

Researches physiological and pathological : instituted principally with a view to the improvement of medical and surgical practice / by James Blundell.

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Blundell, Dr., 1790-1878.
University of Glasgow. Library

Publication/Creation

Southwark : E. Cox, 1825.

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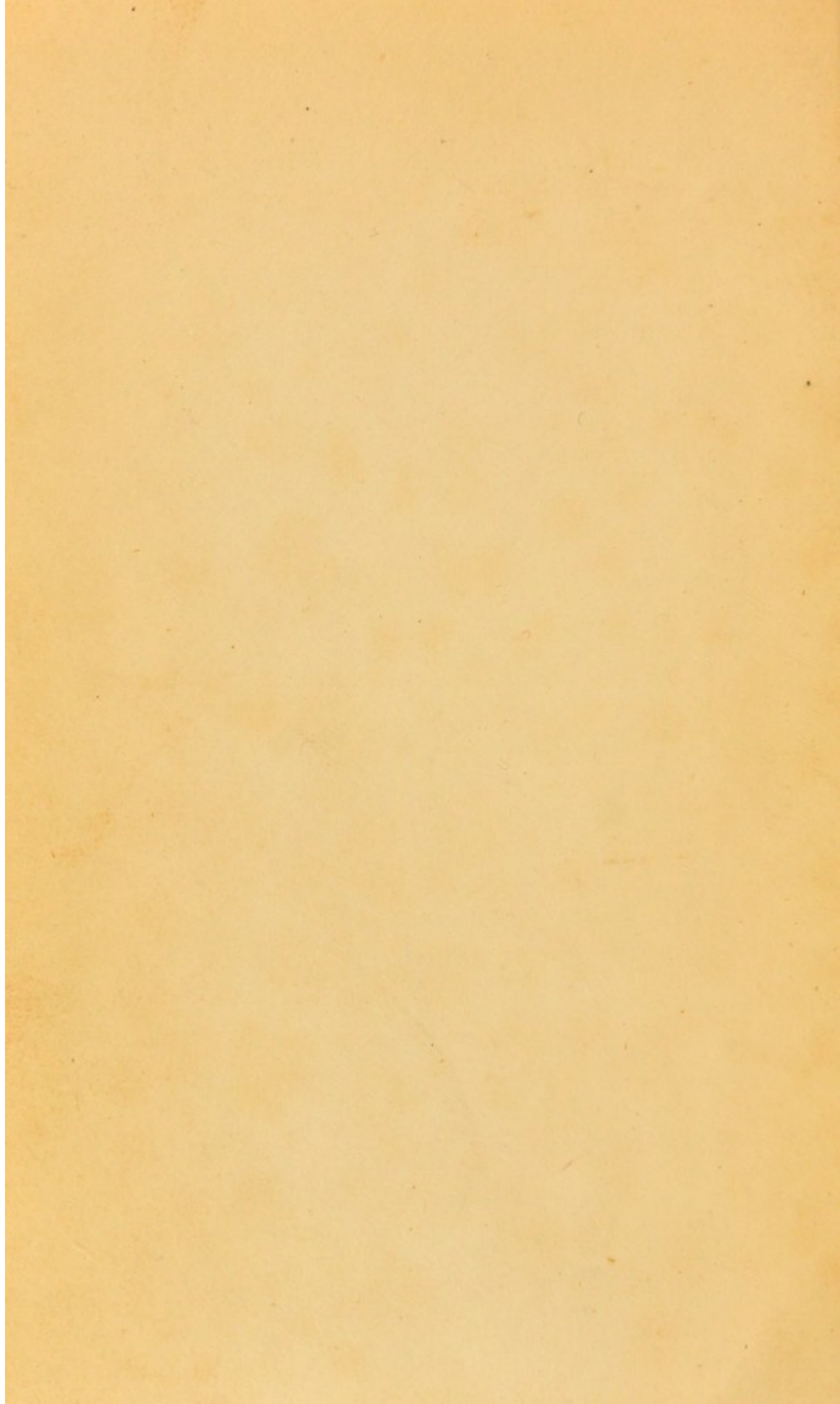
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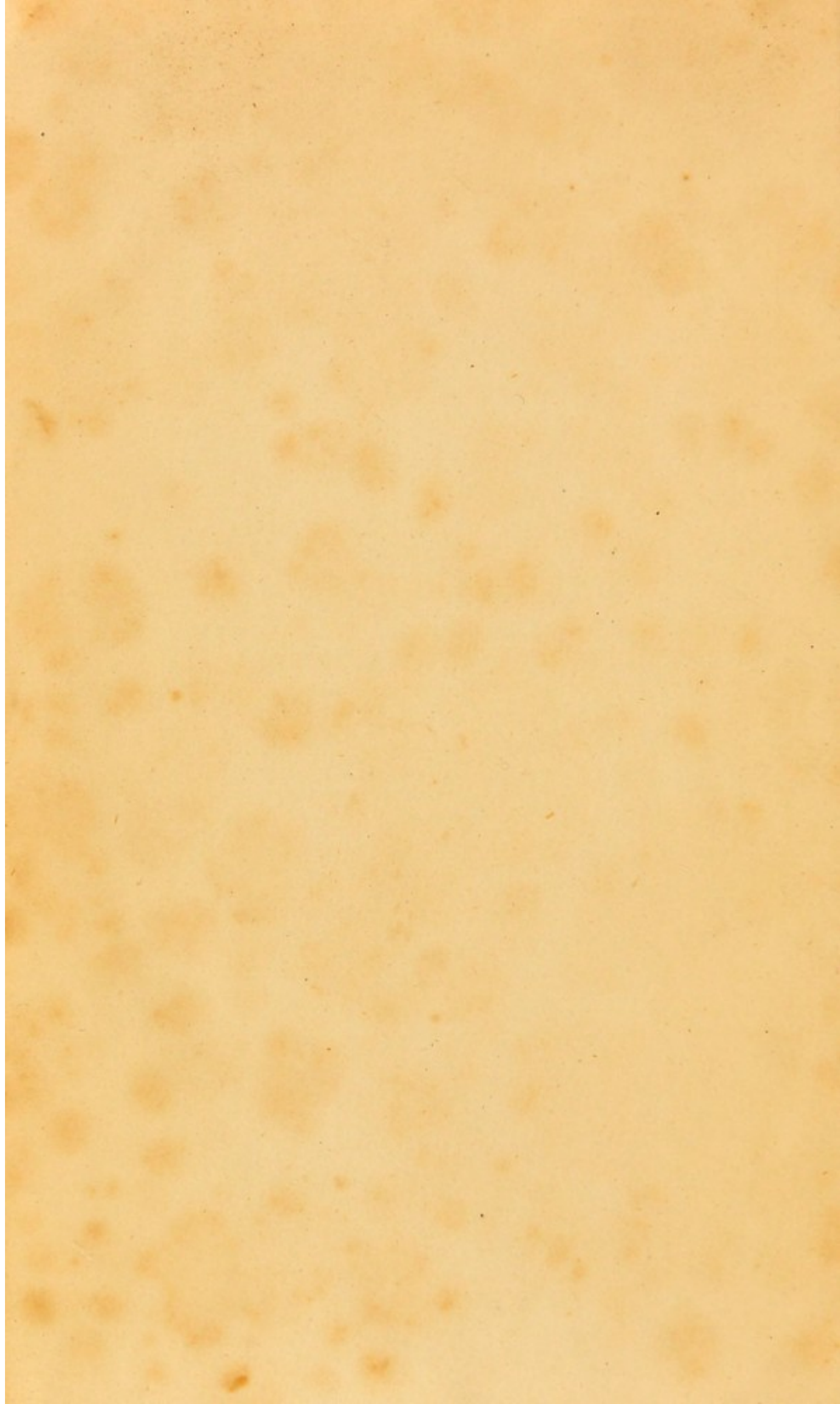
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RESEARCHES
PHYSIOLOGICAL AND PATHOLOGICAL.

ERRATA.

Page 14, line	11, <i>for</i> Dr. Hull	<i>read</i> Mr. Windsor, assisted
	by Dr. Hull—See Med. Chir. Trans. 10—358.	
19, —	13, — three	— two.
— —	15, — two first	— first.
48 — 1,	11, — opposition	— apposition.
65, —	10, — conjuncture	— conjunctures.
69 , —	12, — entered in	— entered.
86, —	4, — eighteen	— eight or ten.
110, —	3, — <i>irregular</i>	— <i>regular</i> .

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RESEARCHES *of the*
University of Glasgow
PHYSIOLOGICAL AND PATHOLOGICAL;
by the Author. with
INSTITUTED PRINCIPALLY
best respects

WITH A VIEW TO THE IMPROVEMENT
OF
MEDICAL AND SURGICAL PRACTICE.

BY
JAMES BLUNDELL, M.D.
LECTURER ON PHYSIOLOGY AND MIDWIFERY AT THE
UNITED HOSPITALS OF ST. THOMAS AND GUY.

"Thou art a blessed fellow, to think as every man thinks; never
a man's thought in the world keeps the road-way better than thine,
and what accites your most worshipful thought to think so?"—
Shakspeare.

"Sat mihi est me Platonem audire."

"Contentinsi del giudizio del tempo il quale per essere padre della
verità è giudice senza passione suol dare delle scritture giusta sen-
tenza."—*Baldes. Castiglion.*

E. COX AND SON,
ST. THOMAS'S STREET, SOUTHWARK;
AND JOHN COX, BERNERS STREET, OXFORD STREET.

1825.

1) Note to page 22. Since the publication of this paper many years ago, and in many cases of accidental division of the peritoneum have been communicated to me by different surgical friends to whom they have occurred in the course of their own practice. In these cases happening in men, women or children, the abdomen has been variously laid open by stabbing cutting tearing or gnawing, and in almost every instance in which the intestines and other viscera have stood clear of grave injury as (stabbing cases) ⁽¹⁾ perhaps excepted, they so frequently do, the patients seem as a matter of course to have recovered. When communicated by men of sense & probity as falling under their own eyes, these cases almost always present the same features. "Yes, the patient got well, no, there was no danger to the peritonitis! No, I never met with any other cases of the same kind in which the patients did not recover. Yes, this is the only case or these are the only cases, (as the fact may be) which have occurred in my practice, and they recovered without difficulty." That the intestines protruded, that they were replaced with difficulty, that it was necessary to enlarge the opening, we are sometimes further told. Owing to the epidemic overfear of peritonitis rife among the surgeons, these cases are generally mentioned as remarkable, "it is very remarkable, (meaning extraordinary) - it is very remarkable that the patient got well." The truth however really is exactly the reverse, it would be very remarkable, if the patient, did not get well, for recovery in these cases is not the exception, but the rule. It is easy to verify all this by making the necessary inquiries. Truth is always the same. James Blundell M.D.

Addition to page 27. Relying ⁽²⁾ on the above principles, in the month of February 1829, in the presence of Dr. Elliotson, and of three eminent surgeons, Bramley Cooper Esq., Aston Key Esq., and Dr. Morgan Esq. Dr. J. B. Blundell (the author of this paper) removed the uterus by incision through the outlet of the pelvis, the organ being brought down per vaginam like the ovum of a six weeks miscarriage. The case was one of bleeding disorganization of the os uteri, of a kind under ordinary treatment uniformly fatal. The patient recovered completely, becoming

(1) ὁ δ' ἰατρός προσελθὼν ἐπειράτο τῶν ἐντέρων ἀπρώτων διαμεινάντων, ταῦτά τε καὶ στήν, καὶ τὸ τραῦμα διαφύλαττεν. Plutarch. Galo Minor. 6.70.
 (2) πρὶν Νίκαρχος ἦκε γούγων, τετραμμένος, εἰς τὴν γαστέρα, καὶ τὰ ἐντέρα ἐν ταῖς χερσὶν ἔχων, καὶ εἶπε τὰ γεγενημένα. Xenoph. Anab. L. ij. 6. v. yet 30 or 40 hours afterwards this man (an officer in the army) was well enough to desert to the Persians in the night. L. ij. 6.3.

blump and well, but died on the 7. February 1849, so that she lived twelve months within a few days after the performance of the operation, for it was on the 12. February in the year preceding, that the uterus was taken away. Death was clearly produced by Enteritis following constipation, in part it may be occasioned mechanically from constriction and altered position of a portion of the bowell tied by adhesion, to which the inflammation resulting from the incision seemed to have given rise. On examining the body after death, Dr. Hodgkin found that the womb had been entirely removed. (See Medical Gazette Vol. iiij p 797. London) and in the Museum of the Royal College of Physicians London it is now (1843) to be seen entire, having been presented to the Museum of the College by Dr. Blundell. The above operation was the second of four, in the other three, the patients did not recover, they were all cases without other hope. The 3rd & 4th operations were performed in consequence of the encouragement derived from the success of the second, and both of these were more neatly executed than the second and successful operation had been. Of these two last cases however it may be observed that the disease was very far advanced. In the opinion of Dr. Blundell the suc-

PHYSIOLOGICAL OBSERVATIONS

AND EXPERIMENTS.

successful case derives its chief value from the proof which it affords of the validity of the general principle on which it is grounded, namely that power of the abdomen to sustain operations of surgery, is greater than British Surgeons especially has been supposed. In these four operations, the womb was removed, by first through the outlet of the pelvis, dividing with a scalpel the uterine attachments; the abdominal parietes were not touched. The two first of these operations, the second of which was the successful one, were the first of the kind performed in Great Britain. Under more favorable circumstances, better success may be anticipated.

(3) James Blundell M.D.

Addition to page 31. In conformity with the above principles the enlarged ovary during the twenty years which have elapsed since the publication of the paper, has now (A.D. 1843) been removed in several instances successfully, in some by shorter incisions, in others by longer. The first operations by long incisions performed in Great Britain were those of ~~W. G. A.~~ the last operations of this kind known to Dr. Blundell are those of ~~W. G. A.~~ done with an admirable combination of caution and enterprise, and the first which in London have been performed with success. At Mr. Walnes operations, two in number, Dr. Blundell was present. The incision was made in the Linea alba, the length was of ten or twelve inches, the ovaries were dropsical, weighed sixteen or seventeen pounds (imperial weight) and were removed entire without topping. There were no

adhesions. The ligatures, several in number, formed a complete skein which stretched from the side of the pelvis to the linea alba where the gut hung forth through the wound. These ligatures, of course contained within the peritoneum, remained for several weeks before they separated and came away. There were ten threads in the first of these two cases and eight in the second, and in this second case, a short but smart attack of phlegmasia dolens supervened. Notwithstanding the wound of ten or more inches, and the peritoneal lesions, to say nothing of the phlegmasias in neither case did diffused and dangerous peritonitis at any time occur. These operations, commencing with those of Lizars, performed all of them (and they are now not few) since the publication of this and the annexed paper, are grounded, as Lizars in relation to his own operations, has with a noble frankness stated, on those general principles which these two papers contain. Of these two papers, the first, (on a few controverted points respecting generation) read before the Medico-Chirurgical Society (now the Royal) in the May of 1819, was published in the year 1819 in the 10th Volume of their Transactions, and the second, (the above paper on abdominal surgery) read before the Society in 1823, four years after the former, was as a Publication in the Transactions declined, so that it did not appear in print, till the year 1825, the date of this volume. That in the course of ages numbers will hereafter be saved by the means here recommended, the use of surgery within the abdomen, there is full reason to hope, and surely this today nothing of those that have been preserved already, furnishes a sufficient answer to all the obloquy with which from well natured but misdirected feeling, the author, and the authors experiments have been assailed - Strike Gentlemen, but hear! You who would join the Cough at the Egyptians, which will you sacrifice, your women or your Cats?

James Whistler M.D.

Though first in order of time, ^(a) this paper stands second in order of place in this volume - (see pp. 34 et seq.) - For the first principle of the paper on abdominal surgery, see this former paper on Generation (pp. 41 and 42. of this volume) the passages marked in the margin with red ink.

See Lizars System of Anatomical Plates (the printed descriptions which accompany them) Part VII. The Brain, second & concluding portion The Dedication. Edinburgh 1825. To the sagacity & boldness of Lizars abdominal surgery is deeply indebted. J. W.

*As a little child playing at blindmans bluff
So is man blinded by prejudice when seeking for
the truth. J. B.*

3

PHYSIOLOGICAL OBSERVATIONS
AND EXPERIMENTS.

On Abdominal Surgery-
SUBSTANCE OF A PAPER

READ BEFORE THE

MEDICO-CHIRURGICAL SOCIETY OF LONDON

IN THE YEAR 1823:

ΠΑΡΑΤΑΞΙΣ ΜΕΤ' ΑΝΑΓΝΩΣΤΗΝ ΔΕ.

NOT PUBLISHED*.

See pages 41 & 42. in this Volume marked with red ink.

OF all the Branches of Surgery there is none, I conceive, which in this country admits of greater improvement than the Surgery of the Abdomen, the great importance of which it is unnecessary to enforce. With a view to this improvement it is, that I have been induced to accumulate the following Facts and Observations; and in laying them before the Profession it may be proper to remark, that where there is no observation to the contrary, I can vouch for their being substantially correct.

* As this abstract was made several months after the reading of the Paper before the Medico-Chirurgical Society, and as some of the Rabbits died in that interval, the results and inferences here stated do not correspond *exactly* with the inferences and results as stated in that Paper.

In four experiments the left kidney was taken out of the rabbit, through an incision upon the outer edge of it, about an inch long, very large for the bulk of the animal: the kidney was drawn up through the wound, and the superior half of the peritoneal attachment, thus put on the stretch, was, together with the vessels, included in the ligature. The rabbits were of a spare habit, and were all under their full size, as they had not reached their puberty. Of these animals,

1. The first died about sixty hours after the extirpation, with inflammation of the abdomen.

2. The second died about four and a half days after the operation, with the same disease strongly characterized.

3. The third rabbit recovered, lived for five or six weeks and then died, from a cause which ill-health prevented me from exploring.

4. And the fourth also for a short time recovered, fattened, and grew, but at the end of five or six weeks it died in like manner, with a sack in the seat of the extirpated kidney, formed by the peritoneum, and filled with a semi-fluid, in colour and consistency like custard; the cyst was not burst; the remaining kidney was, *I think*, enlarged;

the spleen was black, the liver was dark, the kidney was rather pale.

In seven experiments I took out the spleen; four of the rabbits were of spare habit, and of the same size with the former, and three of them were full-grown bucks, with the omentum, kidney and other parts well loaded with fat. Of the full-grown buck rabbits,

5. The first died about three complete days after the operation, with abdominal inflammation.

6. The second died about four days complete after the operation, with well-marked inflammation of the peritoneum, as in the preceding case.

7. The third recovered for a time, and seemed likely to survive, but three months and a half after the operation, it died with a diffused peritonitis, and a large sack between the left portion of the liver and stomach, as big as a large orange, and full of a fluid like whey and custard mixed.

Of the smaller rabbits,

8. The first died five complete days after the operation, with purging and inflammation of the peritoneum.

9. The second recovered for a time, but at the end of six months began to pine away gradually, like the former (7), and died ultimately with inflammation of the abdomen, effusion of coagulable lymph, firm adhesions, and a cyst in the region of the spleen, as large as the kidney of the animal, and full of a thin pus.

10, 11. And the remaining two recovered permanently.

In five rabbits I opened the abdominal cavity over the bladder to the extent of half an inch, in the course of the *linea alba*, punctured the *fundus vesicæ* with a lancet, and secured the aperture by ligature. Of these rabbits,

12, 13, 14. Three recovered completely, and were killed for inspection; and two died.

15, 16. One of them, fourteen days after the operation, with the external wound unclosed; the other, seventeen days after the operation. Both were a good deal emaciated, and there were no decisive marks of peritoneal inflammation.

In two experiments on rabbits under the adult size, I cut off at least one quarter of the bladder at the fundus, with one stroke of the scissors; a ligature had been previously applied.

17. One of these rabbits died seven months afterwards, full grown, and not obviously cachectic, with one of the purulent sacks already described seated internally, immediately over the abdominal wound.

