for the serious maladies, and a remarkable decrease of the death-rate. As the mean ratios of sickness in them on the less healthy stations are the general averages of the stations, and as the mean ratios of death on those in China and the West Indies are also under the general averages, it may be confidently expected that, when all the sanitary conditions of these vessels are known and attended to, the change from wood to iron in the construction of ships will be found of signal benefit to the health of the navy. Superior means of cleansing and ventilating decks, holds, and the bilges, and the interspaces of the ribs and those under the engine-rooms, are already advanced improvements, from which high results may be reckoned on in ironclads; and as those means are perfected, so we may calculate on a reduced rate of sickness and a diminished mortality from yellow and remittent fevers, dysentery, and bronchopneumonia, to which may be assigned three-fourths of the present large amount of phthisis.

I have dwelt on professional matters that will be more acceptable at a naval port, where we breathe the sea-air and look at ships, than they would be elsewhere. I have endeavoured to show:

1. How the charitable views and exertions of those who defended the country at sea in the days of Queen Elizabeth have influenced the medical affairs of the navy; and how the principles established, and the system adopted by them, have been revived, after apparent disuse, as a national instead of a class means of dealing with the residuum of the crews of our fleets: the accumulated funds, by defalcations of pay, etc., by voluntary offerings, endowments, bequests, and Parliamentary grants, being now availed of by the State, to supplement its own pensions, in providing for its maimed, disabled, and worn-out seamen and marines, when aged.

2. That the absolute necessities of the country in war led to the construction of the naval hospitals; and that, although their institution was postponed eighty years after their necessity was recognised, yet, like the fable of Minerva springing armed from the brain of Jove, they were designed on principles that have stood the test of time, and are still considered perfect models of their kind, fulfilling to the utmost their intent and functions.

3. That from the era in the internal economy of the navy, when, as Blane remarked, "impressment began to be less rigidly enforced, and to be deprived of some of its horrors, lemon-juice to be liberally allowed to ships at sea, surgeons to receive encouragement, and medical regulations to be enforced," a very constant progress of improvement in the health of the navy has gone on, until its medical statistics, gathered from every portion of the coasts of the world, exhibit mortality rates not far behind those of the general population of the British Isles.

My task is ended; and, in thanking you, Mr. President and gentlemen, for the attention with which you have listened to these notes on a branch of public medicine, I trust that this visit to Plymouth will, if possible, increase your professional interest in what concerns the health and vigour of that glorious service the British navy.

THE CIRCULATION IN THE ACARDIAC FŒTUS.

By JOHN W. OGLE, M.D., F.R.C.P.,

Physician to St. George's Hospital.

THE question as to what rôle the capillaries or small blood-vessels play in furthering the circulation of the blood, has been often raised, and has lately been brought before the profession in a correspondence between Dr. G. Johnson and his "Reviewer" in the *British and Foreign Medico-Chirurgical Review*.* The subject must be allowed, for every reason, to be an interesting one; and I cannot but regard it as being somewhat strange that its discussion, after having advanced to a certain point, should apparently have been allowed to foreclose without any allusion on either side to certain observations which have been made by several writers in past years, and which would seem to constitute a necessary link in any chain of discussion on so important a subject. I desire in this communication to refer the reader to one or two facts which must be recognised in anything like a complete survey of data forming a basis for such discussion.

In support of the proposition that the arteries alone suffice by their contraction to carry on the circulation, the reviewer of Dr. Johnson had quoted the case of an acardiac fœtus which Sir B. Brodie in 1809 described in the *Philosophical Transactions*, and which had been alluded to erroneously by Mr. Palmer, in his notes on the works of Hunter, as having lived some time after birth. It proved, however,

that Brodie had described the fœtus as having been born dead; and this important statement, which Mr. Palmer had overlooked, had not been referred to by the reviewer. Consequently the supposed instance of viability of such a monster turned out to be not a fact.

