late years than I used, and that is, always when we change a dressing, invariably first to cover the wound with something pure; not to wash the surrounding parts with antiseptic solution, and then, after this has been done, put a dressing on the wound; but before we begin to defile the lotion at all, put on the wound what is pure, and, last thing of all, wash the surrounding parts, which, though they look the same to our eyes, are different *to cælo*. The edges of the dressing are septic; the wound, if it is as it ought to be, is aseptic. I have known such a thing, for instance, as for a gentleman, in dressing a stump after amputation of the thigh, to wash the perineum with a rag dipped in the carbolic lotion one in forty; and then, having so washed the perineum, immediately to squeeze the rag over the wound. Gentlemen, that makes you laugh; but I assure you these are the kind of things that are constantly going on, and disasters happen in consequence; and gentlemen with whom things go wrong invariably say that with them everything has been perfectly done—a thing which, for my part, I am always loth to say. I am not likely to have many more years of active surgical work; and I have felt that when you, sir, gave me this opportunity, it was my duty to speak what I believe to be the truth; for I feel it to be a grievous thing that patients should be hurried out of their lives or deprived of usefulness of limbs, simply for want of sufficient earnestness with regard to the endeavour to obtain complete exclusion of septic agencies from wounds, according to our present lights and our present knowledge. Gentlemen, I thank you most heartily for your cheers; for there was a time when such remarks might have met with a different reception.

A NEW METHOD OF PERFORMING RESECTION OF THE KNEE-JOINT.

By W. MACFIE CAMPBELL, M.D.,
Surgeon to the Liverpool Northern Hospital.

WITH regard to Mr. R. Davy's clinical lecture on this subject, in the JOURNAL of October 20th, it seems to me that the claims to priority in special modes of operation are often made without due consideration.

On November 20th, 1882, I excised the knee-joint of a woman, aged 35, for acute ulceration of the cartilages. The femur was sawn obliquely before and behind, removing the cartilage completely, leaving a wedge-shaped extremity. A corresponding form was cut in the head of the tibia to the depth of about an inch. The advantage obtained was complete antero-posterior immobility. She was discharged cured on February 5th, 1883, sixty-seven days after operation.

This case was shown at a spring meeting of the Liverpool Medical Institution, and the method was familiar to the surgeons present, as appeared from the subsequent debate. To whom is due the original idea, I know not; certainly not to myself, as I had read it shortly before the operation, but where I cannot remember, though I think in some American journal.

I may add that this excision is only possible in cartilaginous disease, or in such a case as Mr. Davy has described, as sufficient healthy bone must be obtainable, and I, for one, would object to remove as much of the femur as he figures.

A CENTRAL site has been purchased in Cambridge by Mr. Reynolds Rowe, upon which he intends to build and endow a large and free open church dedicated to St. John of Jerusalem. The scheme includes an infirmary for the use of members of the University; a training-school and home for nurses ready for engagement in private houses, and an ambulance centre for instruction.

PRESENTATION.—Mr. Edward Walter Witten, M.R.C.S. Eng., upon resigning as Superintendent of the Medical Mission Dispensary, Brighton, has been presented with a gold pencil-case, and a text-book with the names of his co-workers and patients written in, and inscribed, "Presented to Mr. E. W. Witten, as a small token of gratitude and esteem from his co-workers and the people for whom he has laboured. October 18th, 1883."

DONATION.—Since the re-opening of Eaton Hall, the Duke of Westminster has charged all visitors 1s. for the privilege of inspecting the house and gardens. His Grace has recently sent the whole of the proceeds from 11,200 visitors, amounting to the handsome sum of £560, as a donation to the funds of the Chester Infirmary.

AN ADDRESS ON COLLECTIVE INVESTIGATION OF DISEASE.

Delivered at a Meeting of the Lancashire and Cheshire Branch.

By WILLIAM ROBERTS, M.D., F.R.C.P., F.R.S.,
Professor of Clinical Medicine in Owens College, Manchester; Physician to the Manchester Royal Infirmary and Lunatic Hospital.

GENTLEMEN,—Your presence here in such considerable numbers is a sign of the interest which you take in the movement. It is also a sign, I am bound to say, of admiration for our distinguished visitor, Mr. Jonathan Hutchinson.

We are in some sort under a special obligation in this district to take exceptional interest in this movement, because the initial idea sprang up in our midst. To our friend and neighbour Dr. Arthur Ransome is due the first inception of the plan of collective investigation. In 1864 he read a paper at the Cambridge meeting of the British Medical Association "On the Need of Combined Medical Observation," and he there suggested that the machinery of the Association should be utilised for that purpose. Two years ago, the matter was taken up afresh and independently by Dr. Mahomed. Both gentlemen are to be congratulated on the present flourishing prospects of the movement. It is not necessary for me to go over again the arguments in favour of this mode of investigation—this work has been exhaustively done by Professor Humphry, Sir William Gull, Sir James Paget, and others, whose addresses have been published in the BRITISH MEDICAL JOURNAL. Indeed the time is rather gone by for argument; the plan is actually on trial and in operation, and the first fruits are already garnered.

