different speech sound if it is associated with a different speech source.³ This latter observation is particularly relevant to the earliest stage of language learning—the child will be unable to make progress if he cannot equate his sounds with those made by the adults around him—and possibly to some cases of language impairment in the adult since if this ability to abstract is absent there might be no speech comprehension presented with otherwise normal hearing.

In the fairly near future this background knowledge will provide the basis for a speech pattern audiometry which will be truly intermediate between the classical pure tone and speech audiometric tests. Though its test stimuli are computer-generated and far more complex than those which are at present used, they can be tape-recorded and used clinically with quite simple and reliable apparatus. In addition to this we can expect eventually to see the employment of speech therapy techniques which depend on the analysis and display of speech pattern features,⁴ and the use of synthetic speech for the blind⁵ and as a prosthetic speech aid for those who are entirely without expressive speech.

These technical developments which are likely to stem from the use of our present knowledge of speech patterns will probably be paralleled by an increase in our understanding of the mechanisms which underlie the speech mode of perception. The experimental techniques which have made hemispheric specialization more accessible to measurement⁶ and those which are beginning to define the role of short-term auditory memory in speech processing⁷ may be capable of extension so that the process of speech pattern encoding itself is better understood.

This rather superficial survey will have achieved its purpose if I have at least indicated how audiometric stimuli could, to advantage, be related to the nature of speech itself.—I am, etc.,

A. J. FOURCIN

Department of Phonetics and Linguistics,
University College,
London W.C.1.

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Bronchospasm after Althesin Anaesthesia

SIR,—I read with interest the letter by Lt. Col. T. R. Austin and others (16 June, p. 661) in which it was assumed that Althesin was the drug responsible for bronchospasm occurring during anaesthesia. Apparently no tests were performed to substantiate this conclusion despite the simultaneous use of other drugs, including pancuronium. In a recent similar case, to be published in the *British Journal of Anaesthesia*, bronchospasm occurred following the administration of pan-

curonium and Althesin. Further investigations during anaesthesia and also by intradermal testing postoperatively showed conclusively that pancuronium and not Althesin was responsible for the bronchospasm. Anaesthesia is a complex situation and adverse reactions may be caused by any one of the drugs used, by drug interactions, or by other factors.—We are, etc.,

R. M. CLARK
F. R. ELLIS

Department of Anaesthesia,
General Infirmary, Leeds

SIR,—In reply to Dr. M. J. Heslop's letter (30 June, p. 775) commenting on our report of a case of bronchospasm following the administration of Althesin and pancuronium (16 June, p. 661), may we say we were most interested to read of the case of bronchospasm following pancuronium, reported by Buckland and Avery.¹ Until this latter report pancuronium had held an unblemished record over its five-year world wide use for not causing histamine release, even in susceptible laboratory animals in large dosage.² As this information was not available at the time we felt that our patient's problem had probably been caused by the Althesin.

This correspondence emphasizes, we feel, the very real need for the kind of inquiry which Professor J. W. Dundee and Dr. R. S. J. Clarke are embarking upon,³ with a scientific evaluation of the extent of unusual reactions to anaesthetic drugs, and especially new anaesthetic drugs.

In reply to the final point in Dr. Heslop's letter we can only say we have issued the patient with a card pointing out the problem we encountered. The value of skin testing is not proved and there remain plenty of alternative agents should he need further anaesthetics.—We are, etc.,

T. R. AUSTIN
J. ANDERSON
J. RICHARDSON

British Military Hospital
Rinteln, Germany

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Facial Sweating after Food in Diabetes

SIR,—Dr. P. J. Watkins (10 March, p. 583) having told me about facial sweating after food in 1972, I specifically sought this sign in 200 consecutive patients at our diabetic clinic and found two similar cases.

The first patient was a woman aged 32 with severe insulin-dependent, unstable diabetes for six years with advanced retinopathy, nephropathy, gut neuropathy, and orthostatic hypotension. For two years she had suffered profuse facial and neck sweating after chewing but obtained no relief from anticholinergic drugs. The second patient was a 78-year-old woman with three months' history of thirst, polyuria, and vaginitis. She was thin, normotensive, apparently otherwise healthy, and non-ketotic, with a fasting blood sugar of 180 mg per 100 ml. Gustatory sweating was noticed within two weeks of the onset of polyuria. There was no other evidence of peripheral

or autonomic disturbance and no pupillary changes or lesions of the chest or brachial plexus. After diet and four weeks of glibenclamide the glycosuria disappeared and the fasting blood sugar fell to 120 mg/100 ml with disappearance of thirst, etc. Her gustatory sweating has continued, however, and is presumably her only evidence of diabetic autonomic neuropathy.