18. The second rabbit is alive still, and appears large, fat, and healthy.

Into the peritoneum of four rabbits I threw about an ounce of human urine, of a full yellow colour; left it there for an hour; then discharged it, and washed out the peritoneum thoroughly, by injecting tepid water. They all suffered much collapse from this experiment; and while the urine remained in the abdomen among the viscera, they dragged the hinder legs after them, as if slightly paralytic: the injection of the tepid water seemed to soothe them. Of these rabbits,

19. The first, a fat buck, never recovered from a state of collapse, and died in less than twenty-four hours after the experiment. The peritoneum exhibited no obvious marks of inflammation.

20. The second, also a fine fat buck, died in sixty hours, in part, at least, from peritoneal inflammation; there was purging. The inflammation seemed greatest nearest the wound.

21. The third, a rabbit under the full size, of spare habit, was destroyed in nineteen hours, with the most diffused and active peritonitis I ever saw in this animal. In this last rabbit I found small chrystals of urinary salt, scattered all over the peritoneum, from which the urine had been too negligently washed out.

22. The fourth rabbit, also under the full size, recovered completely; and is now, twelve months after the experiment, large, fat, and to all appearance perfectly well.

In seven experiments, I injected into the peritoneal-sack, eleven drachms of the *decoctum quercus*, *Pharmacopœiæ Londinensis*; the rabbits were under the full size and spare. Of these rabbits,

23. One died in fifteen hours, with purging; and, I think, a diffused peritonitis in the incipient state. The extractive of the bark was found lying about in the peritoneum. The intestines were tympanitic.

24, 25, 26, 27, 28. Five others died, between twenty and thirty hours after the injection, apparently from the same cause:

29. And one got completely well.

In experiments 27, 28, 29, the decoction was of the full strength; in experiments 23, 25, 26, it was reduced to half strength; and in experiment 24, to a strength of one-third; it was therefore of the full strength in the rabbit that recovered. The rapidity with which death ensued in these experiments, deserves particular notice.

The peritonitic inflammation, which I have had repeated occasion to mention, in giving the results of these experiments, was marked, in the more decisive instances, by serous effusion; by accumulation of adhesive matter; by the agglutination of the different viscera to each other, and the peritoneum; and, in some of the rabbits, by a thorough injection of the smaller vessels (on the large intestines especially) with blood, so that they exhibited a petechial appearance.

From the facts ascertained by the preceding experiments, the following inferences may, I think, be fairly drawn:—

1st. Large apertures into the peritoneum of the rabbit do not immediately induce a dangerous prostration of strength. In all my experiments, I never once observed any marked collapse in the animal at the moment when the peritoneum was laid open, though I was in full expectation of it.

When urine was injected, collapse was immediately and evidently produced.

2ly. Large apertures into the peritoneal sack, in the rabbit, are not necessarily, nor perhaps generally productive of fatal inflammation. Of eighteen rabbits not only opened, but subjected to further violence, five only died from this cause; (1, 2, 5, 6, 8;) the remainder, thirteen in number, (3, 4, 7, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18,) either recovered, or were carried off by some other affection. As, in the eleven remaining experiments, a strong stimulus was applied to the peritoneum; these, of course, are excluded from the computation.

3ly. In the rabbit, the kidney, the spleen, and a large piece of the bladder may be extirpated without necessarily causing death; though death under the first operation is probable. Of four rabbits, all died ultimately from the renal operation; two, however, not till one or two months afterwards. Of seven rabbits, five died (5, 6, 7, 8, 9) from the splenic operation; and of the same number only three died (15, 16, 17) from the operations on the bladder.

4ly. When the abdomen is laid open, and parts are removed from it in the rabbit, the first danger

arises apparently from collapse, (19); the second from general inflammation, (1, 2, 3, 6, 8, 20, 21, 23, 24, 25, 26, 27, 28); and the last from chronic topical disease, (4, 7, 19, 17.)

5ly. The rabbit's abdomen is very tender, probably no less so than that of man. Of twenty-nine rabbits, twenty-one died more or less directly from the operations performed, some of them, it must be confessed, violent ones; and it should be observed particularly, that five, out of seven rabbits, died from the splenic operation carefully performed; though both cases, hereafter recorded, in which the human spleen was removed, under circumstances to appearance highly unfavourable, terminated in complete and uninterrupted recovery. The general impression left on my mind by many observations is, that the abdomen of the rabbit is, on the whole, no less tender than the human.

6ly. It follows, from the former inference, that success in abdominal operations on the rabbit, furnishes a presumption in favour of success in similar operations on the human abdomen; and therefore, from these experiments, we may infer *presumptively* that moderate openings into the human peritoneum will not necessarily, nor even generally, prove fatal from inflammation or other-

wise; and, further, that certain viscera or parts of viscera, not essential to the welfare of our structure, may be removed from the belly without necessarily, or even generally producing death. The extirpation of the kidney must be highly dangerous; but there is a presumption in favour of the successful removal of the spleen, the ovaries, or even of large pieces of the bladder. But to proceed to observations on the human body.

Of slighter injuries of the abdomen, it is unnecessary to make a large enumeration. Tappings, slight wounds in which the intestines are not laid open, hernial operations with extirpation of small pieces of the omentum, and in Mr. Pott's case of both the ovaries, in general confessedly do well; and where death occurs, after the operation of hernia especially, it is sometimes rather referable to some accidental concomitant, as delay, for example, than to any thing of necessity emanating from the nature of the operation itself.

Of severer injuries of the abdomen, with their results, the following may be adduced as having, with few exceptions, fallen under my own notice, or that of my friends; and, as possessing an authenticity, on which, where there is no observation to the contrary, I can thoroughly rely. These, as it will be perceived, so far as they

furnish inferences at all, confirm those taken from experiments on the rabbit; and form, apparently, a part of one harmonious system of facts, which mutually support each other.

30.—1st. One case, the only one I know of, in which the mouth of the womb was torn off, and came completely away; large bleeding and collapse were produced, but the patient recovered. My friend, Mr. Scott of Norwich, carefully investigated this case, and Dr. Merriman of London is now, I believe, in possession of the preparation.

31.—2ly. One case, in which, from defective formation of the external genitals, the child's head could not readily pass: it forced its way into the rectum, and was born at the anus, occasioning three large rents, two laterally, and one forward; the woman recovered without any very pressing symptom. Mr. Harrison of Greenwich, had the woman ultimately under his own care; and himself, in conjunction with my friend Mr. Gaitskell, obliged me with the relation of it.

3ly. Four cases of chronic inversion of the womb, in which the uterus was extirpated by ligature, at different ages.

32. One, a case under the care of Mr. Chevalier. The woman in this instance was, I think, about

sixty; and for years previously had ceased to menstruate : no bad symptoms seem to have occurred.

33. The second, a case under the care of Mr. Newnham. The woman was about twenty-four. Some difficulties arose from the extreme irritability of the patient, but the greater part of the womb was got away. The preparation of the womb I saw myself. The woman is still doing well, and it is now six or seven years since the operation was performed.

May 1841
Mr. Winslow assisted by
34. The third, a case in which Dr. Hull of Manchester operated. The patient was excessively irritable and intractable, and some difficulties occurred as in the former instance, but the operation succeeded. Dr. Hull himself related this case to me in a conversation between us.

35. The fourth, a case of my own, in which the greater part of the womb was removed by a wire ligature. It came away in eleven days. This patient was of a tranquil torpid habit; and not one bad symptom occurred.

36.—4ly. One case has fallen under my observation, in which a fall from the top of a coach occasioned a transverse rent through the abdominal coverings, above the abdominal rings, on the right side, four fingers broad at the least.

The intestines hung out. The man recovered in a few weeks. The intestines still protrude at this part, pushing the abdominal coverings before them, and forming a ventral hernia. The scar of the rent is still apparent. The man was under the care of Mr. Green, one of the surgeons of St. Thomas's Hospital.

5ly. Two cases, (may be mentioned,) the only ones within my knowledge, in which the human spleen was removed.

37. One that of the soldier whose side was laid open by a sabre wound at the battle of Dettingen, (if my memory serve,) the spleen protruding and lying out for some hours in the dirt. It was removed by the surgeon. The man recovered, and seemed to suffer afterwards no inconvenience referable to the want of the spleen. Mr. Cline used to relate this case.

38. A second, that recorded by Dr. O'Brien in his inaugural dissertation.* The case was under his own personal care. The man was a native of Mexico: the spleen lay out for two days before the surgeon was applied to: the bleeding was profuse: the vessels and other connexions were

* Edinburgh, 1818.

secured by ligature, and the spleen separated completely from the body on the twentieth day of the wound. On the forty-fifth day the man was discharged from the hospital, cured; and observed to some one about this time, that "he felt as well as ever he did in his life." There was bloody urine till the tenth day, (the only bad symptom which occurred during his recovery,) the kidney having most probably received a wound at the time when the side was laid open.

5ly. Three cases may be cited, in which the dropsical ovary was rent, probably extensively, from external violence; they are all that have been brought under my notice, and all terminated favourably. For the full authenticity of the following I pledge myself:

39. An unmarried lady, with dropsical ovary, was thrown on the ground with violence, from a two-wheeled carriage, and struck the enlarged abdomen with considerable force against a stone which lay by the road side. A large discharge of urine followed; she became permanently freed from her dropsy; and marrying, died with a retroversion of the womb, which could not be replaced. On inspection, the remains of a ruptured ovarian cyst were discovered, retroverting the uterus, which was fixed firmly in the retro-

verted position, by means of inflammatory adhesions.

40. This case, which may be relied on as authentic, gives additional probability to one related by the late Dr. Kissam, of New York, who was a fellow student of Mr. Gaitskell at Edinburgh, and much esteemed for his activity and talent. In this, as in the former instance, the lady had an ovarian dropsy of many years standing, clearly distinguishable through the abdominal coverings. No abscess occurring, for several days afterwards, a trocar and canula were introduced into the peritoneal sack, and twenty-six pints of bloody serum were drawn off; the patient, notwithstanding the double injury from the rent and operation, getting well without any alarming symptoms. (*New England Journal of Medicine and Surgery*, Vol. v. p. 225.)

41. The third case deserves notice, especially as corroborated by the two former, which it resembles. There was swelling in the region of the right ovary, equable, smooth and without distinguishable fluctuation; pain shot occasionally in the course of the round ligament down the thigh; the left limb first, and afterwards the right, became œdematous; the general health was little impaired. When straining to reach something

on a high shelf, the patient felt some part give way within her, and examining herself immediately afterwards, she discovered that the circumscribed tumour was disappeared, and that there was general abdominal swelling in its place. For a length of time afterwards she seemed to be recovering from this injury, and died, with a schirrus of the uterine organs, and not, as appeared, from the accident. (*Idem.*)

6ly. May be narrated, two cases, in which an opening was made into the abdomen, with a view of extirpating the dropsical ovary.

42. In the first, the operation failed completely. The woman had never been tapped; the ovary held about a pailful; a schirrous piece, as large at least as the hand, not easily removed, was left in the belly; great collapse occurred, directly the ovarian sack was drawn forth, before it was cut into; but the woman lived between eighty and ninety hours afterwards, without the occurrence of peritoneal inflammation, and died, apparently, from the cachexy produced by the dropsy, and for want of reaction in the system and the wound.

43. In the second case, the ovarian cyst was extirpated by Dr. Nathan Smith, formerly, I be

lieve, of Connecticut. (*See American Medical Recorder of original Papers and Intelligence in Medicine and Surgery, No. 17.*) The sack contained about eight pints; there were no adhesions of extent and importance; the natural connexion of the ovary was as large as a finger, and the patient got well without a bad symptom*. Dr. Nathan Smith is well known to some gentlemen now in London, and would, I have little doubt, if this were deemed necessary for the sake of science, give proof convincing to the most sceptical of the authenticity of this case. These are the only ^{two} ~~three~~ operations that I at present know of, coming so immediately under my notice, as to justify citation; at the ~~two~~ first I was myself present. I question much whether, in the first operations of lithotomy and amputation, the proportion of recoveries was so great as one in two. df

7ly. May be mentioned five cases of laceration in the womb or vagina, occurring during parturition, all of which were ultimately brought under my personal notice, though in one case only was I present when the accident occurred.

44. In the first, the child was born alive by

* I quote from my adversaria, as I have not been able to lay my hand on the original in time for the press.

the natural efforts, and the side of the womb was torn longitudinally, where it unites with the broad ligaments; the woman sinking, of consequence, from flooding. I was requested to inspect the body; the rupture of the womb had not even been suspected during her life.

45. In the second case, the vagina, or neck of the womb, was lacerated behind, to the extent of a large hand breadth; the peritoneum being laid open, a clot of blood as big as the hand was found, after death, in the abdomen; collapse occurred, the patient never rallying thoroughly, though she lived for thirty-six hours.

46. The third case resembled the former; the woman died collapsed in about thirty-eight hours; there was, however, more reaction than in the former case.

47. In the fourth case the womb was torn in front, and the child escaped into the belly; the bladder was not injured. Collapse occurred in this case, and death took place in less than twelve hours.

48. The womb was torn in front in the fifth case also; the child as before escaping into the peritoneal sack. I brought this fetus away by

turning; had my hand among the intestines and on the edge of the liver; felt the large arteries in the back of the abdomen, and grasped gently the empty and contracted womb. The child was brought away dead; the woman recovered pretty completely in the course of four or five weeks, but has never been in a state of robust health since. Her name was Casey; she lived near St. George's church, Southwark; and before her recovery was complete, she came, for greater conveniency, into Guy's Hospital. A few months back, (*i. e.* five or six years after the accident;) I made a careful examination, when no traces of cicatrix were discoverable in the vagina, and the mouth of the womb felt perfectly sound and natural, so that there can, I think, be no doubt that the parts had been torn through above.

Sly. Every one knows the formidable nature of the Cesarian operation, and the very unfavourable circumstances under which it has generally been performed in this country. By a friend of the late Dr. Haighton, however, it has been done three times; once successfully, when the abdominal wound was healed completely by the sixth day, and the woman was able to stir about in her house on the thirteenth day; the constitution was in this case sound, the contraction of the pelvis having been produced by a local

cause, *viz.* fracture of the ossa innominata: the other two cases terminated fatally. Both the latter were performed on very unhealthy subjects; and there was malacosteon. I have been induced to notice these three operations, because, from Mr. Barlow's acquaintance with my valued relative, they have been brought in a manner under my immediate notice. (1)

Such is the small collection of facts, *favourable and unfavourable*, which, with limited opportunities, I have been able gradually to accumulate in the course of the last five or six years; and which to me seem calculated to throw some additional light on the probable success of a more enlarged abdominal surgery. From these, few as they are, I feel conscious that no *certain* inference can yet be drawn, though *presumptive* inferences certainly may, and they seem to me to be the following:

1st. That smaller wounds of the peritoneum, as in tapping, hernia, &c. do not in general induce fatal peritonitis, or other destructive effects; and, therefore, that the common opinion, not perhaps found on paper, but frequently urged in conversation, and apparently operative in practice, I mean, that inflammation in a spot of the peritoneum, will almost invariably diffuse itself over

(1) Since the publication of this paper many years ago . . . (See page 1. of this volume. Manuscript)

the greater part of it, is probably unfounded in truth.

2ly. That extensive divisions of the peritoneum are certainly not of necessity fatal, whether by inflammation or otherwise; and *probably* not generally so.

3ly. That the womb, spleen, and ovaries may be taken away in the mode mentioned, certainly without of necessity destroying life, and *presumptively* without generally destroying it.

4ly. That the womb, when developed from pregnancy, may be torn open; that the child may escape into the peritoneal sack, among the viscera; and that the mouth of the womb may be torn off, not indeed, so far as these cases may be relied on, without great danger, but twice, in seven instances, without death.

5ly. And generally, that the peritoneum and abdominal viscera, though very tender in the human body, will, without fatal consequences, bear more injury, than, from their modes of practice, the British surgeons, especially, seem disposed to admit.

6ly. That all the above inferences, from obser-

uations on the human abdomen, are in unison with those drawn from observations on the rabbit, the one set of inferences mutually supporting the other; and in this we have a fact corroborative of the principle for which I have contended elsewhere, that observation on the brute and human subject, when made with caution, may, perhaps, be found more in correspondence with each other, than some surgeons are disposed, at present, to admit. A contrary opinion, so far as it is erroneous, must exert a very baleful influence upon the progress of surgery.

Whilst the body of facts which have reference to abdominal injuries remains so small, it would, no doubt, be the extreme of rashness, on such authority, to recommend to practice any operations as yet untried, or of rare performance, *unless indeed in those cases in which they secure the only remaining chance of life*. As, however, the facts related evidently create a suspicion, that a bolder abdominal surgery would not be unattended with success, I may be pardoned, perhaps, for endeavouring, on this occasion, to draw the notice of the profession to the following operations, all to appearance feasible, though by no means, all of equal promise; stating distinctly, at the same time, that my design at present, is to recom-

mend them to *consideration* merely, and not to *practice*, except as observed above in cases otherwise desperate.

1st. *A division of both the fallopian tubes, and even the removal of a small piece of them, so as to render them completely impervious, a fit addition, apparently, to the Cesarian operation, the danger of which it would scarcely increase.*—The effect of this operation would be to prevent subsequent impregnation, without, however, destroying the sexual propensities, or the menstrual action of the womb; and as many, besides Mr. Barlow's patient, have, on the Continent, recovered from the Cesarian operation, the possibility of a second need for it should, I think, by all means be precluded. In those cases, also, of contracted pelvis, in which, notwithstanding the excitement of parturition in the seventh month, it is still necessary to destroy the children, by opening the head, and reducing their size, in order to bring them down through the pelvis, I think it would not be amiss to adopt this operation in order to produce sterility. An opening, two fingers broad, might be made above the symphysis pubis, near the linea alba; the fallopian tubes might be drawn up to this opening one after the other, and a piece of the tube might then be taken out. This operation,

much less dangerous than a delivery by perforating the head when the pelvis is highly contracted, may, I think, be safely recommended.

2ly. *The extirpation of the healthy ovaries.*—This operation, even granting it to be safe, can scarcely in any instance be necessary, though it may be observed, by the way, that it would probably be found an effectual remedy in the worst cases of dysmenorrhœa, and in bleeding from monthly determination on the inverted womb, where the extirpation of this organ was rejected.

3ly. *The extirpation of the ovarian cyst in schirrus, combined with dropsy or in simple dropsy.*—This operation will, I am persuaded, ultimately come into general use, and if the British surgeons will not patronize and perform it, the French and American surgeons will. If the dropsical cyst be large and of long standing, the removal will most probably be prevented by extensive adhesions; but if the cyst be small, containing (as in Nathan Smith's case) a few pints only, the adhesions will most probably be few and easily detached. It remains to be ascertained, *by observation*, to what extent adhesions in the abdomen may be cut through, without danger to life.

4ly. *The removal of a large circular piece of the*

cyst in ovarian dropsy, when the sack itself cannot be extirpated.—As rupture of the ovary has cured the disease apparently, by laying the cyst open, and, perhaps, by inducing inflammation, advantage might be expected from this operation, at least as a palliative, though other cysts would no doubt, in many instances, gradually renew the disease.

5ly. *The removal of the cancerous womb, when the ulceration first makes its appearance.*—Might not the womb be taken out above the symphysis pubis, or through the outlet of the pelvis? If above the symphysis pubis, might not the head of the vagina be tied up, and might not the ligature be conveyed by needle, into the vagina, so as to hang out at the pudenda? All the parts about the cancerous womb, and the vagina among the rest, are in such a diseased state, that I expect little from this operation, unless early performed; and then, perhaps, Ozianders' operation of paring away the diseased surface of the ulcer might be preferable, but, I think the propriety of extirpating the womb in these cases ought certainly not to be lost sight of. (2)

6ly. *Extirpation of the puerperal uterus.*—When the Cesarian operation is performed, or when a patient is evidently sinking after rupture of the womb, let it be remembered, that the wound

Relying on the above principles, in the Month of February. . . . (See page 4 of this vol. Manuscript.)

formed by the extirpation of the womb, and which might, probably, be much reduced in extent by drawing the parts together with a ligature, would merely take place of a more formidable wound, that, I mean, formed in the womb by the Cesarian operation, and which by the operation here performed, would, together with the uterus, be taken completely out of the body. No operation, perhaps, can be more unpromising, shall I say more unjustifiable *in the present state of our knowledge*, but I thought it proper to mention it. Experiment on animals, rabbits for example, which have very large wombs, might be of use here: the inverted womb has been four times extirpated with success, when reduced to the original dimensions, 32, 33, 34, 35.

