

the issue raised by the report of the Royal Commission published in 1918.

Since the circulation of that report the medical school has been completed (in 1921) and is being carried on under a different scheme. Its passage has not been a smooth one, but somewhat stormy, for two reasons: First, the steering has been somewhat faulty, owing perhaps to inexperience, but also to failure to recognize rocks ahead. The University of London and the Conjoint Board in England at first refused to recognize it as a teaching school for the final examinations; after protracted negotiations the school is now recognized provisionally. Secondly, the separationists—that is, those who accept the findings of the Haldane Royal Commission—believe that the school will, if it remains a department of the Cardiff College, not be the success that is desired, having regard to the many medical schools of varying excellence that already exist. They point out its failure up to the present, and maintain that a new Council, Senate, and Principal are essential to give all their energy to the establishment of a medical school of first-class grade. For example, an appeal for £100,000 was made only a short time ago for the medical school, but this was quickly followed by another appeal for a much larger amount for the college, and was started by a generous list. The separationists resented this action of "Poohbahism," and felt even more strongly than ever that the medical school will not become the great centre of learning that is wished for so long as it is not allowed to paddle its own canoe.

The world during the last few years has had examples of the success of efforts to establish medical schools embodying twentieth century ideals. Such schemes have had the lavish command of millions of money spent on organization in its fullest sense—on buildings, equipment, professorial staffs with adequate assistants, and research scholarships and foundations. We may look forward with interest to the outcome of the struggle now going on in connexion with the new medical school in Cardiff.—I am, etc.,

June 23rd.

JOHN LYNN-THOMAS.

#### GAS LEAKS AND CARBON MONOXIDE POISONING.

SIR,—The letter of December 8th, 1923, by "M.A., M.D., D.P.H.," cited by Dr. Laird (June 14th, p. 1073), wholly begs the question. He writes, "Wherever gas is really sensed [on entering a house] a lowered condition of health will be found in the whole household, or at least that part of it—mainly, of course, the feminine element—which spends most of the twenty-four hours at home." This lowered health he attributes, without adducing any scientific proof of his attribution, solely to carbon monoxide poisoning—an example of assertion, common as it may be, the very antithesis to the method of scientific men.

A house so shut up as to smell of some small gas leak or products of gas combustion from gas rings is one very badly ventilated, and the people who inhabit it will suffer from that lowered metabolism which is due to sedentary life in stagnant atmosphere with low cooling power. They will also suffer from breathing a warm moist atmosphere and from deprivation of sunlight. The oxidation processes of the bodies of such people will be reduced to a low level by the lack of exercise and exposure to open air, a reduction which is certain and large, while that due to the inhalation of some unknown small amount of carbon monoxide is unproven. It is of no value for Dr. Laird to cite paragraphs from the writings of Dr. John Haldane and others concerning the ill effects of oxygen want. Nobody doubts these. What he and "M.A., M.D., D.P.H." have to do is to prove that, in houses where gas is smelt and people are out of health, there is a significant amount of carbon monoxide in the blood. The measurement of this by Haldane's carmine method is not so difficult that it should be shirked. Every authority agrees that 1 part of carbon monoxide in 10,000 parts of air is negligible in its physiological effects.

There is no doubt that smokers inhale carbon monoxide, and that it may come from coke, anthracite stoves, fires, and exhausts of motor cars, as well as from gas appliances. Teach people to keep their windows open and get out into the sun and air and clean the skies of smoke by using smokeless fuel, of which gas must be one for the sake of coal

economy, and a very great deal will be done for health. At the same time, the gas companies and public authorities must teach that all gas leaks must be stopped and not the least smell of gas tolerated, so that all danger of gas leaks in shut-up rooms is avoided.—I am, etc.,

London, N.W.3, June 17th.

LEONARD HILL.

#### THE CAUSATION OF CANCER.

SIR,—I have read with interest the letter (June 21st, p. 1113) in which Dr. A. B. Smith and Dr. S. M. Smith relate their experience with the vaccine prepared from the organism from cancer isolated by the procedure described by me. In about 40 cases of cancer so treated during a period of two years by Dr. Robert Robertson and myself, and by other medical men with special experience in vaccine therapy, to whom the vaccine was sent for test, the results were very similar. These may be summarized as follows: (1) A focal reaction (swelling of the mass, pain and tenderness, increase in discharge, and, in cancer of the breast with axillary involvement, increased swelling of the arm) was almost invariable. (2) In some, temporary local and general benefit was found; in others, the vaccine seemed to exaggerate the activity of the growth and to hasten the end. (3) In no case was there lasting benefit or anything approaching a cure. The cases were all such as had progressed beyond the stage of complete surgical removal and many were far advanced. The definite focal response suggests a specific relation between the organism and cancer.

