General. From my own experience as a junior hospital medical officer, I would say that little thought is given to the death certificate as a legal testimony, and none at all to its providing scientific data. It is signed solely in the interests of expediency to the doctor or to the relatives of the dead person.

I once attended a hospital post-mortem examination of one of my patients, an emaciated, self-neglected, elderly woman who had died some days after admission to hospital. Finding no specific cause of death—indeed he remarked on the unusually healthy appearance of the organs, particularly the lungs, in a person of her age—the pathologist concluded the woman must have died from malnutrition. Malnutrition is not an “acceptable” cause of death in London, any more than it is in the Middle East. It was at once proposed, in order to avoid “every-one’s wasting time with the Coroner,” to record a death from bronchopneumonia. So the interesting fact of a death from malnutrition, or perhaps, better still, “cause unknown,” was lost, and the nation’s Health Service continues to be planned on the resulting data. This was a case where an inaccuracy was recorded consciously. Unconscious or unheeded misrepresentations probably occur in a large number of certificates signed.—I am, etc.,

Brighton, Sussex.

MARY R. BLISS.

Mendel To-day

SIR,—The centenary of Mendel’s paper seems a good time to comment on Mendelism and human heredity. Professor Arnold Sorsby’s article (6 February, p. 333) raises, in my mind, the question of how far Mendelism is applicable to man.

I agree that the general findings of Mendelism as elaborated by animal geneticists since 1900 apply to man as to other animal species. There are, almost certainly, on the human chromosome complement, pairs of material entities which segregate and recombine like Mendelian factors. Phenomena like dominance, recessiveness, epistasis, modifying genes, polygenic inheritance, linkage, mutation, etc., occur in man. However, these terms are unsatisfactory for describing variations in human characters because, in a wild population like the human species, one cannot make all the necessary experimental crosses, and it is therefore often impossible to say which category of genetic phenomena one is dealing with. More important still, all these phenomena can be masked or mimicked by environmental effects. Neither modern statistical methods nor chance observations of rare instances of breeding provide the evidence needed to fit any but the simpler cases of human heredity to a Mendelian model.

Mendelism appears to be a useful theory for those who can control the breeding of the organisms they study, agriculturalists and the stockbreeder, and those who are professionally engaged in making genetical experiments. Those of us who are interested in wild free-breeding populations must concentrate our attention on environmental variations. For us such phrases as “Genes control development” have no meaning. Where breeding and therefore genes cannot be controlled we must produce what changes we seek through

the environment. For us the environment controls development.

Recent spectacular discoveries in human heredity have been concerned with bringing about changes in environments in what had previously been considered genetically determined disease. Infants who would at one time have become phenylketonuric idiots now grow into intelligent people when phenylalanine is removed from or reduced in their diets. Others, who would have failed to thrive because of an inherited sensitivity to galactose, become normal persons when deprived of galactose. They may still have their inherited inability to use what for most people are wholesome foods, but in their new environments (phenylalanine-free and galactose-free) they are no more abnormal than I, who have inherited inability to taste phenylthiourea.

Many will argue that we need a breeding theory like Mendelism for man; the Eugenists do, but, unless we are ready to delegate to the State decisions about who will have children, and how many, Mendelism will remain unapplied to man. Dunn¹ sets out some of the dangers very clearly. He reminds us that the Nazi race laws of 1933 were drawn up by a committee which included, along with Heinrich Himmler, three German geneticists—Ploetz, Rüdén, and Lenz—and that these laws received much support from other German scientists.

I should like to propose that we make a distinction between human genetics and human heredity. The term “Human Genetics” should be reserved for the study of the part played by nuclear and extra-nuclear cellular structures in human heredity. “Human Heredity” should be the term used for the study of the distribution of characters in families, whether the character has a clear-cut Mendelian type of distribution like polydactyly and albinism or has not, like diabetes mellitus, intelligence, stature, religious persuasion, wealthiness, and nobility. We confuse ourselves and those we seek to teach by overmuch argument about what are at present insoluble problems such as, “Is diabetes (or schizophrenia) due to a recessive gene or to a dominant with incomplete penetrance?”

We would do better research and give better advice to people with problems concerning inheritance if we stopped trying to force our material into Mendelian models, and instead applied ourselves to producing accurate empirical hereditary prognoses.—I am, etc.,

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REFERENCE

- ¹ Dunn, L. C., *Eugen. Rev.*, 1962, 54, 69.