7ly. Should the bladder give way into the peritoneal sack, and I have two preparations of this accident, why should we not lay open the abdomen, tie up the bladder, discharge the urine, and wash out the peritoneum, thoroughly by the injection of warm water? This operation would secure a chance of life, if the urine had not been extravasated long, say above half an hour.

8ly. Small openings, with callous edges, through the neck of the bladder into the vagina, are cured

in France, (as I learn from Mr. Travers) by the actual cautery. When the opening is large it may probably be closed by ligature, without a bad symptom. Mr. Preston, one of my pupils, first suggested to me this operation.

9ly. The injection of astringent into the peritoneum, or into ovarian cyst, has been proposed, in cases of dropsy, to check the exhalation. The experiments related, give little encouragement to the trial of this operation, at least, with the oak bark; or rather, in the present state of our knowledge, they render it altogether unjustifiable.

10ly. In cases of strongly characterized introsusception, though there never, perhaps, can be demonstrative proof of the disease, why should we not make an opening into the peritoneum, when every other remedy has failed, and gently pass the small intestines, fold by fold, through the fingers? In the dog and the rabbit, (the latter animal has a tender belly and a large mass of intestines) I have repeatedly done this without occasioning death, or even producing extensive and dangerous inflammation.

11ly. In the rabbit I have often tied an abdominal artery, and then carried the ligature out of the

abdomen, at the point where the artery lay, by means of a broad pointed needle, instead of drawing the thread forth at the wound. In operating on the human body, would this expedient be advantageous, should further experience lead us to wish the ligature in all cases removed? I have, once or twice, weeks after operating, found the remains of a ligature which had been cut short, lying in the middle of a sack of puriform matter, and, to appearance laying the foundation of chronic disease.

Cancelled by. James Muddell M.D.

~~P. S. Since the substance of the preceding pages was read before the Medical Chirurgical Society of London, in the year 1823, Dr. Ritzius, one of the supernumerary physicians to his Majesty the King of Sweden, has been in London, and has informed me, that the complete removal of the cancerous womb has been, to his personal knowledge, performed on the Continent five times. All the patients recovered from the operation; four of them, he said, were doing well several months afterwards, and one died not, apparently, in consequence of the injury inflicted by the operation, but, as was supposed, from the further progress of the disease in the surrounding parts contiguous to the uterus. The womb was removed through the outlet of the pelvis. There~~

was no hemorrhage requiring a ligature. Dr. Ritzius designs to publish these cases.

Annexed is an engraving of a cancerous womb removed in one of these operations. Dr. Ritzius, who lent me the drawing, saw the woman several months afterwards, at which time she was looking very well. The operator was a M. Sauter, of Constance. These cases suggest many reflections.

(3)

*In conformity with the above principles, ...
(see page 2 of this volume Manuscript.)*

Cancelled by Dr. Blundell M.D.

EXPERIMENTS

ON A

FEW CONTROVERTED POINTS

RESPECTING THE

PHYSIOLOGY OF GENERATION.

*Read before the Medico Chirurgical Society in the
year 1819, and*

*Published in the 10th Volume of their Transactions
in the same year.*

AMONG the various questions which have been raised respecting the generation of animals, there is one, as yet undecided, which has not perhaps been hitherto investigated with all the care it deserves. It may be demonstrated by experiment, that, in this curious process, the male furnishes the semen, and the female the rudiments; but whether these two substances must have access to each other, in order that the young animal may be formed, is a question which still admits of dispute. It is true, indeed, that many naturalists have *asserted*, that contact is necessary; and Spallanzani has even gone so far as to demonstrate that it certainly takes place in the generation of the frog and toad. Still, however, notwithstanding the labours of physiologists

hitherto, we are not, I believe, as yet in possession of any regular system of experiments, which proves that the semen must have access to the rudiments, in those forms of brute generation which most nearly resemble our own. In the present state of our knowledge, the reverse of this position seems, at least, not improbable, as the experiments of Dr. Haighton, a valued relative of mine, have shewn, that evidences of generation may be produced in the ovaries, although the semen has been excluded, previously to sexual intercourse, by the closure of the fallopian tube.

The principal object of this memoir, which I had formerly the honour of presenting to the Medico-Chirurgical Society, is to contribute some little towards the supply of this defect. In it I have endeavoured to shew, that the semen must have access to the rudiments, in order that the young animal may be produced; and yet, that generation, although these approaches are necessary for its completion, may, to a certain extent, be accomplished without them.

As the rabbit was the animal, on account of its natural aptitudes, selected for my experiments, it may be proper, perhaps, before I enter on the recital of them, to premise a few remarks on its

genital system*. In the fallopian tubes, and ovaries, and, I may add, the *external* genitals of the doe, there is little, when we view the organs as they are suspended in the glass, to attract the attention of the observer. It is different, however, with the vagina and the wombs; these are so strongly contrasted with the corresponding parts of the human organs, the wombs, by their tubular form, and the vagina by its length, its laxity, and the largeness of its diameter, that they cannot be overlooked.

The vagina, when full grown, is about four inches long, and so capacious that, without much stretching, it will readily admit the extremity of the fore-finger. Its size, indeed, is so considerable, that it makes an approach to that of the human vagina, and greatly exceeds the dimensions of the same canal in a moderate-sized monkey, preserved in the obstetric museum at Guy's Hospital.

The wombs, the structure of which is scarcely less remarkable than that of the vagina, are two tubular organs, when unimpregnated, about three inches and a half long and about two lines and a

* It is scarcely necessary to remark that this description is not addressed to those who have made a study of comparative anatomy.

half in their diameter; they are therefore, it is obvious, very unlike the human uterus, and rather resemble that of several of our domesticated animals, as the cat, for instance, the bitch, and the females of the rat and mouse tribe. These two wombs, it should be further remarked, communicate with the vagina by two distinct orifices; and they are so completely independent of each other, that the one may be removed without injury to the other, excepting a slight and superficial wound of that part where their necks lie in contact, and cohere.

Both the wombs and the vagina are, in these animals, furnished with longitudinal and annular fibres of a muscular structure, similar in kind to those of the intestines, but grosser and more distinct. In addition to these, along the inner margin of the wombs, from one extremity to the other, there runs a broad strip of fleshy fibres, which may, perhaps, not improperly be denominated the mesometric. I give the muscle this name, because it covers no inconsiderable portion of what may be called the *mesometry*; a delicate double membrane, the production of the peritoneum, which performing, for the tubular wombs, the office of a mesentery, unites them, like the intestines, to the spine. It is allied to the broad ligaments of the human womb.

All these fleshy fibres are animated with a very lively irritability. The mesometric muscle* changes the situation of the wombs. The wombs themselves perform a sort of peristaltic action. The vagina not only performs this action, but an additional movement, which I shall hereafter have occasion to describe.

Such are the most striking characteristics of the genital system in the rabbit, those, at least, which the following experiments require me to notice. I may now proceed to the experiments themselves.

The first set of experiments was instituted with a view of ascertaining whether the semen and rudiments must have access to each other, in order that the young animal may be formed. For this purpose, an incision was made into the cavity of the belly, immediately above the wombs; and these, together with the upper part of the vagina, were pushed through the opening. One of the wombs was then divided near its mouth, in a transverse direction, (just as a piece of intestine might be,) so as to separate it into two portions, the superior and inferior; or, as they may be de-

* Is this muscle allied in function to the round ligaments of the human womb?

signated from the annexed parts, the vaginal and fallopian. After this division the organs were immediately replaced, and the wound was sewed up.

Notwithstanding this violence, in the course of a few days, or a few weeks at farthest, most of the rabbits recovered their health, and at different intervals became fit for the approaches of the male. But though the general health was restored, the recovery was not complete. The operation; as subsequent dissection proved, had the effect of interrupting the canal of the womb, its tubular cavity growing up at the line of division, so that the communication between the vaginal and fallopian pieces became intercepted, and the semen and the rudiments could have no access to each other.

In this condition of the genitals, as soon as the sexual ardour was rekindled, the animals were submitted to the male; and, excepting in one or two anomalous instances, out of ten or twelve experiments, they all became pregnant from the *first* admissions. At different periods from impregnation the sexual organs were examined after death with great care and deliberation, when young animals were invariably found in the sound womb, but none in the interrupted. This, it is true, like the human uterus in extra-

uterine pregnancy, was in many instances enlarged and developed and plentifully supplied with blood, indeed it often appeared as well adapted as its fellow for receiving and cherishing the rudiments; but with all its aptitudes for generation, it lay under one capital defect—its canal was interrupted; it intercepted the access of the semen to the rudiments, and without this access generation could not be accomplished.

To confirm this conclusion, the accuracy of which I doubted at the time, it was determined to submit it to the test of another train of experiments. In these it was my object, to preserve the principle of the preceding operation, the exclusion of the semen from the rudiments; and yet at the same time, to vary its circumstances as much as possible, in order to ascertain how far they had affected the result; for I need not observe, that circumstances often exert a silent and most fallacious influence over our experiments, (our negative experiments especially) to be deprecated the more, because, from its insidious nature, it is so frequently overlooked.

In this second series of experiments, therefore, instead of operating upon rabbits that were full grown, I made use of those only that were under their puberty; and instead of interrupting, as be-

fore, the canal of the *uterus*, I interrupted that of the *vagina*.

The vagina of the doe, it has been already observed, is at least three inches in length; so that although it is interrupted at the uterine extremity, there still remains sufficient room for the male organ. Of this peculiarity I availed myself, in conducting these experiments; and instead of cutting the uterus, I cut the vagina asunder, (near to the mouth of the womb) so as completely to interrupt its canal. In other respects the experiment was conducted as before.

This operation proved dangerous, much more so than the former; a number of the rabbits, however recovered, and admitted, without repugnance, the approaches of the male. The result was decisive. Although the external genitals of these animals were turgid with blood, and the sexual excitement of some was remarkably lively; although too, in some of them, intercourse was renewed at intervals of a week or a fortnight, on the whole, as many as twenty or thirty times, not one became pregnant. Desire itself in one or two instances, seemed almost insatiable; and in the rest, though suspended by coition for a time, in the course of a few hours, or a few days at farthest, it invariably recurred.

The same general appearances were observed on dissection, in them all. The vagina, if the operation had been properly performed, was completely interrupted. In both the ovaries there were *corpora lutea*. In some cases, the wombs appeared to have undergone little change; in others, they were very much enlarged, and evolved as completely as in actual pregnancy; but in no one instance was there the appearance of a single *ovum*, extra-uterine or in the womb. In these, as in the preceding experiments, though in a different manner, the access of the semen to the rudiments had been intercepted, and under these circumstances, notwithstanding repeated commerce with the male, the formation of the young animal could not be accomplished.

In performing the experiments recorded in the preceding paragraphs, there are various little niceties in the mode of operating, the observance of which is necessary to ensure success. The incision which is carried through the abdominal coverings, may be made in the linea alba, and should be eight or ten lines, at least, in length, in order that the parts may be replaced with facility. It should, too, lie as close to the *symphysis pubis* as possible, that the intestines, which in this herbivorous animal are numerous and cumbersome, may not, as they are apt to do

when the incision is higher, protrude at the opening. It is true, indeed, that if the incision is placed in the vicinity of the pubes, the bladder, when it is distended, will fall in the way; but if the operator possess the requisite dexterity, there is no danger of wounding it; and a gentle pressure, persevered in for a time, will occasion it to withdraw into the pelvis. It deserves remark, however, that to produce this contraction, a little perseverance is necessary; for the bladder is not, in this manner, so readily excited to contract, as from previous reasonings on its irritability, we might have been led to expect.

To close the abdominal opening, the Glover's suture will serve as well as any other; nor does the including the peritoneum in the stitches, so far as I have been able to observe, materially increase the risk of a general inflammation. Exemption from this, depends much more upon the habit of the animal, than the niceties of the wound.

And here I may be permitted to remark, in the way of digression, that from various observations*

* Operations for hernia and on the abdominal viscera of rabbits and dogs. The rabbit I suspect is very liable to spontaneous inflammation of the bowels. I have known in women the malignant ulcer of the womb penetrate into the peritoneal cavity,

upon brutes, as well as my fellow-creatures, I cannot forbear imagining, that the risk of extensive inflammation, from local injury of the *peritoneum*, has been exaggerated, perhaps greatly. The high importance of this principle* in surgery, is too obvious to require a comment; already a sufficient number of observations has been accumulated, to induce us to examine it with attention; and I may add, that it is one of those grand practical points, which ought not to be decided by a few casual facts, much less by authorities, however venerable; but, like every other principle of a solid philosophy, by various, deliberate, and unbiassed experiment and observation.

If in performing this operation (as in the first set of experiments,) the womb is divided, the incision should be made transversely near its mouth, in order that we may leave the fallopian piece as large as possible, for the reception of the *ova*, in case the genitals should have power to form them. It ought, too, to be carried from four to six lines into the mesometry, in order that the pieces thus

between the *rectum* and the *uterus*, without exciting a general inflammation of the belly.

* This paper was read before the Medico-Chirurgical Society in the May of 1819. Finding that the principle here referred to did not excite so much attention as I conceive it ought to do, I was afterwards led, in the paper on abdominal surgery, again to bring it under the notice of the profession.

liberated, and moving out of ~~op~~position with each other, may not reunite so as to form anew a continuous canal. If, on the contrary, (as in the second scheme of experiments,) the vagina be divided, a ligature should be applied to the orifice of that piece of it which remains annexed to the womb, and the ligature should be fastened to the margin of the external wound. This precaution ensures the escape of the thread*, and at the same time prevents the pieces of the vagina from falling into ~~op~~position, and renewing the continuity of the canal. α/-

When the genitals are mature, the rabbit very frequently dies from this operation, which, in consequence of the large size of the vagina, is more violent than the former. It is better, therefore, on this account, as well as for reasons already assigned, to operate before puberty. Previously to this change the parts are comparatively small, and the interruption of the vagina

* In operating upon the viscera of small animals, I have occasionally used a very slender ligature, have cut it short, and left it. In two rabbits, which had apparently recovered after the vagina had been tied in this manner, a general inflammation of the belly came on about six months afterwards, in the winter, when the health of the animals was impaired by the severity of the season. On inspection after death, it was found, that the ligature still adhered to the vagina, and it *seemed* to form the centre from which the inflammation had spread.

does not, as we might have been led from previous reasonings to expect, prevent the subsequent developement of the sexual organs. But to return from these details.

Although it appears probable, from the preceding experiments, that the complete process of generation requires the access of the semen to the rudiments, it seems equally certain, from a variety of appearances which I noticed in the course of my experiments, that to a certain extent, though imperfectly, it may be accomplished without it. These appearances I shall now proceed to state.

In both the uterine and vaginal experiment, the womb, though it contained no fetuses, in many cases enlarged, as in extra-uterine pregnancy. Its structure, too, became thoroughly developed; it received more copious supplies of blood; in short, it frequently seemed as well prepared as its fellow, for receiving and cherishing the rudiments*.

The ovaries, too, I may further add, although

* It deserves notice, that, in the uterine experiments, it was generally the fallopian portion of the womb to which the semen was not applied, and not the vaginal to which it was applied, which appeared to undergo these changes in the highest degree.

there was no genuine impregnation of them, were very obviously excited. The vesicle in different parts of them germinated; its fluids increased; the delicate covering opened; the little cavity discharged its contents, and corpora lutea formed in all their perfection. As this appearance of the *corpus luteum*, notwithstanding the interception of the semen, is of considerable importance, and may help to clear away an objection to which the experiments lie open, it becomes necessary to examine it with attention.

The corpus luteum in the rabbit, as long as it remains, is, I think, *always* marked by pretty strong characteristics, though its appearance differs considerably with its age. A mammillary projection of the ovary, an augmented vascularity, a minute cavity, which, when the luteum is cut through, recalls to mind the appearance of a printed asterisk (*), constitute the leading characteristics; and by these, I may add, it is so decisively marked, that, although the parts are on a small scale, an experienced eye may detect it at a glance. Colour is of little use in distinguishing these bodies in the rabbit. The younger the *luteum* is, the more prominently the characteristics appear †.

† In giving the name of *corpus luteum* to the appearance here described, I merely adopt the nomenclature of preceding physio-

Now, these lutea, thus characterized, were distinctly produced both in the uterine and vaginal experiments. In the uterine experiments I had an opportunity of contrasting those of the fruitful and sterile ovary with each other, and yet, after the most deliberate examination, I could not discriminate the slightest difference

logists; and in stating my belief that this appearance is the result of impregnation, or, at most, of the sexual excitement when exalted to its highest pitch, I am only advancing an opinion, which is, I conceive, *as far as respects the rabbit*, confirmed by observation. I have frequently examined the ovaries of the doe, in the virgin condition, and during heat; and in one or two cases, after the animal had been under the influence of long-continued and lively desire. In the two last instances I have never found the appearances described, though I dare not, from a negative observation of this kind, deny, that, under these circumstances, their formation is possible. In the first case, on the contrary, I have invariably discovered them, and older or younger in their appearance, according as they were examined sooner or later after impregnation. There can, therefore, I apprehend, be little doubt, that these appearances occurring *in the rabbit*, are the result of conception. This fact is sufficient for my reasoning. It may, indeed, seem irreconcilable with the opinion which a veteran physiologist has formed, respecting the nature of the human corpus luteum (*see Philosophical Transactions*); but so long as it appears to be confirmed by observations, conformably to sound philosophy, it cannot be denied. I am far, however, from wishing rashly to impugn the opinion of Sir Everard Home. Truths once proved must be admitted, and their apparent inconsistency demonstrates our ignorance, not their incompatibility.

between them. It deserves notice, also, that in some instances they were more numerous upon the prolific, and in others upon the barren side of the genitals.

In these experiments, it may be further remarked, the fallopian tubes, as well as the ovaries and wombs, seemed to be excited by coition. I observed repeatedly, in those experiments in which the vagina was interrupted, that the abdomen of the doe enlarged in a few days after the sexual commerce; and that enlargement, never noticed before, and gradually decreasing* in a few weeks afterwards, if the male was excluded, might by repeated coitions be carried to a very great degree. There is now in my possession, a doe with an interrupted vagina, which has admitted the male from twenty to thirty times. In this animal, in consequence of these repeated connections, the abdomen has gradually acquired so large a size, that it considerably exceeds the bulk of mature gestation, and reminds one of the tumour of an ascitic which requires the trocar. These enlargements, I have ascertained from repeated dissections, result from the accumulation of a humor in the wombs. This humor, various in its consistency and colour, is,

* It did not, however, subside completely.

however, generally fluid and pale, and turbid, and always, so far as my experiments have extended, forms albuminous concretions at a temperature below boiling heat. Even in the uterine experiments, (for the preceding remarks refer to the vaginal only,) the same essential appearances were observed; the wombs, in consequence of impregnation, became filled, on the sound side, with fetuses, and on the barren with the humor described.

These facts are very significant. The formation of the lutea, the developement of the wombs, and, above all, the repeated accumulations of fluid there, in consequence of coition, all seem to indicate the descent of the rudimental material; and reflecting upon them, I cannot forbear imagining that the tubes were excited, that they really transferred the rudiments to the womb, and that these rudiments engendered the watery accumulations there, in the abortive attempts of generation. This notion receives some little countenance from the generation of oviparous animals; for in many of the different species referred by naturalists to this class, the rudiments may be discharged independently of preceding impregnation. The common fowl is an example of this; the frog, the toad, and a numerous tribe of fishes. This opinion, however, is merely con-

jectural, and I must acknowledge candidly that it is the less entitled to confidence, as it rests on a sort of accidental observation, made subordinately, perhaps with some degree of remissness, at a time when others of greater importance in the inquiry occupied a principal share of my attention. This remark I take the liberty of introducing here, as I conceive it to be the duty of every experimental inquirer *himself* to distinguish between his conjectures and demonstrations, and thus, by the exercise of a philosophical frankness, to prevent error from insinuating itself from its association with truth.

