Crawford and Clayton			Hamilton <i>et al.</i> ¹	
Water Type	No. of Rib Samples	Mean lead (p.p.m. Dried Ash)	No. of Rib Samples	Mean lead (p.p.m. Dried Ash)
Hard	27	23.63	22	29.0 ± 3.0 (S.E.) ± 18.4 (S.D.)
Soft	28	33.54	22	34.5 ± 2.9 (S.E.) ± 13.6 (S.D.)

histological examination of the samples is inadequate. Further, any reference to plumbosolvency is a "red herring" to the basic problem and should be treated as a separate issue, which is amenable to a solution (see Dr. Crawford and Mr. Clayton's reference to Glasgow water). Apart from invoking very unusual conditions relating to chemical elements in particular chemical forms which are supposed to be avidly taken up by the body from drinking water, the total daily intake of most, if not all, elements present in U.K. waters constitute a negligible contribution to total intake, which is overwhelmingly provided by diet.^{4,5} It is incorrect to consider the lead content of water simply in terms of hardness or softness; there are many types of hard and soft water, and under some conditions the lead content of hard water can exceed that of soft water supplied in lead pipes. The concentration of the chemical elements in water and also in relation to defined geographical regions is a function of the regional availability of the elements combined with changes brought about by various types of technological processing during purification of water.

The incidence of some forms of cardiovascular disease can be described in relation to regional environmental factors⁶ but at present our knowledge of the importance, if any, of the chemical elements as causative agents is almost non-existent. For the studies undertaken by Dr. Crawford and Mr. Clayton, ancillary studies describing the lead content of the individual's diet and the domestic plumbing system, together with the range of variability in lead content of drinking water are required in relation to the samples of rib taken for chemical assay. With a few exceptions, permissible levels for the concentration of the chemical elements in drinking waters will undoubtedly be based upon medical evidence. At present the necessary information is sadly lacking, though much effort is being directed to improve the quality of available information. A careful evaluation will have to be made between that provided by medical evidence and the practical considerations of supplying drinking water. Until the required information is available, great care must be exercised when correlating morbidity attributable to the elements present in drinking water.

The quality of drinking water in the U.K. is of a very high standard, but the presence of many elements in the water is inevitable. There appears to be no evidence that intake of drinking water is harmful to populations residing in the various natural geological regions of the U.K. The question of plumbosolvency is acknowledged and can be remedied. It would be most unfortunate if, with the increasing attention paid to chemical elements present in water, the known toxicity of some elements when ingested in large amounts is taken to indicate that they are harmful at low concentrations—that is, acceptance of a linear response between dosage and effect over all ranges of concentration.

The present dilemma, if it really exists, is the lack of medical evidence upon which permissible levels for the concentration of elements in water can be identified. At low concentrations interactions between elements and resulting synergistic effects may occur, but in terms of human health the elements present in the diet and the standard of dietary intake for individuals are more likely to find expression in morbidity related to the abundance of the chemical elements. With the exception of plumbosolvency for soft water supplied in lead pipes, many individuals in the U.K. who are supplied with water derived from wells or leats may be exposed to some elements that periodically, because of rainfall, may present intermittent hazards to health, though in many instances the presence of algae or bacteria may be the primary cause of illness.—I am, etc.,

E. I. HAMILTON

"Woodchurch,"
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¹ Hamilton, E. I., Minski, M. J., and Cleary, J. J., *Science of the Total Environment*, 1972, 4, 1.

² Hamilton, E. I., *Nature*, 1971, 231, 524.

³ Hamilton, E. I., *Calcified Tissue Research*, 1971, 7, 150.

⁴ Ministry of Agriculture, Fisheries and Food, *Survey of Lead in Food*. London, H.M.S.O., 1973.

⁵ Hamilton, E. I., and Minski, M. J., *Science of the Total Environment*, 1972/73, 1, 341.

⁶ Hamilton, E. I., and Minski, M. J., *Environmental Letters*, 1972/73, 1, 375.

