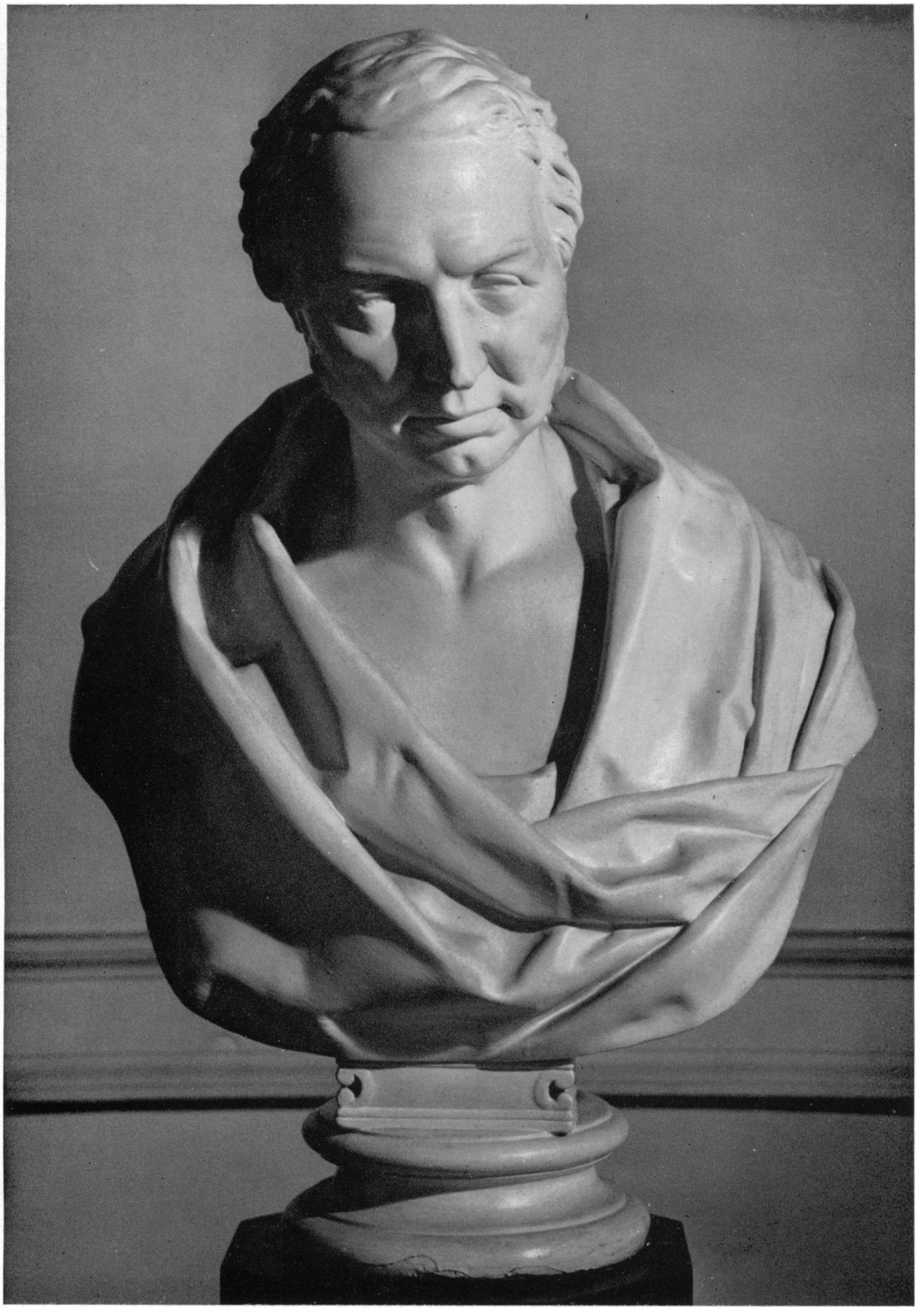

Fig. 1
A bust of James Carson in the Liverpool Medical Institution.
(Sculptor and date unknown.)

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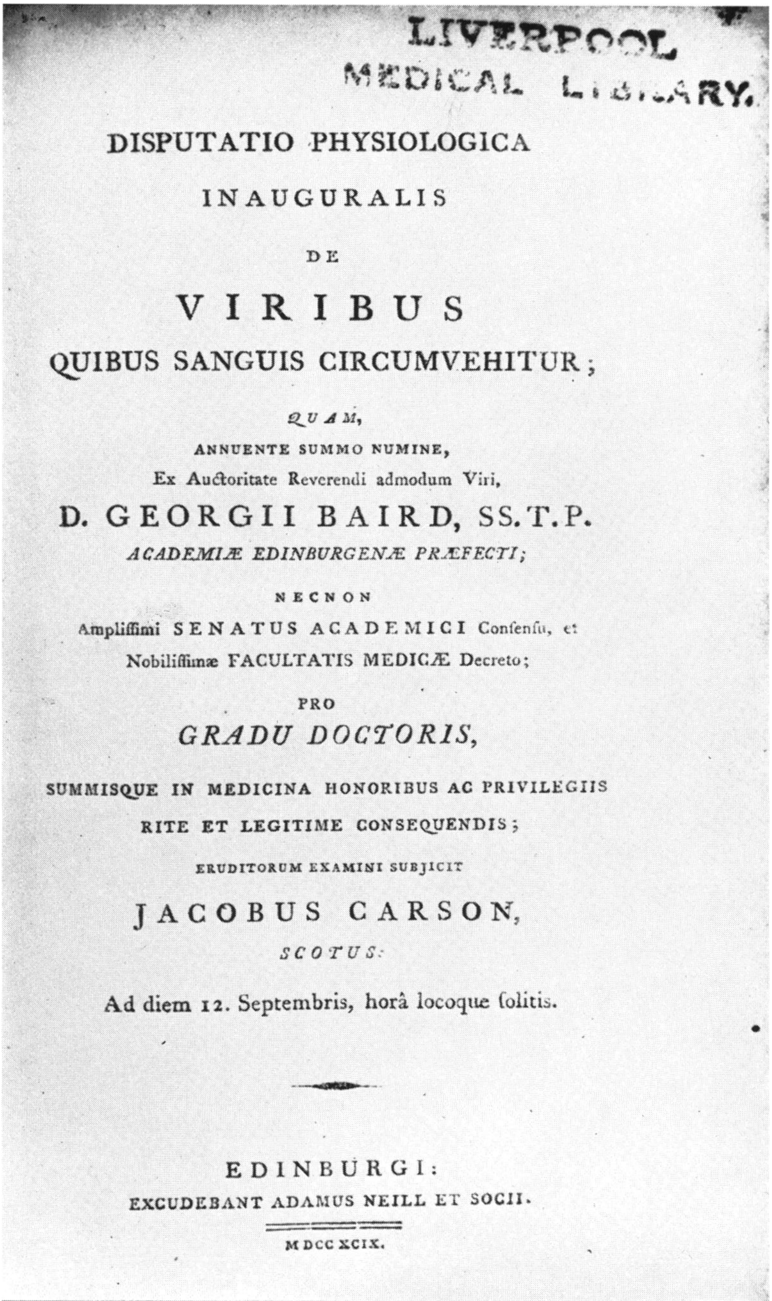


DISPUTATIO PHYSIOLOGICA

INAUGURALIS

DE

VIRIBUS

QUIBUS SANGUIS CIRCUMVEHITUR;

*QUAM,*

ANNUENTE SUMMO NUMINE,

Ex Auctoritate Reverendi admodum Viri,

D. GEORGII BAIRD, SS.T.P.

*ACADEMIÆ EDINBURGENÆ PRÆFECTI;*

NECNON

Ampliſſimi SENATUS ACADEMICI Conſenſu, et

Nobiliſſimæ FACULTATIS MEDICÆ Decreto;

PRO

*GRADU DOCTORIS,*

SUMMISQUE IN MEDICINA HONORIBUS AC PRIVILEGIIS

RITE ET LEGITIME CONSEQUENDIS;

ERUDITORUM EXAMINI SUBJICIT

JACOBUS CARSON,

*SCOTUS:*

Ad diem 12. Septembris, horâ locoque ſolitis.

EDINBURGI:

EXCUDEBANT ADAMUS NEILL ET SOCII.

MDCCXCIX.

Fig. 2
Carson's Thesis for his M.D.