On the whole, then, it seems probable, judging from the appearances related, that generation may be carried forward to a certain extent, although the access of the semen to the rudiments be intercepted. Under these circumstances, the young animal cannot be formed, it is true; but corpora lutea may be generated; the wombs may be developed; and the rudiments, if we may judge from the facts already stated, may even be transferred to the uterine cavity by the play of the fallopian tubes,

It should be remarked, however, in dismissing this part of our subject, that these imperfect attempts at generation do not always equally occur.

Corpora lutea, I believe, will be found to form invariably after sexual intercourse, if the genitals are excited at all; but in some anomalous instances, there is no consequent developement of the wombs, and in others, no accumulation of the uterine fluid. The first of these failures has occurred to me once in twelve experiments, and the last of them five times*. But these *negative* irregularities merely prove, that, under circumstances, the genitals may be more extensively excited at one time than another. They by no means invalidate the principle which it has been my endeavour to establish on positive facts, that the ovaries, tubes, and *uterus*, are capable of an imperfect excitement, even when the semen and the rudiments are kept apart from each other.

Against the experiments and reasonings advanced in the preceding pages, various objections may be urged, to which it may now be proper to advert.

And first, it may be objected that sterility is sometimes an accidental occurrence. We frequently observe it in human generation. In the

* In one or two instances the orifice formed by dividing the uterus remained open in the fallopian piece. This accounts for some of the failures of uterine accumulation.

experiments under consideration it would perhaps have occurred, although the interception of the semen, to which it is ascribed, had not taken place. To these objections, however, I would reply, that in the rabbit the accidental failure of impregnation is rare, and does not occur in one doe out of twenty, if the animal be in health; that the appearance of the genitals, and the behaviour of the female when the male was admitted, both of them indicated inclination and aptitude for generation; that these experiments were not solitary, but frequently repeated; and that sterility was not an accidental occurrence, in a single instance only, but an invariable result of them all. Nor must it be forgotten that the formation of the *lutea*, and the evolution of the *uterus*, are themselves sufficient proofs that the genitals were not accidentally inactive; nor that in the uterine experiment, in which the semen was intercepted on one side only, there were undeniable proofs of the generative excitement in the formation of the young animals on the other.

But there is another objection to which the experiments lie open, which, on a cursory consideration, at least, may appear to bear with considerable weight. In these operations either the wombs or the vagina were cut asunder. It may be asserted therefore that sterility ensued, not so

much in consequence of the interception of the semen, as from the debility induced in the genitals by operative violence; the germs afterwards perishing because the soil was become unfriendly.

To this plausible objection, however, it might be sufficient to reply, that from the form of the parts the injury of the operation is merely local; that when the vagina is cut through, before puberty, the genitals suffer so little from it that they are afterwards brought to maturity in the same manner as if no operation had been performed; and that in both sets of experiments, whether uterine or vaginal, the wombs frequently become enlarged and developed, and like a fruitful and well-dressed soil (to resume the figure already adopted), are brought into high condition for raising the rudiments to perfection. To obviate this objection, however, in a still more satisfactory manner, the following experiments were instituted.

I divided the vagina of two young does, just before their puberty; but instead of securing the uterine piece to the verge of the abdominal wound, I allowed it to remain in apposition with the other. In consequence of this method of operating the parts reunited; the canal of the vagina was renewed; and the sexual desires appearing

a few weeks after recovery, both the rabbits became impregnated. The inference is obvious.

The second set of experiments, turning on the same principle, was executed on the wombs themselves. In these both the wombs were divided, the one in two, and the other in three places, in such a manner, however, that the incision was not carried completely across into the mesometry; so that the pieces were retained in mutual apposition, and reunited without interruption to the uterine canal.

The result of these experiments was decisive. From the very method of operating it is obvious the wombs were more roughly handled in this than in any of the preceding experiments; accordingly a larger number of the rabbits died; and yet, notwithstanding this violence, the very first doe which recovered, produced no less than nine fetuses from her *first* intercourse with the male. Indeed so complete was the action of the *uterus*, that there was not one of the little masses of rudimental matter which it failed to mature; and it was found, on a careful comparison of the wombs with the ovaries, that the number of fetuses and corpora lutea was the same. To these remarks I may add, that the human womb, although it have been cut or torn, or partially

destroyed by ulceration, still retains the power of maturing the rudiments. Healthy children have been born, not only after recoveries from uterine rupture and the Cesarian operation, but even at the time when the neck of the womb had been ulcerated. A case of this kind has lately fallen under my own knowledge ; and others are recorded by obstetric writers.

There yet remains a third objection, which, it is conceived, may be completely obviated, though at first view it wears a very formidable aspect. The vagina of the rabbit is very long and very large ; its course is not direct ; the organ of the male can neither fill it nor penetrate to the orifices of the wombs ; how then can the semen be injected into the uterine cavity, even granting that it might meet the rudiments there ?

This objection, felt in all its force by those who have examined the genitals merely in the preparation glass, falls at once when they are viewed in the rabbit while it is living ; or, to avoid unnecessary severity, immediately after the dealer has killed it.

Both the vagina and the wombs perform a sort of peristaltic action, the wombs somewhat obscurely, the vagina in a manner scarcely less lively

than the intestines of the animal themselves. This canal indeed, during the heat, is never at rest; it shortens, it lengthens, it changes continually in its circular dimensions; and, when irritated especially, will sometimes contract to one-third of its quiescent diameter. Now this peristaltic action, resembling the intestinal, is itself sufficient to explain the transmission of the semen*. In addition to this action, however, the vagina performs another, easily comprehended on inspection, although, as frequently happens, the verbal description of it may perhaps appear a little obscure. The action to which I here allude consists in the falling down, as it were, of that part of the vagina which lies in the vicinity of the wombs; so that it every now and then lays itself as flatly over their orifices as we should apply the hand over the mouth, in our endeavours to stop it. So close is this application, that I have sometimes fancied I could perceive externally something resembling a little dimple, occasioned by the sinking of the surface of the vagina into the orifice of the womb. How well adapted the whole of this curious movement is for the intro-

* There is some little reason for surmising that even the human vagina can perform a sort of peristaltic movement. Two facts have been related to me which lead to this opinion, but they are of a character too delicate for public exposure.

duction of the semen at the opening, it is needless to explain. The mere performance of it furnishes no contemptible argument in proof of that approach of the semen to the rudiments for which I have been contending.

Before I close these observations (already perhaps too diffuse) I cannot forbear adverting to some other points of the genital physiology, which they may contribute to illustrate.

It has been asserted by some naturalists, that the corpus luteum is an evidence of genuine impregnation. It seems *certain*, however, from the facts related, that this evidence cannot be relied on; for the luteum, in these experiments, was generated under circumstances in which, as the event proved, impregnation was impossible. Indeed there seems to be little reason for doubting, that the corpus luteum may be produced, even independently of the sexual intercourse, by the mere excitement of desire in a very high degree. Mr. Saumarez has recounted experiments, in his "*New System of Physiology*," in which the *luteum* appears to have been generated in this very manner. I have now in my possession a preparation, (for which I stand indebted to Dr. Cholmeley and Mr. Callaway,) consisting of the ovaries of a young girl, that died of chorea, under seventeen

years of age, with the hymen, which nearly closed the entrance of the vagina, unbroken. In these ovaries, the corpora lutea are no fewer than four. Two of them, it must be acknowledged, are a little obscure; though an experienced eye, I conceive, would readily detect them. The remaining two are very distinct, and differ from the corpus luteum of genuine impregnation, merely from their more diminutive size, and the less extensive vascularity of the contiguous parts of the ovary. In every other respect, in colour and form, and the cavity which they contain, their appearance is perfectly natural, indeed so much so, that I occasionally circulate them in the class-room, as accurate specimens of the luteum upon the small scale.

On this point I have been the more explicit, both as the principle is of some importance in forensic medicine, and as it removes at once an objection to which these experiments lie exposed, and which is taken from those of Dr. Haighton. In these experiments, very ingenious, and extremely beautiful, my valued relative has shewn, with his usual accuracy, that the corpus luteum may form though the fallopian tube have been obliterated in some part of its course, and the access of the semen to the rudiments therefore have been intercepted. When, however, he infers

from this, in opposition to the principle asserted in this memoir, that the ovary has been impregnated, notwithstanding the interception of the semen, he certainly falls into one of those errors, from which the most wary physiologist is never absolutely exempt; for the corpus luteum in the rabbit is not a certain evidence of impregnation.

The appearances related, I may further remark, afford, when combined with others, a plausible proof that the semen sometimes penetrates as far as the ovaries; a point which has been much controverted.

In the varieties of human generation, we sometimes meet with extra-uterine pregnancies, in which the ovum not only lodges in the tubes, or the peritoneal cavity, but in the ovary itself. Indeed, this form of the disease seems on the whole to be the most common. Now, if it be true, as I have endeavoured to prove, that the young animal cannot be formed unless the semen have access to the rudiments, it is evident, that in these pregnancies, in which the fetus is generated among the graafian vesicles, the semen must have made its way up to the ovaries themselves. It must not, however, be too hastily inferred from this, that the semen always penetrates into these

remote recesses of the genitals. Facts have been related, which give a shade of probability to the conjecture, that without the contact of the semen the rudiments may sometimes descend into the uterus; and certainly, although the opinion is not without its difficulties, it is not impossible that they may meet each other there*.

There is yet a third point in the physiology of generation, which the preceding experiments may contribute to elucidate. It has been contended by some naturalists, and not without show of reason, that the semen in generation is transferred to the blood-vessels; and as the purgative or emetic, when ejected into the veins, exert their peculiar influences on the stomach or the bowels, so also, in their opinion, this active fluid, transmitted by the absorbents, makes its first impression on the vascular surface, and its second, by a similar sympathy, on the genitals themselves. What effects might be produced by injecting the semen directly into the veins when the genitals are in a state of excitement, I shall not venture to determine; as yet I am in possession of no decisive experiments upon the point, and it would be a mere waste of mind to speculate without them. It seems evi-

* Is the transfer of the semen beyond the womb the cause of extra-uterine pregnancy?

dent, however, from the facts related, that after transmission through the absorbents and their glands, the semen retains no such generative influence. It will be readily conceded, that when a rabbit admits a large male, in vigorous health, and in the flower of its age, as many as twenty or thirty times, a large quantity of the genital fluid must be imbibed by the absorbents of the vagina, yet neither in the uterine nor the vaginal experiment, in which these repeated coitions sometimes took place, was impregnation by absorption accomplished. The simple exclusion of the semen from the rudiments always prevented the formation of the young animal; in the vaginal experiments it was not produced at all; in the uterine it was formed on that side only where the womb remained pervious.

On a review of the whole inquiry, it will, I conceive, appear not improbable that, for the completion of generation, the semen must have access to the rudiments; and yet that notwithstanding the necessity of these approaches for its completion, the process to a certain extent may be accomplished without them. These are the two leading propositions which it has been my endeavour to establish; at the same time I have subordinately attempted further to shew, that the corpus luteum is not a proof of genuine impregnation; that the semen, at least

occasionally, penetrates as far as the ovaries; and that however copiously this fluid may be absorbed into the vessels, it is incapable of giving rise, by any impression there, to the complete circle of the generative actions.

Whether these principles of brute generation may be transferred to our own, I will not venture to determine. Analogical arguments, generally the best that physiology furnishes, are, it must be admitted, never absolutely demonstrative; but as the generation of the rabbit, in its other principles, resembles that of the human female, there seems to me but little reason for supposing that there is an essential difference here.

Now granting this, and there seems to be nothing unreasonable in the assumption, we may, I think, from the whole inquiry, draw a few practical inferences; and these I am the rather induced to state, because some well-meaning persons of weak minds, incapable of perceiving the connexion between speculation and practice, are led to condemn investigations of this sort as idle, and therefore, as unjustifiable and cruel, because, until they are instructed, they do not know how to use them. And,

First, it may be inferred, that when the womb is extirpated by ligature there is no danger, lest

from commerce afterwards with our sex, an extra-uterine pregnancy should be produced. And,

Secondly, it follows, that when the Cesarian operation is performed, the possibility of a second need of that operation may be precluded by removing, on either side, a small portion of the fallopian tube, so as to intercept its calibre.*

In the third place, it is obvious that when the pelvis is so small, that delivery cannot be accomplished after seven months are completed, without breaking open the head of the child, the need of this murderous operation may be prevented by the same simple expedient. The sexual desires would, I believe, remain, but sterility would be produced; and though certainly not wholly unattended with danger, the operation is perhaps safer and less painful than the extraction by embryotomy, which has hitherto been used as a substitute; it demands, too, no sacrifice of a helpless fetus, and would require performance but once.

Should these suggestions be verified, and brought into operation in the practice of future ages, I think it must be admitted that they amply justify the experiments which give birth to them, and render them a benefit to mankind.

* See page 25.

SOME REMARKS
ON THE
OPERATION OF TRANSFUSION.

IN common with many of my most intelligent friends, I have long entertained an opinion, that notwithstanding the improvements which have been already made in surgery, there are still many operations of importance which may be added to the science. Among these operations, transfusion may, I think, fairly rank as one; and it is with a view of keeping this valuable operation before the profession, and in the hope of adding somewhat to the body of facts by which it may be still further illustrated, that I am induced again to make it the subject of a memoir.

Without pretending to give an enumeration of all the cases in which transfusion may be tried, with a fair prospect of advantage, I may observe, that there are some cases, in which the practical utility of it is both great and obvious.

1. I remember being called once to a poor woman in my neighbourhood, who had lost a large quantity of blood after her placenta had been taken away. When I saw her, the hemorrhagy was stopped, but she was evidently sinking; and, notwithstanding the assiduous use of all the ordinary remedies, she died in the course of two hours after the first eruption of the blood.

2. By a friend of mine, on the other side of the water, I was requested, some two or three years ago, to give advice in a case very similar to this. The blood came away from the womb, during, and after the birth of the placenta; and the patient died in the course of three or four hours afterwards, throughout greater part of which time she was obviously sinking, notwithstanding the ordinary remedies were actively tried.

3. A poor fellow, in one of our hospitals, lost a great quantity of blood in consequence of an injury of the leg; but, although it was pretty evident that death must ensue, he continued to breathe afterwards for two or three hours.

Now, in cases of this kind, (and I might enumerate others which have fallen under my per-

sonal notice,) when the patient is gradually sinking, and the bleeding is suspended, there is a fit opportunity for trying the operation of transfusion; and, unless we are prepared, in the face of opposing facts, to deny the utility of the operation altogether, it must, I think, be admitted that it would be used, in such emergencies, with the fairest prospect of preserving the patient's life.

It is not, however, in conjuncture of this kind only that the operation deserves consideration: there are, too, other cases in which, transfusion should not be lost sight of, although its utility in them is certainly much more dubious.

We know that in hanging or submersion, death, at first, is apparent only, and not real; for a certain period after respiration stops, resuscitation is still possible. Now, that death from bleeding may also for a time be apparent, is by no means unlikely; and it is not impossible, therefore, that transfusion may be of service, if performed within a given period even after the breathing has been stopped. Under this impression it was that I instituted the following experiments; and although the results have not corresponded with my wishes; and although too they do not by any means form a complete body

of information on the point, with the view of making an opening in the subject, I am induced to record them.

4. Into the right carotid of a dog, which weighed about ten pounds avoirdupois, I introduced a tubule, directing its extremity towards the heart, and drew off about half a pint of blood, which was all that could be abstracted. At first the blood flowed with impetuosity for somewhat less than a minute, after which there was very little farther discharge; and apparent death was produced, respiration ceasing, and the abdominal muscles becoming relaxed, within about four minutes from the time when the artery was opened, and within about three minutes from the time when the blood ceased to flow in an impetuous stream. About one hour (64') after the attack of apparent death, a large quantity of blood was transfused into the jugular vein towards the heart from the carotid of another dog; but no signs of returning life appeared.

5. I opened the carotid artery of a dog, the same in size as the former, and introduced a pipe, as usual, the blood flowing away for about half a minute in an impetuous stream, and sluggishly for two minutes longer, so that about half a pint altogether was withdrawn; the breathing ceasing

entirely, and the abdominal muscles becoming relaxed, about four minutes from the time when the discharge began.

About half an hour (34') afterwards, I transfused into the jugular vein, as before, blood taken from the carotid artery of another dog, without however producing any signs of returning life; and about an hour after the bleeding, and half an hour after transfusion had been performed, the hot bath was tried, but without obvious advantage.

6. I drained another dog from the carotid artery, in the same manner as the two preceding; about half a pint of blood being drawn away as before. Apparent death, marked in the ordinary way, by a total cessation of the pulse and respiration, came on four minutes after the blood began to flow.

About ten minutes after the attack of asphyxia, I replenished the animal, by the jugular vein, with blood taken from the carotid artery of another dog, but no signs of returning animation were produced.

7. Into the left carotid artery I introduced a pipe, as in the former experiments, with its extremity towards the heart; the dog weighed

thirteen pounds three ounces avoirdupois; and about eight ounces of blood were got away. Apparent death came on about two minutes from the time when the blood began to flow, though a few convulsive respirations occurred, for some two or three minutes afterwards.

About twenty minutes after the apparent death commenced, I injected into the jugular vein, towards the heart, as before, several ounces of blood taken from the carotid artery of another dog; and very soon afterwards, I tried the artificial respiration actively, together with the hot bath for an hour, without, however, producing the slightest indication of returning life. On examining the heart subsequently, I found the left side of it empty, and the right charged with blood; and it should be remarked particularly, that this blood was clotted.

8. I introduced a tubule into the left carotid of a dog of middling size, directing its extremity towards the heart, and got away about half a pint of blood; the blood first issuing in an impetuous, and afterwards in a sluggish stream. Nine minutes after the blood began to flow, (not earlier,) the circulation ceased to be distinguishable in the femoral artery; and it was not till

fourteen minutes after the opening of the carotid that apparent death occurred; so that in this, as in the subsequent experiment, (9) thirteen or fourteen minutes, instead of three or four, elapsed before apparent death was produced.

Thirty-five minutes after the attack of apparent death, and about fifty minutes after the artery was opened, I transfused blood by means of a tubular apparatus, not above four inches long, into the carotid artery, instead of the jugular vein of the animal, directing it towards the heart; and though the quantity which entered ~~in~~ could not be exactly ascertained, as the blood was not seen, there is reason to believe it was considerable, for the emittent dog was very languid afterwards.

About fifty minutes from the commencement of apparent death, I attempted resuscitation by the hot bath; and a thorough inflation of the lungs, in which natural respiration was imitated, as exactly as possible, for half an hour together, but without producing the slightest sign of returning life. This experiment succeeded exceedingly well in all its parts.

9. In a dog, which had not acquired its full size, and of a habit somewhat delicate, I opened the carotid artery of the right side, introducing

the tubule towards the heart. For about half a minute the blood flowed in an impetuous stream; and then, for several minutes longer, it escaped very slowly from the tubule, which was kept clear from coagula, by the occasional introduction of a stilet. The whole quantity of blood drawn, however, scarcely exceeded half a pint.

At first the respiration was tolerably natural; but when the animal was pretty well emptied, it became disordered; deep sighings frequently occurred, and about twenty minutes from the time when the artery was opened, (not earlier,) respiration stopped altogether, the pulse disappearing completely in the femoral artery; the abdominal muscles becoming very flabby; and the animal lying, to all appearance, dead.

About five minutes after respiration was stopped, and about twenty-five minutes from the time when the blood had ceased to flow with impetuosity from the tubule, I injected into the jugular vein, towards the heart, blood taken from the carotid artery of another dog; the operation being continued for three or four minutes. The effects were immediate, shuddering first occurred, then respiration, then vascular action, and ultimately complete recovery. I am disposed to think, that the heart of some dogs is

much more retentive of its irritability than that of others; and that, in bull-dogs especially, the cardiac irritability is tenacious and permanent. This dog, though delicate, was very savage, and probably had a taste of the bull-dog blood; so that the heart was, perhaps, more tenacious of irritability than ordinary, which may account for its recovery.