Pruritus Vulvae

SIR,—Only a man would suggest that women will give up wearing tights, however bad the itch (Mr. C. N. McFarlane, (2 June, p. 533). Only a man would suggest that stockings with "hold-up" tops really work, except in the most statuesque females. However plausible the explanation given for the recommendation not to wear tights (is it, by the way, supported by any factual evidence?) I suspect that most women would prefer to put up with their symptoms rather than give up the garment which has liberated them from the last vestiges of stays.

Perhaps Mr. McFarlane would have more success if he told his patients to wear cotton pants under their tights, and change them frequently.—I am, etc.,

SALLY FORD

Tayport, Fife

SIR,—Mr. C. N. McFarlane (2 June, p. 533) states that the wearing of tights is "one of the foremost causes of pruritus vulvae seen in gynaecological and general practice today." He omits from consideration two other possible factors which are equally "pushed" by advertisers in women's magazines—"biological" washing powders and "feminine" deodorants. These products are quite unnecessary refinements on soap and water; the washing powders have been shown to cause irritation of the hands and I contend that they are much more likely to cause vulval irritation than the wearing of tights.

Compared with conventional stockings, with suspenders, and even hold-up stockings, which are both uncomfortable, irritant, and restricting, tights are an immense improvement in female dress, and Mr. McFarlane should provide proof for his statement before he condemns them out of hand.—I am, etc.,

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"The No Touching Epidemic"

SIR,—Dr. P. N. K. Heyling's Personal View (14 April, p. 111) was a joy to read. Your correspondents so far have not commented on the special value of "touching" in psycho-geriatrics; it is quite indispensable in this field of medicine.

The primeval gesture of the hand-shake stretches back into pre-history, and is so deep-rooted that its function in the province of relationships dies hard; it is surely the last social grace to survive in the mentally confused. By the manner of the hand-shake a wealth of meaning can be conveyed. For a brief moment the patient knows that the doctor, to the exclusion of all else, is wholly giving and caring. In this sense it is a sacramental act. When the mental handicap is complicated by blindness, deafness, aphasia, or dysphasia—where the "sound of the voice and the look on one's face" may be of no avail—the daily hand-shake is mandatory.

The doctor with the no-touch technique, whose visit to the ward is confined to a brusque "sick parade" for those who are brought to his notice as "poorly," rejects a basic clinical approach to mental illness and remains out in the cold—as indeed do his patients.—I am, etc.,

C. F. J. CROPPER

Bath

Arrhythmias in the Guillain-Barré Syndrome

SIR,—Increasingly the cardiovascular complications of the Guillain-Barré syndrome are being stressed and it is possible that dysrhythmias may contribute to the mortality of this condition. In this context I wish to report a patient with intermittent atrial fibrillation.

A 59-year-old white man was admitted with one week's history of difficulty in walking, aching of his legs, and weakness of his arms. He had suffered four days of diarrhoea two weeks before admission. His general condition was unremarkable, with a regular pulse of 80/min and a blood pressure of 130/90 mm Hg. He had bilateral facial palsies and marked weakness of the limbs, especially of the legs, with diminished muscle tone and absent reflexes. Sensations were intact, though he described hyperaesthesia of the feet. Nerve conduction studies showed gross slowing in all limbs; the protein content of the cerebrospinal fluid was 50 mg/100 ml without cells. Vanilmandelic acid excretion and urinary steroids were all normal, but there was a loss of the normal diurnal variation of plasma cortisol.

Ten days after admission his weakness had advanced but there was no respiratory embarrassment. His pulse was 100/min and irregular, and an E.C.G. showed atrial fibrillation. He was not distressed and there were no signs of heart failure. The following day he spontaneously reverted to sinus rhythm, the whole attack lasting

12 hours. His E.C.G. was monitored and he went into atrial fibrillation on a second occasion, reverting to sinus rhythm after six hours, again without treatment. His neurological condition showed steady improvement and he was discharged walking with sticks.

The cause of his atrial fibrillation remains obscure though toxic myocarditis is a possibility. He had no obvious chest or other infection nor evidence of ischaemic heart disease either clinically or on E.C.G.s taken before and after exertion.