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# JAMES CARSON, M.D., F.R.S., OF LIVERPOOL*

*by*

LORD COHEN OF BIRKENHEAD

JAMES CARSON (*Fig. 1*) was originally a minister of the Church of Scotland but decided to devote himself to medicine, and became a student at Edinburgh, where he graduated in 1779, in his twenty-eighth year. He later practised as a physician in Liverpool being attached to the Workhouse Fever Hospital and lunatic asylum. The researches which he carried out and published in Liverpool were to lead to his election as an F.R.S. on 1 June 1837. Twice he became a candidate for a Chair of Medicine, first at Edinburgh on the death of Dr. Gregory, and subsequently at Glasgow on the death of Dr. Freer. He was, however, unsuccessful, and he remained in Liverpool until ill health compelled him to retire to Malta. He died on 12 August 1843.

He is mentioned by none of the major medical historians except in a sentence by Garrison who writes in his *History of Medicine* (1929), 'In 1895, Carlo Forlanini (1847–1918) introduced the treatment of phthisis by artificial pneumothorax, which had been suggested by Carson (1842) and was introduced in America by John B. Murphy (1898).'

But the date is wrong; 1842 should be 1822. And Carson not only suggested artificial pneumothorax in phthisis but saw it performed over seventy years before Forlanini's operation. Yet it was not the suggestion that artificial pneumothorax might be carried out, but the steps which led to it and their subsequent development which, in retrospect, point to Carson as one of the foremost physician-physiologists of his time.

My purpose in this lecture is to deliver him from an obscurity into which he should never have been allowed to recede.

Sir Charles Dodds in his Linacre Lecture (1960) on 'A Riddle of the Seventeenth Century' was puzzled by the scant, if any, impact of the demonstration of the circulation of the blood on their practice of medicine by Harvey or his successors in the eighteenth and early nineteenth centuries.

For Carson contrariwise, the ever-permeating purpose of his physiological observations and experiments was to strengthen not only his understanding of disease, but also his ability to control it.

Carson's work stems from that of Harvey whom he greatly admired. In 1815 he wrote:

> Two centuries have now nearly elapsed since the circulation of the blood was first taught by Dr. Harvey. This theory has long surmounted the opposition against which it had for a period to contend from inveterate prejudices and jealous ambition; is universally acknowledged to have been fully proved by its author; and is deservedly ranked among those discoveries which are supposed to confer the greatest honour upon human nature.

* Faculty of the History of Medicine and Pharmacy. The Gideon de Laune Lecture, 1962.



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But Carson emphasized that there are two aspects of Harvey's thesis. The first is, and I quote from Franklin's translation of *De Motu Cordis* (p. 87), that 'the blood is driven round a circuit with an unceasing circular sort of movement'; and the second, 'that this is an activity or function of the heart which it carries out by virtue of its pulsation, and that in sum it constitutes the sole reason for that heart's pulsatile movement'.

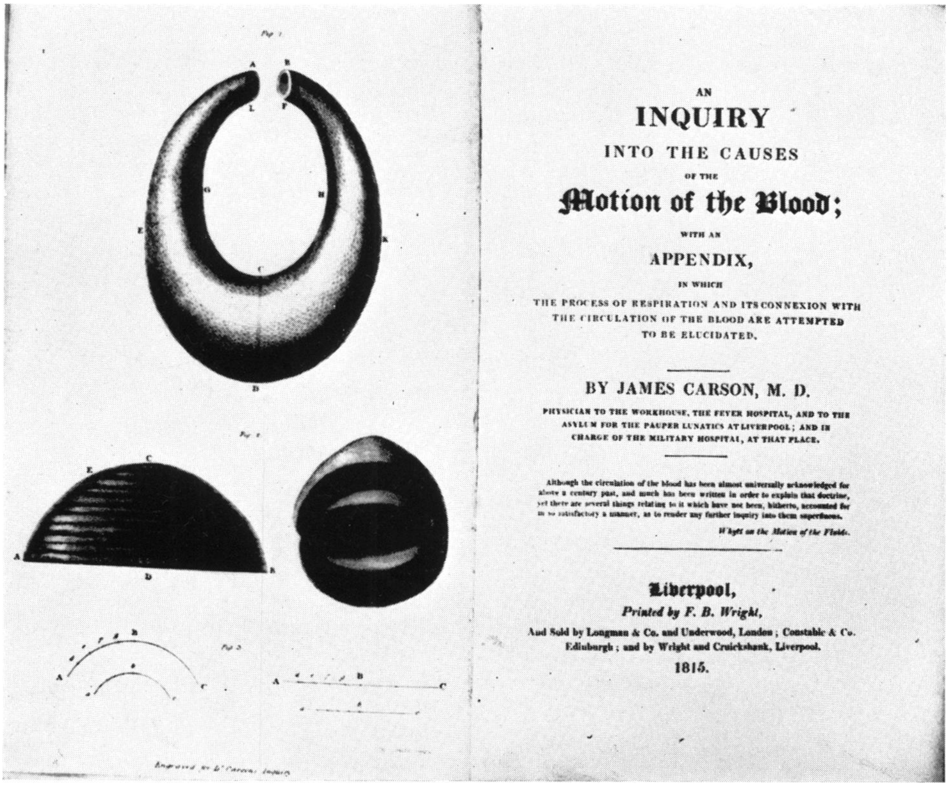
Carson accepted the first thesis, but rejected the second. He tells how he came to doubt this vis-a-tergo as the sole cause of the circulation, as a result of a meeting at the Royal Medical Society of Edinburgh when he was a student several years older than his fellows because of his earlier studies as a minister. Referring to this meeting over thirty years later he relates what for many of us must parallel personal recollection:

> At this visit a paper was read, advocating the inutility and even danger of an acquaintance with mathematics and the physical sciences, as part of a medical education. I disapproved of the doctrine, and intended to give it my reprobation, a privilege which was allowed to a visitor; but I waited to hear the sentiments of the members of the Society, and while I was still hesitating about the time of addressing the Society, and considering perhaps which I had to say, the President declared the debate to be closed. I retired from the meeting, not at all well satisfied with myself, for allowing such pernicious doctrines to pass without receiving the condemnation they deserved. The subject took exclusive possession of my mind; and in the fancied oration in which I was refuting the doctrine, and in which I passed a sleepless night, the first born of a numerous progeny from the same parent, I selected the circulation of the blood as a part of a medical education, to the understanding of which the condemned sciences were necessary. After successive unsuccessful trials to get the blood round, according to the scheme of Harvey, I began to discover the vestiges of power which had not been noticed by physiologists, and which appeared necessary to the accomplishment of the blood's circulation.

Next morning he told his friend, Mr. John Murray, who was afterwards a distinguished writer and chemist, that he believed that he had discovered the causes of the motion of the blood, and that he proposed to make this discovery the subject of his thesis. Murray felt that so unorthodox a view might be unacceptable, yet Carson did make it the subject of his thesis—*De Viribus Quibus Sanguis Circumvehitur* (*Fig. 2*)—which was presented and accepted in 1799, though Carson had reason to believe that some of the most eminent members of the Faculty condemned it in no measured terms.

In this thesis Carson discusses on theoretical grounds, mainly of hydrodynamics, his objections to the vis-a-tergo view, and here he first suggests the rôle which the elasticity of the lungs may play in the return of blood to the heart. But it is in his later work—*An Inquiry into the Causes of the Motion of the Blood* (*Fig. 3*)—published in 1815, that his more mature conclusions and the results of some experiments are presented.

Although he concedes that 'a certain degree of the motion of the blood in the veins is produced by the force of the arteries; or in other words, by a vis-a-tergo' he emphasises that this cannot be, as Harvey had asserted, the sole cause. He points out that the aorta is to the capillary bed as the apex of a cone to its base (*Fig. 3*), thus the force of ventricular systole, subject as it is to the need to stretch the arterial walls, and to overcome the resistance of the passage



AN
INQUIRY
INTO THE CAUSES
OF THE
Motion of the Blood;
WITH AN
APPENDIX,
IN WHICH
THE PROCESS OF RESPIRATION AND ITS CONNEXION WITH
THE CIRCULATION OF THE BLOOD ARE ATTEMPTED
TO BE ELUCIDATED.

BY JAMES CARSON, M. D.

PHYSICIAN TO THE WORKHOUSE, THE FEVER HOSPITAL, AND TO THE
ASYLUM FOR THE PAUPER LUNATICS AT LIVERPOOL; AND IN
CHARGE OF THE MILITARY HOSPITAL, AT THAT PLACE.

