alcohol has been reached. It is well known that the alcohol concentration in urine exceeds that in blood by about one third, and that urine values reflect fairly closely the venous blood values which were present about 30 minutes before the urine collection.[10 11] Under experimental conditions in which alcohol is taken as a single drink, and in which the time of the peak concentration in the blood is known—or can be foretold—useful deductions can be made from the analysis of a single sample of urine. But in ordinary life the subject may still be absorbing alcohol, or he may not have emptied his bladder for a long time, and in these circumstances accurate deductions of blood alcohol levels cannot be made. It is for this reason that the B.M.A.'s Alcohol and Road Accidents Committee[12] recommended "that two specimens of urine should be taken, the second about 30 minutes after the first, and the results of both analyses given to the Court." This recommendation is adopted in the White Paper,[1] and at a police station a suspect who objects to the taking of blood will be given the alternative of providing urine samples. The observations of Payne and his colleagues confirm the relative inaccuracy of this method of determining tissue alcohol concentration, especially if a second sample shows a higher concentration than the first. Their results emphasize how essential it is not to rely on a single urine sample.

The main point of conflict between the conclusions of Payne and his colleagues and those of other investigators concerns breath alcohol analysis. These authors conclude that "the wide variations between breath and blood analyses virtually exclude the use of breath testing for the accurate determination of blood alcohol concentration." This sweeping statement runs counter to almost all the evidence adduced by other workers, and it is necessary to look very carefully at the techniques used for analysis. The infra-red analyser which they used may be precise if all the physico-chemical conditions are properly regulated, but such a method is known to be fraught with technical difficulties. It has not yet been tested by other observers, and the *in vitro* calibration of the instrument was apparently accurate to within only ±10% when vapour in equilibrium with blood was used. It is possible that this poor correlation is due to an inherent lack of reliability in the new method of breath analysis. This could have been checked if the authors had compared their results with those given by a well-established instrument such as the Breathalyzer or Kitagawa-Wright apparatus. On theoretical grounds breath is to be preferred to venous blood for analysis, since it is in equilibrium with arterial blood. It is true that the Breathalyzer, for example, tends to underestimate the blood alcohol level, but the underestimate is not gross.[13 14] In one well-designed study,[13] in which observations were confined to the post-absorptive phase, when breath and venous blood values *should* correspond, the correlation between these values was in fact impressively close, and the results of breath analysis were notably consistent. Other workers in this country[10 16] have also found a good correlation between blood and breath alcohol levels. Moreover, in the U.S.A. the view of the National Safety Council,[15] which is generally accepted, is that: "Tests made on instruments for breath analysis if conducted in the manner prescribed by the authors of the respective methods will give comparable and reliable results for estimating the concentration of alcohol in the blood."

Payne and his colleagues describe one other finding which is at variance with those of other workers, and which has some practical importance. In two instances they found unexpectedly high alcohol levels in the breath 40 and 45 minutes after a drink had been taken. It is well recognized that a "mouth-alcohol" effect may persist for some time after drinking, but none of the numerous investigators who have reported their results has found this to be longer than 20 minutes.[13 17-21] Difficulties can arise with breath testing apparatus if tests are made too early, or if the subject vomits, regurgitates, or belches. These possibilities must be taken into account in any screening test. In general, however, as Payne and his colleagues point out, the tendency of breath tests to underestimate blood alcohol is advantageous to the suspect, and is a point in their favour when they are used as an aid to law enforcement.

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Medical Microbiology as a Career

In his recent Foundation Lecture to the College of Pathologists, published at page 189 of the *B.M.J.* this week, Dr. J. W. Howie, Director of the Public Health Laboratory Service, poses four questions, all concerned with the training, status, and functions of those doing diagnostic bacteriology. The lecture begins with the interesting statement, which will be news to many people, that "in most parts of the world," other than Britain, "progressively fewer young doctors are making careers for themselves in medical microbiology." In some countries, including the United States, much of this work has for long been done by science graduates.

A general falling off in recruitment from the ranks of medicine in other countries than ours could have important consequences if we accept the lecturer's own answer to his first question. This is simply whether a training in medicine and general pathology is necessary to the proper practice of medical bacteriology. Dr. Howie supports an affirmative answer with several telling anecdotes illustrating the value to the bacteriologist of a clinical training. Perhaps one example he gives is rather extreme—namely, of a bacteriologist who was the first person connected with a patient to recognize rather obvious signs of appendicitis—but other

instances will occur to readers of long experience. And no one will question the greater value of the services which a bacteriologist can render when he understands their bearing on the diagnosis of a difficult case. He must be prepared if necessary to see the patient, and he may be asked to perform whatever investigations he thinks may be helpful. Short of this, he should be at liberty to examine a specimen in whatever way he thinks advisable in the light of the clinical information he has, or even to question the necessity of a laborious investigation he has been asked to do, as in another instance cited by Howie.