"Paralysis of the heart" in neuropathy was described by Osler¹ in 1892 and tachycardia was noted by Guillaín,² Kernohan,³ and others. Davies and Dingle⁴ described five patients with Guillaín-Barré syndrome, all on respirators, who had tachycardia, one of whom showed ventricular ectopic beats on the E.C.G. Of 15 recent cases seen in this hospital, eight had a pulse rate between 120 and 160/min and half showed non-specific S-T changes in the E.C.G. This tachycardia was out of proportion to any expected rise associated with a pyrexia and tended to occur at the height of the illness.

It may be that the mortality of around 10%⁵ seen in this condition despite effective positive pressure respiration may be in part due to cardiac arrhythmias, and on the premise that these are potentially treatable E.C.G. monitoring of all patients showing a sustained tachycardia is advisable.

I would like to thank Dr. P. Millac for permission to report this case and for helpful criticism.

—I am, etc.,

IAN M. STEWART

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¹ Osler, W., *The Principles and Practice of Medicine*, p. 778. Edinburgh, Pentland, 1892.

² Guillaín, G., *Archives of Neurology and Psychiatry*, 1936, 36, 975.

³ Haymaker, W., and Kernohan, J. W., *Medicine (Baltimore)*, 1949, 28, 59.

⁴ Davies, A. G., and Dingle, H. R., *Journal of Neurology, Neurosurgery and Psychiatry*, 1972, 35, 176.

⁵ Miller, H., *British Medical Journal*, 1966, 2, 1219.

Public Attitudes to A.I.D.

SIR,—With reference to Dr. J. J. Slomes' interesting comments on A.I.D. (12 May, p. 366), may I point out that the refusal by the Editor of the *Jewish Chronicle* to publish an advertisement for a Jewish donor may not be at all a consequence of an "attitude" against A.I.D. but derive entirely from Jewish law. A.I.D. is perfectly permissible in Jewish law provided the donor is *not* a Jew, and therefore any non-Jewish donor will be permissible but a Jewish one will not be so. The Editor of the *Jewish Chronicle* acted in accordance with Jewish law as laid down recently in a responsum published by a leading world rabbinical authority.¹ The reasons for this ruling are not relevant here; suffice it to say that they are partly an attempt to avoid any incestuous unions.—I am, etc.,

D. PARIENTE

London N.W.11

¹ Feinstein, M., *Iggeret Moshe*, New York, Gros Bros., 1970.

Vitreous Surgery

SIR,—In your leading article on this subject (5 May, p. 258) you state that "the failure of chemical and drug therapy in the treat-

ment of vitreous haemorrhage" has led to the need for mechanical means of removing opaque material from the vitreous. I would like to point out that Dr. J. V. Forrester and I recently reported¹ on the treatment of vitreous haemorrhage by injection with urokinase. A diabetic patient 38 years of age showed a remarkable return to vision from perception of light to 6/36 after an injection of this enzyme. He had been virtually blind in his left eye for four years as a result of a massive intravitreal haemorrhage.

Since then we have studied another four patients and have had good results in all cases. We think that urokinase opens up an exciting new prospect in the management of vitreous haemorrhage and that chemical and drug therapy should not be completely discarded at this early stage.—I am, etc.,

J. WILLIAMSON

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¹ Williamson, J., and Forrester, J. V., *Lancet*, 1972, 2, 488.

Aetiology of Acute Glomerulonephritis

SIR,—Your leading article (14 April, p. 66) draws attention to the association between impetigo and acute glomerulonephritis. In underdeveloped countries scabies is common and may be complicated by superinfection with pyogenic bacteria. I wish to report on a series of patients with acute glomerulonephritis admitted to a mission hospital in Southern Africa.

Among 4,695 admissions between 1 June 1971 and 31 May 1972 there were 33 patients with acute glomerulonephritis; 14 (43%) were suffering from infected scabies, nine (27%) gave a history of recent upper respiratory infection, and two (6%) had impetigo. The remaining eight (24%) gave no history of recent infection, though some of them had uncomplicated scabies. There was no significant sex or age difference between the groups. The seasonal incidence in the group with septic scabies is, however, of interest; all these patients were admitted between September and March and 10 of them (71%) in the five-week period from 27 November—that is, midsummer. These patients were not all living in the same area. Patients in other groups were evenly distributed throughout the year.

