especial risk from the use of cardiazol. Such evidence in any particular case is provided by the presence of failure either of congestive or anginal type, or, in the absence of these severe signs, by diminished exercise tolerance. In certain cases also typical cardiographic changes may be observed. Now in only one of Dr. Good's cases was, apparently, electrocardiography carried out, while in only one (Case 3) is there any hint of the exercise tolerance. Still more important, this latter omission makes it impossible for him to tell us whether the exercise tolerance improved or deteriorated after the exhibition of cardiazol; and surely this should be the focal point of the whole investigation.

Dr. Good admits that he had the temerity to use cardiazol in Case 3, which showed frank failure of anginal type, while he was "restrained in only one case—a heart case in which compensation was poorly established." In a scientific investigation of this type the ill-founded conception of "compensation" would be better replaced by a simple scientific statement of exercise tolerance and the presence or absence of failure. Case 3 is actually of peculiar interest because the exercise tolerance appears to have greatly increased as a result of cardiazol therapy, and recently Bourne and Wittkower showed that "in true angina of effort, relief of underlying anxiety by psychological treatment results in improvement or loss of cardiac pain." It would be of interest to know the nature of the mental symptoms in this case.

Such important observations as those recorded deserve the benefit of precise analysis, and it cannot seriously be maintained that "weak heart sounds" or even thickened radial arteries are, by themselves, satisfactory evidence of myocardial defect. Dr. Good comes perilously near to assessing his cardiac cases, as Lewis has said, "on a count of murmurs."—I am, etc.,

G. H. H. BENHAM.

County Mental Hospital, Prestwich, Nov. 15.

Aetiology of Influenza

SIR,—I have been much interested in reading Prof. Major Greenwood's epidemiological reflections on the air war (November 16, p. 677). It would be of real value if someone qualified to do so, perhaps the author himself, could summarize clearly the evidence that influenza is an infectious disease transmitted by droplet infection or by direct contact. I am not attempting to deny that this is so, but so far as I can find the evidence is mostly indirect and would permit of alternative explanations, whereas the facts of its distribution are very difficult to explain in such a manner. The recovery of virus and the transmission of a disease to ferrets, and from ferrets to mice, was interesting but indirect and by no means conclusive evidence. The virus is irrecoverable from many epidemics of influenza and inconsistent in type, and it is quite uncertain that the disease in the ferret represents the human disease.

There is the evidence of the popular opinion that influenza is a contagious disease, but while this is admissible as a link in a chain of evidence it has often proved very fallacious, since this has been the opinion in regard to such diseases as ergotism, which have not subsequently proved to be infectious diseases. The direct infectiousness of influenza is so generally assumed that it would be salutary to have a review of the evidence before too many schemes are devised to prevent the spreading of this disease, and too many conclusions are drawn about its nature.

An alternative possibility which suggests itself is that the causal agent of influenza lives symbiotically in many human beings like a primrose in a hedge, and that certain seasons favour its development to such an extent that it becomes a nuisance and a parasite, and that still more favourable seasons allow it to "flower" still more abundantly, just as certain wild flowers have "years." It will be seen that the difference of concept is considerable. Although influenza should be considered as infectious in that the agent may spread from one human being to another, the epidemic is explained not so much by this factor as by the sudden lighting up to parasitic activity of a quiet commensal already living in the body. The extraordinary explosiveness of influenza pandemics would thus be no more surprising than the speed with which the woods become carpeted with bluebell flowers. All this is very hypothetical, and there are other possible hypotheses. The only excuse for mentioning this one is to suggest that the present conception of the transmissibility of influenza is also a hypothesis not yet adequately based on sufficient evidence.

(An example of the type of disease which I have been attempting to describe is given by certain types of helminth infestation in animals. In these a mild degree of infestation is so constant as to be considered normal, and the host and the invader are so mutually adapted that normally no symptoms result, but occasionally outside circumstances upset the balance and allow of much severer infestation with the worm, whereupon the animal becomes ill and may even die. This state of affairs probably also exists in the human in the Tropics in certain types of hookworm infestation.)—I am, etc.,

Beaminster, Dorset, Nov. 19. R. E. HOPE SIMPSON.