I have before me the first volume of a series of reports intended to be issued by the General Committee, and I may be permitted to compliment the editors, Professor Humphry and Dr. Mahomed, on this new birth of a wholly new kind of medical literature. I can only regret that this little volume is not in the hands of every member of the Association. The object in view is only half accomplished unless these reports as they come out pass into the hands of every member, or at least every contributor, by some automatic process without the trouble of separate ordering and paying. I hope the Committee will be able to see their way to do this. I may say that I was one of those whose confidence in this mode of investigation was not strong, but after a careful examination of the first number of the *Collective Investigation Record* my opinion has been greatly modified. No one can read it attentively without acquiring a feeling almost amounting to a conviction that the movement is one of real importance, and that it will result in a large addition to our knowledge that could not have been got in any other way. I should like to refer for a moment to the admirable report on the communicability of phthisis drawn up by Dr. Burney Yeo. I think no candid person can read this report, and the detailed evidence on which it is based, without coming to this practically important conclusion, that no healthy person should be permitted to occupy the same bed with a sufferer from pulmonary consumption, and that no person with a hereditary predisposition to tuberculous disease should be allowed to have continued and intimate personal contact with a phthisical patient. And I cannot help adding that a grave responsibility would in my opinion be incurred by a medical man knowing this evidence, no matter what his theoretical opinions may be, who would permit such cohabitation and close personal contact.

One of the most promising fields of labour for combined observation is the self-study of disease by medical men. In regard to serious organic fatally tending disorders, this would, indeed, be a task too tragic for contemplation, but in regard to a number of minor ailments this method is admirably adapted. Take, for example, the dyspepsia of healthy persons, or primary dyspepsia. Judging by the number of books written on the subject one would imagine that we knew a great deal about dyspepsia.

I am satisfied that, of exact knowledge, we have very little. The dyspepsia of healthy persons is a very common and peculiarly human disorder; it causes an immense amount of misery, but it kills no one, and the pathologist ignores it. The several types of it have never been scientifically defined, and its case-history remains un-

written. Its signs and symptoms are of a nature to baffle any kind of investigation except self-study. You want to know the precise kinds of dolorous sensations experienced, their exact time-relations to the phases of digestion, the degree of reaction and chemical characteristics of the dejections from the stomach at different periods after the meal, the nature of the gaseous eructations, and the properties of the saliva at various times during the painful gastric struggle. How can you get at all this unless medical dyspeptics set to work on their own cases? If you wish to know what excellent work of this kind may be done by practitioners in the country, I would refer you to a paper on "The Chemistry of Acid Dyspepsia," by Dr. McNaught, of Newchurch, read before the Manchester Medical Society last December, and of which a short abstract is published in the *BRITISH MEDICAL JOURNAL* for December 30th, 1882. There are, I believe, 10,000 members in the British Medical Association, and I venture to say that at least one in ten is an habitual dyspeptic of one type or another. If 500, or even 100 of these, following the example set by Dr. McNaught, and, emulating his zeal and skill, were to furnish the General Committee with the particulars of their own cases, we should know more about this preventable, and I believe wholly unnecessary, torment in a twelvemonth than can be gathered from all the books. Moreover, it stands to reason that an exact knowledge of dyspepsia as it affects healthy persons is an essential pre-requisite for the successful study of those disorders of digestion which are associated with organic changes.

Another question on which collective investigation may be able to throw a valuable light is the use of alcohol. The medical profession has hitherto reposed, like the rest of the civilised world, in the belief that the moderate use of alcohol was beneficial, or at least harmless. But this opinion is now challenged on what is put forth as scientific grounds. Certain insurance-offices have promulgated statistics which are alleged to show that the value of life is greater among total abstainers than among the temperate users of alcohol. If this be so, it is important that we should have the facts established on unimpeachable evidence. The statistics in question appear to me, however, to be insufficient to warrant this interference. Everyone who has had a large experience in examining applicants for life-insurance knows that, in spite of every care, persons are smuggled in as temperate who are taking dangerous quantities of alcohol. Moreover, considerable numbers of those who are strictly temperate when admitted lapse afterwards, in the course of years, into habits of real, though not perhaps of notorious intemperance. So that the comparison instituted by these statistics is really not between abstainers and temperate drinkers, but between abstainers on the one hand and a mixture of the temperate and intemperate on the other. The question as to the limits up to which alcohol may be beneficially—or at least, safely—used, is, for us in this country, with our tendencies to excess, one of transcendent interest. We are greatly in need of more precise information on it, and I know not how we are to obtain this information except by a system of collective observation. Such information would not only be valuable to us in advising our patients as to the use of alcohol, but would also be most useful to the social reformer and legislator, who look to medical science for light and guidance to direct them as to whether they should devote their attention to projects designed to regulate the use and suppress the abuse of alcohol, or to projects which aim at total prohibition. In an inquiry of this sort, the first step is to establish some standard scale of temperance. The words "temperate" and "moderate" are too vague and elastic to form a basis of scientific comparison; and I should like to suggest to the central Committee that they should undertake for us the construction of such a scale. Total abstinence would, of course, form the zero of the scale. But as total abstainers, at least life-long abstainers, are only a small group, there might be joined with them the habitual abstainers—that is to say, persons who habitually are total abstainers, but who occasionally take small quantities of alcohol. There would, we might rest assured, be no appreciable difference between these two groups in regard to their vital statistics, and together they form a very numerous class of the population. For our first grade or degree of temperance, we might choose a limit indicated by the experiments of Parkes, Anstie, and Count Wollowicz. These observers came to the conclusion that a healthy man might consume from an ounce to an ounce and a half of absolute alcohol daily, without appreciable injury. Suppose we were to fix our first degree of temperance at a limit of one ounce of absolute alcohol daily; our second degree of temperance at a limit of two ounces of alcohol daily; our third degree at four ounces daily, and so forth. I only mention these numbers by way of illustration, and not for ultimate