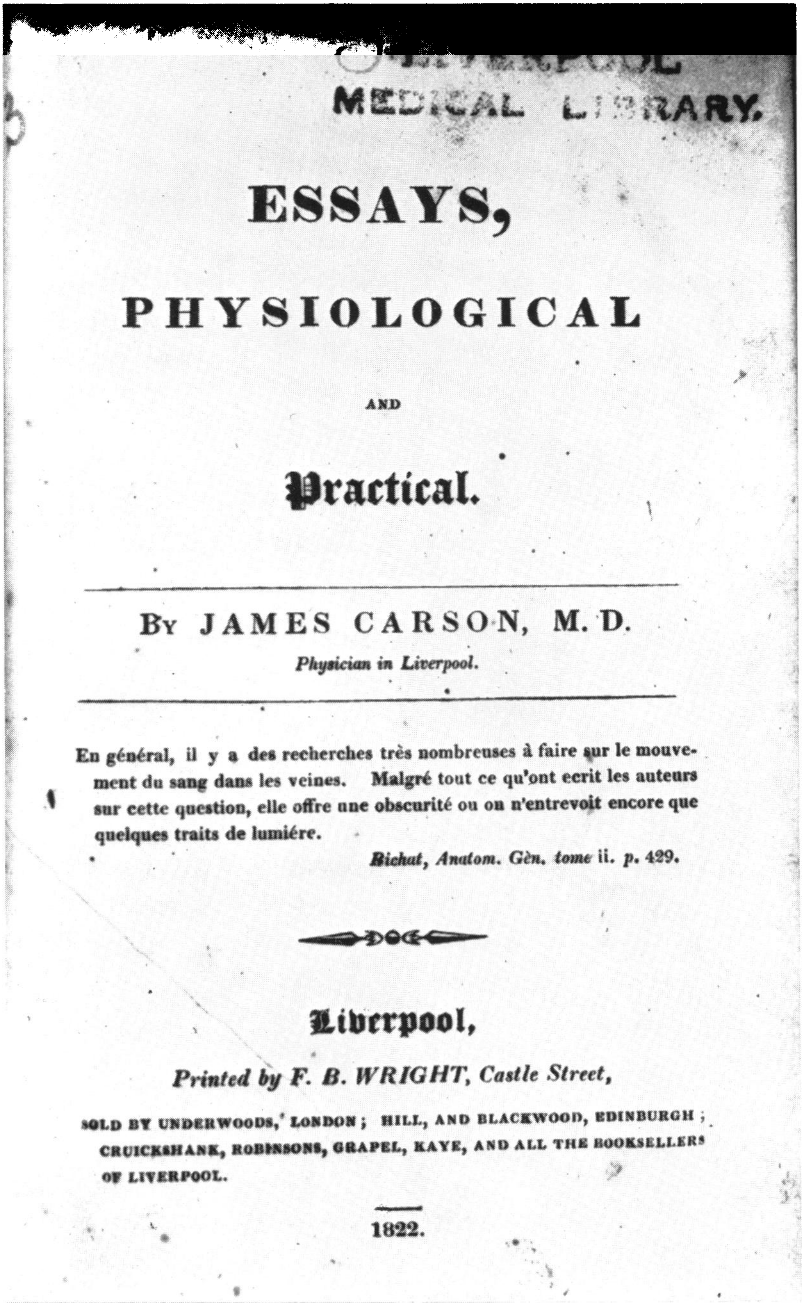
Although the circulation of the blood has been almost universally acknowledged for above a century past, and much has been written in order to explain that doctrine, yet there are several things relating to it which have not been, hitherto, accounted for in so satisfactory a manner, as to render any further inquiry into them superfluous.

*Whytt on the Motion of the Fluids.*

Liverpool,
*Printed by F. B. Wright,*
And Sold by Longman & Co. and Underwood, London; Constable & Co.
Edinburgh; and by Wright and Cruickshank, Liverpool.
1815.

Fig. 3
Title-page and frontispiece of the 1815 *Inquiry*. The frontispiece shows (1) Carson's concept of the double cone from aorta to its broadest section at the periphery and narrowing again as it returns to the heart; on (2) the circular muscle fibres of the ventricular wall.

https://doi.org/10.1017/S0025727300027848 Published online by Cambridge University Press





# ESSAYS,

## PHYSIOLOGICAL

AND

## Practical.

BY JAMES CARSON, M. D.

*Physician in Liverpool.*

En général, il y a des recherches très nombreuses à faire sur le mouvement du sang dans les veines. Malgré tout ce qu'ont ecrit les auteurs sur cette question, elle offre une obscurité ou on n'entrevoit encore que quelques traits de lumiére.

*Bichat, Anatom. Gèn. tome* ii. *p.* 429.

**Liverpool,**

*Printed by F. B. WRIGHT, Castle Street,*

SOLD BY UNDERWOODS, LONDON; HILL, AND BLACKWOOD, EDINBURGH; CRUICKSHANK, ROBINSONS, GRAPEL, KAYE, AND ALL THE BOOKSELLERS OF LIVERPOOL.

1822.

Fig. 4

*Essays, Physiological and Practical* reproduced Carson's later experiments and his work on artificial pneumothorax.

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of blood through the arteries and arterioles, is quite inadequate to complete the circle. Again he observed that when one of the distal limb veins is opened the escaping blood shows no pulsation. He then turned to comparative anatomy, and stressed that

Had the blood been circulated by the heart alone, this organ might have been expected to bear, in different animals, some proportion to their size; but this is not the case, the heart of the ox does not bear nearly the same proportion to the bulk of his body, that the heart of a dog bears to his.

He suggests also that if vis-a-tergo were the sole explanation of the circulation, then the superior and inferior venae cavae are ill-positioned, since the forces of the blood returning through these opposing veins would neutralize one another.

Carson then proceeds to examine three other factors which might, on theoretical grounds, be expected to play a part in the return of blood to the heart, namely, the contraction of muscles on movement, the pulsation of arteries emptying the veins which accompany them, and capillary attraction. These, he dismisses after subtle argument, as of no great moment. He then quotes Dr. Darwin who maintained that the veins take up the blood from the arteries by absorption in the manner that a sponge imbibes water. But Carson asks, 'What is the cause of this absorption?' He examines also the view of Dr. Wilson of Newcastle-upon-Tyne, who regarded cardiac diastole as an active suction process designed to create a vacuum so that the blood enters from the veins. After careful dissection of the cardiac musculature, he concluded that diastole follows *relaxation* of the muscle fibres and is not an active process, and that the stimulus to systole is derived from the blood which the different parts of the heart receive at every diastole 'and, perhaps in the most extensive degree, from the uneasy distension sustained by the muscular fibres at the moment of full dilation', thus anticipating the view that stretch is the effective stimulus to contraction of cardiac muscle.

John Hunter supposed that blood is a living structure, and is capable of generating its own motion, but Carson repudiates this and with it the assertion of a contemporary author, to which I shall later refer, that 'it is vain to attempt to apply the laws of hydrostatics to the blood, a living fluid, moving in living tubes'.

Having examined and dismissed these earlier views Carson turns to his own thesis. He points out that the substance of the lungs is powerfully elastic; stretch a piece to two or three times its ordinary length, and it will 'upon the removal of the distending force resiliate briskly into its former dimensions'. From the first inspiration drawn at birth the lungs are distended by the weight of the atmosphere, so that they are in contact with the chest wall and diaphragm. Carson knew that if openings be made into the chest cavity, the lungs collapse, and he therefore tried to ascertain 'the strength of the collapsing effort of the lungs' by experiment:

On the 30th March, 1815, the windpipe of a bullock which had been newly slaughtered was laid bare from the larynx to its entrance into the chest. It was divided transversely near the top, and dissected from the other parts as far as the chest. The bullock was placed upon his back with his shoulders higher than his buttocks. The windpipe was then bent into a pitcher full of water, so that the top of it reached ten inches below the surface of the water. An opening was then made into each side of the chest to admit the air. The sound of the air passing through the orifices into the chest was distinctly heard for some seconds, without the surface of the water in the pitcher being in the least agitated. In my impatience, I raised the windpipe about two inches, when bubbles of air issued from the surface of the water and in a few seconds subsided, after which, another strong ebullition prevailed for a few seconds more.

He carried out two further experiments on sheep in 1815 which showed qualitatively the resilience of the lungs. But it was in his communication to The Royal Society in 1820 'On the Elasticity of the Lungs', reprinted in his *Essays, Physiological and Practical* in 1822 (*Fig. 4*) that he described experiments designed to measure the elastic recoil of the lungs. For this purpose he used a glass apparatus (*Fig. 5*) consisting of a large globe holding nearly two quarts with tubular openings at each end, A and B. At one end was a glass tube BC about three feet in height and the other a shorter tube AD. To D a piece of dried gut was bound a few inches in length, and to the other end of the gut was fixed a cylindrical tube of bone, metal or wood, also of a few inches in length, and of a diameter corresponding with the diameter of the windpipe of the animal which was to be the subject of the experiment. For these experiments he used several species of animals—cats, dogs, calves, bullocks and sheep. His early experiments were inconclusive since he found that opening the chest often damaged the lungs, so he used the diaphragmatic approach to induce pneumothorax. In calves, sheep, and in large dogs, the resiliency of the lungs was found to be balanced by a column of water, varying in height, from one foot to a foot and a half; in the lungs of oxen and other animals of their size, it was proved to exceed considerably this force, and in rabbits and cats, the column of water varied in height from six to ten inches.

