenon of the disease to direct us. Whatever induces debility, or lessens the actions of a part, renders it less able to resist the growth or multiplication of hydatids. Excepting in those rapid cases which I consider as altogether hopeless, the disease usually occurs late in life, in a part the vascular action of which is much enfeebled, and which, by its projecting and now somewhat pendulous state, cannot but carry on a still more feeble circulation. It

is always much exasperated by those causes which tend to lessen the circulating powers, particularly cold and meagre diet, and most of all anxiety. The internal remedies which have promised only a temporary success, have been what are called tonic. We cannot doubt the tonic powers of arsenic, when we see its effects in intermittents and chronic headaches. We ought however to be aware of the difference between metallic poisons and vegetable narcotics. The latter may by degrees be so familiarized to the constitution as to become almost a part of diet: but no continuance will enable us to encrease the dose of arsenic to any considerable quantity, should even the stomach permit it. I have seen ulceration produced in several parts of the sound skin by such an attempt.\*

The advantages of chalybeates are well known, and above all a generous diet, warm cloathing, a mild climate, and an equal flow

\* See Morbid Poisons, page 137. Mr. Simmons's case confirms this fact.

spirits. The latter is certainly as important as any other remedy : we often see the disease exasperated to an astonishing degree after the sentence of amputation has been pronounced. In every stage of the disease tepid bathing may be found useful, as it is now clearly ascertained to be a remedy which, judiciously used, stimulates without producing subsequent debility. During a paroxysm of pain it is very likely to mitigate its excess. Whether hemlock baths owe their efficacy to any thing more than their warmth and the narcotic particles inspired remains to be proved.

Topical applications may be varied according to the state of the disease. If arsenical solutions are applied, it should in my judgment be only to the sloughs. Justamond remarks that the arsenical caustic is the only one that can be depended on, when fungus arises after the extirpation of any encysted tumor : He advises it to be applied early and freely. If as I suspect this fungus arises from some part of the cyst or tunica being left, is it not probable that the



effect of the arsenic may be to extinguish the life of the remaining fragment? Mr. Cline, you have seen, applies lapis septicus to the whole internal surface of the cyst when it is not removed. I have seen red precipitate answer the purpose, though not so soon. But neither precipitate nor arsenic should be considered as caustics, as they do not produce their effects by chemical combination, like the caustic alkali, but by inducing so much inflammation that the part dies in consequence.\* When therefore arsenic is used, it should only be to the sloughs or tunics, as the pain it produces on the fungus is much more violent than the advantage derived from it authorizes. If the part is truly a slough, no pain will follow; nor will any other effect be produced: if fragments of tunics, their life may be destroyed without giving the patient unnecessary pain.

\* Of this difference Justamond must have been aware, when he found that arsenic would not affect the sound skin.

Such

Such is I fear the extent of our knowledge even in palliatives ; and when the carcinomatous period is much anticipated and its progress rapid, these are likely to avail us but little : For though I have never heard of a rapid case occurring in a warm climate, yet when it has commenced in a cold one, it rarely allows time for the preparation for and completion of a voyage, before more mischief will be done than can ever be repaired.

There still however remains a remedy, which, if not found useless before you receive this, is at least entitled to a trial. As far as can be collected from the accounts of the effects of nitrous acid, it seems generally agreed that it quickens the circulation, increases the appetite and spirits, and mends the health. Some of its advocates have observed, that if mercury produces its effects by exciting a new action, nitrous acid has the same advantage, and instead of the debility which follows the use of the old remedy, the patient feels himself stronger and more chearful. If it should be found

by these means to enable the constitution to resist the growth of hydatids, it may be occasionally resorted to, particularly after the cow-pox fever, should that induce too much debility.

From so hopeless a prospect the mind turns with some relief though encreased anxiety to an institution founded on the most benevolent sympathy, and conducted with more fidelity and tenderness than the first institutor could expect. But this like most others exhibits only the short-sightedness of human nature! How often does the recollection of my visits to the cancerwards at the Middlesex Hospital remind me of the expressive language of the bearer of my former letter, when he acquainted me with his reasons for leaving Bristol for Madeira. "I saw," said he, "those who on their arrival took their morning exercises on horseback, in a short time confine their rides to the Wells and home. After this I saw them conveyed thither in carriages; soon after I saw them no more! Such was the gloomy prospect that determined him  
to

to quit a spot too often the last resource of those who suffered with his own complaint." Is not such the prospect without a single exception in the disease we are now speaking of? What therefore must be the suggestions of those who have constantly before them,

— crudelis ibique

Luctus, ubique pavor, et plurima mortis imago.

VIRGIL.

Or, as our own poet more expressively says,

————— Despair

Tended the sick, busiest from couch to couch,

And over them triumphant Death his dart

Shook, but delay'd to strike, tho' oft invoc'd.

MILTON.

You cannot suspect that it is any part of my wish to under-rate the design or execution of this well-intended plan. But I cannot help thinking, if its founder had been aware of the sentiments that must arise in the mind of each sufferer from the prospect before her, he would have directed them to be placed in different wards. Perhaps if  
they

they were lodged in different houses, with small pensions and the assistance of medical attendants, their lives might be prolonged and their sufferings mitigated. There might also be the advantage of tracing the disease with more certainty to its issue, as each patient would have less inducement to quit such an asylum.

Adieu, dear Stokes. This is a long letter, and perhaps you will say to little purpose. But if it offers a single new idea that you can improve, I shall be of a different opinion. Let me go further and suggest a few advantages that may be derived from this correspondence. If our hopes of the proposed remedies are few, we may at least cease to feel surprised that no former ones have succeeded, and have the more encouragement in attempting a different mode of cure. If the prospect of succeeding is more remote, we are, I trust, assisted in our prognostic.—In the most forlorn state, to know the worst is often more consolatory than a state of anxious uncertainty; but in cases comparatively mild, to be confirmed by a knowledge

ledge

ledge of the most probable laws of the disease, that though incurable it is likely to be neither often painful nor necessarily fatal, is of itself the most important remedy yet known. The mind is thus relieved of that anxiety which of all things most debilitates the actions of life, and renders every part less capable of resisting such a disease. If recourse is had to the knife we may form a future prognosis with some certainty, and the patient and her friends may often enjoy the unalloyed satisfaction of each other's society. Lastly, we shall not be at a loss in varying our palliatives, all of which are of that class which must tend to protract life and to increase its enjoyment.

Adieu, &c. &c.

JOSEPH ADAMS.

THE END.

*By the same Author,*

Preparing for the Press, a New Edition, much enlarged in the number and description of Poisons,

OBSERVATIONS ON MORBID POISONS,  
PHAGEDÆNA AND CANCER.

In which most of the controversial parts and the chapter on Cancers will now be omitted.

