always, under such circumstances, give an opinion in favour of amputation, from an impression that the experience I have already had of the possibility of complete recovery, will be still further strengthened and confirmed.

The subject is extremely interesting and practical, and worthy, I think, of being placed before your readers as a theme for discussion in the JOURNAL.

Association Intelligence.

MEETING OF COMMITTEE OF COUNCIL.

THE Committee of Council will meet at the Queen's Hotel, Birmingham, on Tuesday, the 1st of April, at One o'clock.

Agenda.—Financial Report for 1861; Poor-Law Medical Legislation; Annual Meeting in London; and other business.

PHILIP H. WILLIAMS, M.D., *General Secretary.*
Worcester, March 19th, 1862.

SOUTH-EASTERN BRANCH: EAST KENT DISTRICT MEDICAL MEETINGS.

THE third meeting was held at the Saracen's Head, Ashford, on March 13th; H. WHITFELD, Esq., in the chair. Eighteen members were present, and one visitor. Mr. Bottomley, the President of the Branch, sent a letter saying that he much regretted his professional engagements prevented him from joining the meeting. Several other members were unable to attend from the same cause.

Papers. 1. Mr. RIGDEN read a paper, On the Advantages of Early Turning in Cases of Labour, when the Head presents in an Unusual Way. He first adverted to a series of papers published by Mr. Figg in the *Medical Times and Gazette* of 1860, advocating turning in all cases of labour, and giving statistics of practice, proving that labours were thereby expedited, and unattended by any bad consequences either to the mother or to the child. Mr. Rigden thought such meddlesome practice not likely to be followed by the profession generally. He had had, however, a number of face and frontal presentations lately, in which he had adopted early turning with advantage. As soon as the presenting part, he said, has been detected, and it is evident, from its position above the brim of the pelvis, that there will be great difficulty in propelling it into and through the passages; in such cases, I have lately effected delivery by version. Although this procedure is not, I believe, generally adopted, and it certainly is not taught in our more modern books on midwifery, yet it is by no means new. Smellie, Granville, Boivin, Lachapelle, and several other distinguished authorities, have occasionally followed this practice. It is true that occasionally the head, while above the brim of the pelvis, may be controlled by the hand of the operator, and placed in a more favourable position; and I have in a few cases been able to make the change; but much more frequently I have failed, and thus, probably, time has been lost. The liquor amnii has escaped; and version, which may have been at first an easy operation, has become a very difficult one, and dangerous to the patients. After referring to Dr. Churchill's statistics on face and forehead presentations, Mr. Rigden went on to state the result of his own practice. In the ten cases in which I have thought it proper to adopt this procedure, no ill effects have resulted to the mother. Delivery has been accomplished quickly, and with comparatively little suffering. Eight children were born alive; one had apparently died during or shortly before the commencement of labour; and one was in a state of decomposition. In thirteen other cases of face and forehead presentation, which were left to the unaided powers of nature, four children died, and two of the living ones were delivered by forceps. I have still very great confidence in the powers of nature in completing these cases; but, seeing that instrumental assistance is often ultimately required, and that the whole process, under favourable circumstances, is attended by prolonged anxiety and suffering, it seems advisable to have recourse to version early, particularly when it may be done without danger to the mother or child.

In answer to Mr. GARRAWAY, the author of the paper said he would advise turning in all face-presentations, and that he had used chloroform in some of his cases. Mr. REID considered the operation of turning a serious one. Statistics showed that one case in three was fatal to the children. He thought cephalic version may be adopted in forehead presentations, and related a case which was thought to require forceps. On finding, however, an ample pelvis, he attempted cephalic version, and was astonished to find the head slip round into a vertex presentation. If the pelvis was large, he thought cephalic version may be as easily accomplished as turning. Mr. Rigden, in reply to Mr. WHITFELD, said one out of the number of his cases was a first child.

2. Mr. WHITFELD read a paper on the Hereditary Transmission of Mental and Physical Impressions. [This paper has been received for publication.]

3. Mr. SANKEY of Dover mentioned that he had continued the use of Belladonna in the very obstinate case of Epilepsy given at the last meeting, with the effect of lessening the fits, but only during the time of employing the medicine.

Nineteen members dined together after the meeting; Mr. Whitfeld in the chair.