These experiments confirmed what Carson had written in 1815:

In searching for the causes by which the chambers of the heart were dilated after contraction, it was ascertained that this condition of the organ was in part to be ascribed to the form and position of its fibres, in consequence of which simple relaxation was accompanied by a certain degree of dilatation; but particularly to the supporting of a part of the atmospherical pressure that would have rested upon the convex surfaces of the heart or its envelope by the resilient or collapsing efforts of the lungs. It was urged that the abstraction of a part of the ordinary pressure of the atmosphere from the convex or external surface of the heart, or from the convex surface of the pericardium, was perpetual and was, therefore, always ready to cooperate with the dilating faculty of the heart itself as that was alternately renewed; and that the conjunction of these powers was fitted during the intervals of contraction to dilate the chambers, to the utmost extent, or at least to the extent of the capacity of the pericardium.

In consequence of the dilatation of the ventricles by the causes which have just been stated, the valves at the roots of the arterial trunks yielding from the greater pressure from without become securely closed, and the resumption of blood by the heart from the arteries is completely prevented; but the passage of blood from the auricular into the ventricular cavities is not obstructed; the blood, therefore, by which the former chambers were dilated pursues the less resisted course, and occupies the space left by the dilating ventricles. Any deficiency in the full dilatation of the ventricles, which in the healthy condition of these parts can scarcely occur, will readily be supplied by the projectile force of the contracting auricles. By the dilatation of the

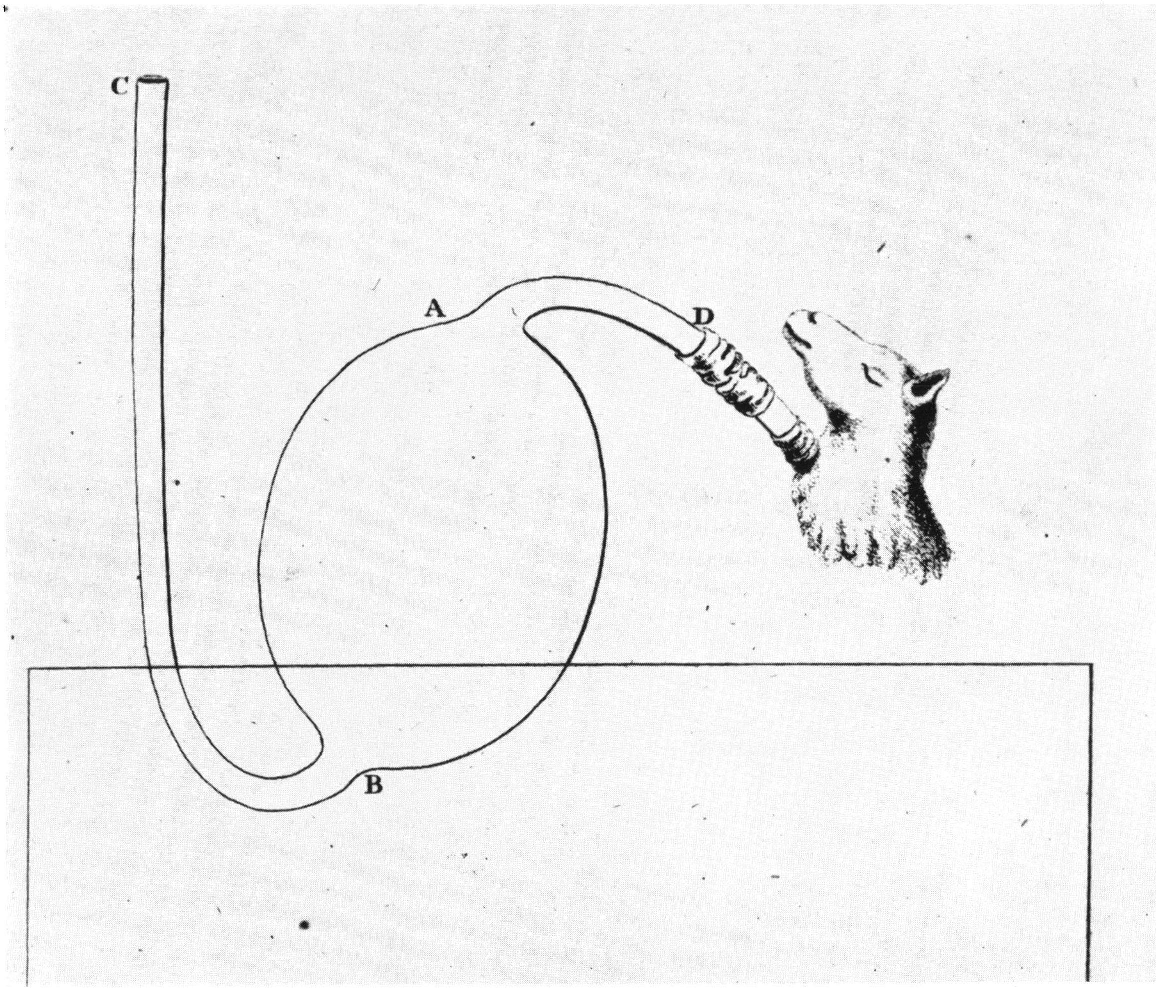
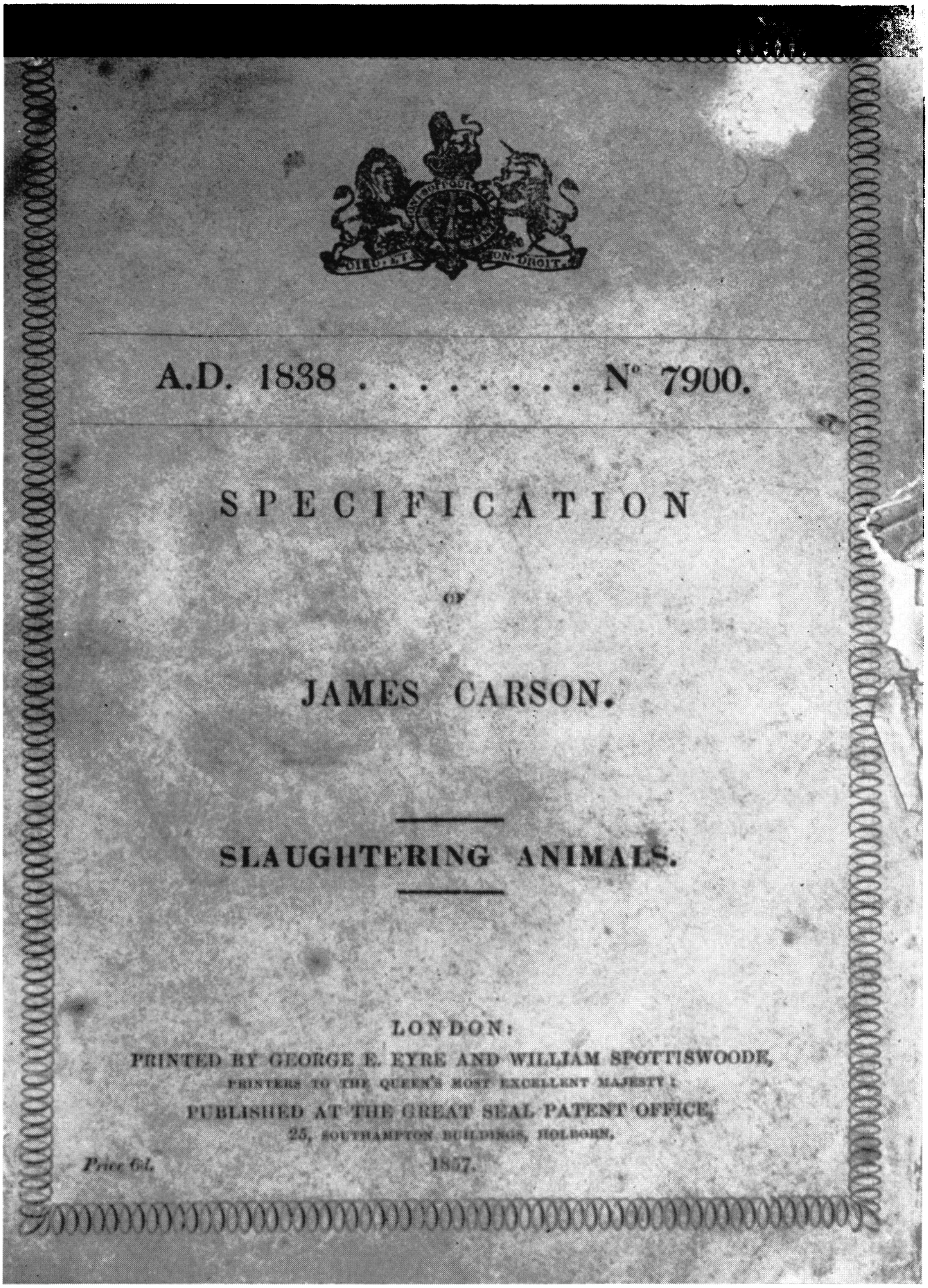



Fig. 5
Measuring the resilience of the lungs.