Still the fœtus without a heart had lived *until* birth, and had originally been supposed by Brodie to have furnished an example of the circulation having been carried on apart from the influence of a heart—i. e., of any *vis à tergo*. This supposition, however, was subsequently set aside by the statement of Dr. Young, that, whenever an acardiac fœtus exists, there is always a second fœtus, well developed and possessing a heart, as an attendant; and this dictum was strengthened by Sir Astley Cooper, who not only described in the *Guy's Hospital Reports*, vol. i, 1836, a case of an acardiac fœtus which was a twin, the other fœtus having a heart; but went further, and showed the means or channel of communication between the two fœtuses, proving by injection and dissection that the heart of the perfect fœtus drives the blood through the vessels of the acardiac one by means of direct anastomosis between the arteries and veins of the two fœtuses at the junction of the umbilical cord with the placenta. Appended to this communication by Sir Astley Cooper was a declaration by Brodie acquiescing in Young's statement, accepting Cooper's explanation, and correcting the mistake which he had himself previously made in supposing the circulation in the acardiac fœtus to be independent of the heart.

The above facts are those (if I read it aright) concerning this question of the independence of the circulation, which have been elicited by the discussion between Dr. Johnson and his reviewer. Taking an interest in the subject, and being led by the discussion to follow it out to some extent, I found that other facts also exist connected with it, which should not be lost sight of, and which, as I before said, have been brought forward by those engaged in the subject in past years. Of these, I propose to give a short summary, by way of adding something towards the interest and completion of the discussion.

In the first place, I would allude to a paper on Acephalous Fœtuses* in the first volume of the *Guy's Hospital Reports*, 1836, p. 218, by Dr. Hodgkin. This was, in fact, a letter to Sir A. Cooper, and contains the history and description of several monstrous productions, and also an abstract of what had hitherto been written on the subject by English and foreign writers, including Brodie's case before alluded to. In this paper, Hodgkin specially dwells on "the mode of circulation through the bodies." He refers to the fact, previously alluded to by Breschet, that in almost all cases of acephalous fœtuses the heart has been wanting; and he observes that, in a large majority of cases, the acephalous fœtus has been accompanied by one or more perfect children—stating that the "inquiry immediately presents itself, how far the circulation in the monster was connected with that of the perfect child, and depended upon it;" and to this idea, started by Young, he had alluded in his Thesis in 1823.† He refers to the fact that Dr. John Clarke (and quotes the case) was the only author who had previously demonstrated the vascular connexion of the two placentæ. (See *Phil. Trans.*, 1793, p. 154, "A Description of an Extraordinary Production of Human Generation.")‡ He then notices the views held by various authors as to the maintenance of the circulation in the fœtus in the absence of a heart, observing that Méry and Lecat referred it to the direct impulse of the mother's heart; that Ponjol and Winslow concluded there was no circulation—the first thinking he found no other vessels than veins both in the fœtus and umbilical cord, whilst the latter found only arteries. Monro and Tiedemann both consider that the circulation is maintained by the impulse given to the blood by the vessels themselves, although they do not agree as to the course taken by the blood, Tiedemann considering that the blood is conveyed from placenta to fœtus by the umbilical arteries; and in this view Breschet coincides, endeavouring to support it by analogy with the circulation in fishes and mollusca.

Dr. Hodgkin then gives in detail, which it would be too long to recapitulate, the views held by those who have described this kind of malformation, as to the distribution of vessels and the course of the fluids in the foetal mass. Appended to the above paper by Hodgkin is an account of the structures of the placenta and imperfect fœtus by Sir A. Cooper,§ which was referred to in the recent controversy in this JOURNAL, and to which I have alluded already. In this account, as before said, Cooper demonstrates the mode of support and circulation of the

* He points out that the term "acephalous" comprehends not only those instances where the head is wanting, but also those in which this and every still more considerable degree of deficiency may be met with.

† This Thesis I have had no opportunity of seeing.

‡ The mother was a patient in the General Lying-in Hospital in Store Street, London.

§ The placenta and fœtuses had been sent by Hodgkin to Cooper for examination.