adoption. It would, of course, be necessary to translate these degrees into actual measurements of the beverages in common use. This would be an easy task if we knew the exact alcoholic strength of our spirits, wines, and malt liquors. It is very desirable, for many reasons, to possess this knowledge; and the Excise would render a valuable service to medical men and to the public, and greatly promote the cause of temperance, if they insisted that the alcoholic strength should be plainly indicated on every bottle of spirits and wine, and on every cask and bottle of malt liquor. The requisite information is, I believe, in their hands already, and it would be no great matter to make it public in this useful way. Now suppose we had our standard scale of temperance established, it would then be possible to draw up the medical history of individuals and families, belonging to the several grades on the scale, with some approach to accuracy. I am quite aware that there exists a very considerable range of varying tolerance of alcohol among individuals and families; but there is also a mean or average tolerance, and if the numbers were sufficiently large—say 3,000 or 4,000—we should get at this mean. Again, I am aware that the habit of most people, in regard to alcohol, is too irregular to furnish the materials of reliable statistics; but among the millions of Britain there are scores of thousands whose habits, in regard to alcohol, are of clockwork regularity, and it is among these that the materials must be sought for sound statistical information. Practitioners in the smaller towns and country parts have exceptional facilities for obtaining a trustworthy knowledge of the alcoholic habits of individuals and families; and they could furnish us with a vast mass of invaluable materials for an inquiry of the kind here contemplated. If we possessed statistics of this nature, we should be able to arrive at conclusions of incalculable value. We should know, in the first place, the influence of abstinence and of the usage of varying quantities of alcohol on longevity, both in regard to individuals and their descendants. But longevity is not everything—longevity alone is, indeed, but a poor measure of that which is best in human life. We should like to know what influence, if any, the use of alcohol has on the higher attributes of our nature. It is difficult to believe that the habitual use, even in a most temperate degree, of so subtle and so potent an agent as alcohol, is without influence on the moral and intellectual development of the individual and his descendants, and therefore of the race and nation. Nothing within my experience is more astonishing than the different impressions produced by an examination of the alcohol-question as it regards the individual on the one hand and the race or the nation on the other. Nowhere among civilised communities, I suppose, is there seen so much physical, moral, and intellectual ruin of individuals from alcohol as in this country, and yet the national death-rate is one of the best in the world, and the moral and intellectual vigour of the race is, as we fondly believe, unsurpassed. Again, if we take a wider view, and survey the nations which abstain from the use of alcohol—the vast Mussulman populations and the other millions of Asia—we recognise that they are, upon the whole, far inferior to the alcohol-bred races of the West. The Japanese are, so far as I know, the only Asiatic people who have for centuries consumed alcohol largely; and it is curious to note that they display a mental receptivity and a love of progress which are in marked contrast with the stagnation around them, inasmuch that they have been called the Europeans of the East. These strange contrasts—and they might be multiplied—are not, of course, wholly due to alcohol—far from that; but it might be hazardous to conclude that alcohol was not a contributory factor. And it would not be altogether a satisfactory consummation to find, after banishing alcohol from our midst, that the nation had become somewhat poorer in manliness and intellectual worth. In any case, our duty as scientific inquirers is clear and simple; it is, if we can, to find out the truth, and, having found it, to make it known.

I have now great pleasure in calling upon Mr. Jonathan Hutchinson to address you.

CHOLERA IN PRUSSIA.—From an official return lately published, it appears that there have been nine epidemic outbreaks of cholera in the Prussian states. In 1831-32 the number of deaths was 41,738 (3.21 per 1,000 of the population); in 1837, 13,325 (0.96 per 1,000); in 1848-51, 86,498 (5.34 per 1,000); in 1852-54, 51,582 (3.06 per 1,000); in 1855-56, 30,823 (1.80 per 1,000); in 1857-58, 4,080 (0.24 per 1,000); in 1859-60, 2,166 (0.12 per 1,000); in 1866-67, 120,714 (6.21 per 1,000); in 1873, 28,656 (1.14 per 1,000). The total number of deaths has been 379,582. During the same period there were 241,187 deaths from epidemic small-pox.