Cut Fingers in Factory Workers

SIR,—In reply to my friends Messrs. W. R. D. Mitchell and H. Blacow Yates (November 16, p. 685), there is nothing novel in the method of flexor tendon repair. Two No. 60 linen thread sutures carried on a No. 18 half-circle needle are inserted about a quarter of an inch from the ends. Two more are used for fine adjustment as near the end as is compatible with an efficient hold. They are tied second. The wound in the skin, enlarged as required by vertical incision avoiding the midline, is closed by end-on mattress sutures. A dorsal plaster slab applied to the forearm and flexed fingers remains for three weeks.

I am grateful for their endorsement of my concern over the increasing incidence of cut flexor tendons and the risk of permanent loss of use; but they question if even prompt suture is as successful as it is easy. I admit their stricture is valid, and that even early suture is not always the final triumph that my words implied. This should engage the earnest attention of orthopaedic leaders. Meantime our immediate duty is to ensure that the lesion does not go totally neglected for days and weeks, as so often happens now.—I am, etc.,

Bristol, Nov. 20. A. WILFRID ADAMS.

Application of a Thomas Splint

SIR,—The following detail in the application of a Thomas splint may be found useful to the many these days giving instruction to first-aid units. Number 2 assistant in applying extension to the leg grasps the heel by the inner hand—that is, left hand for a left leg—and the front of the ankle by the outer hand. Number 1 assistant slips on the ring along the foot, Number 2 removing the upper (outer) hand. As the limbs of the splint are swung to the horizontal to pass the ring round the heel, Number 2 grasps the heel by the outer hand on the near side of the ring and removes the inner hand. As soon as the ring passes up the leg the inner hand resumes its grasp, but now in the upper position over the ankle. The change over of hands in this way allows the manipulation to be carried out in the smoothest manner and without fumbling. The replacement of the heel grip maintains the extension without variation in tension, so liable to occur when the heel grip is relaxed and the upper grip (often too far forward on the foot) takes all the strain.

The details were worked out by the members of the mobile unit.—I am, etc.,

Nov. 15. G. L. DUNCAN.

Mass Radiography

SIR,—The question of miniature chest radiography is most interesting. As a radiologist I have the feeling that we may be tackling this problem from the wrong angle. Miniature photography of the fluorescent screen image cannot to-day produce results at all technically comparable with radiography using the full-size film. Shortage of raw materials or lack of labour can be the only rational reasons for adopting inferior techniques, unless, possibly, one is influenced by certain convenience of developing and storing the tiny negative. Why not investigate the present high cost of *x*-ray film? Surely ample supplies of cheap film would entirely wipe out any of these roundabout methods of achieving less perfect results. There are many conditions in the chest for which we should search as well as for tuberculosis, and the majority of these

require radiographs of high technical quality. Frankly, unless it is a question of shortage of raw materials I entirely fail to see why we as a profession should tolerate anything but the very best for our patients. They certainly would not allow it if we used our positions of influence to enable public opinion to control governing policy.

The next step after mass chest radiography is likely to be bulk radiographic investigation of the gastro-intestinal tract. I believe that schemes could be perfected whereby very large numbers of patients could be investigated with full co-operation between radiologist and other members of the professional team. Mass radiography of the intestinal tract would seem almost certainly to call for highly protected screening units, and the expenditure of some ten to twelve small films and one or two larger films per patient. The basic cost of such investigations would be infinitesimal in comparison with the savings effected in industry and war production even with films at their present price, but I feel that such price is artificially high.—I am, etc.,

Belfast, Nov. 15. DOUGLAS BOYD.

Euthanasia

SIR,—The conclusion arrived at by many well-known members of our profession is that the act of dying is usually not in any way so terrible to the dying individual himself as it seems to his friends present at the death-bed. In my *Aspects of Death* (London, fourth edition, 1922, p. 514) I referred to the prevailing medical evidence in support of this conclusion, quoting the opinions of such eminent men as Sir Henry Halford, Sir Benjamin Brodie, Sir William Gull, Sir W. S. Savory, Dr. William Munk, Sir William Osler, Sir J. F. Goodhart, Prof. H. Nothnagel, Prof. C. A. Ewald, and I would now add Sir Frederick Treves, Prof. A. E. Hoche, Prof. G. Perthes, and Prof. J. A. Ryle (1940). Dr. T. Bodley Scott (1914) went so far as to write: "The so-called agony of death is, in my experience, a chimera."