* See Dr. Johnson's article on the Physiology and Pathology of the Circulation in the number of the JOURNAL for May 10th; and correspondence in the numbers for June 3rd and 10th.

second or imperfect foetus, which receives through its own umbilical artery from that of the perfect foetus the current which, after entering the aorta and circulating through arteries and veins, returns by the umbilical vein of the imperfect into that of the perfect foetus. Following the description by Sir A. Cooper is the letter of Sir B. Brodie before referred to, in which he expressed satisfaction with the arguments of Dr. Young. In the same year (1836) was published, in the tenth volume (p. 204) of the *Dublin Journal of Medical Science*, an account which Dr. Houston had given to the Medical Section of the British Association at Bristol of a human foetus without brain, heart, or lungs; with observations on the nature and cause of the circulation in such monsters. In this he refers to a paper which he had previously read before the Surgical Society of Ireland in connexion with the case, and minutely describes the dissection of the specimens; and, after adducing and examining the views of other physiologists on the circulating organs in the monster, such as Tiedemann, Brodie, Breschet, Monro (alluding to the case of Dr. Clarke), he characterises as untenable the view of Astley Cooper that the blood is driven by the heart of the perfect foetus from the vessels of the perfect into those of the imperfect foetus, and back again; and seeks to establish, by arguments drawn from dissection, that the blood passes into the imperfect foetus in a normal way—i. e., by the umbilical vein, and back by the umbilical artery; but that in the foetus itself the arteries and veins change functions, so to say, the circulation being inverted—the blood entering by the veins, and returning by the arteries. He repudiates any supposed influence of the heart of the perfect foetus on the circulation in the imperfect one, and attributes to the placenta an inherent power of drawing in and ejecting again the blood which traverses its vessels; and suggests that, by means of this and like properties in the foetus itself, the circulation may be carried on between them without any other influence whatever. Subsequently Dr. Marshall Hall wrote a paper in the *Edinburgh Monthly Journal*, 1843, in which, adopting Dr. Young's idea, he rebutted at length the conclusions both of Sir A. Cooper and of Dr. Houston. He objected to Cooper's view that the blood is carried from the perfect into the imperfect foetus through the umbilical artery by means of anastomosis between the arteries of the two beings, as constituting an inversion of the order and course of the circulation in the cord; and to Houston's view, as being an inversion of the circulation in the foetus, and because it leaves us without a power to which the circulation of the blood in the imperfect foetus can be referred. As a proof that the action of the heart of one foetus *does* extend to the umbilical cord of a second foetus, Dr. M. Hall quotes a highly interesting case from Lallemand of the birth of twin foetuses. After division of the umbilical cord of the first-born and ligation of the foetal extremity, it was found that from the end connected with the placenta an unusual amount of blood was pumped out in jerks or pulsations—"lancé par saccade". This led to the suspicion that another foetus was in the uterus—as proved, indeed, to be the case.

Dr. Hall asserts that there is no proof whatever of the capillaries possessing any intrinsic power of moving or circulating the blood, but that the influence of the heart may and does extend to and through the capillary vessels; moreover, that the heart's influence in the case of a second perfect foetus extends not only along the cord and through the placental circulation, but through a second series of capillaries in the body of the imperfect foetus, illustrating this extended heart's influence by what occurs in the case of fishes—the blood being distributed first through the capillaries of the lungs, then through capillaries of various organs of the body, and lastly again through those of the portal circulation; also by experiments, which he relates, on the fin of the eel. He observes: "My view of the subject is this. I suppose the heart of the perfect foetus to propel the blood to the imperfect foetus, as Dr. Young suggested; but not immediately from its umbilical artery into that of the other, according to the view of Sir Astley Cooper, but through the capillaries of the placenta into the umbilical vein, and through the umbilical vein and a series of capillaries to the aorta; and not, according to the views of Dr. Houston, in an inverted direction. There may appear to be some difficulty in conceiving how an anastomosis between the umbilical artery of the perfect foetus with that of the imperfect can exist without arresting or impeding the flow of blood towards the placenta in the latter. The fact is to be explained, I believe, on the principle of lateral action. As a stream of air or of water, passing by other air or water, carries with it the adjacent particles of the latter; and as a stream of air or water, passing rapidly along one tube immediately by the orifice of another tube containing a similar fluid, either at rest or flowing with less rapidity in the same direction, draws the fluid from the latter; so the blood propelled with energy along the umbilical artery of the perfect foetus, immediately by a branch anastomosing with that of the imperfect one, will draw by lateral action the blood contained in the latter into its own rapid cur-

rent. The blood in the artery of the imperfect foetus is thus drawn by the blood propelled in that of the perfect foetus towards and into the placenta. This effect will also be proportionate to the greater velocity of the blood propelled by the heart of the perfect foetus. Thus, then, in reality, and on two different principles, the blood is propelled to, and attracted from, the imperfect foetus, by the power and action of the heart of the perfect one, and along the customary channels."