From these experiments, a variety of inferences may be drawn, which, although they do not lead us to expect much from transfusion, when the asphyxia of hemorrhagy has been produced, are sufficient, I think, to bear us out in the assertion, that in cases of this kind, in which there is no other hope, the operation may deserve consideration: and we may infer,

1st. That the time which intervenes between the opening of an artery, and the attack of apparent death, varies exceedingly in different individuals, even when the artery remains unclosed, and the bleeding therefore is not obstructed. In some of these dogs it was about two minutes, in some four, in one fourteen minutes, (8) and in one twenty (9).

2ly. That after the cessation of respiration, and the relaxation of the abdominal muscles in the

dog, the animal very speedily becomes irrecoverable by the process of transfusion ; for it will be observed, that when the dog was suffered to be in a state of apparent death for sixty-four, (4) thirty-four, (5) twenty, (7) nay, even ten minutes, (6), it could not be resuscitated ; and this too, although in Experiments 7, 8, the operation of transfusion was assisted by the stimulus of the hot bath, and an artificial respiration very diligently executed. Whether this principle may or not be transferred to the human body, admits a doubt ; but the affirmative is probable.

3ly. That one impediment to the resuscitation of the animal in these cases, arises, perhaps, from the coagulation of the blood in the heart, for concretions were found there on inspection in Experiment 7 ; it should be added, however, that this was ascertained to be the case, by inspection, in one of the dogs only, for the others were not examined ; and that in other animals, as, for example, the ox, the blood has been found by an excellent experimental observer, Mr. Thackrah*, to be fluid in the heart, half an hour after the animal has been knocked down : nor must it be forgotten, that the blood of the dog has a much stronger tendency to speedy

* See Thackrah's Inquiry, p. 58, London, 1819 ; a valuable present to Physiology.

coagulation than the human blood. We must not therefore lightly make this inference general, nor transfer it without consideration to the human body.

4ly. That although the dog cannot, perhaps, in general, be resuscitated, even a few minutes after the carotid artery has been fairly laid open, and distended by the introduction of a pipe; and although resuscitation be improbable, if respiration has been suspended in consequence of bleeding, yet, now and then, recovery is possible; since, in Experiment 9, a complete resuscitation was accomplished, about twenty-five minutes after the artery had been opened, and four or five minutes after the animal had lain to appearance dead. And here it may be observed, transiently, how necessary it is not to draw conclusions hastily from a few experiments, but, on the contrary, to multiply them as much as may be, since it is by performing the same experiment repeatedly that important exceptions are sometimes ascertained.

5ly. It follows from the preceding inferences, that if we are called to a patient fifteen or twenty minutes after the carotid artery has been laid open, the patient may be still respiring; and therefore, that resuscitation by means of transfusion, may not perhaps be found in every instance

impracticable. If the eighth pair of nerves were divided on one side only, the recovery might be permanent; and if it were cut through on both sides, the patient might be expected to live afterwards for a few hours.

6ly. In some of these experiments, in which the carotid was laid wide open, the blood, towards the close of the operation, came away sluggishly, and in small quantities; so that the discharge might have been arrested by the mere pressure of the finger. When persons cut their throat, I strongly suspect, that by the pressure of the finger, or, by putting a fold or two of a handkerchief into the wound, the flow of the blood might sometimes be so far obstructed, as to prolong the patient's life till further assistance could be obtained. If this assertion be true, it cannot be made too generally known.

7ly. The preceding experiments do not enable me to decide, whether, in apparent death from bleeding, a preference should be given to the injection of blood into the jugular vein or the carotid artery; but, I think, on the whole, the blood ought to be injected into the carotid artery, toward the heart, in order, if possible, to renew the circulation through the coronary vessels, on which, I suspect, the irritability of the heart depends. But to proceed.

The formation of blood is the principal end of the chylopoietic viscera, and of their auxiliaries; and it seems, therefore, not improbable, in those cases in which the action of these viscera is interrupted by schirrosity of the pylorus, or other causes, that their operations might be superseded by the injection of blood into the veins, so as to supply the vessels in a direct manner with that blood, which in health is the result of sanguification. As, however, opinions derived from reasonings of this kind are exceedingly uncertain, until they are brought to the test of experience, I have been induced to institute an experiment, not unattended with labour, with a view of demonstrating that dogs certainly may be supported for a length of time by the transfusion of blood only, without the aid of food taken into the alimentary tube; and the following is a brief statement of the results.

For three whole weeks I nourished a dog by the mere transfusion of blood into the external jugular vein. The aggregate quantity infused during this period, was eighty-four ounces, avoirdupois, nearly; that is, on an average, four ounces a-day; and during the whole time the animal was allowed to swallow nothing except water, and, indeed, never manifested any marked desire for solid aliment. The blood, during the

first eleven days, was injected by the syringe, but during the remainder of the term it was transmitted direct from the artery of the emittent dog, by means of a tube. The kind of blood used was the arterial.

2ly. At the end of this period I found the dog, which weighed twenty-six pounds two ounces when the experiment began, had lost seven pounds two ounces of its weight; yet, although somewhat emaciated, and in its general health a good deal disordered, it appeared still to be in no danger whatever from inanition.

That this dog had been effectually nourished by the blood with which it was supplied, there can, I think, be little doubt; for had this not been the case, the animal must have been exhausted or nearly so, after a fast of three weeks. And as dogs, a little above the middle size, when they are kept without food, lose from eight to sixteen ounces of their weight in the course of twenty-four hours, it is evident, even without nice adjustment of the calculation, that unless nourishment had been derived from the blood, the loss of weight in three weeks must have been more considerable than seven pounds two ounces.

3ly. In the course of this experiment the

dog's health was a good deal impaired; in part, perhaps, in consequence of the large wounds in the skin, which in my method of operation it was necessary to make, in order to get repeatedly into the jugular veins; and in part also, indeed principally, (I conceive,) in consequence of the manner in which the blood was injected. For as this was a first essay, and not so well conducted as it might have been had I possessed more experience in the operation, instead of injecting the blood daily or twice a-day in quantities of two or three ounces only, so as not to overload the vascular system, I generally transfused the blood at longer intervals of one, two, and once or twice of three days; and much larger quantities of blood, eight or ten ounces at least, were thrown in together. The consequence of this large supply was such a surcharge of the vascular system as would be produced in a full-grown man by the injection of not less than three or four pints of blood into the veins, with no other preparation than a fast of one or two days. Accordingly the animal seemed to be a little distressed by it, though not so much as might have been expected from previous (*a priori*) reasoning. The pulse became irregular, and intermitted frequently; there was languor, general tremor, and sometimes, but not invariably, a

disposition to sleep. On examination after death too, I found that the spleen and liver were decidedly larger than natural; the spleen weighed one ounce and three-fourths, avoirdupois, and the liver one pound three ounces and a half; and the right side of the heart was, in a greater proportion than ordinary, larger than the left.

4ly. Why it is that the body of the dog wasted considerably, though four ounces of blood were, on an average, daily supplied to the veins, it is not easy to demonstrate; but I think it highly probable that this wasting was occasioned, not by any defect in the nourishing properties of the blood itself, but partly by the insufficiency of the quantity injected, and principally by the disorder of the system which the injection occasioned. That the latter part of this opinion is correct I incline to believe from an observation I made more than once, I mean that the dog wasted more rapidly during the first twenty-four hours after the injection, than during the twenty-four hours which followed them.

From these facts and observations it may, I think, be inferred:

1st. That the dog may be nourished a length of time without the help of food, by transfusing

into the veins the blood of another individual of the same species, either by the tube or the syringe.

2ly. That the blood which is supplied to the vessels in this manner does not support the body so effectually as an equal quantity would do, if derived from sanguification.

3ly. That the health is liable to be much impaired, by operations of this sort; and that enlargement of the heart, the spleen, and the liver, may be produced by them, in the course of three weeks.

4ly. That these effects, it is probable, in the present state of our knowledge, are not inevitable, nor of equal degree in all cases; but are rather to be attributed to the circumstances of the operation, than the nature of the operation itself.

5ly. Whether these principles may be transferred from the dog to our own species, is at present uncertain; but till we have proof to the contrary, they furnish a strong presumption, that the human body may be nourished by the injection of blood.

In the progress of knowledge and the decay of prejudice, should the method of nourishing by transfusion be practised hereafter on the human subject, I suspect it will be found, that small quantities of blood are sufficient to support the body in a state of languid life; and that ill health is not inseparable from the operation, provided it be performed in a dexterous and judicious manner, and provided, especially, the blood be injected frequently, in small quantities at once.

But to conclude this part of my paper. There are perhaps various cases, in which blood may be transfused with advantage, but three more especially deserve consideration: — those, I mean, in which the patient is dying for want of nourishment; those in which the patient is already dead, to appearance, in consequence of copious bleeding; and those cases, lastly, in which the breathing still continues, although it is pretty evident from the course of symptoms, that death must ensue in consequence of the loss of blood which has been sustained. These cases, under the present modes of management, are all of them desperate.

Of the kinds of blood proper for the operation of transfusion, when performed on the human body.

When the blood of one genus of animals is added, in small quantities, to that of another genus by transfusion, we have reason to believe, (in the present state of our knowledge,) that no dangerous consequences will ensue; and I have heard Dr. Haighton assert, that after taking a few ounces of blood from the dog, he has afterwards transfused that of the sheep, in its place, without producing dangerous symptoms. Now, if further experiments, multiplied and varied, should thoroughly confirm this principle, we may hope to find hereafter, that the blood of animals may be safely thrown into the human vessels, in small quantities daily, for the purposes of nourishment, instead of the human blood, which it must be more difficult to procure.

Although, however, the blood of one genus of animals may, perhaps, without fatal consequences, be sparingly mixed with large quantities of the blood of another genus, all the facts, which have hitherto come to my knowledge, go to prove, that if an animal be drained of the blood in its larger vessels, and replenished with large

quantities of blood derived indifferently from another genus, great danger, and in general death itself will ensue.*

10. I took a dog of smaller size than ordinary, but perfectly healthy, and drew off about four ounces of blood from the femoral arteries, when all the usual symptoms, dyspnæa, struggling, cessation of the circulation, relaxation of the abdominal muscles, and a complete asphyxia were produced, though, it should be observed, the quantity of blood abstracted was small.

I then drew four ounces of blood by measure, from the human arm, suffered it to lie in the cup between fifty and sixty seconds, and injected it into the femoral vein. At first the dog seemed to be a little recovered from the operation, and both respiration and circulation were renewed; but it died on the table in the course of a few minutes, not from the entrance of air, nor from excess in the quantity of blood injected, (for both these accidents were precluded,) but, apparently, in consequence of the substitution of human blood for the canine, or else, from the

* Does there exist a genus of animals whose blood may be safely substituted for the human?

deterioration of the blood, occasioned by its lying in the cup of the syringe. The dog, before the experiment, was perfectly healthy.

11. From the femoral artery, of an old dog, of the terrier breed, valued a good deal for his hardihood and fondness for rat-catching, I drew off eleven ounces of blood, distress, dyspnæa, jactitator, and suspension of respiration being produced by the operation, so that it was obvious, from the character of the symptoms, as well as from the quantity of blood which had been taken away, that unless transfusion were performed, the animal could not survive. I then drew two ounces of blood, from the arm of a healthy man, and suffered this blood to remain in the cup of the syringe for thirty seconds, before it was injected into the femoral vein of the animal. The operation was performed five times, about ten ounces of human blood being injected; and every time, before the blood was poured into the vessels, it was suffered to lie at least thirty seconds in the cup. By the bleeding apparent death was produced, and the animal revived, as usual, in consequence of the injection; the pulse, imperceptible before, returned distinctly enough into the femoral artery, beating, without obvious intermission, one hundred and twenty times in the minute; and when the dog was loosed from the

frame, it was able to walk about, and seemed to be pleased when noticed, and encouraged by the usual caresses. Notwithstanding this partial recovery, however, the dog still remained languid, feeble, and evidently unwell; and though originally of hardy constitution, it died in the course of the following night, about twelve hours after the operation was performed. The entrance of air into the veins was carefully guarded against in this experiment, and it appears, from the experiments which follow, that death was not occasioned by the mere exposure of the blood in the cup.

12. I took a large, lively, and very healthy bitch, and drew off, from the femoral artery, as much blood as could be abstracted, amounting to seven ounces. The ordinary symptoms, distress, gasping, struggling, together with profound fainting were produced.

As soon as the discharge was finished, blood taken from the human arm was infused, to the amount of six ounces nearly; this was received into the cup of the syringe as it flowed from the arm, and thrown directly, but tranquilly into the vein. In consequence of this injection the dog clearly recovered a little; it became sensible, and respired more regularly though not without

occasional gaspings; the abdominal muscles too became firm, and the blood circulated so freely, that it gushed out afresh from the femoral artery. The recovery, however, was by no means so complete, as when canine blood is injected; the action of the heart remained irregular, intermittent, and labouring; and the dog, after gasping, and gaping convulsively, made ineffectual attempts to vomit, and died in the course of a few minutes.

These symptoms are very similar to those which occurred in the following experiment, and to those which are produced in death from bleeding. That death was occasioned, in this instance, by the substitution of human blood for the canine, there can, I think, be little reason to doubt; and this is the more remarkable, because the blood was injected without being suffered, as in the previous experiment, to remain in the cup, and because the animal, immediately before the experiment was performed, was lively and well. It should be observed, too, that the quantity of blood drawn away, was not very large considering the size of the animal, and that a temporary revival was produced; circumstances which make the death the more decisive. The experiment occupied but little time, and no untoward accidents occurred.

13. From the femoral artery of a large, lively, and very healthy dog, I drew off as much blood as could be obtained by means of the tubule, in the usual manner, to the amount of ~~eighteen~~ *eight or ten* ounces, when the usual symptoms supervened; and I then threw in about five ounces and a half of human blood, in quantities of half an ounce at once. The blood was measured by means of the syringe, and it was not before injection suffered to lie out in the cup. In consequence of the hurried and careless manner in which the apparatus had been put together, a few bubbles of air got into the veins.

By this injection the dog was revived a little, as in the former instances, but by no means so much so as it commonly is by the injection of blood taken from its own species. It noticed caresses, breathed more regularly, acquired firmness of the abdominal muscles, and had such a renewal of the circulation, that the blood issued afresh, though slowly, from the femoral artery. In the course of a few minutes, however, the same symptoms occurred as those which preceded the death of the former dog: the animal gasped, yawned convulsively, and vomited; and after lying about an hour, on its side, in a state approaching to faintness, it expired. On examining the heart after death, I found the right

side was full of blood, and the left was comparatively empty; the greater part of the blood was in a fluid state, and the few coagula found there were very small.

It will be proved, by future experiments, that the few bubbles of air which entered the veins in conjunction with the blood were not capable of producing death. Had the blood of another dog been transfused, instead of the human, I have no doubt that the animal would have been permanently resuscitated, and the many experiments which I have made enable me to form an opinion on this point.

14. I took a young dog, of small size, very lively and healthy, before it was made the subject of operation, and withdrew four ounces of blood from the femoral artery, it being all that could be abstracted; the usual symptoms were produced, though the asphyxia was not so deep as in the preceding experiment.

I then, without delay, threw in three ounces of blood by means of my syringe, not suffering it to lie out in the cup, excepting at the very commencement of the operation, when there was some pause, in consequence of one of the ligatures not having been taken away. By this in-

jection, the dog was resuscitated more completely than in the three preceding experiments, and though very unwell, and feeble, it was able to walk about. Two hours after the injection, however, there was thirst, languor, and debility; and the pulse was so small, and weak, that it could not be distinctly ascertained whether it intermitted or not. These symptoms gradually subsided, and on the third day the animal appeared to be rapidly recovering, but drooping, a second time, one or two days afterwards, it died on the sixth, with dropsy of the pericardium. An ounce of fluid was accumulated in this membrane; but there were no signs of inflammation, nor was there dropsy of the other cavities.

15. Having laid bare the left carotid artery, in a brisk, and very lively young dog, I drew away about eight ounces of blood, by means of a pipe introduced into the vessel, with its extremity towards the heart; faintness was produced, but not a total cessation of the breathing.

I then, without delay, threw five or six ounces of human blood, into the external jugular vein, by means of the improved transfusing instrument hereafter described. By this injection the animal was a little resuscitated, the pulse became distinguishable enough in the femoral

artery, and the blood flowed so briskly along the vessels, that when the ligature was removed from the carotid, it gushed out in a full stream. Notwithstanding this partial resuscitation, however, the dog when taken from the table was exceedingly languid, and lay at full length, on its side, in a state approaching to syncope, breathing slowly and cool; and in the course of half an hour or an hour afterwards, it died.

This experiment is the more decisive, because the death of the animal cannot be attributed to plethora, injection of air, or the production of too deep an asphyxia, by the abstraction of the blood, as all these accidents were carefully precluded. It is one of the last experiments which I have made, and I do not hesitate to assert, after all the experience I have had of this operation, that if canine blood had been injected instead of the human, the dog would certainly have recovered. It deserves remark, that a drachm or two of blood, of an arterial tint, taken from the carotid of this dog after the injection, did not become solid at the end of seven minutes, although the blood of the dog naturally concretes in one or two. Now, as the human blood, like that of the horse, is remarkable for the slowness with which it coagu-

lates, this fact goes to prove that the blood transfused, still retained the properties of human blood although it had been circulated for some minutes in the vessels of the animal. Whether the vessels of the dog, when the animal lives, as in experiment (14), possess the power of assimilating the human blood to its own is uncertain, but the affirmative is probable.

These experiments acquire additional strength, when associated with others instituted by Dr. Leacock,* of Barbadoes, a few months before; experiments to which I am wholly indebted for my first notions on this point. From these it appears, that if a dog be drained of its blood until apparent death be produced, it may, indeed, be revived, for a time, and very completely too, by replenishing it from the sheep, but it generally dies in a few days afterwards.

Connected with my own, these experiments of Dr. Leacock possess a peculiar interest, for though they harmonize with them in the general result, they differ from them materially in their circumstances. It was arterial and not venous blood; the blood of the sheep and not the human

* Inaug. Diss. Ed. 1817.

that was substituted, and it deserves particular remark, that in Leacock's experiments, the transfusion was not performed by the syringe, a method of operating with which he was unacquainted, but simply by the tube.

From these facts it appears clearly that the human blood cannot safely be substituted in large quantities for that of the dog. It is certain that death was not produced accidentally, from the hurry of injection, or from plethora; from suffering the blood to accumulate in the cup of the syringe, or the dog, to remain too long in a state of asphyxia, for in some of the experiments, these accidents were carefully obviated, particularly in the last.

16. The only experiments that I know of, in which the human blood was substituted for that of the dog, by an operation similar to those described above, without destroying the animal, are those performed, six or seven years ago, by Mr. Goodridge, of Barbadoes, a gentleman who was at that time finishing his medical studies, at the united hospitals. In these experiments, probably in consequence of the natural vigour of its constitution, and, perhaps, from the smaller quantity in which blood was injected, the animal was enabled to struggle through the consequences of

the operation, but even in these cases, for some hours after the transfusion, a variety of unfavourable symptoms occurred.

As it is clear, from the preceding experiments, that the blood of one sort of animals cannot, with impunity, be substituted indifferently, and in large quantities, for that of another sort of animals; it follows, of course, that in performing the operation of transfusion on the human body, the human blood should alone be employed, at least, until we have discovered some other kind of blood as well suited to the vessels, as that which they naturally contain.

Provided the blood transfused be derived from an animal of the same species with that which receives, it seems to matter but little whether that blood be arterial or venous. In most of those experiments in which, after draining the dog of its own blood, I resuscitated the animal, and preserved its life, by supplying it with blood taken from the vessels of another dog, arterial blood was injected, in preference to the venous, because a full supply of this kind of blood might be more easily obtained. To satisfy myself, however, that venous blood possesses the resuscitating power, as well as the arterial,

17. I opened the femoral artery of a dog, not much larger in the body than a large cat, and drew off about four ounces of blood, (more could not be obtained,) when faintness, and relaxation of the abdominal muscles were produced. Respiration, however, was not suspended, nor was the asphyxia complete; but, from observation drawn from other experiments, I have no doubt, that, unless transfusion had been performed, the dog would have died.