It is possible that "Watkins's sign" may be more common than has hitherto been realized.—I am, etc.,

A. FREEDMAN

Department of Medicine,
Prince of Wales Hospital,
Randwick, New South Wales

Rootless Wanderers

SIR,—Your leading article (7 July, p. 1) is very timely and I would like to make some further comment on it.

We psychiatrists working in the community have expressed for some time our very great concern over the lack of understanding by the legislators for the implementation of an efficient community psychiatrist service as part of the future comprehensive psychiatric set-up. The Local Authority Social Services Act 1970 unfortunately initiated the split between the hospital and community sectors of the service, and the proposed reorganization of the National Health Service will, I fear, finalize this dichotomy. The community sector of the psychiatric service plays an integral part in any overall psychiatric scheme, and one part cannot function efficiently without the other. The only way to build up such a service is, in my opinion to bring together all the psychiatric facilities, services, and personnel under one authority, the only logical one being the future health authority.

If it is proposed to run down and eventually phase out the psychiatric hospitals, as indeed appears to be Government policy, then this can be attempted only after, not before, efficient community psychiatric services have been established in the respective areas. Furthermore it should not be forgotten that the psychiatric units attached to district general hospitals equally require the help of the community psychiatric counterparts. So let us put the emphasis where it seems to fit best into the envisaged psychiatric service of the future—namely, to encourage the building up of appropriate community services under psychiatric guidance and management.—I am, etc.,

U. P. SEIDAL

London N.15

Excretion Urography in Acute Renal Failure

SIR,—We have read with great interest the very careful report of Dr. W. R. Cattell and others (9 June, p. 575) on urographic findings in acute renal failure. Over the past two years we have carried out a similar study on 25 oliguric patients without any obstructive disease. In 20 cases a diagnosis of acute tubular necrosis was made, based on the sudden onset of renal failure, the presence of an aetiological factor, typical urinary findings, and a typical clinical course including a diuretic phase and complete recovery. In five cases subacute glomerular or vascular disease was diagnosed on histological evidence (subacute glomerulonephritis in two, polyarteritis in two, and malignant

Diagnosis	No. of Cases	Nephrogram			Presence of Pyelogram
		Early (<5 min)	Late (>10 min)	Absent	
Acute tubular necrosis	20	12 { 7 faint 5 dense 2 faint	4	4	7
Subacute glomerular or vascular disease	5		1	2	1

hypertension in one); only one of these patients recovered some renal function.

Urography was performed during the first two weeks of acute renal failure, always before the onset of the recovery phase, 140 ml of methylglucamine iohalamate (Contrix 28; 28% iodine) being given in a single injection lasting less than five minutes. Plain films and tomograms were taken before and up to 24 hours after the injection of the contrast medium. The findings are summarized in the table. In the 20 cases of acute tubular necrosis it may be seen that, contrary to the experience of Dr. Cattell and his colleagues, we found no evidence of a nephrogram in four cases and a delayed nephrogram in four others. Nevertheless, all patients recovered completely and further examination failed to reveal any other associated renal disease. Our findings in subacute glomerular or vascular disease are similar to those of Dr. Cattell.

The reason for the discrepancy between our observations and those of Dr. Cattell and his colleagues in acute tubular necrosis is not immediately apparent. It may be related to the fact that we have studied a larger number of patients and thus observed the full spectrum of urographic patterns in acute tubular necrosis. Alternatively, it may result from the criteria used to assert the presence of a nephrogram. In our experience it may be difficult to decide whether or not a nephrogram is present. An increase in radiodensity noted between control and post-injection films may be due to slight changes in technical conditions despite constant kilovoltage (progressive warming up of the tube, instability of the power supply, physical characteristics of individual film cassettes). As criteria for the presence of a nephrogram we require an increased radiodensity of the kidney not only in reference to control films but mostly in reference to adjacent structures such as the psoas or the sinus fat. We conclude that, unfortunately, the nephrographic patterns are not sufficiently distinctive to lead to an accurate diagnosis in non-obstructive acute renal failure.—We are, etc.,

J. M. VANDENBROUCKE

J. P. GILBEAUX

C. VAN YPERSELE DE STRIHOU

Renal Service and Department of Radiology,
University of Louvain Medical School,
Louvain, Belgium

Contraindications to Smallpox Vaccination

SIR,—Dr. A. B. Christie (23 June, p. 714) raises some interesting points. He says "vaccination as a routine is no longer recommended in Britain, and contraindications are therefore irrelevant." This is only partly true because not all children born in this country will reside here for the rest of their lives and they may go, with their parents, as residents or visitors to countries in Asia—Pakistan, India, and Bangladesh—where smallpox is still endemic and vaccination is required. Furthermore, I have found

quite a number of parents who still wish to have their children vaccinated despite the fact that routine vaccination is no longer recommended, because they feel that primary vaccination if carried out later in adult life may constitute a greater risk than primary vaccination in childhood.