As a rejoinder to the above, it would seem that Dr. Houston read a second communication on the same physiological question before the British Association in August 1843 (*Dublin Journal*, Jan. 1844); and this in its turn was followed by further observations by Dr. M. Hall, in which he again sums up his views as follows. "According to my view of the blood in the acardiac foetus, the blood is propelled by the heart of the perfect foetus through *both* placenta, along *both* umbilical veins, into *both* foetuses, with *equal* velocity. The difference between the two cases is observed in the umbilical arteries, and in these only; for, whilst the blood is propelled rapidly along that of the perfect foetus by means of the *immediate* action of the heart, it is propelled slowly along that of the acardiac foetus by the action of the same heart, diminished in its energy by having already propelled the blood through the vessels of the placenta, of the cord, and of the foetus, just enumerated. In these arteries, the blood of the perfect foetus comes into contact with that of the acardiac foetus through the medium of anastomosis. At this point, the rapid current of the former attracts the slow current of the latter, by 'lateral action',* precisely in proportion to the degree of the difference of their velocity."

I should say also that Dr. M. Hall alludes to the opinions of Dr. Graves (*System of Clinical Medicine*, 1843, pp. 82, 485) and Dr. Carpenter, both of whom thought the views of Dr. Young and Sir A. Cooper regarding the circulation in the acardiac foetus to be erroneous. Dr. Graves had quoted the acardiac foetal circulation as illustrative of the possibility of derangement of vital action of any part or organ, and the production of congestion and inflammation independently of the action of the heart or of the general circulation—a view opposed by Müller, but entertained by several German physiologists, who attributed to the blood an automatic movement or power of propulsion. He had also, in treating of inflammation and the motor powers which cause and regulate the circulation, etc., dwelt at length on the attracting and propulsive power of the small vessels, strengthening his position by arguments derived from the known action of minute vessels in certain lower animals and in plants, in furthering the movement of fluids; quoting also from a paper written in 1842 by Dr. Holland of Sheffield, on the Forces by which the Blood is circulated in Capillary Vessels. Dr. Carpenter, in his work on *Physiology*, had also accepted and propagated the view that in even the highest animals a capillary power exists, which is sufficiently strong to maintain the circulation by itself when the power of the central organ is diminished.

In Dr. Carpenter's recent edition (1869) of the *Principles of Physiology*, edited by Mr. Power, allusion is made at p. 300 to the paper above quoted, by Dr. Houston, and to the controversy between him and Dr. M. Hall. It is considered that in Dr. Houston's case it seemed impossible for the heart of the twin foetus to have occasioned the movement of blood in the imperfect one; and that some cause present in the latter must have been sufficient for the propulsion of blood through its vessels. It is also thought that Dr. Houston's reply to Dr. M. Hall, above alluded to, was satisfactory.

Such, then, are the additional observations which I find bearing on the discussion of a capillary circulation, so far as it is supported or not by the conditions of circulation in an acardiac foetus. I have ventured to seek a place for them in the pages of the *JOURNAL*, thinking they would be considered as possessing some interest at the present time.

P.S.—Since writing the above, my attention has been drawn to a communication (which I had overlooked) of Dr. Dickinson to the *Medico-Chirurgical Transactions*, containing a description of a foetus born without heart, brain, or liver. In this, it appears, he concludes to adopt the view suggested by Dr. Young and worked out by Sir A. Cooper, as regards the dependence of the circulation in the acardiac foetus on the heart of the healthy twin, and observes as follows. "The umbilical vein of the healthy child receives a large communicating vessel from the corresponding channel in the cord of the monster. This must be looked upon as a main trunk, which receives branches in two directions—from the placenta on one side, from the cord of the monstrosity on the other. The contrariety only remains until the two streams are mingled in their common channel. The blood passing from the placenta by the venous radicles would be mixed with the blood coming from the monster by its umbilical vein, and both together

* Dr. M. Hall has a small chapter in which he adduces other instances of the influence of this principle of lateral action of the blood-circulation in the economy.

would be conveyed by the communicating vessel into the umbilical vein of the healthy child. A similar explanation would, of course, hold good as regards the arteries."