After suffering the animal to lie in this state for some minutes, I injected, by means of the syringe, the blood of another dog, (about three ounces,) when the animal became so much revived by the operation, that it began to look about it, and was pleased when caressed, though unable, when loosened, to get down from the table. The venous blood issuing slowly from the emittent dog, I was compelled to transmit it in a very gradual manner, and some of it actually coagulated in the cup, in which it was detained for several seconds.

For many hours after the operation, the dog seemed languid and ailing, but on the third day it recovered very much, and ultimately got completely well; the languor in this case, being, I think, fairly attributable to the exposure of the

blood in the cup of the syringe, before it was transmitted to the animal. It may be noted here, by the way, that from this experiment we have proof, that blood may remain for several seconds in the cup, not indeed without becoming deteriorated, but without being thereby rendered completely unfit for the purposes of life.

18. I took a lively, resolute dog, weighing 20lbs. avz. and drew off all the blood which could be got from the carotid artery, by a pipe of large bore, introduced, as usual, with its extremity towards the heart. The quantity got away was thirteen ounces avoirdupois; and the dog lay to all appearance dead, respiration ceasing entirely, and the abdominal muscles becoming relaxed, insomuch that my assistants thought that the animal was gone beyond recovery, and I feared the same myself. Asphyxia having been induced in this manner, I drew blood from the jugular vein of another dog, and injected it direct, by means of the impellor, the dog becoming resuscitated immediately, breathing, making a noise (as he had done before the blood was abstracted) expressive of anger and impatience, and struggling with some violence. The blood flowed afresh from the carotid artery of a bright arterial tint. When loosed from the table, the animal,

though languid, was able to walk about briskly; indeed, after the operation he appeared decidedly better than my dogs do in general, and after remaining languid for a day or two, he recovered completely. The general health was impaired less in this, than in the preceding experiment; the reason of which I conceive to be, that the blood, flowing from the emittent dog more rapidly, was more promptly transmitted through the syringe, and was therefore less exposed, and less deteriorated by lying in the cup.

I suppose there can be little doubt that, to save a patient, by transfusion, from the death of hemorrhagy, it would not, by any means, be necessary to pour in a quantity of blood, equal to that which had been lost. That quantity of blood only would be required, which would enable the heart and arteries to act, and which would support the patient long enough to allow the vessels to accommodate themselves to the diminished volume of the blood, and to gain time for a new supply from the digestive organs. What, however, is the smallest quantity of blood which would be sufficient to support life, I have not hitherto laboured much to ascertain; and the rather, because it is exceedingly doubtful, whether conclusions of this kind can be trans-

ferred, with any useful degree of certainty, from the dog to the human subject.

19. I once, however, laid bare the femoral vessels in a dog, and drew off, in the course of two minutes, about ten ounces of blood from the artery, by means of a pipe, which was introduced in the usual manner, with its extremity towards the heart. After the blood had been abstracted in this manner, the pulse disappeared in the opposite limb, and the blood ceased to flow from the tube, excepting in slow drops, at long intervals: so that, after the experience which I have had in these experiments, I should not hesitate to assert, that, unless transfusion had been performed, the animal must have died.

After the dog had been suffered to lie in this state for a few seconds, I threw in, by means of the syringe, about two ounces of blood, in place of the ten, which had been taken away; when the animal was very decidedly resuscitated; respiration returned; the pulse became obvious; and the blood, upon slackening a ligature which had been applied, came out from the tube inserted into the femoral artery, in a pretty full stream. It appears, therefore, from this experiment, not only that blood transfused by a syringe, is capable of resuscitating an animal, but further, that a small

quantity only (in this experiment one-fifth of the blood which had been lost) is adequate to the purpose.*

From these experiments we may, I think, venture to presume, until we have proof to the contrary,

1st. That, in transfusion, venous blood may be successfully used, although, perhaps, arterial blood is preferable.

2ly. That an animal may be saved from the death of hemorrhagy, by the transfusion of a much smaller quantity of blood than that which it has lost.

3ly. That the blood of one genus of animals cannot be indifferently substituted, in large quantities, with impunity, for that of another genus; and, therefore, that if an operation be performed upon the human body, human blood only should

* I think it is worth enquiry, whether, when there is a deficiency of the blood, the action of the heart and arteries may be kept up for a while, so as to gain time, by the mere injection of water into the vessels; in other words, whether blood, diluted with water, will support the actions of life, and to what degree it may be diluted.

be employed, until some other blood be found which is equally congenial to the vessels.

To these inferences may be added the following remarks. It seems not improbable that animals of one genus possess the power of assimilating to their own the blood of another genus, provided they live for days after it has been infused into their veins; as it is not easy to conceive, how life can continue for a length of time afterwards, (Experiment 16,) unless such assimilation be accomplished.

Although the blood of one genus of animals cannot be injected largely into the vessels of another genus, without danger to life, it is not unlikely that small injections of this kind may be safely tried. The importance of this principle, in the operation of nourishment by transfusion, is obvious.

Although an animal may be resuscitated by the transfusion of venous blood, I suspect that, of the two varieties, the arterial blood is the more efficacious; but into this enquiry I forbear to enter further at present.

It is clear, from these facts and inferences, that although the blood of the mammalia may be

essentially the same in all the genera, the different kinds of blood differ very importantly from each other. It is an interesting, and, perhaps, a difficult enquiry, Whether any genus of animals be furnished with a kind of blood congenial to the human veins? That of the horse is the most promising.

Blood may be received into a cup, and passed through a syringe, without being thereby rendered unfit for the purposes of life.

In performing transfusion there can, I conceive, be no doubt that blood ought to be transmitted by the tubule merely when this method is practicable; but as we should probably meet with obstructions in operating in this way on the human body, I have been led to make experiments with a view of ascertaining whether blood may not be absorbed and propelled by means of a syringe, without becoming unfit for the purposes of life; for transfusion may easily be performed in this manner:—

20. Into the carotid artery of a very healthy dog, weighing about seventeen pounds avoirdupoise, I introduced a pipe, with its extremity towards the heart, and drew off eight or ten ounces of blood, (as much as could be got away), the ani-

mal not ceasing to respire altogether, though struggling, stretching of the limbs, and the usual outcry which sometimes precedes dissolution were produced. The muscles became very relaxed.

After the dog had lain in this languid state for a few minutes, I injected several ounces of blood in quantities of half an ounce at a time, suffering it to become a little inspissated in the cup of the syringe before it was infused.

By this injection the dog was revived a good deal, so that it was able to walk; but for some hours afterwards it remained dull and languid, and disposed to lie about. The day after the operation it seemed, as usual, to be a good deal better, but declined a little in the evening, and the day following it refused its food, and seemed so languid and unwell, that I really feared it would die. In the course of a few hours more, however, it recovered from this attack, and a few days afterwards became completely well. Whether the ill health, greater than ordinary in this case, was occasioned by the entrance of air together with the blood, by the inspissation of the blood, or by the transfusion of too small a quantity, I am unable to determine; but of the causes enumerated, I think the inspissation of the blood to be the most probable.

21. I drained a very fat brown dog, with hanging ears, and, to appearance, of tender constitution, of all the blood which could be got from the left carotid artery, by a pipe directed towards the heart; the animal struggled hard, but continued to respire occasionally.

By means of the impelling syringe, I then threw into the external jugular vein blood taken from the carotid artery of another dog; but found on loosening the animal, that it was languid, and disposed to lie quiet more than ordinary. For four or five days after the operation, this dog continued in an ailing condition, was dull, and ate and drank but little; but after this period, it began to gather strength very fast, and soon became completely well.

The languor and ill health, it deserves remark, were in this, as in the former case, of longer duration, and more severe than I have generally found them; perhaps in consequence of the tenderness of the animal, and its very great corpulency: for on so fat a dog I never operated before. That the injecting apparatus was airtight is certain. This experiment was made in the presence of Dr. Ritzius, of Stockholm.

22. I procured a dog of delicate habit, and not full grown, and drew off from the carotid artery

all the blood which could be got out by means of the tubule, to the amount of four ounces and a half: somewhat less than the full measure usually obtained from dogs of the same bulk as this animal. Complaint, struggling, gasping, syncope, and relaxation of the abdominal muscles were produced, and in this condition, approaching to complete asphyxia, the dog was suffered to lie for a few seconds.

I then, by means of a syringe, transfused arterial blood taken from another dog, to the amount of four measured ounces; the blood, owing to some impediments in the operation, lying some three or four seconds, at least, in the cup before it was transfused. In a few seconds the dog recovered, and on opening the carotid artery, the blood was found to break out afresh; not sluggishly, as in the experiment with human blood, but in an impetuous stream; and the dog, upon being untied, got up without difficulty, and leaped down, in a somewhat playful manner, from the table. Though languid for a few hours afterwards, in the course of a day or two it got completely well, and never at any time after the operation appeared to be in danger. This experiment was made with the very same syringe which I employed in those experiments already related (10, 11, 12, 13, and 14); in which the

human blood was transfused instead of the canine; and though the results of the two sets of experiments were so different, the operation was performed exactly in the same manner in both.

23. I drained a dog from the left carotid artery, in the same manner as in the preceding experiment, abstracting, as usual, all the blood which could be got away. The dog continued to respire a little occasionally, but was evidently dying, and the circulation was stopped.

After waiting a few seconds, I threw into the jugular vein, by means of the syringe, blood taken from the carotid artery of another dog; the recovery of the animal being sudden, and on the whole complete. It remained a little languid for three or four days afterwards, but ultimately got well, without the occurrence of a single alarming symptom. The syringe was air-tight.

24. I drained a young dog from the left carotid artery, in the usual manner, so that the circulation stopped completely, and the animal scarcely respired.

Then, by means of the syringe, I injected into the left jugular vein blood taken from the arteries of another dog, with the effect of com-

pletely resuscitating the animal, which a day or two after the experiment seemed to be unusually lively and well, and not a single bad symptom was observed afterwards. For the first three or four hours, the pulse intermitted a little, and I suspect that this symptom is not uncommon. Mr. Coleby, of St. Thomas's Hospital, gave his assistance in this experiment.

From the preceding, corroborated by experiments 17 and 18, it seems obvious enough, that although the blood of the dog become deteriorated, by passing through the syringe, it is not thereby rendered unfit for the purposes of resuscitation and life. To confirm this principle, however, in my opinion of great practical importance, I have been induced to institute another scheme of experiments, of which the following is a brief and faithful detail.

25. I introduced pipes into the carotid artery and jugular vein of a dog, directing their extremities towards the heart, and then drew off, by measure, six drachms of blood into the bottom of a conical wine-glass; absorbing it into a syringe, and afterwards injecting it into the jugular vein. This was done several times, till about six ounces of blood had been transmitted in this manner;

after which I paused for a few minutes, and then performed the operation twice again in the same mode, about six ounces of blood being transfused each time; so that in all the three operations about eighteen ounces of blood were passed through the syringe, or, to assume the least estimate, twelve or fourteen.

During the operation the action of the heart was disturbed, and the dog, though able to walk about, was languid and ill immediately after it was finished. A day or two afterwards, however, it became very lively and well, took its food largely, and did not appear to suffer a single bad symptom at any subsequent period.

26. Into the jugular vein and the carotid artery of another dog, I again introduced pipes, with the extremity towards the heart, and transmitted blood by means of a syringe and wine-glass from the artery to the vein, in the same manner as in the preceding experiment, at four different times, an interval of a few minutes being interposed between each of the four injections. The second and third injections were sparing, of a few drachms only, but the first was much more abundant; and the last injection was long continued and very copious, insomuch that the blood was

observed to issue at length from the artery of an unusually bright arterial tint, as if it were rendered doubly arterial; in no other experiments have I observed this phenomenon to be so conspicuously marked. Owing to the imperfection of the apparatus, about a drachm of air got into the veins.

In consequence of some alarm and agitation the pulse intermitted before transfusion began, but during the operation these intermissions became more frequent, occurring every five or six beats, the blood flowing sometimes impetuously and sometimes sluggishly from the carotid artery; towards the close of the experiment these intermissions became less frequent, and in a few minutes afterwards ceased altogether.

The entrance of the air did not occasion any unusual symptoms. The intermissions of the pulse, constituting the most remarkable symptom, must certainly be attributed in part to alarm, for it was observed before the operation commenced; but it seems also to have arisen partly from the unequal and impetuous stream in which the blood was thrown into the heart. It is obvious that it cannot be ascribed to the arterial nature of the blood which was injected into

the veins, or the changes it suffered in passing the syringe ; for although the arterial characters of the blood were heightened as the experiment proceeded, and some of the blood at least had passed the instrument more than once, these irregularities, instead of increasing, became less and less frequent.

27. By means of an instrument very similar to the "Impellor" described in the close of this Memoir, I transferred blood direct from the femoral artery to the femoral vein of the dog, absorbing the blood from the cup of the instrument and discharging it promptly into the vein, without suffering any large accumulation and exposure in the cup. The operation was continued for ten minutes together, but the blood was not observed to issue in the end of the experiment of an arterial tint brighter than ordinary. The dog was of the lap-dog breed, very tender, and similar in constitution to that which was used in (Ex. 21.)

Under this operation the pulse sank down from 140 beats in the minute, its natural frequency, to 50 or 60 beats large and distinct; the dog in the same space respiring twelve times, a slight catch or spasm following each respira-

tion. When taken from the frame the dog appeared to be languid and unwell, but it was perfectly sensible, wagged its tail when caressed, and walked, not without debility, several yards before it lay down, the pulse thirty or forty minutes after the operation, still not exceeding 60 beats in the minute. Notwithstanding these unfavourable symptoms, in the course of a few days the dog became completely well, the recovery being more speedy than I had expected: whence it is evident, that although the blood had perhaps been impaired by transmission through the syringe, it was not rendered wholly unfit for the purposes of life. Subsequent experiments, however, render it exceedingly doubtful whether the bad symptoms produced really arose from the mere exposure of the blood in the cup, and the transmission through the syringe, as in these it will be observed the same symptoms did not occur. This dog was of tender habit.

28. I took a dog of the sandy-coloured pug breed, apparently a hardy animal, and putting pipes as usual into the femoral vessels, I transmitted blood, by means of the instrument used in the preceding experiment, from the artery to the vein direct, for eight minutes together, the blood flowing during the whole time, and towards the

end of the experiment exhibiting the arterial characteristics in a higher degree than ordinary. After waiting about half an hour I performed the transfusion for eight minutes longer; and then pausing another half-hour, I operated a third time also for eight minutes, a few small concretions forming on this occasion in the cup: so that transfusion was carried on altogether for about twenty-four minutes, the blood flowing freely during the whole time. Assuming therefore, what had indeed been ascertained by the preceding experiment, I mean, that in the course of two minutes about half a pint of blood came away from the femoral artery when kept open by the tube, twelve pounds of blood, at least, must have been transmitted through the instrument; and the same blood must of consequence have passed the syringe repeatedly, for the whole weight of the dog did not equal twelve pounds; and this conclusion is confirmed by the highly arterial character which the blood had acquired when the operation concluded. After remaining languid for a day or two, this dog recovered like the former.

In the progress of this experiment, made with greater dexterity than the preceding, it is remarkable that very little derangement of the system occurred. The respiration was indeed a

little hurried, but the dog remained lively and vigilant, and his pulse, irregular from fear before the operation was begun, became very irregular as it proceeded, and beat, as usual in dogs of this size, about 150 times in the minute. Its temperature underwent but little change. The regularity of the pulse during this operation is very remarkable, especially if we consider its great irregularity in the preceding experiments. It must be recollected, however, that the transfusion was performed in this instance on the femoral vessels which are remote from the heart, and not on those of the neck. The formation of concretions deserves notice, as it proves that the blood in this animal remains fit for its peculiar functions, although certain parts are begun to concrete.

29. I procured a dog weighing 15lb. 2oz. avoirdupoise, and inserting pipes into the carotid artery and jugular vein, instead of the femoral vessels, I suffered the blood to flow, taking care to return it into the jugular by means of the *impellor*, as fast as it issued from the artery. A slight disposition to coagulation manifested itself; and some clots formed in the emittent tube, not however in sufficient quantity to close it. The syringe was air-tight, but one bubble,

accidentally absorbed from the cup, was injected into the vein.

For twenty-five minutes the operation was continued unceasingly, during all which time the animal did not appear to be in any way remarkably disturbed, and immediately afterwards the pulse (naturally 140) beat 160 times in the minute, and the respirations were twenty-six. No intermissions of the pulse were observed, and, throughout the operation, the instrument playing well, the blood flowed in a moderate and equable stream.

From a subsequent observation I ascertained that when the syringe was worked with the same degree of rapidity as it had been during the experiment, a full half-pint, that is eight ounces avoirdupoise, were transmitted in the course of a minute; and it follows, therefore, that in the course of twenty-five minutes, during which the operation was continued, twelve pounds and a half of blood at least, probably more, must have passed through the instrument, though the whole weight of the dog scarcely exceeded fifteen pounds. The subject of this experiment was a large puppy of delicate habit, with hanging ears, and with a taste of the spaniel blood. The whole length of tubes and surfaces over which the blood

passed in its way from the artery to the vein, was, as I ascertained by measurement, at least one foot and a half.

Three hours after the operation the dog seemed disposed to lie about and was languid, but could walk very well, and there were 160 beats of the pulse, and thirty respirations in the minute. Next day the animal ailed but little; the day afterwards it was much better, and took plenty of food; and the day following, that is the fourth, reckoning that of the experiment as the first, the general health appeared to be completely restored. This experiment succeeded perfectly well in all its parts, and seems, in conjunction with the former, to prove clearly that blood may be transmitted through the syringe, and that, too, repeatedly, not indeed without injury, but without thereby becoming unfitted for the purposes of life. The experiment is the more satisfactory, because the dog was small and tender.

We must not, however, suppose that experiments of this kind are wholly unaccompanied with danger, or that success invariably attends them, even when performed upon the dog.

30. Introducing pipes in the usual manner into the carotid artery and external jugular vein of

a dog, I transmitted the blood through the syringe for two or three minutes together, after which the flow from the artery became sparing, and I suspended the operation for two or three minutes longer, to give the animal an opportunity of recovering. When the operation was renewed, the tubule slipped accidentally from the carotid artery, and two or three ounces of blood were lost before the artery could be secured; at length, however, the pipes were replaced, and the operation proceeded, the blood flowing slowly from the arterial tubule, either in consequence of partial closure, or, as I strongly suspected, from the want of a full action of the ventricle; and the blood in the cup assumed a slightly gelatinous consistency. On this occasion the operation was continued for two or three minutes as before, at the end of which, the dog was seized with a few spasmodic contractions of the dorsal muscles, and died without a further struggle.

The cause of death in this experiment is not obvious. The transmission of blood through the syringe, the partial coagulation of it, the disturbance of the circulation by the injection of the blood along the jugular vein into the heart, the loss of two or three ounces of blood when the tubule was pulled out of the artery, the impression of fear under which the dog laboured, may

all be supposed to have occasioned it. From viewing this experiment, however, in connexion with the preceding, (exp. 27.) I am inclined to think, that the partial coagulation of the blood, during its slow transmission, in the latter part of the operation, when the blood flowed sluggishly from the carotid artery, was the most probable cause of death. The quantity lost was not sufficient to kill the animal, unless there were some peculiarity of constitution in this animal not usually found in dogs; and the result of former experiments shews, that it was not the ordinary deterioration of the blood by the syringe that destroyed it, and that it did not die from fear, or from a disturbance of the current of the blood, occasioned by the operation. In exp. 27, in which the blood during its transmission through the syringe became, as in this experiment, a little inspissated, very dangerous symptoms were produced, though the animal surviving the first shock of the experiment, recovered rapidly afterwards.

From the preceeding facts we may, I think, infer:—

1st. That blood, although it have been passed through the syringe, and repeatedly, is still capable of supporting the life and health of the body. With one exception, all the dogs on which these

operations were performed, eleven in number, recovered, and some of them got completely well within two or three days afterwards.