This kind of relationship between ward and laboratory works very well when the bacteriologist has the clinical knowledge which, combined with his own skills, places him on an equal footing with his clinical colleagues. The alternative is that the laboratory should be the servant of the clinician, simply doing what is demanded of it, no more and no less. This relationship of master and servant is common on the Continent, and clinicians who insist on it cannot expect more than the mechanical and unimaginative service which they consequently get. This attitude could indeed be responsible for unwillingness among medical graduates to enter on a laboratory career. This is not to say that all investigations require the same degree of skill and experience. Some, such as examination for tubercle bacilli, can with advantage be mechanized. Moreover, the science graduate or senior technician, in a laboratory accustomed to thorough work, will often do more with a specimen than is actually requested. When asked to look for dysentery bacilli he may find *Giardia* cysts, or in pus supposed to be pyogenic or tuberculous he may detect actinomycotic granules. The facts may be ascertained, and may, as in these examples, be conclusive. In others they are not, and to assess the significance of detecting a given organism in a given situation from a patient with certain signs may call for knowledge based on medical training as well as laboratory experience.

The second question, whether microbiology provides a worth-while career to a medical man, will receive only one very emphatic answer from those who should know best. This career combines the satisfaction of an exact science with the wider and human interests of a vital share in clinical medicine. It has always provided opportunities for contributing even to the saving of life by the rapid diagnosis of acute infectious disease. During the past 25 years it has provided far more than that—what amounts practically to the duty of directing treatment with antibiotics and other antibacterial drugs. Only the laboratory can provide the information on which this can rationally be based, but apart from this the bacteriologist should know more about the properties and uses of these drugs than his clinical colleague, whose knowledge has to be spread over the enormously wider field of pharmacology generally and clinical medicine in all its aspects. The functions of the bacteriologist also include prevention, whether by immunization or by hygienic measures. He must know where pathogenic bacteria come from and how their migrations can be checked. He has transformed the study of epidemiology by introducing exquisitely exact methods of bacterial identification, and been almost wholly responsible for the recent revolution in methods of disinfection and sterilization. A career offering this variety of interests needs no commendation.

The two further questions are concerned with training. When and how bacteriology should be taught to the undergraduate is a controversial question, but everyone will agree with the suggestion that students showing special aptitude and interest should be encouraged by their chiefs to consider a laboratory career. If indeed ours is one of only a few countries in which such recruitment is still adequate, we have a special responsibility for ensuring that medically qualified bacteriologists shall continue to play the essential part which they have in the past. It might be thought from what has been said of their achievements, and because of the diminishing incidence and lesser severity of many infectious diseases, that in some directions there is less to be done than formerly. This would be an error. Difficult problems in diagnosis still remain, and in place of specific diseases, some of which are in process of being overcome, we are faced with a growing amount of non-specific and often more dangerous infection in patients whose resistance has been impaired by such innovations as corticosteroid, antimitotic, and immunosuppressive drugs, and by increasingly ambitious surgery. These problems, and that posed by acquired bacterial resistance to antibiotics, provide as great a challenge as any that has had to be met in the past.

Vaccine Therapy in Asthma

The use of bacterial vaccines is now restricted mainly to the prevention of certain infectious diseases when it is possible to produce good immunity with a small number of doses. A rather different problem is the control of illness in which the patient may have become allergic to part of his normal bacterial flora. The problem here is to desensitize rather than immunize, if these two processes may be regarded as different.

"Infectious asthma" is a disease in which infection of the upper respiratory tract has a dominant role in the cause of the patient's illness. What proportion these cases form of all cases of asthma is debatable, but most clinicians believe it to be small. In this group vaccines might play a useful part in treatment. A practical problem here is whether autogenous or stock vaccine should be used for desensitization. While it would seem that the isolation of the organism provoking disease and its incorporation in a vaccine ought to yield better results than stock, in fact this procedure is not particularly common. This may be due to the lack of close liaison between allergist and bacteriologist.

To study this problem in practice requires the design of clinical trials in which groups of patients selected by a random process are given either vaccine or placebo. It is easy to arrange that neither the patient nor the doctor knows the nature of the injected material. Provided the groups are followed up for long enough (usually over a year) and there is a satisfactory scoring system for the clinical condition, statistical analysis of the results is possible. A number of such trials have been reported, and there seems little doubt that vaccine is in no way better than placebo. Nevertheless, every trial is apt to be followed by criticism of its design, together with a new trial to overcome the defects of the first.

A recent paper by V. J. Fontana and colleagues[1] points out that despite the many previous trials—and they list 17 dating back to 1909—"the indications for the use of bacterial hyposensitization are still open to controversy." They decided to carry out a new trial, treating 30 children suffering from infectious asthma. The first year was devoted to recording

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