Reports of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, MARCH 11TH, 1862.

B. G. BABINGTON, M.D., President, in the Chair.

OBSERVATIONS ON THE TACTILE SENSIBILITY OF THE HAND. BY EDWARD BALLARD, M.D.LOND.

THE author, not being acquainted with any very extended researches into the tactile sensibility of the surface of the body, submitted this paper as the first of a series upon the subject. The method he employed for ascertaining the sensibility of the parts examined was that known as Weber's; but inasmuch as the results of this method vary according as the points of the compasses are laid in the direction of the long axis of a part or transversely to it, he employed the sum of the numbers, in English inches and decimals, obtained by an observation in each direction as representing the true sensibility of any part. He considered that the hand, being *par excellence* the organ of touch, and possessing on the whole the highest amount of sensibility, and giving thus readily a standard for comparison of subjective impressions made elsewhere, should be the organ first examined. The paper was based upon the results of observations made upon 142 points upon the surfaces and borders of the author's own hand and fingers—in all, therefore, of 284 separate observations. The author deduced in order the general sensibility of the hand and its surfaces and borders; and, separately, of the metacarpal portion, fingers and thumb. He compared these

several parts between themselves, and pointed out the relative sensibility of the lateral halves of the hand, these being related to the freer motion imparted to the radial half; and of the centre to the sides, as showing at what parts of the hand the sensibility is highest at any given distance from the wrist. The following were some of the more important deductions. The most sensitive spot of all was the tip of the index finger, in which he differs from Weber, but agrees with Valentin. The sensibility of this spot is represented by the number ·35 in. The spot of lowest sensibility (5·0 in.) was on the dorsum of the hand, opposite the base of the fifth metacarpal bone. The palmar surface of the hand was in all parts more sensitive than the dorsal; but this was not the most sensitive part, for next to the tips of the fingers stood in order the two borders, the radial border being more sensitive than the ulnar. Weber had pointed out that the sensibility of the hand increases from the base towards the extremity; but the author exhibited this fact by accurate numbers, and demonstrated not only the increase but also the rate of increase on each surface and border of the hand, and of each finger separately. He found the most rapid increase in sensibility at the spot where the fingers actually commence, not where they apparently commence, and not at the clefts, but opposite the metacarpo-phalangeal articulations, and again at the middle of the last phalanges on approaching the tips of the fingers. On the whole, the most sensitive finger is the index, and the sensitiveness shades off towards the ulnar side of the hand; and the most sensitive portion of the index, next to the tip, he found to be its radial side. Of the little finger, the most sensitive part is the ulnar side, and he connected these facts with those parts entering into the constitution of the borders of the hand at large. Of the palmar surfaces of the fingers, that of the index is the most sensitive; of the dorsal surfaces, that of the ring-finger is the least sensitive. The radial side of the index is the most sensitive, and the sensibility shades off as the fingers are further removed from the radial side of the hand, till it becomes least upon the little finger. The ulnar side of the little finger is the most sensitive, and the sensibility becomes less as the ulnar side of the hand was distanced; but the high sensibility of the index was provided for by its ulnar side standing next in rank to that of the little finger. Of the three intervals between the fingers, that whose approximating surfaces possess the highest sensibility is the interval between the index and middle fingers. The thumb was considered separately, and was regarded as a finger not having a metacarpal element. Appended to the paper were tables exhibiting the observed sensibility in each direction at the several spots examined, and four photographs, on which were marked the sums of the observations at each spot.

ADDITIONAL EXPERIMENTS ON THE POISONOUS EFFECTS OF COAL GAS UPON THE ANIMAL SYSTEM.

BY C. J. B. ALDIS, M.D.

In the paper which Dr. ALDIS brought under the notice of the Society a short time ago upon this subject, the experiments were made with the gas as it issued from the main, but in those now to be related common gas, diluted with atmospheric air in different proportions, was used. The experiments were made with Mr. Henry Banister, at the works in Horse-ferry Road, on the 7th and 14th of February, 1862.

In the first experiment, common gas and atmospheric air in equal proportions, were administered to a rat placed under a glass vessel. The head dropped in one minute, and it became insensible, with slight convulsion, in one minute and a quarter; the respiration was hurried, the eyes staring, spasmodic jerking of the body, followed by death in two minutes and a half. The *post mortem* appearances were similar to those already described, except a darkish patch on the anterior part of the pleura of the right lung, like that produced by smoke.