2ly. That although blood which has passed the syringe retain its fitness for the animal purposes, it probably becomes deteriorated by this operation, especially if it lie for a few seconds out of the vessels, and be slightly inspissated in consequence. In exp. 22, 23, 24, 25, 28, 29, the dogs suffered but little, but in exp. 20, 21, 26, 27, dangerous symptoms were produced. How long the blood may lie out of the vessels without becoming wholly unfit for the vital purposes, has not been ascertained, though the principle is well worth investigation.

3ly. That the deteriorated blood, after it has, been thrown into the vessels, undergoes a sanative process, by which it again becomes thoroughly congenial to the functions of the animal; for it will be observed, that most of the dogs, though languid for some two or three days subsequently to the operation, became very lively and well a few days afterwards.

4ly. And probably, that dogs of tender constitution suffer more from operations of this kind than those which are more hardy; other circum-

stances being the same. This plain, but important principle, must be born in mind when we think of transferring these conclusions from the dog to the human subject. The preceding principles certainly hold true in the Pathology of the dog, but whether they hold true also in human Pathology must be ascertained by future observation; the affirmative is in a high degree probable.

Of Transfusion from the Arteries of one man to the Veins of another.

In general, there must, I am aware, be considerable difficulty in obtaining arterial blood from the human body for the purposes of transfusion; but persons may be induced occasionally, sometimes from motives of affection, and sometimes for hire, to submit to the opening of an artery.

If blood is to be transferred from the arteries of one man into the veins of another, it would be necessary, in the first place, to lay bare a vein on the fore-arm of the patient to the extent of an inch or more, and then to bind a ligature between the hand and the intended opening, in order to prevent the escape of the blood when the circulation is renewed; after which an opening should be made with a lancet longitudinally, (in the course of the

vein I mean) so as to allow the ready entrance of the small venous tube, which should be slipped into the vessel towards the heart, and retained there by pressure of the finger.* The patient who is to receive the blood being prepared in this manner, the operator should next prepare the person who is to supply it. Of the different arteries which may be fixed on as adapted to an operation of this sort, perhaps the radial, ulnar, or anterior tibial are the principal, and of these three I think the radial is decidedly to be preferred, for its conveniency in other particulars as well as on account of its easy access. The artery elected, a tourniquet should be put on the limb, between the intended opening and the heart, in readiness to be constricted, should emergency require, and then, to the extent of an inch or two, the artery should be laid bare with the scalpel, extensively enough to secure the aperture against the obstructions arising from a deficiency of room. This done, the artery should be detached a little, say to the extent of half an inch, and a ligature

* Should the small quantity of air lodging in this tubule be found to produce injurious effects, the tubule may be inserted first into the flexible tube, and then, after the whole tubular apparatus has been filled with blood, as hereafter explained, the tubule may be slid into the vein; but unless the operator be dextrous and prompt, the blood will become deteriorated, and even clotted, by lying in the inanimate apparatus.

should be put on the vessel below, close against the part where it remains connected with the cellular web and the vasa vasorum; and about an inch from this ligature, a pair of spring forceps should be applied, which, closing of themselves, may press the sides of the artery together, so as to close it completely. The artery should then be laid open with a lancet to the extent of a line, (one-eighth of an inch) so as to allow the ready entrance of a small tube, and this opening should be made in the course of the vessel, as the artery, I find, when divided across, contracts more in its calibre, and, if the incision be a little too deep, is in danger of breaking completely through. A small tubule, about the size of a crow-quill, in connexion with a flexible tube, about as large as a goose-quill, should then be introduced into the artery with its extremity towards the heart, and should be secured there by means of a ligature; the ridge, or shoulder, near its orifice, will give it a bearing line, and prevent its slipping out. The operation being brought to this point, the small arterial forceps are opened by pressing the handles together, so as to give passage to the blood and expel the air from the tubes; after which, the forceps are suffered again to close, and the tubular apparatus being full of blood, the flexible tube is firmly connected with the tubule in the vein. With a moderate share of dexterity, the junction may be easily

accomplished in a few seconds, and before the human blood, of slow coagulation, has time to become clotted; provided the venous tubule be made to unite with the flexible tube in the way of a plug. By pressing the tubule home into the flexible tube, and giving it at the same time a semi-rotatory movement, the junction may be made sure. The apparatus being thus prepared, the blood should be admitted to it by opening the spring forceps, and by means of the same instrument: the rapidity of the flow, as well as the moment of its cessation, may be regulated with great nicety. After the operation has been concluded, the wounds and the injured vessels must of course be managed on the general principles of surgery; and it may not be amiss to remark, that if, with a view of facilitating the operation, the artery of the person who emits the blood have been detached a little from its bed, when ligatures are afterwards applied, those ligatures ought to be applied close to that part where the vessel still remains in connexion with the cellular web, in order that the adhesion of the sides of the artery may not be interrupted, for want of connexion with the vasa vasorum.

The instruments required for this operation should, I think, be shut up in the same case with the impellor hereafter described. Those which I would recommend, are the following:—a tourni-

quet, a scalpel, a lancet, an eyed probe, a blunted needle and ligatures, a pair of spring forceps, venous tubules, a smaller and a larger, and two flexible tubes, each about six inches long, one connected with a smaller and one with a larger arterial tubule to meet the varying calibre of the vessels, and both capable of uniting, in the way of plug, with either of the venous tubules.

Of the Transfusion of Venous blood by means of the Impellor.

When, in consequence of the want of arterial blood, it becomes necessary to transfuse the venous, a different method of operating must be adopted; and, in the present state of our knowledge, I would recommend in such cases, the employment of the impellor.

In operating with this instrument, a chair is to be procured, and by means of a gimlet an aperture is to be worked into the outside of the back of it, at a convenient elevation, and into this aperture the vice of the apparatus is to be very firmly screwed. By means of the solid stem which projects from its apex, the cup is to be fixed erect in the gripe of the vice, and the large outer cup, containing the impelling part of the instrument, is to be filled with tepid (96°) water, if this can be procured, so that the whole of this part of the appa-

ratus, inclusive of the syringe, may be covered in completely by the water, in such manner that the entrance of *air* into the instrument by leakage, whether of the joints or of the syringe, may be effectually precluded. The apparatus being thus far prepared, the inner cup is to be put on, care being taken to fit the tube which projects below from the apex of this inner cup, to the corresponding tube which springs up from the cylinder with which the syringe is connected, and which lies in the bottom of the larger cup; because, if the exact apposition of one tube to the other be neglected, a difficulty may needlessly arise in putting the inner cup into its place, in consequence of the two tubes interfering in an obstructive manner with each other. Half a pint, or a pint of water is then to be poured into the inner cup and pumped briskly through the instrument, so that the air may be expelled thoroughly, the water taking its place; and the extremity of the flexible tube which springs from the instrument is towards the end of the operation to be bent down into water contained in a tumbler, the pumping being continued, and this with a view of ascertaining by the appearance of bubbles, whether there be any fissure at which air enters. The exact temperature of the instrument is not of importance, but the water should, in preference, be milk warm. The apparatus being thus prepared, the operator with his lancet may lay bare, as before, a vein on

the fore-arm of the patient, to the extent of an inch at least, taking care to cut down completely through the cellular web, and then, by means of the same instrument, he may, as in the former operation, make a longitudinal incision at least a line in length, and large enough to allow of the ready entrance of the venous tubule to be introduced to the extent of two or three lines thoroughly, but with the utmost gentleness, with its extremity towards the heart. A ligature should not be used. The tubule should be retained in its place by the finger of the assistant who holds the arm. The orifice of the tubule should not have a cutting edge

After the tubule has been introduced, the chair should be brought close to the edge of the bed; an ounce or two of water should be poured into the cup if void, the syringe should be thrown into action, so as to fill with water the flexible tube which may have drained itself empty, and then the venous tubule, previously inserted into the vein, should, by means of pressure combined with a slight semi-rotatory movement, be plugged pretty firmly into the flexible tube, in order to prevent the juncture from bursting open when the blood is impelled. After the apparatus has been fitted together in this manner, the person who is to supply the blood takes his

seat on the chair; his arm is opened by the lancet as in ordinary venesection; any superfluous water lying in the cup is removed by a piece of sponge; and the blood, instead of being received into a basin in the usual manner, is directed into the cup of the transfusing instrument, and by the play of the syringe, impelled direct into the vein of the patient, without being suffered at any time to accumulate largely in the apex of the cup. As the object of the syringe is merely to give impulse, it ought not to be worked by long strokes, but by short and sharp movements, care being taken that the plug be every time pushed home, so as to bear down upon the nozzle plate and prevent any accumulation in the barrel of the instrument. If the syringe be worked in this manner, the blood will be little more exposed when transmitted through the impellor, than it is when passing direct from artery to vein by means of a tube; since, in order that the impulse may be given, it is enough that the blood be admitted but a little way into the barrel of the syringe:—those who are acquainted with the principles of hydrostatics, must be aware that the entrance of a few minims would be amply sufficient for this purpose.

For supplying blood, men are preferable to women, as they bleed more freely and are less liable to faint. If blood can be procured from

the arms of two persons at once, it would sometimes perhaps be desirable. Spirit sufficient to exhilarate and rouse the circulation, may be advantageously given to those who are to furnish the blood before the operation is begun. If the blood, flowing slowly from the arm, show a disposition to clot in the cup, or if the supply of blood fail, the person who furnishes should remove his arm, and a little water poured into the cup of the instrument may be pumped through the syringe so as to displace the blood; thus the apparatus being cleared of blood, will be secured against clot, and kept in a condition to proceed with the operation. If a large bubble of air be absorbed into the syringe, the operation should be suspended, the instrument should be separated from the venous tubule, and the blood and air should be cleared out of the apparatus by immediately pumping through it a few ounces of water, which should be at hand in a small jug furnished with a spout. It would be easy to contrive an instrument which would measure the quantity of the blood injected, but I think it better to avoid complexities of this kind. Of the quantity of blood thrown in, an opinion may be formed from the feelings of the person who emits it, or by the size of the stream of blood which flows from the arm, and the continuance of the flow; or by putting water into the cup after the operation, and working

the instrument with the same measured movement, and with the same degree of rapidity, and for the same time as during the operation. The measure of the water passed through the instrument in this manner, will give nearly the measure of the blood transfused. The time during which the operation is continued, should be ascertained by the watch.

It is of great importance, in using this instrument, that the outer cup be filled thoroughly, so as to cover in the head or upper end of the barrel of the syringe, to the depth of an inch, or an inch and a half at least; as it is this complete submersion of the impelling portion of the apparatus, with all its joints and vents, which is the only security against the entrance of air.

Till it be ascertained (and the affirmative is probable) that human blood may lie out of the vessels for one or two minutes, without material injury, we ought, in operating, to prevent the blood from accumulating in the cup of the instrument. Should it, however, be found hereafter, that the blood may be suffered to gather in the cup, without thereby becoming materially unfitted for its offices in the vessels, then, if this instrument is used at all, it may be proper, during the operation, to suffer an ounce or more

of blood to accumulate, in order to prevent more certainly the cup from becoming empty, and thus to preclude the absorption of air.

The minute dexterity required in managing this instrument may be easily acquired by any person who will accustom himself to pump blood, or even water, through it; and those who are frequently performing the operation of venesection must have many opportunities of doing this. When the operation is completed, a pint of water ought to be immediately worked through the apparatus, in order to clear out the blood, and prevent it from lying in the tubes and becoming clotted there; and, as soon as occasion serves, the whole should be unscrewed and cleaned out more completely. If the wire-springs which raise the valves are injured, others may be very easily made by giving a piece of wire a spiral form, like that of the spring which has been destroyed. If the leather valves are spoiled, others may be made of a piece of soft alum-leather. Both the wire and the leather should be stored in the case for this purpose. The owner of the impellor ought by all means to make himself master of its structure, and to acquire the little skill which may enable him to set it to rights for himself. Laundry of St. Thomas's-street, Southwark, now manufactures these instruments.

Of Transfusion by the Syringe.

Should it be found hereafter, by numerous pointed, and therefore decisive experiments and observations, that human blood may lie out of the vessels in the cup for several seconds, without becoming thereby unfit for the vital purposes, there is yet another mode in which transfusion may be accomplished, by the syringe alone, I mean, and this method of operating, if feasible, may be adopted in preference to the preceding, on account of its greater simplicity. In this method of operating a good syringe is required, capable of containing two or three ounces of blood, and furnished with a pipe for the vein, (like that used in the former instrument,) about two inches long, and made to fit by plugging, and a semi-rotatory movement, into the nozzle of the syringe.

The blood is to be drawn into a conical vessel, for example, a tumbler, and while flowing into this vessel, held by an assistant, it is to be absorbed into the syringe. When charged, the syringe is to be held with its tubular nozzle upwards, and the piston is to be pushed slowly onward till the blood begins to issue, in order that the air, which from its greater specific levity, will

rise to the upper part of the instrument, may be thoroughly expelled. The syringe, together with the tubule springing from it, now charged with blood only, is to be slid into the vein of the patient, properly laid open for the purpose, and without delay or hurry the blood is to be injected in an equable stream; the operation being repeated as often as the quantity of the blood to be injected may require:—the syringe being of a known capacity will measure the blood.

This, with some little improvement, was the method of operating adopted in Brazier's case, (p. 139.) In this method also Mr. Goodridge operated on his dogs, as before described. The simplicity of the operation, and the portability of the instrument, are its great recommendation. A common syringe, if a good one, might be used in this manner if the emergency were pressing. When the syringe is charged for the first time, air may be previously expelled from the instrument by charging it with water. The vein must be laid thoroughly bare, as the cellular web, if not divided completely, will slip over the orifice in the vein, and obstruct the introduction of the instrument; and the opening must be made sufficiently large and free to allow of the ready entrance of the tube. Perhaps a probe might be passed under the vein with advantage.

Miscellaneous Remarks on Transfusion.

It may be objected to transfusion in every shape, that the tube may excite inflammation of the vein. In weighing this and similar objections, however, it should not be forgotten, that in the present state of our knowledge, it is proposed to perform the operation in the most desperate cases only, when it seems to be the sole remaining mean of saving the patient's life. There is much good sense in the familiar maxim of Celsus, and in the present case it is peculiarly applicable; for, surely, it is better to incur the uncertain risk of venous inflammation, than to leave the patient to his fate. Besides, the pipe should not be secured in the vein by ligature, but by the pressure of the finger merely, or the blood may be injected by an artery. In the latter case the risk of venous inflammation will be completely obviated, and in the former (probably) it is small.

It will, perhaps, be further objected to transfusion, that the operation is likely to be obstructed by the coagulation of the blood; but this objection is unfounded. In the preceding experiments the operation, though performed on the blood of the dog, was not impeded by concretion; yet

the canine blood coagulates in one-sixth of the time which is necessary for the coagulation of human blood, as the following experiments prove; and it is clear, therefore, that in transfusing human blood, coagulation can furnish no insuperable impediment.

31. I drew off, into a conical wine-glass, about three drachms of blood from the femoral artery of a dog; it began to coagulate in about 10 seconds, and was completely solid in about 80. In a second experiment which I made, the blood began to coagulate in about 10 seconds (as before), and was completely solid in about 60.

32. A patient being seized with an arterial epistaxis, I collected some of the blood of a bright florid tint. A full minute elapsed before even minute coagula made their appearance, and the blood did not become wholly solid till four or five.

33. A few drachms of venous blood were taken from the arm of a girl liable to attacks of epilepsy. It was full 60 seconds before even minute coagula began to make their appearance about the sides of the vessels, and six or seven minutes elapsed before a general coagulation of it took place.

34. I filled a syringe with blood taken from the same patient as the preceding. When retained there for one minute only, it was found, on expulsion, to be thoroughly fluid, and it was but slightly inspissated after remaining there for two minutes.

That air may enter in conjunction with the blood may also be objected to this operation; and I have been induced, therefore, to make the following experiments and observations, with a view of ascertaining how far this objection is just.

35. Into the femoral vein of a dog (scarcely larger in the body than a full-sized cat) I threw about five drachms of atmospherical air in the direction of the heart, in quantities of about a drachm at a time; the whole operation occupying about five minutes. The quantity of the air was measured by means of the syringe. In consequence of this operation, dyspnæa was produced, together with irregular action of the heart; the dyspnæa, however, not coming on as soon as the air might be supposed to have entered the heart, but a minute or two afterwards.

During this operation the dog sighed deeply, and a slight dyspnæa was produced; the pulse

too became unequal, and the muscular system tremulous. As soon, however, as the animal was liberated it leaped from the table, licked its wound, and seemed pleased with caresses. On the following day it was languid and restless, and the muscular tremor continued; the pulse intermitted occasionally, and the dog vomited once. In other respects it appeared tolerably well, took food greedily, and revived completely by the third day. This dog was very delicate; and the small size of the animal considered, the quantity of the air injected was large; yet all the symptoms may be imputed, in part, at least, to the alarm which the operation excited.

36. About three drachms of air were blown from my lungs towards the heart, into the femoral vein of the dog, which had been made the subject of the preceding experiment; the greater part of the air being introduced at once. The respiration, circulation, and general health of the animal, seemed to be but little deranged by the experiment, even at the time; and the dog suffered so little subsequent inconvenience, that a day or two afterwards it was led into the country; nor did any urgent symptoms ultimately occur.

37. Mr. Coleby, well known at St. Thomas's Hospital, procured a large dog, laid bare the fe-

moral vessels, introduced pipes, with their extremities toward the heart, and by means of the *impellor*, transmitted blood from artery to vein, for several minutes together without ceasing. The syringe used in this operation, of faulty construction, was not air-tight, and in consequence a considerable quantity of air was, by little and little, worked into the vessels along with the blood. Before, too, the instrument was put into action, the air was not expelled by charging it with water, so that when the syringe began to play, all the air contained in the tubes, the cylinder, and the barrel of the syringe below the plug, was injected into the animal's veins.

In consequence of this operation, the dog suffered a great deal of distress, with frequent and irregular pulse for six or seven days afterwards, at the end of which it was killed and examined, when, as I am informed, a considerable quantity of air was found in the cavities of the heart, mixed up with the blood.

These facts considered, then, it seems probable that the entrance of a few drachms of air into the vessels, would be attended with considerable distress and even danger; but it must be recollected, that if the operation be carefully performed by a competent person, with a

proper instrument, there can be no risk, lest air should enter the vessels in large quantities; and the *probability* is, that a bubble or two of air only would occasion little if any inconvenience. It is not objected to the capital operations in surgery, that an unskilful operator may lay open an artery, or that this or other accidents may happen in a moment of negligence even to the most dextrous.

In making these experiments on the dog I did not warm the instrument, unless the weather was unusually cold; and though, in the present state of my information, I have recommended tepid water in operating on the human subject, I have a suspicion that heat tends to exhaust the irritability, and destroy what may be called the life of the blood.

Whether large quantities of water may or not be safely injected into the human vessels, is, I think, uncertain; but there is reason to believe at present that small quantities will occasion no inconvenience, and the injection of small portions only is necessary in performing this operation. Both water, and wine and water, were injected into my dogs without fatal, or even serious consequences; but into this question I forbear to enter.

In pointing to the advantages which belong to transfusion by the syringe, in preference to transfusion by the tubule, I shall not enter into details. I refrain, therefore, from enlarging on the facility of the operation, or its uses in physiological research, and shall content myself with touching on those advantages which appear to be the most important. This operation may be performed with promptitude, for the human blood is always at hand; and the instrument may in many cases be procured in readiness, as the danger of uterine bleedings, at least, may frequently be foreseen. Promptitude of operating is an advantage of capital importance; for there is reason to *surmise*, from the preceding experiments, (4, 5, 6, 7, 8.) that the apparent death of bleeding soon becomes irremediable. Another advantage arising out of this method of operating is, the abundance in which the blood may be procured. A dog, below the middle size, and this variety, perhaps, is principally found about our houses, generally dies after it has given off from ten to twelve ounces of blood; but much larger quantities of human blood might be obtained on an emergency from the friends of the patient, or for hire.