DIEU ET MON DROIT

A.D. 1838 . . . . . . . . . Nº 7900.

SPECIFICATION

OF

JAMES CARSON.

SLAUGHTERING ANIMALS.

LONDON:
PRINTED BY GEORGE E. EYRE AND WILLIAM SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY:
PUBLISHED AT THE GREAT SEAL PATENT OFFICE,
25, SOUTHAMPTON BUILDINGS, HOLBORN.

*Price 6d.* 1857.

Fig. 6
The patent (No. 7900) giving the specification for slaughtering animals.

auricles, the valves, in the auricular passages, sustaining less resistance on their internal surface, become securely closed; but at the other openings those by which they communicate with the venous trunks no obstacles are interposed. The blood, therefore, in the large venous trunks, is relieved from a part of the ordinary pressure in the direction of the heart; it necessarily takes the course in which it meets with the least resistance, and continues to move in that course until the resistance is equalised by the full dilatation of the auricles.

The modern physiologist writing that the venous return to the heart depends chiefly on the fact 'with each inspiration there is an increased negative pressure in the pleural cavity which aspirates blood towards the heart' (Samson Wright, 1956) is recording what Carson asserted nearly 150 years ago.

Two further examples will suffice to illustrate the fresh light which Carson's views shed on old problems. Both are culled from his own writings.

Harvey had noted

in dissections so much blood is found in the veins, but little in the arteries; much in the right ventricle, little in the left one. [This, Harvey attributes to the fact that] blood can pass from the veins into the arteries in no other way than through the heart itself and the lungs. When these latter have stopped moving at the end of expiration, the blood is prevented from flowing out of the small branches of the artery-like vein into the vein-like artery, and thence into the left ventricle of the heart. As, however, the heart does not cease to move when the lungs do, but continues thereafter to beat and to survive, the left ventricle and the arteries go on discharging blood systematically round the body and into the veins, but receive no replenishments through the lungs, and are thus virtually emptied.

In his paper on 'The Vacuity of the Arteries after Death' communicated to The Royal Society in 1821 and reprinted in his *Essays* (1822), Carson rejected Harvey's ascription of the empty arteries to the last struggles of the left ventricle, and demonstrated that the cause of the empty arteries and the distended veins is the resilience of the lungs fixing

the channels in which the blood will flow in its course towards the heart after death. . . . The arteries are powerfully elastic, and when their coats are relieved from the distending force of the heart become of a diminished calibre. Valves stationed at the roots of the arteries prevent the return of blood from these vessels into the chest. After the small part of the aortic system intervening between the heart and the confines of the chest shall have been, as it usually is found to be, filled with blood, the blood in the rest of this system will sustain no diminution of pressure from the side of the heart. [But, continued Carson] no obstacle exists in the way of the blood in its course to the chest through the veins.

He then described experiments in which openings are made into both sides of the chest to secure collapse of the lungs resulting in death. On dissection, the muscles were remarkably red, and on being incised poured out blood; the liver was like red morocco; and a ligated part of the descending aorta was found to contain a small cylinder of coagulated blood. He compared the appearances of two rabbits, one killed by lung collapse, and as a control, another by thrusting a sharp instrument between the vertebrae of the neck; in the former, muscles and organs were full of blood; in the latter

scarcely was the vestige of a blood vessel to be observed on the surface of the intestines or stomach, which had a pale appearance . . . the flesh was white, and when cut into appeared to be dry discharging at some parts a drop or two of blood . . . the trunks of the veins were swollen and rounded, whereas in the other rabbit, they appeared flat and to contain a thin layer of blood.

From these experiments, and similar observations on sheep killed in various manners, Carson concluded

that the difference of the distribution of the blood after death from that in which, according to the Harveian theory, it must exist in the living system, arises chiefly from the elastic power of the lungs; and that the emptiness of the arteries and of the smaller vessels observed after death, admits of a satisfactory explanation from the supposed operation of this cause, combined with that of the elasticity of the arterial canals.

The second problem which Carson's experiments solved was the mode of action of the diaphragm. The accepted view, according to Carson, was that when the diaphragm contracts it descends and the abdominal muscles relax; when the diaphragm relaxes, the abdominal muscles contract 'sympathetically' and push the diaphragm and viscera towards the thorax. Carson questioned this view on four grounds. One, that observation showed that the abdominal muscles were not tense during expiration; two, that in the concave scaphoid abdomen associated with marked wasting, contraction of the abdominal muscles should have the opposite effect; three, extensive abdominal wounds with extruded bowels do not impede diaphragmatic respiration; and four, that in experiments in which the abdominal muscles were completely severed or removed diaphragmatic respiration continued. Carson then proceeds to show that when the diaphragm contracts and descends its power overcomes the resilience of the lungs, but when contraction ceases, the diaphragm returns to its original position because of the resilience of the lungs, and quite independently of the abdominal muscles.

The first suggestion of artificial pneumothorax as a therapeutic measure was made by Carson in 1822 in a paper on 'Lesions of the Lungs', read before the Literary and Philosophical Society of Liverpool and republished in his *Essays*. He noted in his experiments that unilateral pneumothorax had a very temporary detrimental effect, and that some days later the animal would survive an opening in the chest on the opposite side. In one experiment, for example, he incised the left side of a rabbit's chest; the animal lay stunned for a few seconds, then leapt up, took food, and appeared in all respects well. Five days later an incision was made on the right side. The breathing became instantly short, rapid and laboured; the animal was extremely restless and so weak that it could not stand for more than a few seconds, and for two hours, death seemed imminent. Then, contrary to Carson's expectation, the animal began to recover gradually, and four hours after the operation began to leap about the appartment, to breathe easily and to take food. Carson argued, rightly, that the intact mediastinum prevented the opposite side of the chest from being affected in a unilateral pneumothorax. He then considered the application of this knowledge to disease. He pointed out that if the tendo achilles is ruptured its two ends must be apposed by 'suitably contrived machinery' to ensure healing.

Applying this reasoning to lung cavities and abcesses, he argued that their obstinacy in healing is due to the elasticity of the lungs keeping their walls on stretch and preventing 'salutary contact'; indeed, the elastic spring of the fibres tends still further to increase the breach. He ascribes the frequent spontaneous cure of wounds *ab extra*, by sabres or bullets, to the frequently associated lung collapse. But he recognized the potential risks of complete collapse of a diseased lung:

> When any considerable vessel has been ruptured in the lungs, the air vessels of the lung become filled with blood. If in these circumstances the lung were reduced to a state of collapse, the blood contained at the time in the air vessels, would be thrown into the trachea, and might cause death by suffocation. In the same manner, if a lung in which there was any considerable abscess filled with matter were suddenly reduced to a state of collapse, the matter contained in the abscess and in the air vessels of the lung, would be thrown into the windpipe, and might bring the patient into great and imminent danger.

Thus he is led to urge repeated partial pneumothorax:

> To obviate these dangers, the plain and simple means are to reduce the lung thus situated to a state of collapse by degrees only. This might be accomplished by admitting a small quantity of air into the cavity of the chest, at one time, and allowing an interval to exist between the successive admissions, which may be necessary before the lung shall be brought to a state of complete collapse.