The second experiment was made with one-fourth common gas and three-fourths atmospheric air. The respiration of the animal became hurried in three minutes, the head dropped in five minutes, and death occurred in eleven minutes. During the autopsy, a smoky-looking spot was seen on the pleura.

In the third and fourth experiments, the proportion of gas to air was one-fifteenth.

The former experiment was not sufficiently prolonged to show the deadly influence of the gas in this diminished quantity; but in the latter the rat soon began to pant, and the head fell in ten minutes, springing of the body was observed, with twitching of the ear, followed by coma in seventeen minutes. The eyes were open; the respiration became laborious in forty-one minutes, when death ensued. The surface of the skull was intensely red; the brain was congested; the blood was very fluid and bright-coloured; the pleura was of a bright red colour, and the right side of the heart was distended with darkish blood.

MANCHESTER ROYAL INSTITUTION: MEDICAL SECTION.

MARCH 5, 1862.

L. BORCHARDT, M.D., in the Chair.

CASES AND SPECIMENS.

Scirrhus of Stomach. Mr. GREAVES exhibited a specimen of cancer of the pylorus, in which life had been prolonged for a very considerable period by the use of nutritive enemata. A discussion ensued upon the subject of the absorption or partial digestion of nutritive matters thus exhibited, the secondary digestive function of the cæcum being pretty generally supported.

Laryngeal Phthisis. Dr. ROBERTS exhibited a specimen of very extensive ulceration from this disease, and made some remarks upon the local application of caustic solutions.

Carbolic Acid. Mr. LUND exhibited a specimen of pure carbolic acid prepared by Mr. C. Calvert. He had used it very successfully in its undiluted state in the treatment of lupus; and mentioned a case where healthy cicatrisation had ensued from its application, in a week or two, after several years of unsuccessful treatment by other remedies. He also spoke very highly of its beneficial effects, when largely diluted, on sloughing wounds. Other members confirmed this experience; and Dr. Roberts also recommended its internal use in cases of dyspepsia attended with fœtid fermentation.

Ascarides in the Horse. Dr. BROWNE exhibited some specimens of these worms, and pointed out their distinctive characters.

PAPER.

Apnœa Neonatorum. Mr. GREAVES read a paper, in which he entered at considerable length into the physiology and practical bearings of this subject. It will probably be published *in extenso*. He referred to the vital condition of the child during labour, and in the period between expulsion and the full establishment of respiration. He considered the various forms of still-birth to be divisible into: 1. Syncope, or apparent anæmia; 2. Simple apnœa, or delayed respiration; and 3. Congestive apoplexy. These states being dependent on the suspension of the placental circulation, lung-breathing not having yet been set up; or, in the third form, being suspended after partial performance. Mr. Greaves further referred to the retardation of the fœtal pulse during a pain, as proof that the contractions of the uterus acting on the placental vessels produce a temporary state of apnœa in the child, similar to that

resulting from the absolute separation of the placenta; the effect being, in either case, to cause a congested state of the ventricles of the fœtal heart. This state constituted, in his estimation, the main excitant of the *besoin de respirer*, as shown by the sudden relief immediately following the first inspiration, and the consequent rise of the pulse from 60 or 70 to 130 per minute. In dealing with the practical part of the subject, the author recommended frictions to the epigastrium, cold affusions, and, if necessary, artificial respiration; but reprobated the use of the warm bath. In the apoplectic state, bleeding from the cord was advisable; and it was suggested that, in every case, a small loss of blood might do good by relieving the congestion that exists even in the most apparently anæmic cases. The writer finally contended that the danger of an early division of the cord has been greatly exaggerated, owing to a misconception of the state of the placental function during the conclusion of labour. The pulsation in the cord has frequently been supposed to indicate a continuance of the placental circulation, which is evidently disproved by the fact that the umbilical portion still continues to pulsate after division. An interesting conversation followed the reading of the paper, in which several members took part.

Correspondence.

MEDIAN LITHOTOMY.

LETTER FROM HENRY THOMPSON, ESQ.

SIR,—I have read with much interest the cases of median lithotomy, and the remarks upon them, which Mr. Prichard has recorded in last week's JOURNAL.