Intussusception in Africa

SIR,—In the *B.M.J.* for 9 January (p. 106) I read with interest the comments by Mr. N. H. Hira on caecocolic intussusception following his report of finding an ileo-ileal one in an inguinal hernia. In the University Hospital, Ibadan, in 1963 I reviewed 119 cases of intussusception seen there in the previous five years, and also 100 cases of caecocolic intussusception seen in a slightly longer

time; the findings of these surveys modify two of the points made by Hira as follows. Of the 119 cases of intussusception 76 were caecocolic, and at least 16—at least because a further 16 were not accurately identified in the records available—were ileocaecal. I feel that this might be a more realistic proportion than the very small one quoted by Elebute and Adesola.¹

The second series of 100 cases of that very abstruse disease of caecocolic intussusception showed that this illness took place whether there was a caecal mesentery or not; progress thereof was certainly facilitated by the presence of this structure, but in the large number of patients with a fixed caecum the intussusception was halted around or before the hepatic flexure. Details of this will be soon published, but it is enough to say that after pondering the findings in these hundred patients caecal mobility was dismissed as a factor in the initiation of caecocolic intussusception. And, though I have not the exact figures available here, I remember that in several hundred abdominal examinations carried out at the same hospital over several years during surgery for a variety of conditions other than intussusception no significant difference was noted in the incidence of mobile caecum than that in the United Kingdom.

Caecocolic intussusception poses a fascinating aetiological mystery; there is some evidence that it occurs in sharply defined geographical areas in Africa, and certainly none of the theories put up to date explains anything like the whole odd pattern of the disease.—I am, etc.,

Ekwendini,
Malawi, Central Africa.

GRAHAM J. COLE.

REFERENCE

- ¹ Elebute, E. A., and Adesola, A. O., *Brit. J. Surg.*, in press.

Diagnosis of Amphetamine Addiction

SIR,—In his letter of February 6 (p. 384) Dr. P. G. M. G. Walker rightly draws attention to the fact that we must now refer to drug dependence instead of drug addiction or habituation. The W.H.O. Committee¹ suggested that the component which is common to all forms of drug abuse is *dependence*, whether it be psychic, physical, or both. The characteristics of dependence vary with the drug which is involved; in consequence the particular type of drug must now be designated. Although the Committee’s definition of drug dependence enables a common term to be applied to drug abuse, it does not help the practising doctor to diagnose dependence on particular drugs any more easily, as I pointed out in a recent symposium at the Royal Society of Medicine.

The diagnosis of drug dependence in clinical practice depends upon the recognition of repeated compulsive consumption of drugs on a periodic or continuous basis. This consumption originates from the patient’s own active desires; in attempting to make a diagnosis the doctor must rely on the elicitation of an accurate history from the patient. This may be linked with a subjective general impression about the patient, but Dr. Walker points out that in the case of amphetamine/barbiturate dependence a positive diagnosis is difficult. In the case of narcotic drugs also a positive diagnosis is difficult on the history alone.

If the doctor looks for objective clinical signs he may obtain some more definite evidence of dependence. The constricted pupils, the injection scars, and other signs of pharmacological action of the drug in a patient whose history suggests narcotic dependence makes the evidence more definite. Studies in experimental animals support the validity of such evidence.^{2,3}

Dr. Ashcroft and his colleagues suggested that the chewing and teeth-grinding sign may be a comparable diagnostic sign in patients who are dependent on amphetamine.⁴ They indicated that there may be pharmacological evidence in rabbits which supports their clinical observation. The value of such pharmacological evidence for forecasting and interpreting drug action in man has been examined by Litchfield.⁵ He points out that almost all drug effects seen in man can be demonstrated to have a counterpart in animals, but it must be recognized that drug effects seen in animals cannot readily be translated into their counterpart in man.⁶ Dr. Walker is conscious of the difficulties of making a differential diagnosis between the clinical signs in patients who are tense and anxious and in patients who have the clinical features associated with dependence on amphetamine or amphetamine/barbiturate mixtures. Such difficulties do not, however, justify any suggestion that pharmacological findings are not valid for the interpretation of observations on the actions of drugs in man unless definite proof to this effect is produced.

Dr. Walker also suggests that a positive diagnosis of amphetamine dependence in practice is impractical unless biochemical facilities are available. It is of some importance to consider the meaning and implications of the term "diagnosis" in relation to drug dependence. Administration of nalorphine or substitution of dummy tablets for a narcotic drug induces the abstinence syndrome on the basis of which a definite diagnosis of drug dependence can be made. In the case of morphine, and to a lesser extent the barbiturates, toxicity is associated with withdrawal of the drug. Dummy tablets rarely, if ever, produce placebo effects. In the case of amphetamine, substitution of dummy tablets for an amphetamine/barbiturate mixture produced a placebo effect in 41% of patients who said they could and in 45% of patients who said they could not do without the mixture. All these patients were believed by their doctors on the basis of the history and general impression to be dependent on the mixture. Clearly, therefore, patients who were known to be taking purple hearts were not dependent upon the pharmacological action of the chemical compounds which are normally present in these tablets.^{7,8} The use of dummy tablets therefore provides a valuable clinical test of dependence on the pharmacological activity of a drug.