Recognizing that consumption 'crops the flower of the human race' and resists 'all our efforts to arrest even for a moment the slow but steadily onward pace of this fatal malady', he held that the trial of any rational method of treatment was justified. The first opportunity Carson had of testing the method was on 26 September 1822. James Sloane, an eminent merchant of Liverpool, the last of five brothers, the other four having died of consumption, had returned from the West Indies in the last stage of consumption. Soon after, he heard of Carson's paper and determined to have the operation. This was carried out by Mr. Bickersteth, the most eminent of contemporary Liverpool surgeons, but on incising the chest between the sixth and seventh ribs, the customary inrush of air was not observed. The patient thought that he was somewhat relieved but gradually worsened and died a month later. After death, extensive adhesions were found binding the lungs to the chest wall and mediastinum which prevented collapse. Carson surmised that since adhesions were commonly found associated with tuberculous infection of the lung, these would often prove a contraindication to artificial pneumothorax. But, he inferred, they might be a blessing in disguise, for if a sufficiently deep incision could be made into a lung abscess, it would drain, and the lung would not collapse. Unfortunately, in the one case he records, that of Mr. Johnstone, a merchant of Liverpool, the incisions failed to reach the abscess, and the patient succumbed. Carson appears, however, to have had success with a dependent opening for unilateral empyema and records that 'complete cures might often be expected'.

Carson was to put his experiments on pneumothorax to yet another use. In 1838, he took out a patent for a *new mode of slaughtering animals intended for human food* (*Fig. 6*).



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The effect of this method of slaughtering is [he writes], to retain the lymphatic and lacteal fluids, and indeed, all the finer juices of the body at any point in the same proportion in which they existed while the animal was still alive, instead of being accumulated in the large vessels and discharged out of the body in a mass and becoming a nuisance.

Fig. 7 illustrates the method.

A tube *A*, pointed at one end *B*, and perforated near the point by several holes *C*, is to communicate at the other end with an air-tight bag *D*, filled with air and of a size fitted to different animals; a button *E*, is to be placed on this tube with a slight concavity on the side facing the point, and at such a distance from that point as to admit all the holes in it to be within the cavity of the chest, into which it is to be introduced. A tube thus armed is to be introduced by a simple perforation (to be made by a scalpel or lancet) into the cavity of the chest between the fifth and sixth rib on each side of the chest; at the same time, as far as the button will allow it, the concave side of the button is to be filled with some greasy substance and pressed firmly against the side of the animal to prevent any air escaping along the outside of the tube. The air is then to be pressed gently out of the bags, through the tubes, into the chest, the lungs instantly collapse, and in a minute or two, the animal expires.

He shows the method of securing the animal and also at *J* the point where the instrument enters the chest. I can find no record of whether the method was ever used in practice, and if so, how extensively.

On the evidence which I have provided, none will dispute Carson's pioneering role in revealing the influence of respiration on the circulation.

The only earlier observation which I have found was that of Stephen Hales in his *Statical Essays* of 1733, where are recorded his studies on blood pressure by inserting a glass tube into the arteries. He writes 'when the blood has subsided a little in the tubes which were fixed to the arteries of these dogs, it would as in the horses, rise on a sudden considerably on deep sighing'. But Hales makes no reference to the important contribution which the inspiratory suction power of the thorax, due to the elasticity of the lungs, makes to the circulation; there can be no doubt that the credit for this must be given to Carson. Twenty-seven years after his graduation thesis, and eleven years after his publication *On the Motion of the Blood*, Dr. (later Sir) David Barry, in his *Experimental Researches on the Influence Exercised by Atmospheric Pressure upon Progression of Blood in the Veins* (1826) makes no reference to Carson, but propounds a similar thesis. He observed that if one end of a bent tube be introduced down the jugular vein, and the other end rest in a vessel containing water, then the water within the tube can be seen to rise during each inspiration and sink during each expiration.

Then followed the work of Poiseuille (1828), of Ludwig (1847) and in 1853, of Donders, who first measured intra-thoracic negative pressure, or the elastic pull of the lungs in man, and showed how it varied with the depth of respiration.

Though Carson's main medical works are on respiration and the motion of the blood, they are by no means confined to these topics.

His more physiological essays included those on animal heat, absorption and muscular motion (*Fig. 8*). But apart from the last in which he describes some of the mechanical factors involved in muscular contraction, he makes no

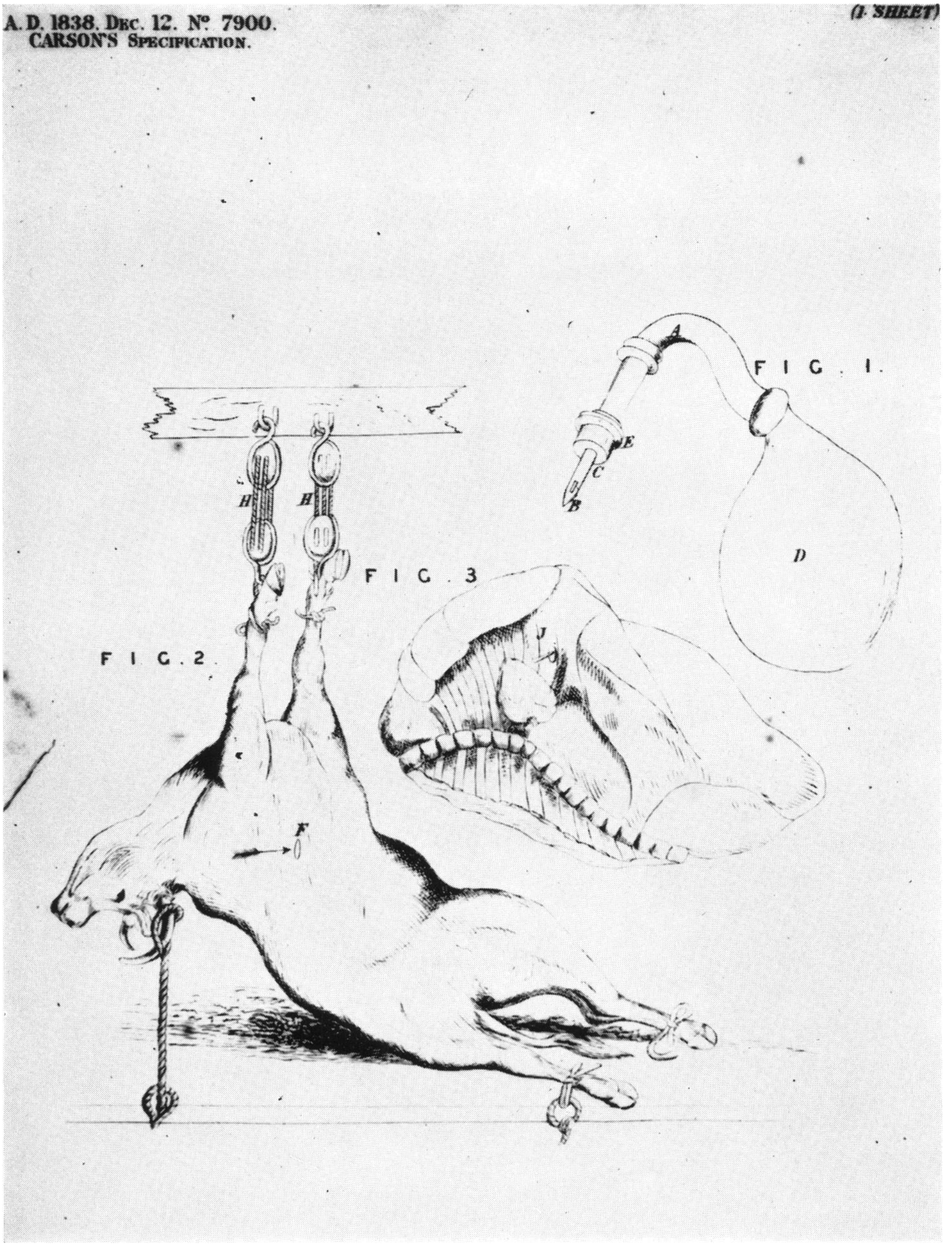
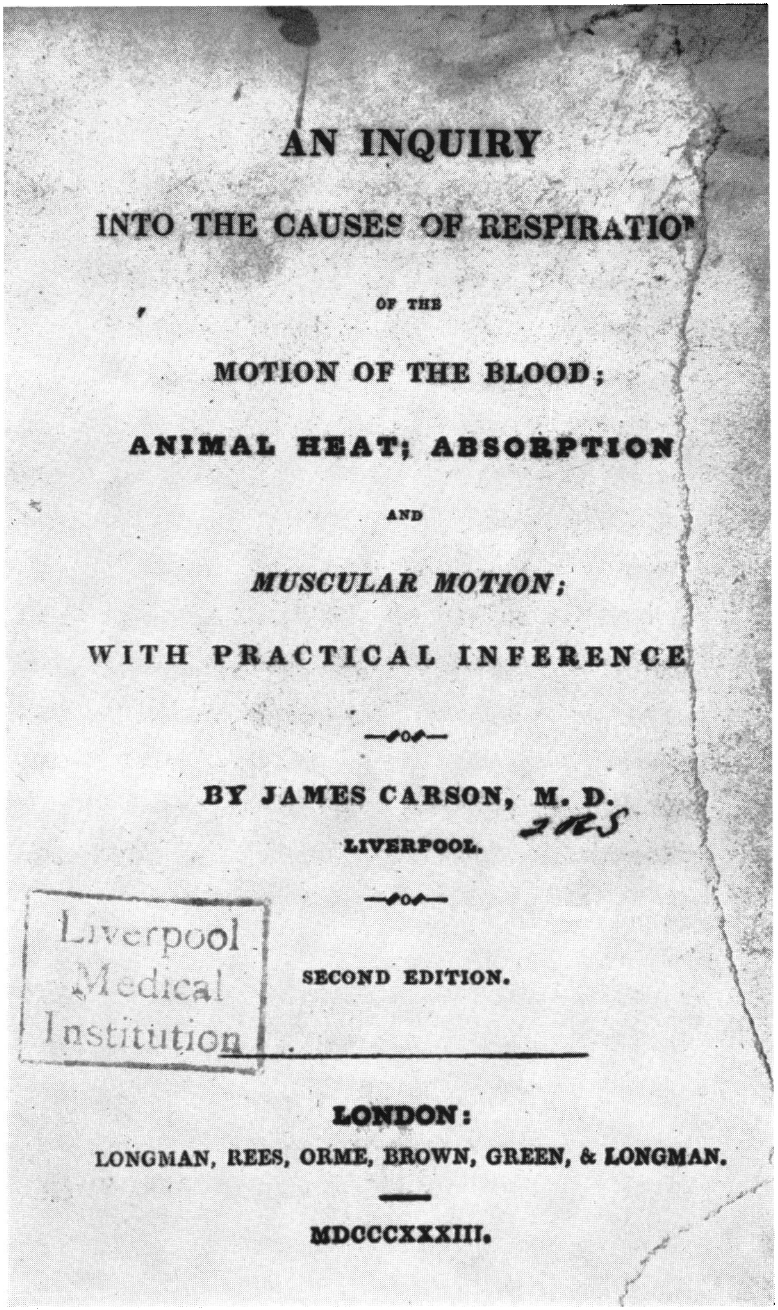