My principal object in troubling you with a line now is to correct a misapprehension which appears in the paper respecting some remarks of mine at the Pathological Society of London, on a case in which Mr. Cadge of Norwich performed the median operation last autumn. I had no intention whatever of stating "that the operation had the credit of being more liable to be followed by urinary infiltration *under* the prostate, than the lateral;" and I am very glad, if such an impression was conveyed, to have this opportunity of correcting it. I stated that I doubted whether, *in cases of considerably hypertrophied prostate*, especially where the lateral lobes were much enlarged, the median was a desirable operation to perform, because, in dilating the neck of the bladder to extract a stone of full size, the floor of the prostatic urethra, or posterior commissure of the gland, was apt to split, and infiltration to take place in the situation pointed out. I have examined the organs in three fatal cases of median operation in enlarged prostate, finding this condition in each. And these observations led me by no means to conclude absolutely, but nevertheless to suspect, that in such cases the median operation might be less applicable than it is to cases of the ordinary kind. Where the prostate is much enlarged, the deep incision of the median operation, but just involving the apex of the gland, leaves a large portion of the urethral canal, from an inch and three-fourths to two inches and a half (sometimes even more), to be dilated; and, if the lateral lobes are large, rigid, and unyielding, and the floor, as it often is in such cases, be thin, the dilatation is apt to split this as the weakest part, and so to expose the patient somewhat to the danger referred to.

So much for this point. On the general question, permit me to add that I cannot but regard the median operation as a valuable addition to our resources against stone in the bladder, and that I cordially agree with Mr. Prichard in believing that the profession is under great obligation to Mr. Allarton for the promulgation of his method. I have just had the opportunity of expressing this opinion in the Lettsomian Lectures of the present year, as well as of endeavouring to indicate the cases for which the operation appears to me to be best fitted, and will not enlarge on that topic here. Nevertheless, I think there are grave reasons for doubting whether it can compete, *as a rule*, in cases of large stone—say from eight or ten drachms upwards—and in the cases of enlarged prostate referred to, with a well performed lateral operation.

My friend Mr. Cadge of Norwich informs me that between forty and fifty cases of median lithotomy have been performed there within the last three or four years, and that the mortality is quite as great as that experienced before by the lateral method. But it is right to state that there has been a full proportion of unpromising cases among them. This seems, so far, to support the opinion first expressed.

I am, etc., HENRY THOMPSON.

16, Wimpole Street, March 17th, 1862.

THE UNITY OF THE HUMAN RACE.

LETTER FROM F. J. BROWN, M.D.

SIR,—In the JOURNAL for the 8th instant, there is a short article relative to the unity of the human species.

Allow me to call your attention to the compromise proposed by a French physician, Dr. Sagot. He reconciles the differences between the monogenists and the polygenists. He says that God created the distinctions of race at the time of the Dispersion. If we accept this explanation, the distinctions of language and the distinctions of race arise from the same cause, viz., a miracle, and date from the same period (Tower of Babel).

Allow me, sir, further to say one word on the existence of a pre-Adamic race of men. It is quite possible that men have existed on the earth at different epochs; created suitable for the state of the planet at the time of their existence. Our religious history begins with Adam; and the nature of Adam, we are taught, is shared by every human being on the earth. When the present creation shall be swept off the face of the earth, then it may please God the Creator to people the earth anew. In this way there may be a succession of creations. We know that God is creating every moment, for He is the "Lord and Giver of Life"; yet he may clear off all creatures from the surface of the planet periodically, so as to make the earth better capable of sustaining higher organisations.

I am, etc., FREDERICK J. BROWN.

Rochester, March 10th, 1862.

DEATHS FROM ETHER.

LETTER FROM CHARLES KIDD, M.D.

SIR,—Would you permit me to say that it was I who used the words deaths "*after* the inhalation of ether, not *from* the inhalation," and not you? I do not want to set up any false pretence. How this list, which includes nearly all that the Boston Committee have been working at, can "complicate, rather than throw light on the subject," is past my comprehension.

More than half the cases sent are decided deaths from ether; many have been deaths while the patient was under ether on the operating table. It is the nature of the death from ether to be slow. You are obviously confounding such accidents with those from chloroform, which are sudden; the reason of the two modes of death having been well explained by the Biological Society of Paris.