I entirely agree with Dr. Christie that a certificate of contraindication should be accepted for travellers going from one smallpox-free country to another. The relevant paragraph in the International Health Regulations (supplied to me by the Department of Health) states as follows: "If a vaccinator is of the opinion that vaccination is contraindicated on medical grounds he shall provide the person with reasons, written in English or French, underlying that opinion, which health authorities should take into account."

Though there is no agreement or obligation imposed by this article most immigration authorities accept it, though there are some notable exceptions; but during the recent episode of smallpox in London, which was in itself a most unusual epidemiological event, many countries took entirely different views of the situation and many, in my opinion, over-reacted. When the first case of smallpox was diagnosed, London, or more specifically the London Metropolitan Boroughs, were declared a smallpox zone but not the whole United Kingdom. Some countries accepted the World Health Organization definition of the declared zone, but others required a certificate from anyone from the United Kingdom irrespective of whether a certificate of contraindication was made available. As a result many families to my knowledge either had to cancel their holidays or accept the risk of travelling abroad with a certificate of contraindication which in all probability would not be accepted as valid in the circumstances.

In 1970 a considerable advance was made by the Council of Europe by agreeing to create a single area for the health control of international traffic in Europe. Many, but not all, European countries are a party to this agreement but there is always a tendency when a case of smallpox occurs for people to lose their sense of proportion and I would put in a plea for a review of this 1970 agreement, certainly as far as the European countries are concerned. At least a distinction should be made as to what does and what does not constitute a smallpox zone.—I am, etc.,

J. A. DUDGEON

Department of Microbiology,
Hospital for Sick Children,
Great Ormond Street,
London W.C.1

Latent Morbidity after Abortion

SIR,—We read with interest your leading article (3 March, p. 506) which commented favourably on the report by Margaret and Arthur Wynn¹, on the consequences of induced abortion. Having now examined the

report carefully, we find in it ambiguities and inaccuracies that seriously diminish its value as a scientific appraisal of the risks of induced abortion. Some examples of its deficiencies follow.

(1) The appropriate denominator in examining variation in abortion rates with time is the population at risk—that is, women in the childbearing years. The use of number of live births as a denominator can artificially inflate the abortion rate, if the live birth rate decreases with time. Data shown by Klinger² illustrate this well.

(2) The statement quoted from Klinger³ "It is clear that induced abortion plays an important role in the development of a subsequent child"—is prefaced in the original with "these relationships have not yet been studied in detail." By removing this prefatory clause Klinger's statement is given a misleading air of certainty.

(3) The table referring to "Abortion and Subsequent Prematurity: Hungary 1964" is attributed to Horsky⁴ but does not appear in this paper. It does appear in somewhat different form (the prematurity rate specific for the number of induced abortions and time period) in both of Klinger's papers.^{2,3} Horsky's paper does not deal with the matter of abortion and subsequent prematurity at all. Further, if the relationship between induced abortion and prematurity is to be properly examined, maternal age and parity must be taken into account, since the prematurity rate is known to vary with both of these variables.⁵

(4) A paper by Matsunaga⁶ is quoted to show that the infant death rate in Japan due to congenital malformation in 1960 (after legalization of abortion) was 30% higher than in 1947 (prior to legalization of abortion). Again, little can be concluded about the contribution of abortion to this increase without making the congenital malformation death rates specific for both maternal age and parity, both of which are known to affect the incidence of malformations.⁵

(5) In the context of examining sterility following induced abortion, the Wynn Report cites a Norwegian paper⁷ in which sterility is found to occur in 3.4% of women who have had an abortion. However, 45.8% of the women in that study were seeking an abortion on medical grounds. These women, therefore, had conditions such as pelvic and metabolic disorders very likely to be associated in themselves with sterility.

We are puzzled by the inclusion of references at the end of the paper by Margaret Wynn which are not cited in the text. For example, those by Maramatsu,⁸ Muller,⁹ and Potts.¹⁰ The first paper presents no data on the influence of induced abortion on subsequent pregnancy. The second supports access to abortion and deals with the consequences of unwanted pregnancy. The third offers no support for the Wynns' position and, although data are not presented, it states that in Czechoslovakia no relation-