Fig. 7
Showing Carson's drawing of the method of slaughter.



AN INQUIRY

INTO THE CAUSES OF RESPIRATIO[N]

OF THE

MOTION OF THE BLOOD;

ANIMAL HEAT; ABSORPTION

AND

*MUSCULAR MOTION;*

WITH PRACTICAL INFERENCE[S]

BY JAMES CARSON, M. D.

LIVERPOOL.



SECOND EDITION.

LONDON:

LONGMAN, REES, ORME, BROWN, GREEN, & LONGMAN.

MDCCCXXXIII.

Fig. 8

This 1833 publication is largely a revision (Carson calls it a second edition) of his 1815 volume; but included are essays on animal heat, absorption, and muscular motion *with practical inference.*

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original experimental contribution, but seeks to draw inferences from the then current and, as it proved, grossly inadequate data described by others.

There is to be detected, in most of his writings, a preoccupation with the part in all physiological processes played by respiration. His explanation of animal heat after a long discussion of the inadequacy of earlier theories, is that in the lungs, part of the air is converted from a gaseous to a liquid form while still in the lungs and whilst coursing in the blood vessels; thus heat is conveyed to the periphery; in the tissues the changes are reversed, and heat evolved.

The then current theory of absorption was that taught by John Hunter, namely, that food was absorbed by the lacteals which Aselli had described in 1622, and that absorption from tissues was wholly lymphatic. Carson's view was closer to that now accepted, namely, that both veins and lymphatics play a part in absorption. He writes:

> The office of absorption is twofold; it consists, in the first place, in conveying from surfaces, both internal and external, whatever is fitted for the nourishment and repair of the system; and, in the second place, in taking up and carrying off such constituents of the arterial blood, as may be unfit for the former purpose, or such matters as having for a season constituted a part of the system, and discharged a useful purpose, have, by the changes they have undergone, or by a solution of their adhesion, become useless and noxious, and can no longer be retained with benefit or safety. I allude solely at present to the matters which are, or may become contents of the sanguiniferous and lymphatic systems. The first office, that of supplying the system with nourishment, is I contend, performed solely by the lymphatics, considering the lacteals and imbibers of the lungs, a part of them; the latter, or that of depurgating the system, by the red veins.

One of the essays attributed to Carson is that on *Digestion* (*Fig. 9*), said to have been written in 1834, but published in 1863, twenty years after Carson's death. In this, he writes, he has 'long regarded the doctrine of digestion by the solvent powers of the gastric juice, as erroneous', and he seeks to demonstrate that digestion is a form of animal and vegetable fermentation. But there is much in this posthumous essay of Carson's which leads me to doubt its authenticity.

Carson had held since his student days, and indeed we have earlier learned that it was the impetus to his earlier work, that physiological function must ultimately be explained in terms of mathematics and the physical sciences.

Nowhere in his explanations of the circulation or of respiration does he have resource to 'a vital force'. Again, in his essay on 'The Vacuity of the Arteries after Death' he refers scornfully to the followers of Harvey who have been driven to the unphilosophical conclusion 'that the motion of the blood is exempted from those laws, to which the hand of the Creator has subjected the motion of all other material substances'. This, as he calls it, 'pleasing apology for ignorance' because of the great authority of its more recent adherent, John Hunter, had become so general that 'the language of the Schools on this subject may be said to be, that it is in vain to apply the laws of hydrostatics to the motion of the blood, a living fluid flowing in living tubes'. And Sir Gilbert Blane is amongst those whom he criticized for referring to physical investigation applied to this subject as 'the vain parade of a science foreign to medicine'. It is, therefore, difficult to believe, when in referring to Spallanzani's experiment with *in vitro* digestion of gastric juice in which the phials containing meat and the juice were introduced under the armpits in order to ensure body temperature, that it is the voice of Carson which speaks in the following words:

But this is not a fair mode of ascertaining the effects of the gastric juice out of the body, for the influence which life may be supposed to have on the solution of the food would be secured in this case. The affinities connected with life would extend to substances in contact with any part of the system: substances placed under the armpits, are not placed, at least, in the same circumstances with those entirely unconnected with a living animal.

It is for these reasons that I believe that the *Essay on Digestion* was written by another hand, but possibly found amongst Carson's papers long after his death, and published in his name.

I cannot today speak of Carson the man. Many incidents in his life point to an arrogance, which might have resulted from frustration, of which his own writings give ample evidence. In 1808, occurred one of the *causes célèbres* in the annals of poisoning—the trial of Charles Angus (*Fig. 10*) for the murder of his wife's half-sister, Miss Burns, who lived with him. Carson was credited with having cheated the gallows of its due by the evidence he gave for the defendant, but I must leave this fascinating story too for another occasion.

What I have endeavoured to do today is to pay a long neglected tribute to James Carson, the scientist. He deserves a permanent niche in the saga of medical history first, because he realized that Harvey's momentous demonstration of the circulation of the blood did not explain adequately the cause of its circular motion; secondly, by mathematical reasoning and original scientific experiment, all too rare amongst physicians of his day, he showed the important part played by the elasticity of the lungs in circulatory and respiratory function; and thirdly, unlike Harvey and many of his successors, he sought to apply his knowledge to the art of medicine, as exemplified by artificial pneumothorax in the treatment of lesions of the lung.

But for the medical historian, I would hope that this lecture conveys another lesson. Carson's 'weighty and solid' fame is virtually unrecognized in the standard histories of medicine. Might it not be that an earnest search in local medical archives will unmask 'full many a gem of purest ray serene'? For too often history supports Bacon's contention that 'Fame is like a river, that beareth up things light and swoln, and drowns things weighty and solid.'