As Dr. Walker indicates, chemical tests for amphetamine, and for that matter most other drugs of dependence, are complicated to perform. In spite of this, laboratories such as this one, which perform these tests routinely, carry them out rapidly and reliably. In positive cases very small quantities of amphetamine can be detected in the urine. However, our experience indicates that whenever a drug is detected in the urine of animals or man it is most important not to depend on any one test alone for diagnosis of drug consumption whether it be clinical or chemi-

cal. Evidence from the history and the physical signs, and clinical tests for the pharmacological activity of the suspected drug, should be considered together with evidence from the chemical analyses of the urine, whenever this is possible, before a definite diagnosis of drug dependence is made.—I am, etc.,

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- ⁵ Litchfield, J. T., *J. Amer. med. Ass.*, 1961, 177, 34.
- ⁶ — in *Drugs in Our Society*, edited by P. Talalay, 1964. Oxf. Univ. Press, London.
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- ⁸ — *ibid.*, 1963, 59, 14.

Post-mortem Caesarean Section

SIR,—I was greatly interested in your article on post-mortem caesarean section (23 January, p. 204), because it so happens that I twice did the operation in India, and in both cases got a living baby.

The first was one of pelvic deformity where a spinal anaesthetic had been given, but I suspect that it had gone too high, because the patient ceased breathing, and in spite of every effort died before the operation. Having made sure that she was dead, I quickly whipped out the baby, which promptly yelled. (Nowadays, of course, it might be possible to resuscitate such a patient.)

The second case was more dramatic, a woman dying of pneumonia, with that terrible respiratory whoop that these patients get in the last stages. I was operating on another case when one of my assistants wheeled the dying woman into the theatre. I carried on with my operation. The whoop continued, but soon became a death-rattle, and, a few moments later, silence. I again did a very rapid caesarean and extracted a living but rather feeble premature baby which died the next day.

I thoroughly agree with you, it is no use fiddling about with lower-segment section, antiseptics, etc. (though this feels strange); the great thing is to get the baby out quickly, without damage and without panic.

I wonder if you are right in allowing "10 or 15 minutes" to elapse in attempts at resuscitation, with a maximum of 25 minutes? I should put the limit at five minutes or less. I do not think that if the foetal heart can be heard, or there are reasonable grounds for thinking that the baby is alive, precious moments should be wasted in making elaborate tests on the mother, still less in obtaining the permission of the relatives. After all, a competent surgeon is unlikely to be mistaken; incidentally, I believe that one of the quickest tests of death is to put a small drop of formalin on the conjunctiva, which promptly turns red if life is not extinct.

In skilled hands the time from making the incision to extracting the baby is less than a minute, and in the highly unlikely event of the incision's jerking the mother back to life she could be quickly anaesthetized with gas and oxygen, which would presumably be kept ready. Even so, a live baby is better than a moribund mother.

In 102 B.C. the alleged originator of the operation must have abided by some of your excellent principles, or of mine, and thereby changed the history of the world.—I am, etc.,

Bulawayo,
S. Rhodesia.

H. WILLIAMSON.

Plantar Warts and Athlete's Foot

SIR,—May I reply to Dr. C. Starkie's letter (13 February, p. 451) regarding barefoot gymnastics? In 1960 I did a survey on the incidence of plantar warts in Leicester schoolchildren (Annual Report, P.S.M.O., 1960). In the course of my research I requested from the advisers on physical education a list of all barefoot activities (gymnastic dancing, showers, and swimming) in all Leicester schools. The comparison of figures of plantar warts' incidence in different schools did not prove that barefoot work contributes to the frequency of occurrence. One school, for instance, which did not do any barefoot work at all, had an incidence of 61 per 1,000, and another, in spite of the many barefoot activities, had only an incidence of 9 per 1,000.

We all know that the numbers of verrucas in schoolchildren are constantly rising, but in my opinion we are still in the dark about the mode of infection and some more research is needed in this field.—I am, etc.,

School Health Service,
Leicester.

I. DUB.

Nursing Procedures

SIR,—At a time when the recovery of some patients in our hospitals is jeopardized by a shortage of trained nurses certain traditional time-consuming procedures should be reassessed for their usefulness.

Fluid charts are laborious to keep and under conditions obtaining in many wards of dubious accuracy. This statement is supported by discrepancies often revealed when daily "in" and "out" totals are added over a period of 10 days and the resultant figures assessed in relation to the clinical status. Whilst an accurately charted fluid intake is of great importance in some situations, in others only a measure of the total 24-hour-urine output is required. This can most simply be obtained by funnelling of urine as soon as passed into a container, whose content is measured once in 24 hours with a plastic measuring jar. The progress of many patients with oedema fit enough to leave their beds for a few moments is best followed by daily weighing.

In this way many hours of valuable nursing time can be saved with the additional advantage of obtaining more accurate and relevant information.—I am, etc.,

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What is a Therapeutic Community?

SIR,—Dr. S. Jacobson (20 February, p. 525) rightly points out that there are many therapeutic roads in social psychiatry leading to mental health. This being so, surely there