A more convincing demonstration of this relation any of your readers can obtain for himself. Let him take some freshly and aseptically removed cancerous tissue, and for a beginning I would recommend a rapidly growing transplantable mouse tumour, such as the Imperial Cancer Research Fund strain 63, which experience has shown me to be highly infected. In each of four specimens of this examined during the past three years I have found the following appearances to be clearly represented. The same phenomena are found, however, with varying clearness in human cancers of all kinds. Place, say, eight small fragments of the growth in as many tubes containing any ordinary culture medium. Horse serum (3 parts to 1 of distilled water) is specially suitable because of the uniformity of the film obtained with it. Incubate at 37° C., remove drops from the vicinity of the tumour at intervals of twenty-four hours, forty-eight hours, etc., and make films, which are dried at room temperature and stained with any ordinary stain, Leishman being one of the best.

At the end of twenty-four hours the observer will find unstained globules, which often possess a glistening sheen, and vary in size from specks just visible at  $\times 1000$  magnification up to comparatively large masses. Let him search a little and he will find, lying in a group of globules, some of the same unstained material drawn out into beautiful rods, and, after a more diligent search, his interest will be quickened by discovering the same dead-white matter creeping out as longer and thicker filaments which branch and then, it may be, abruptly end in a bulbous globule of the same material. In this phase the strange living thing often begins to take on the stain deeply and to exhibit an internal structure. From an early stage he will find evidence that this uncouth order of living matter is springing directly from the cancer cells, and this belief will be dramatically confirmed if, as he moves the slide, there suddenly sweeps into the field a host of the bright rods or threads radiating into the surrounding medium from one and the same cell. By this time he has recognized that he is dealing with some kind of fungus which, after having lived in an invisible form the submerged life of a parasite, is now struggling to organize itself afresh in visible shape in the more liberal conditions of the test tube. He will find, moreover, in every early culture that some of the globules exhibit minute staining points, which become separated in the medium and can be traced through all stages as they enlarge into cocco-bacillary elements of familiar shape and staining characters.

At this point he will come up against problems of an

extremely elusive nature, problems in which his bacteriologist friends can give him little help, and he will be wise to follow the trail of others who before him have struggled through the jungle. In case he suspect prejudice I will refrain from referring him to my own papers, but will point him to other recent workers on general bacterial morphology, who are free from the compromising entanglements of cancer. Burning with the conviction of one who has seen the light he will find it difficult to tolerate the glib criticism that he is wasting his time on contaminations. He will be not a little surprised to find that these critics apparently do not realize the extreme improbability of the same class of "contaminations" being wafted in the same sequence over many years into the test tubes of Hort of London, Young of Edinburgh, Löhmis and Smith of Washington, Mellon of the Mayo Clinic, Wade Manalang, Reed, and Orr, also of America, Almqvist and Bergstrand of Sweden, and de Negri of Holland! Confident in the assurance of truth, he will find himself ranged in sympathy alongside a new school of scientists, who smile as they think of the malign imp which for wellnigh a generation must have sat on the rim of the test tube chuckling as the bacteriologist wrestled with the spectre of contamination.—I am, etc.,

Edinburgh, June 21st.

JAMES YOUNG.

SIR,—Dr. Archibald Leitch concludes his answer to Dr. Brand (June 14th, p. 1071) as follows:

"It follows that, as we have experimentally produced epithelioma, adenocarcinoma, and sarcoma frequently [the italics are mine] by special irritants (and on the contrary have failed with other irritants), the agents successfully employed cannot be dismissed as predisponents, but are to be regarded, even on Dr. Brand's own showing, as primary causes." Dr. Leitch, in using the word "frequently," admits that his special irritants do not *always* cause cancer. Consequently, on his own statement, we must assume that there is some other condition or specific agent necessary to enable his special irritants to cause the disease "frequently," else why not always?—I am, etc.,

June 15th.

G. W. F. PAUL.

SIR,—Dr. Brand is really incorrigible. He stated, as an essential part of his argument, that cancers were invariably identical in their nature, onset, course, and termination. We gave him some half-dozen examples of tumours by which he could test the strength of his hypothesis, but he sees no reason to modify his contention after careful reflection. (1) "They are all malignant tumours." Some are always so, some are usually so, and some are rarely or never so. (2) "They are identical in onset, beginning in every case at one definite point." Some of them have a very indefinite point of origin, some of them have not infrequently two definite points of origin. (3) "The termination in each case is fatal." This is not a fact. (4) "The course in each is the same." On the contrary, nothing could be more variable. (5) "The essential quality of the disease is the same in all." The only thing they have in common is that they are neoplasms. These points are not matters of opinion: they are matters of knowledge; and Dr. Brand, by his answer, unfortunately conveys the impression that his knowledge of the subject is not so profound as I wish to believe it is.