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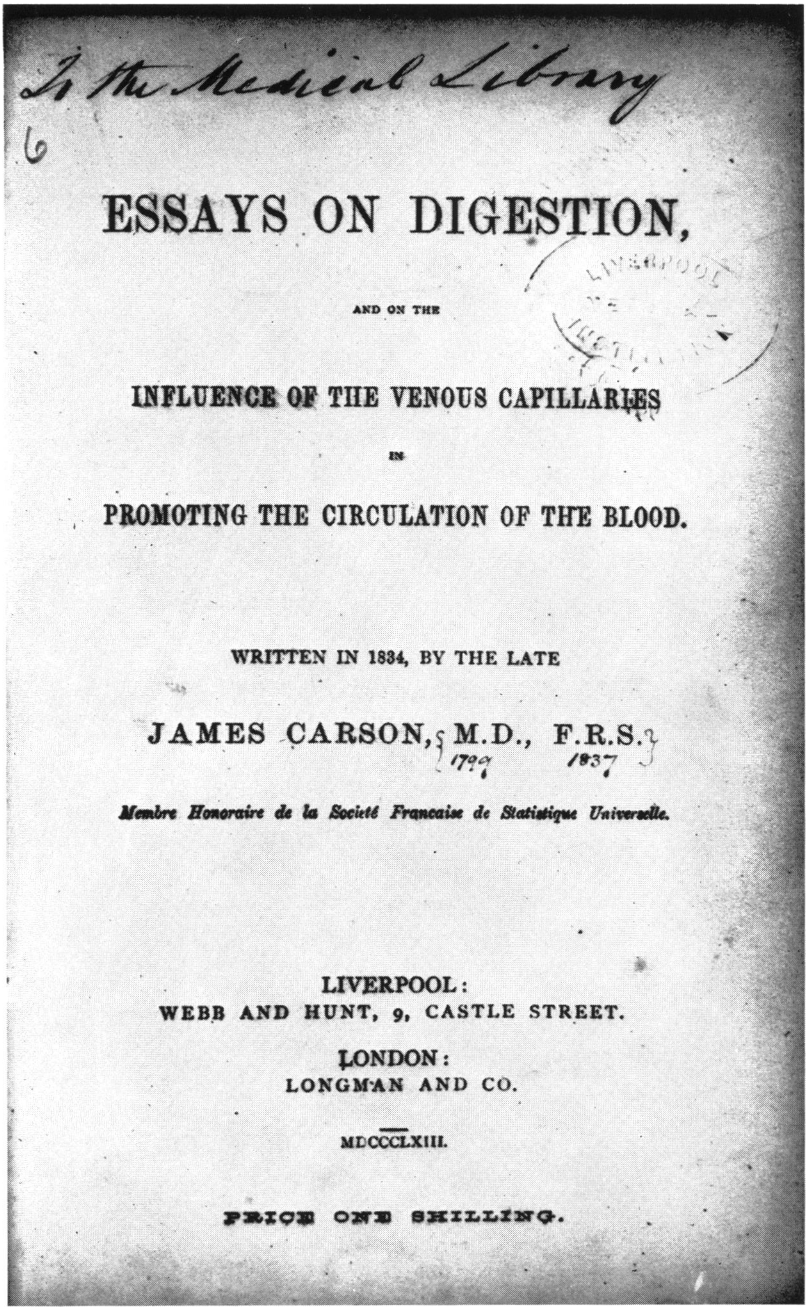

ESSAYS ON DIGESTION,

AND ON THE

INFLUENCE OF THE VENOUS CAPILLARIES

IN

PROMOTING THE CIRCULATION OF THE BLOOD.

WRITTEN IN 1834, BY THE LATE

JAMES CARSON, M.D., F.R.S.

*Membre Honoraire de la Société Francaise de Statistique Universelle.*

LIVERPOOL:
WEBB AND HUNT, 9, CASTLE STREET.

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Fig. 9
This posthumous essay, though attributed to Carson is, on intrinsic evidence, most unlikely to have been written by him.

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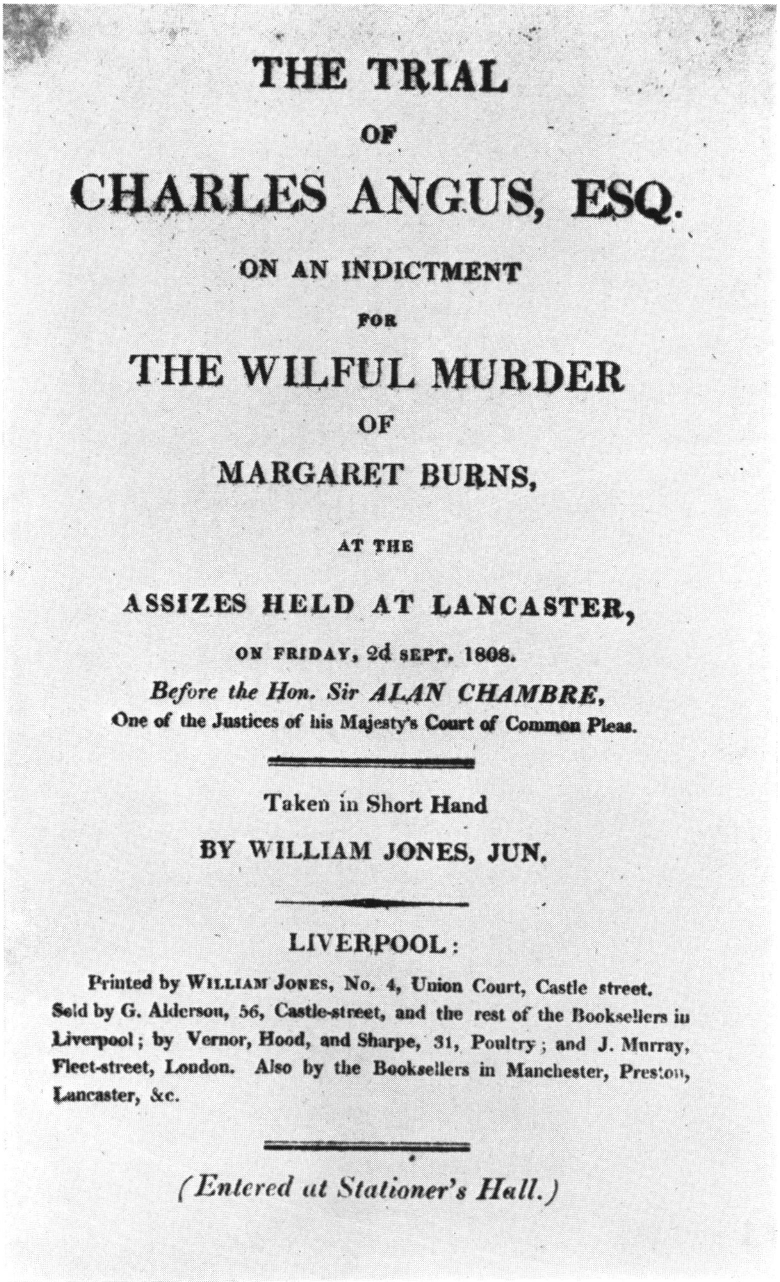

THE TRIAL

OF

CHARLES ANGUS, ESQ.

ON AN INDICTMENT

FOR

THE WILFUL MURDER

OF

MARGARET BURNS,

AT THE

ASSIZES HELD AT LANCASTER,

ON FRIDAY, 2d SEPT. 1808.

*Before the Hon. Sir ALAN CHAMBRE,*
One of the Justices of his Majesty's Court of Common Pleas.

Taken in Short Hand

BY WILLIAM JONES, JUN.

LIVERPOOL:

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*(Entered at Stationer's Hall.)*

Fig. 10
A verbatim report of the trial in which Carson gave evidence for the defence.

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4. *A letter to the members of the Parliament of the United Kingdom*, 1812.
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Many of Carson's papers were first read before the Literary and Philosophical Society of Liverpool. The Proceedings of this Society were not published until its thirty-fourth session in 1844. A list of papers read before that date is, however, available and the following list gives us the dates and titles of Carson's contributions, which were later included in other publications or communications to learned societies.

7 December 1821: Physiological remarks on certain functions of the lungs.
5 April 1822: Proposal for curing diseases of the chest, by producing artificially a collapse of the lungs.
5 December 1822: On the most economical method of killing animals, for the food of man.
7 November 1823: On the circulation of blood in the head.
3 December 1824: Inquiry into the sources of animal heat.
7 October 1831: On the sources of animal heat.
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18 March 1839: On the sense of muscular motion.

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## BRITISH AND FOREIGN MEDICAL REVIEW 1836–47

DR. RICHARD HUNTER, The National Hospital, Queen Square, London W.C.1, writes:

I am trying to trace the set of the *British and Foreign Medical Review*, I–XXIV, 1836–47, kept by its editor Sir John Forbes, and annotated by him throughout with names of authors of reviews etc. On his death in 1861 he left it 'to his good friend and faithful publisher, Mr. Churchill, as a small mark of esteem and regard' (*Memoir*, 1862, p. 54). Enquiry at Messrs. J. & C. Churchill revealed that it is not now in their possession and indeed must have disappeared or been disposed of at least fifty years ago and possibly before the turn of the century. I should be most grateful if any reader could guide me to its present whereabouts.