But of all the advantages derived from transfusion by the syringe, by far the most important is,

the opportunity it offers of throwing human blood into human veins. There seems reason for presuming, from facts already related, that the blood of one class of animals cannot be substituted in large quantities for that of another, with impunity; and hence it becomes of the utmost importance that we should be able to supply the human vessels with human blood,—an excellence, which transfusion by the syringe eminently secures.

Some Account of Six Cases, in which Injection into the Human Veins was attempted.

38. By two gentlemen in my neighbourhood I was called to a case in which a woman was dying, in consequence (as was supposed) of a loss of blood, which occurred during the birth of the placenta. As I entered the room, thirty or forty minutes after the messenger had been dispatched from the house to request my attendance, the patient ceased to respire; and *five or six* minutes afterwards, about sixteen ounces of blood procured with ease, by venesection, from two men, (relatives of the patient,) were thrown, by means of a syringe, into the bleeding vein of the arm. No signs of resuscitation were observed. The vein was laid bare with a lancet. The blood was infused without difficulty. The operation was

performed by the syringe simply, in the mode recommended (p. 127.)

39. A young man, of somewhat muscular make, a patient in Guy's Hospital, lost a large quantity of blood, from the bursting of an artery, and appeared afterwards, for *two* or *three* hours together, to be evidently sinking from inanition. This patient I was requested to see; but in consequence of my being from home, on professional business, time was lost, and I could not perform the operation till the man had ceased to respire for three or four minutes. Assisted, however, by a surgeon of talent and enterprize, Mr. Key, I injected sixteen ounces of blood, by means of the impellor, into the bleeding vein, exposed by the lancet; but, with the exception of one single sigh, no signs of returning life were perceptible. An esteemed and intelligent pupil of mine, Mr. Lord, furnished the blood; and there was no difficulty whatever in obtaining it by venesection as fast as the operation required.

In the first of these cases, it will be observed, the syringe was used; in the second, the impellor; and they prove, that in either mode, the operation is of easy performance: both, though indecisive, render it doubtful whether a man can be resuscitated by an injection into the *veins*,

performed three or four minutes after the last respiration. Artificial breathing was not tried.

40. By a friend of mine, on the other side of the water, I was called to a patient, evidently sinking from a hemorrhage, which had made its attack during the birth of the placenta, though two or three hours elapsed before death actually occurred. In this case three or four ounces of blood were injected by the *syringe*, before the respiration ceased, without of course producing any obvious effect, the quantity being small; but as a full supply could not be obtained from the lady who offered to furnish the blood, the operation was necessarily abandoned. Women, it has been observed already (p. 123.) are not so capable of supplying a sufficient quantity of blood as men are.

41. A lady bled largely for puerperal fever, sank into that state of collapse, which, as men of observation know, is the precursor of certain dissolution. At the request of friends, who were anxious that a remedy, which promised little, should have a trial, about six ounces of blood, taken from her father by venesection, were infused into the bleeding vein, by means of the syringe. Mr. Williams of St. Thomas's-street, laid the vein bare with a lancet, and the tubular nozzle of the syringe was repeatedly introduced

into the vein without any difficulty. No decisive effect of any kind was produced by the operation, and the lady died of the fever, without suffering any extraordinary symptoms.

42. A poor fellow in Guy's Hospital, (his name was Brazier) between thirty and forty years of age, lay at the point of death, in consequence of the extenuation produced by obstinate vomiting, arising, as afterwards appeared, from schirrosity of the pylorus. At the request of Dr. Cholmley, and the expressed wish of the patient, the late Mr. Henry Cline and myself injected, by means of the syringe, twelve or thirteen ounces of blood into the vein usually laid open in venesection, when no ill symptoms, fairly referrible to the operation, were produced. During the first thirty hours afterwards, there was an increase of the strength, and the man appeared mending; but at the end of this period, he began again to sink into a state of collapse, similar to that which had preceded the operation, and died about fifty-six hours after the injection. Not a single bad symptom occurred when the blood was introduced. Could the operation have been repeated, it is not improbable that his life would have been prolonged.* (p. 78.)

* See in Med. Chir. Trans. vol. x. a fuller account of this case.

43. A young man, of short stature, but rather broad and muscular, came into Guy's Hospital, under an attack of hydrophobia, then verging towards its close. By the medical officers of the institution, I was requested, in this case, to lend my assistance in performing the operation of Majendie, which consists in the abstraction of some twenty or thirty ounces of blood, and the injection of two or three half-pints of tepid water into the veins, from one to several hours after the bleeding.

From this man I directed, that thirty ounces of blood should be drawn; but I understand, from the very respectable surgeon, who operated, that after the prescribed measure had been abstracted, a further quantity, of uncertain amount, was lost during the pause enjoined by Majendie, between the bleeding and the injection of the water, in consequence of the restlessness of the patient, and the resulting difficulty in securing the orifice of the vein.

Thirty or forty minutes after the bleeding, preparation was made for the injection of the water. A surgeon of the hospital prepared the vein, and introduced the tubule; but it appeared at this time so evident that the poor fellow was dying, that those whose office it was

to decide, deemed it most prudent that the water should not be injected, and in this opinion I thoroughly concurred. By direction of the Assistant Physician to the hospital, a few drachms of spirit and water were infused, under the hope of raising the circulation, but without producing any decisive effect; and death ensued, with the usual distressing symptoms, about fifty or sixty minutes after the bleeding. The essential part of Majendie's operation, the injection of water I mean, was not performed here. Death was, I think, accelerated by the largeness of the bleeding; but this may be disputed. Respecting the operation of Majendie, I think it better to give no opinion favourable or repugnant till further information.

Whether it be possible to save a patient, when sinking from hemorrhage, by injecting blood before respiration is stopped, these cases do not enable us to judge; but we may, I think, infer from them,

1st. That transfusion, especially that variety of it, in which the syringe alone is employed (p. 127.) may be performed with facility.

2ly. And in the present state of our knowledge, that the operation is not attended with any obviously dangerous symptom, provided the blood be

promptly transmitted, and the injection of air be precluded.

3ly. And till we have proof to the contrary, that when a patient has ceased to respire for a few minutes, much is not to be expected from the injection of blood into the *veins*. What effects might be produced by the injection of venous blood into the common carotid of the right side, towards the coronary arteries, remains to be ascertained.

But, to conclude. The preceding paper contains all the facts, favourable and unfavourable, which are come to my knowledge, and which seem calculated to help the mind in judging, respecting the operation of transfusion. On perusing them, every one who is in the habit of reflecting, will, of course, form an opinion for himself: having, however, thought a little on the subject, I may be permitted to state my own persuasion to be, that transfusion by the syringe is a very feasible and useful operation; and that, after undergoing the usual ordeal of neglect, opposition, and ridicule, it will, hereafter, be admitted into general practice. Whether mankind are to receive the first benefit of it, in this or any future age, from British surgery, or that of foreign countries, time, the discoverer of truth and falsehood, must determine.

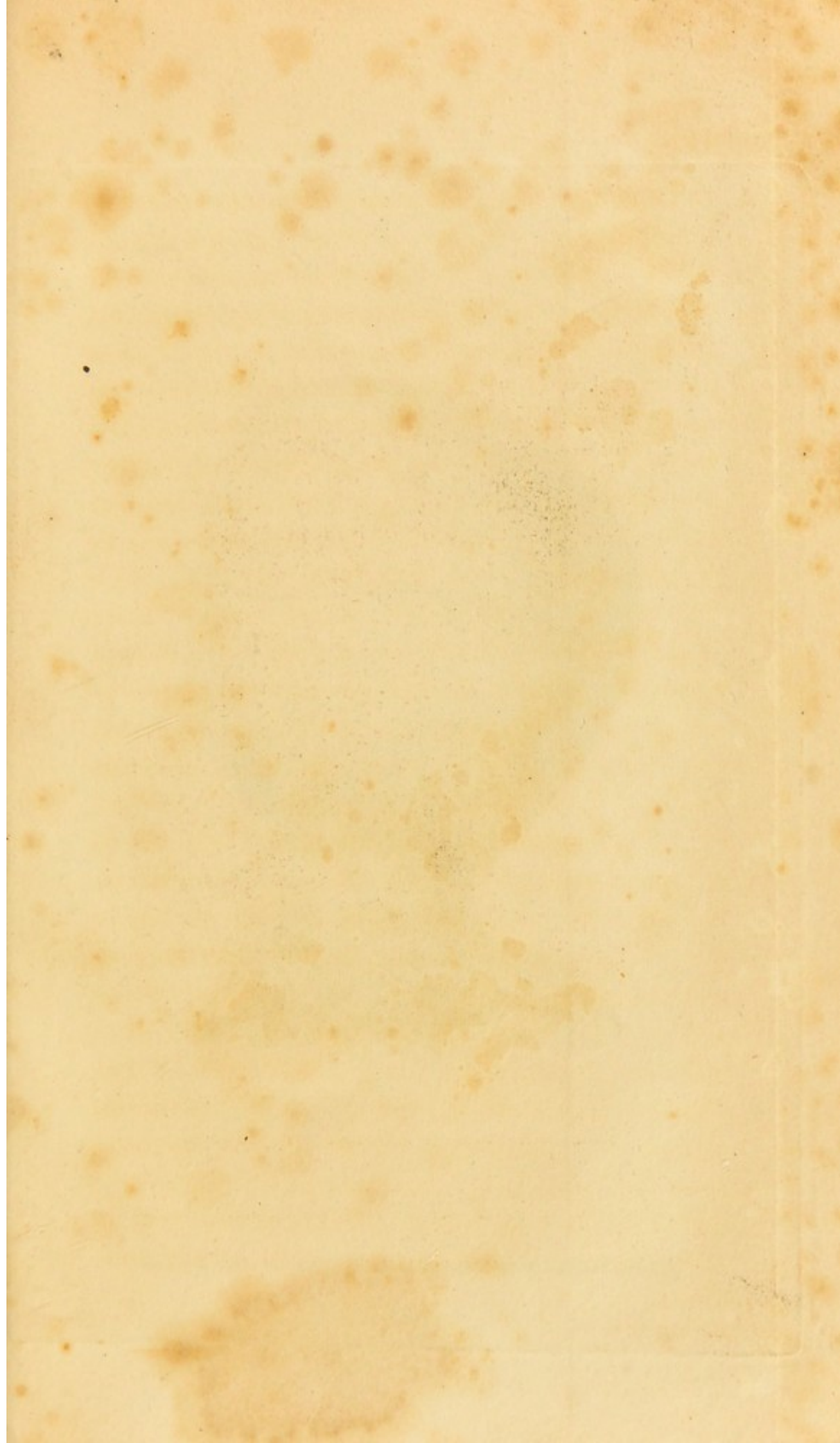


Plate 1



London. Published Nov. 20. 1824. by E. Cox & Son. St. Thomas's Street. Borough.





Fig. 1.

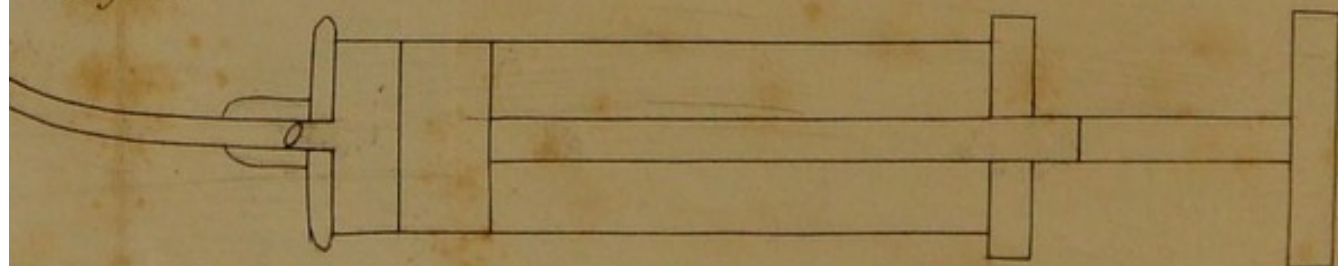


Fig. 2.

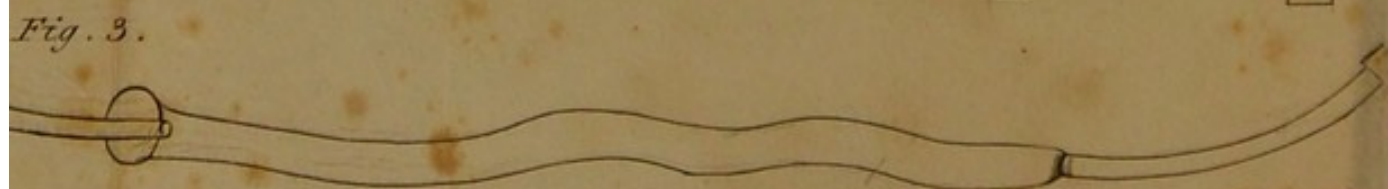


Fig. 3.

DESCRIPTION OF THE PLATES.

PLATE I.

Represents a front view of the Cancerous Uterus, extirpated by Säuter. The drawing from which this engraving was taken, was made from the preparation by Dr. Ritzius; see page 31.

PLATE II.

Fig. 1. The Impellor mounted, p. 120, 121, 122, 123.

Fig. 2. Injecting Syringe, p. 127, 128.

Fig. 3. Tube for transfusion from arteries to veins direct, p. 116, 117, 118.

PLATE III.

Fig. 1. Perpendicular section of the Impellor.

A. C. B. The inner cup.

A. D. B. The outer cup full of water.

D. E. The vice.

a. b. A line on the inner cup above which the blood should not be suffered to rise.

c. d. The upper valve, armed with *two or three* folds of soft alum leather.

e. f. The under valve, *idem*.

g. h. i. k. The syringe.

l. The plug.

B. A mark on the piston-rod indicating the elevation to which the plug *l.* has been raised.

The space covered with dots represents that portion of the instrument which is filled with blood.

The space covered with lines represents that portion of the instrument which is full of water.

m. n. The flexible tube.

n. o. The venous tubule.

When the piston of the syringe is raised, the blood is drawn from the cup A. C. B. down pipe C. through the upper valve *c. d.* into the syringe; and reflux from that part of the instrument which lies below the under valve *e. f.* is prevented by the closure of the valve.

When the piston is depressed, the blood is expelled through the valve *e. f.* along the pipes *m. n. o.*; and reflux into the cup A. C. B. is prevented by the closure of the valve *c. d.*

Except the plug joint at *n.* all the joints at which air might enter and mix with the blood, are covered with water, by which means the instrument is rendered air-secure.

With a view of avoiding complexity, the springs which raise the valves are not represented.

Fig. 4. The curve of the wire forming the spring which raises the valves.

Fig. 2. The valve with the springs mounted.

Fig. 3. Spring forceps.

Fig. 1.

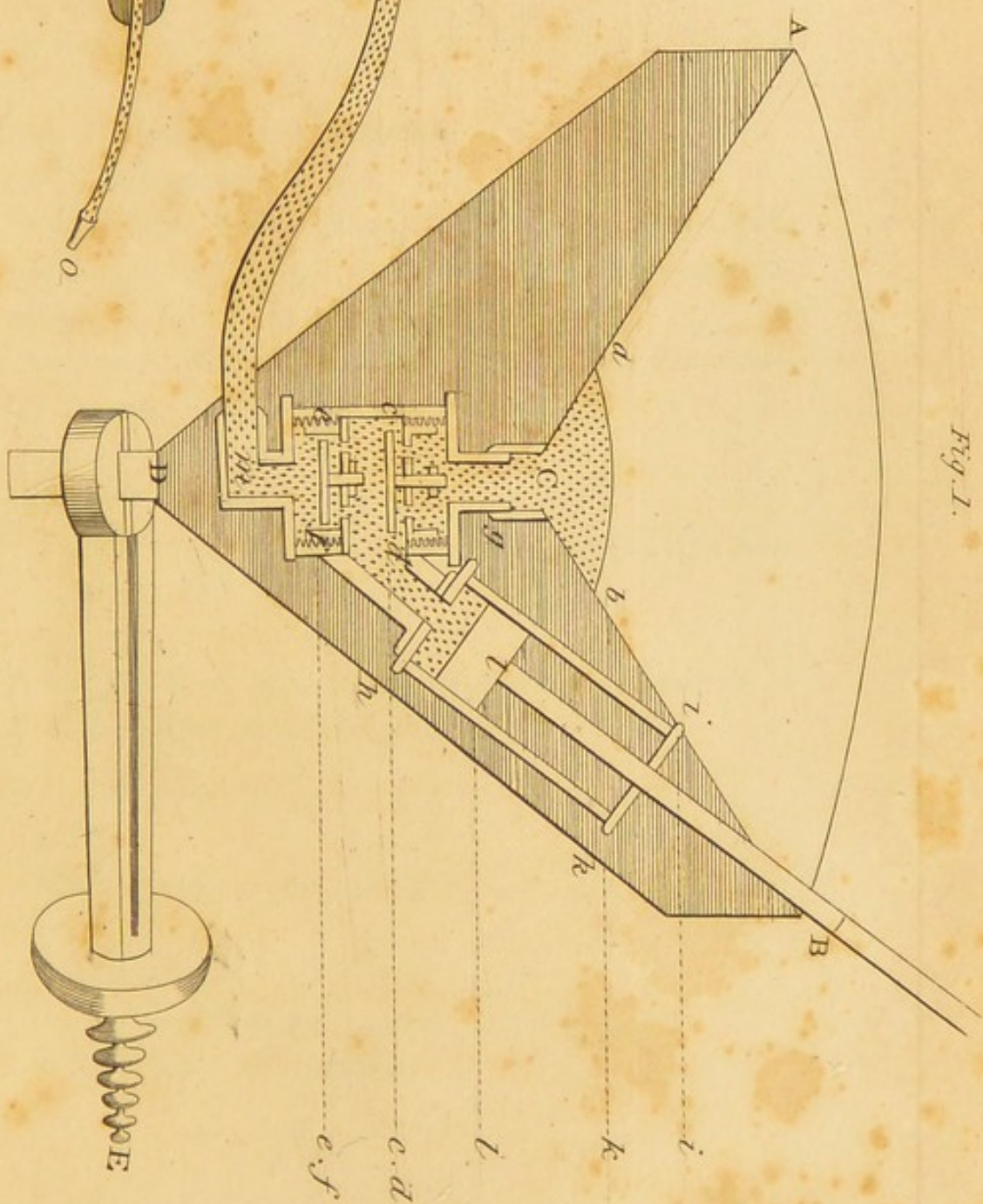


Fig. 4.



Fig. 2.

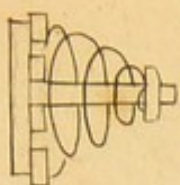


Fig. 3.





Plan for a complete Case of Transfusing Instruments.

1st. Instruments required for the transfusion of blood from artery to vein, (p. 116. et seqq.)

1. A Tourniquet.
2. A Scalpel.
3. A Lancet.
4. An Eye-probe, blunt-ended.
5. A pair of spring Forceps.
6. Two flexible Tubes, about the size of a goose-quill, one a size larger than the other, armed with metallic tubule for introduction into the artery.
7. Two metal Tubules, one a size larger than the other, for introduction into the veins, (see p. 119.)
8. A Jug with a spout.
9. A blunted needle.
10. Tape to tie up the arm in the operation of venesection.
11. Ligature, silk.

2ly. Additional Instruments required for the Transfusion of blood by means of the Impellor.

12. The Impellor.
13. A Gimlet.
14. A Vice to screw into the chair, and support the Impellor.
15. A Sponge.
16. A piece of soft Alum Leather, to form valves.
17. Wire to form valve springs.

3ly. Additional Instruments required for the Transfusion of blood by means of the Syringe merely.

18. A Syringe capable of containing three or four ounces of blood, with a nozzle capable of uniting in the way of plug with either of the venous tubules, (p. 127. and seqq.)

Plan for a Case cheaper and less complete than the preceding, but containing all the articles necessary to the performance of transfusion by the Syringe simply. (p. 127.)

1. A Lancet.
2. A Syringe (art. 18.)
3. Two Venous Tubules (art. 7.)
4. An Eye-probe blunt-ended.
5. Tape to tie up the arm.