A propos of foetal circulation, Dr. Stokes, of Dublin, recently mentioned to me that he had tried once to find out what he could hear with the stethoscope on examining the abdomen of a pregnant cat. The cat was within a few days of her bringing forth, and the experiment was made with care and repeated several times. The cat had four, or perhaps five, kittens; but nothing was heard but the well-defined sound of a single foetal heart. In answer to my question, whether he had detected twins in the human subject by the stethoscope, Dr. Stokes observed that he had never examined a case of twins or triplets. He remarked, "It is said that twins have been detected by auscultation. I suppose it was by the different intensity of sounds in different portions of the hypogastrium."

THE CLIMATE OF SIDMOUTH, WITH RESULTS OF METEOROLOGICAL OBSERVATIONS FROM 1865 TO 1870.*

By J. INGLEBY MACKENZIE, M.B. Cantab.,
Physician to the Sidmouth Dispensary; Fellow of the British and Scottish Meteorological Societies.

THE temperature of the body in health, and the deductions to be obtained from variations from the normal temperature in different diseases and in various stages of the same malady, have been an object of research to many of our profession both at home and on the Continent. It does not appear that the profession, as a rule, have made observations on the air of the room in which such temperature has been taken, or of the external atmosphere at the same period—a circumstance which undoubtedly would make a considerable difference in the temperature indicated.

The temperature, the moisture or dryness of the air in which we are existing, must have great influence on our human economy. The depression caused by a "muggy" day; the brightness which is proverbially the concomitant of sunshine, without entering more into detail—a matter of but little difficulty—are ready evidences of this fact. So also the climate in which we live—whether the air be dry or moist, the temperature equable, or extremely cold or hot—ought to be a study equally deserving our attention with the phases of disease which are in no small measure dependent on changes of temperature, etc. The Registrar-General's weekly returns, with which are now published the temperatures, afford most palpable proofs of increase and decrease of disease with the alterations in the temperature of the air.

Having called attention to these points, before I proceed to the subject of this paper—the climate of Sidmouth—I must remark that the public, and I fear sometimes the profession likewise, look rather dubiously on remarks on climate made by a member of our profession, as if it were impossible but they should be, from interested motives, put in too favourable a light. Fortunately, I am enabled to place in juxtaposition, in confirmation of my own observations on the climate of Sidmouth, some observations taken by a most careful observer, not liable to any bias, which coincide entirely with mine; and, in addition to this, I am able to state that the instruments and their positions have been examined and approved by Mr. Glaisher.

The works on climate of Sir J. Clark and Dr. Scoresby Jackson, though most admirable as wholes, cannot but fail of necessity, from the magnitude of the ground they cover, to be sometimes in error. The account of Sidmouth in the former is founded on observations taken one mile out of Sidmouth for a period of less than one year only. The account in the latter is obtained from some other publication. Here let me suggest the publication of a "Dictionary of Climate", with short descriptions written by observers conversant with the localities of which they write, which would doubtless be valuable in time as a work of reference.

Sidmouth lies in the eastern part of Devon, almost centrally in the district bounded by the Axe and Exe. It lies at the centre of the segment of a circle which forms the great bay bounded on the west by Berry Head and on the east by Portland Bill. Its latitude is 50 deg. 43 min. 40 sec. N., longitude 3 deg. 13 min. 3 sec. W. The town of Sidmouth lies in a winding land-locked valley trending downwards towards the sea, bounded on each side by lofty hills 500 feet in altitude, and closed in on the northern side by another range of hills. It is exposed, therefore, to the south only. It is stated, and apparently on