His favourite and frequently repeated dictum is: "Since cancer is a specific disease, it must have a specific cause." Actually, this means no more than if he were to say, "A beetle has four legs, therefore it must be a quadruped." When he is told that some students of cancer problems (being as free from prejudice as he desires us to be) doubt this specificity he is surprised that they should deny "so obvious a fact," and he is sure they can only be a small coterie. I am not aware that any vote has been taken to show how miserably small this "coterie" may be, nor can I recall any important scientific question that was settled by the counting of heads.

Dr. Brand is firmly convinced that, although we may have succeeded in obtaining cancers experimentally in animals after the prolonged use of certain irritants, the results are primarily dependent on some "x agent of

cancer" which, unknown to us, did the trick for us. I can think of no conclusive argument nor any crucial experiment that would dispose of this idea. It can be maintained with equal force against the microbic theory of infectious diseases: it may be that it is not the tubercle bacillus but some little unseen virus which it generally carries on its back that is the cause of tuberculosis. It may be that disease in general, as thousands of presumably sane people believe, is a product of the imagination. In Dr. Brand's words, these things "have never been disproved because they cannot be." But it will be enough for us, in an endeavour to show that the agents with which we can induce cancer are the prime causes of these cancers, if we bring forward in our communications an increasing number of observed facts and leave the ultimate judgement to common sense—that peculiar attribute which everybody is sure he possesses in greater abundance than his neighbours. And Dr. Brand can help us if he will come off his pontifical chair, make himself acquainted with the evidence, and criticize our conclusions with a mind free from dogma.—I am, etc.,

ARCHIBALD LEITCH.

The Cancer Hospital Research Institute,  
London, S.W.3, June 21st.

#### ACUTE PUERPERAL COMPLETE INVERSION OF THE UTERUS.

SIR,—While I was aware of the rarity of this condition from the statements in textbooks, it was only on reading the article in the *JOURNAL* of May 31st (p. 955) that I realized how few cases have been published, and in consequence think it may be worth while to describe one which I met with about two years ago.

About two hours before her admission to the Royal Victoria Hospital, Dover, the patient had been delivered by a midwife. On admission the uterus was completely inverted outside the vulva with shreds of placenta adherent. The patient was unconscious and in a condition of profound shock, almost pulseless. Intravenous saline was given, and at once without the least difficulty I succeeded in reinverting the uterus, the cervix being quite large. Nevertheless, in spite of all measures the patient never recovered from the condition of shock and died in two hours. The account published makes me wish that I had waited, but it is difficult to believe that in this case the trifling effort to replace the uterus, no anaesthetic being needed, added much to the shock.

It is difficult to account for the severity of the shock in these two cases. I operated on a man during the war who had all his intestines prolapsed through a wound, and who recovered, after an operation with anaesthetic. It is hard to see why total inversion of the uterus should produce a shock so profound, and it would be interesting to know whether it generally occurs.—I am, etc.,

June 5th.

A. R. JORDAN, M.D., F.R.C.S.,  
Surgeon to the Royal Victoria Hospital, Dover.

#### TREATMENT OF UTERINE FIBROIDS.

SIR,—In Mr. Cuthbert Lockyer's "Remarks on the treatment of fibroids of the uterus," published in the *JOURNAL* of June 14th (p. 1037), there is this sentence:

"When we read in the literature of 1923 that '90 per cent. of all fibroids may be dispersed by x rays or radium without serious risk or discomfort' (a quotation of Nigel Stark's), and when Bécélère states that his experience of 700 cases teaches him that 'the only conclusion permissible is that the sole obstacle to successful treatment is the submucous myoma,' we are completely staggered by such exaggerated claims."

The quotation was made by me in a paper on the surgical treatment of fibroids read at a meeting of the Royal Medico-Chirurgical Society of Glasgow and published in the *Glasgow Medical Journal* (March, 1923). At the same meeting there was read, and in the same journal there was published, a companion paper by Dr. James R. Riddell on x rays in the treatment of uterine fibroids. He and I had been associated in the treatment of several patients suffering from fibroids, we had frequently talked over the merits and demerits of the modes of treatment, and our papers were the outcome of those discussions.

I give now the quotation and its context in order to make