Nevertheless, although the act of dying is usually not so terrible to the dying individual as is still commonly supposed, there are occasional cases of extremely painful chronic incurable disease in which the patient longs for artificial release from almost intolerable suffering. For such exceptional cases I would join myself to those who wish to see the law altered, so that, with properly considered limitations, at the patient's repeated earnest request, if more than one experienced doctor certified the case as incurable and the pain and distress as almost unbearable, painless death might be administered. I regard Nature as one of the manifestations of God, and believe that we human beings have been gradually permitted to acquire more and more control of Nature—of which we ourselves constitute a part. We have been enabled to avoid much suffering and death by aseptic surgery, anaesthetics, anodynes, chemotherapeutic means, etc. Why should we not in exceptional cases of chronic painful incurable disease relieve the patient of his life at his repeated earnest request? One often hears Nature blamed for cruelty, which we ourselves—a part of Nature—might nowadays, and possibly are intended to, prevent.—I am, etc.,

London, W.1, Nov. 18. F. PARKES WEBER.

Pathogenesis of Non-pulmonary Tuberculosis

SIR,—A reading of the article on the pathogenesis of non-pulmonary tuberculosis by Dr. M. C. Wilkinson (November 16, p. 660) shows that the author has considered a large and valuable material, but the discussion and conclusions seem subject to criticism.

Most workers would agree that in the presence of haematogenous tuberculosis involvement of glands may not always be obvious, though a primary complex with a glandular component seems to be a necessary precursor of such lesions. Further, it is well known that an active primary complex may give rise to rapid dissemination—for example, such a complex is often found in patients dying of tuberculous meningitis, in whom also other non-pulmonary lesions may be present.

Infection of a group of lymphatic glands is evidence that there is some effort to resist the wider spread of disease. If this were not so all those who develop the primary complex would be likely soon to show manifest pulmonary or non-pulmonary tuberculosis. Nevertheless, a quite definite proportion of those who have shown a glandular involvement in the primary complex do later develop other tuberculous lesions.

In claiming that "patients suffering from gross tuberculous adenitis do not under favourable conditions develop tuberculosis in other parts of the body," Dr. Wilkinson revives an old heresy, promulgated by Marfan in 1886 but repeatedly refuted by the later improvement of diagnosis by radiography. One of us (B. C. T.) published recently an exhaustive study of 324 cases of tuberculous adenitis (*Tubercle*, April and May, 1940, pp. 217, 260) to which Dr. Wilkinson makes no reference. Apart from numerous cases of skeletal, skin, and organ tuberculosis, pulmonary tuberculosis of adult type developed in fifty-four patients with fourteen deaths, and further cases with an increased mortality are expected as time goes on.

Dr. Wilkinson surveyed 110 patients with tuberculous cervical adenitis and forty with tuberculosis of abdominal glands. Reports were received of eighty-one "at periods varying from a few months to seven years after discharge." Since no fresh lesions had developed elsewhere, it was concluded that patients treated for glandular tuberculosis do not develop fresh tuberculous lesions. This conclusion is not justified from a follow-up of only 51% of the patients and for the periods stated (how many were followed for the full seven years?). Even seven years is a short period in tuberculosis. Any follow-up of children is statistically valueless unless it is carried through to adult life, since phthisis rarely occurs before adolescence.

In view of the importance of continued and thorough observation of patients with gross glandular tuberculosis, we feel that it is desirable to correct any false impression arising from Dr. Wilkinson's conclusions.—We are, etc.,

F. A. H. SIMMONDS.
BRIAN C. THOMPSON.

County Sanatorium, Barnet, Nov. 21.

Universities and Colleges

UNIVERSITY OF LONDON

The following candidates have been approved at the examinations indicated:

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