as yet uncontroverted authority, that the bay, at the head of which Sidmouth is situated, was hollowed out by the action of the Gulf Stream, dividing at the Land's End; part passing to the westward; the eastern portion working up and forming the bay. And it has been argued from this hypothesis, that this is one of the reasons for the warm winter-temperature of Sidmouth. Observations on the temperature of the sea have been but rarely taken on this coast; though, by the exertions of Mr. N. Whitley, they have been taken in some parts of Cornwall; and the Scottish Meteorological Society are actively pursuing the same object, not only on the North British coasts, but also in Sweden and Iceland. The general impression is, that the sea is susceptible of less change of temperature than the land; and, if so, we may fairly argue that land lying close to the sea is liable to enjoy a climate more equable than that further distant. And, if the temperature of the adjacent sea be elevated, as suggested in the above hypothesis, doubtless it will affect the neighbouring land. That the climate of Sidmouth was not unknown to previous generations, the fact of its having been the chosen resort of H.R.H. the late Duke of Kent, and that it was here some of Her Majesty the Queen's earliest days were spent, is sufficient evidence.

The points to be observed in the climate of Sidmouth are: *a.* The equability of its temperature; *b.* Its comparative dryness—though possessing a moist atmosphere—in relation to the other parts of the county; *c.* The rapidity with which the soil dries after rain, thereby rendering exercise available for the invalid on almost every day in the year.

a. The Equability of its Temperature.—The avoidance of the extreme of heat or cold is what is generally understood by equability of temperature, or, in other words, where the difference between the highest reading of the thermometer and the lowest is smallest in the day, month, or year.

The average daily range of temperature during the years 1865-1870 inclusive was only 13.4 deg.; that of the summer months being 15.1 deg.; of the winter, 11.8 deg. In the years 1856-1863 (omitting 1862), the same averages (by Mr. Heineken's observation) were severally of mean daily range, for seven years, 9.8 deg.: summer months, 11.1 deg.; winter months, 8.2 deg. Hence during the winter months the temperature does not vary above from 8.2 to 11.8 deg., or in the summer from 11.1 to 15.1 deg., during the twenty-four hours.

The extent of the variation of the thermometer in each month in the year, between the extreme highest and extreme lowest, is only 30.6 deg.

The coldest days in 1870 were the 23rd, 24th, 25th, and 31st days of December. In a list of fifty-one places given by Mr. Glaisher in his Quarterly Report, on which the temperature was specially observed, it was found to be as low as 1 deg. at Taunton (that is, thirty-one degrees below freezing-point); 9.8 deg. at Greenwich (or twenty-two degrees below that point). The thermometer at Sidmouth never indicated a lower temperature than 19.4 deg., or twelve degrees below freezing-point, being ten degrees warmer than Greenwich, and eighteen warmer than Taunton.

The extreme range in the course of the year, during the six years (1865-70) to which reference has been made, is 55.6 deg.; that is, calculating the extreme from the hottest day in summer to the coldest day in winter during the whole of that period.

In proof of the equability of the climate, I have drawn up a table giving in columns the mean temperatures respectively of Sidmouth and Greenwich in each month during these years, showing thereby that, though the annual mean temperatures do not vary by more than half a degree, the temperature in each winter month at Sidmouth is warmer than that of London by from one to four degrees nearly; and, on the other hand, the temperatures of the summer months are proportionally cooler. The same has been proved above in reference to the extreme of cold, and might be also of the extreme of heat.

b. Its Comparative Dryness.—Devonshire is supposed to be, or has the credit of being, one of the wettest counties in England. According to an average for the year 1869, calculated by Mr. Pengelly of Torquay from the data in Symons' *Rainfall*, and published in the *Transactions* of the Devonshire Association for the Advancement of Science, it stands only seventeenth in degree of wetness or quantity of rainfall (or sixteen counties were wetter than Devonshire in 1869). The same gentleman, in another part of his paper, states: "During four years, Prince's Town was the wettest station in the county, and Sidmouth and St. George's Clyst the driest." Again: "The mean daily rate of rainfall on wet days during the four years (referred to) was .41 inch at Sheepstor, where it was greatest, and .18 inch at Sidmouth and Broadhembury, where it was least." From the same table it may be gathered that, of forty-two rain-gauge stations in Devonshire in 1869, the quantity of rain which was registered at Sidmouth was less than at any other station, with one exception, in Devonshire, the mean an-

* Read before the Public Medicine Section at the Annual Meeting of the British Medical Association in Plymouth, August, 1871.