It would therefore appear that acute glomerulonephritis secondary to a pyoderma, and especially septic scabies, may in some areas be very common.—I am, etc.,

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Dogs and Transmission of Toxoplasmosis

SIR,—*Toxoplasma gondii* oocysts from naturally infected domestic cats (Prof. S. Pampiglione and others, 5 May, p. 306) would seem to be of importance epidemiologically¹⁻³ and can remain viable in soil for long periods.^{1,4} In several standard texts and papers the dog (in which *T. gondii* infection is also common) is implicated as a possible source of human toxoplasmosis. This, in the light of present knowledge, is misleading, and it should be stressed that formation of oocysts of *T. gondii* is known only in

domestic cats and some wild felids.^{5,6} They are not shed by dogs (not even when immunosuppressive drugs are used⁷) or various other vertebrates that have been given toxoplasmosis experimentally.^{8,9} Nor has *T. gondii* been isolated in excretions or secretions of dogs successfully infected orally with oocysts.⁷ Thus there is as yet no evidence for droplet infection, and the family dog would not appear to be important in the transmission of toxoplasmosis to man.

The primary source of infection in the case of dogs is probably raw meat containing tissue cysts. Filth flies have been shown to carry *T. gondii* oocysts and can contaminate food for up to 48 hours after contact with infected faeces¹⁰ (contrary to popular belief, in nature faeces are often only partially covered with soil by the cat after being excreted). Many dogs are in the habit of snapping at flies and frequently succeed in catching and eating them. The significance of this in the transmission of canine toxoplasmosis remains to be determined.

As discussed elsewhere in a review of the symptoms, transmission, prevention, and treatment of toxoplasmosis,¹¹ human infections are usually acquired (1) transplacentally, (2) by the ingestion in raw meat of microscopic tissue cysts, or (3) by the inadvertent ingestion of oocysts, either directly or via transport hosts such as flies and cockroaches. In addition, accidental infection of laboratory workers with *T. gondii* (some cases being illustrative of transmission by wound contamination) and transmission of toxoplasmosis by leucocyte transfusion have been reported. There is a risk of transmitting *T. gondii* by normal blood transfusion.¹¹—I am, etc.,

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¹ Fleck, D. G., Chessum, B. S., and Perkins, M., *British Medical Journal*, 1972, 3, 111.

² Peterson, D. R., Tronca, E., and Bonin, P., *American Journal of Epidemiology*, 1972, 96, 215.

³ Wallace, G. D., Marshall, L., and Marshall, M., *American Journal of Epidemiology*, 1972, 95, 475.

⁴ Ruiz, A., Frenkel, J. K., and Cerdas, L., *Journal of Parasitology*, 1973, 59, 204.

⁵ Janitschke, K., and Werner, H., *Zeitschrift für Parasitenkunde*, 1972, 39, 247.

⁶ Jewell, M. L., Frenkel, J. K., Johnson, K. M., Reed, V., and Ruiz, A., *American Journal of Tropical Medicine and Hygiene*, 1972, 21, 512.

⁷ Kühn, D., Oppermann, W. H., Rödel, H., and Centurion, H., *Berliner und Münchener tierärztliche Wochenschrift*, 1972, 85, 309.

⁸ Draper, C. C., Killick-Kendrick, R., Hutchison, W. M., Siim, J. C., and Garnham, P. C. C., *British Medical Journal*, 1971, 2, 375.

⁹ Miller, N. L., Frenkel, J. K., and Dubey, J. P., *Journal of Parasitology*, 1972, 58, 928.

¹⁰ Wallace, G. D., *American Journal of Tropical Medicine and Hygiene*, 1971, 20, 411.

¹¹ Markus, M. B., *South African Medical Journal*. In press.

Isolation in the Control of Dysentery

SIR,—I refer to Dr. H. G. Easton's letter (31 March, p. 798) and wish to express my general agreement with the views stated.

The reference to "patients" in my paper (17 March, p. 669) should have been qualified by the addition of "in hospital" as this was the context in which the statement was made. I accept that it is desirable that patients with mild symptoms be managed at home, provided that the attending doctor regards the family as the infected unit and takes appropriate precautions, especially in respect of members of the household who may, by virtue of their occupation or activi-