

normally engendered by these heat treatments, and this would appear to answer the recommendation made in your concluding paragraph.

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ATARALGESIA

SIR,—The article by Dr. Hayward-Butt in your issue of Nov. 16 will be of considerable interest to all anaesthetists, particularly to those who are attracted by the problems of obstetric anaesthesia.

It is evident that there is in many places a groping towards a new form of pain relief in operative obstetrics.

Davies¹ referred to five cases of caesarean section in which 25 mg. of chlorpromazine was given intravenously 30–45 minutes before operation, anaesthesia being induced and maintained with a nitrous-oxide/oxygen mixture, plus succinylcholine when relaxation was required.

More recently, Scott and Gadd² suggested that a number of forceps deliveries could be satisfactorily undertaken following the intravenous injection of 100 mg. pethidine and 25 mg. chlorpromazine.

Dr. R. E. Molloy, who arrived at this hospital a couple of months ago, informs me that when he left the Whittington Hospital a substantial number of cases had been conducted there in this manner, with fully satisfactory results. I have commented upon this trend in a general review shortly to be published,³ and have referred also to a number of forceps deliveries which, in cooperation with Mr. J. D. O'Sullivan at St. Mary's Hospital, Portsmouth, I conducted satisfactorily with 150 mg. pethidine and 50 mg. chlorpromazine given intravenously. Although this series was prematurely concluded, owing firstly to intervening vacations, and then to my leaving for Pittsburgh, it was obvious to us that the method had considerable potential value, though modifications would be advisable.

Evidently Dr. Hayward-Butt is also engaged in this movement. He mentions that he has given his "unit mixture" (number of units unspecified) in four cases of caesarean section (three times intramuscularly, once intravenously). One would like to know in more detail the exact dosage given, the length of time between administration and the delivery of the child, and the precise condition of all the neonates. We still know too little about the placental transmission of the drugs in question to be able to ignore these details. I used chlorpromazine in my cases, but it might well be that in a larger series the sympathicolytic and other effects mentioned by Dr. Hayward-Butt (though not recorded in my patients) might encourage the use of 'Pacatal' in its place. If an opiate antagonist is to be used (and again I must quote the fact that in my own cases and those seen by Dr. Molloy there was apparently no pressing need for one, judging by neonatal depression, though the results would probably have been even more impressive had such an antagonist been incorporated in the mixture) I would suggest that levallorphan, rather than amiphenazole, be chosen. Levallorphan is apparently almost specific against narcotics, and is virtually innocuous both locally and generally, even when given in high doses intravenously—as is well recognised in this unit.⁴

Despite Dr. Hayward-Butt's preference for intramuscular injection, on the ground that it leads to less mental clouding, I would favour the intravenous route as there must be a more precise prediction of onset of effect, and in emergency cases (such as obstetrics) a 30-minute delay would be contra-indicated.

I earlier used the term "conducted" because I am not happy about the question of nomenclature. Dr. Hayward-Butt uses the word "ataralgesia" to describe the state of each of his patients. Mine, however, certainly had some

"disturbance of mind": I could converse easily with some, but others appeared to be in a semidrunken state, and a high proportion had amnesia for some of the events of delivery, though none recollected feeling pain. I had, perhaps clumsily, dubbed this state as one of "hypoaesthesia," in an attempt to indicate a diminution of awareness without loss of consciousness. As must be obvious, I have no Greek.

A further point which interests me, and one which will perhaps be the most intriguing to all anaesthetists, is how Dr. Hayward-Butt conducted the five abdominal operations for which no anaesthetic was given. If no relaxant drug was used, then this would imply a most interesting supplement to our knowledge of the pharmacology of pethidine and pacatal. If relaxants were employed, were the patients intubated, was their respiration assisted or controlled, and did they accept such manoeuvres with ataralgesic tranquillity?

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CORONARY AND AORTIC ATHEROSCLEROSIS

SIR,—Dr. Elkeles' report (Oct. 12) on the greater prevalence of aortic calcification in older women as compared with men, in contrast to coronary-artery disease, is in good agreement with our findings, not quoted by him, first published in 1954¹ and subsequently amplified.^{2 3}

Our actual prevalence-rates for these shadows at various ages were within the general range reported by Dr. Elkeles, and our diagnostic criteria, verified by comparing pre-mortem radiographic findings with post-mortem specimens from the same individuals,⁴ appear to be similar. We felt rather humble when we found that Rokitansky already knew of the greater prevalence of aortic atheromata in women.^{1 5} Subsequently, Barr, in an extensive review,⁶ referred to an even earlier report by Hasse.⁷

Since you have lately given much space to atherosclerosis, I am taking the liberty of drawing your attention to an extensive epidemiological study of atherosclerosis from which these data have been taken. We have studied about 1200 men and women aged 40 and over, chosen strictly at random from a population of about 33,000 employed clothing workers in New York City.^{2 3} Most were of Italian or Jewish extraction and were closely comparable from a social, economic, and occupational point of view. Our data are among the very few currently available,⁸ relating to the prevalence of manifest atherosclerosis and associated factors in representative samples of population segments. Since most of Dr. J. N. Morris' extensive data on ischaemic heart-disease in various occupational groups in Britain are based on incidence- rather than prevalence-rates, a comparison with our data cannot be readily made.

Among the Italian-American men in our group, the prevalence of manifest coronary-artery disease, diagnosed on the basis of a history of an acute heart attack, unequivocal angina pectoris, or clear electrocardiographic findings, was about the same as for the other groups studied in the United States.⁸

In the Jewish men, on the other hand, the prevalence-rate was twice as high ($P < 0.01$). No ethnic difference in the prevalence of coronary-artery disease could be detected among the women. These findings were confirmed by analysis of certain insurance records, as we have reported. Radiological evidence of calcification in the thoracic and abdominal aorta likewise

1. Boas, E. P., Epstein, F. H. *Arch. intern. Med.* 1954, **94**, 94.

2. Epstein, F. H., Boas, E. P., Simpson, R. J. *J. chron. Dis.* 1957, **5**, 300.

3. Epstein, F. H., Simpson, R., Boas, E. P. *ibid.* p. 329.

4. Hyman, J. B., Epstein, F. H. *Amer. Heart J.* 1954, **48**, 540.

5. Rokitansky, C. *Handbuch der pathologischen Anatomie: Handbuch der speziellen pathologischen Anatomie*, vol. 2, p. 544. Vienna, 1844.

6. Barr, D. P. *J. chron. Dis.* 1955, **1**, 63.

7. Hasse, K. *Krankheiten der Zirkulations- und Respirationsorgane*. Leipzig, 1841.

8. Measuring the Risk of Coronary Heart Disease in Adult Population Groups; a Symposium. *Amer. J. publ. Hlth*, suppl. April, 1957.

1. Davies, J. I. *Canad. Anaesth. Soc. J.* 1955, **2**, 327.

2. Scott, J. S., Gadd, R. L. *Brit. med. J.* 1957, **i**, 971.

3. Crawford, J. S. *Brit. med. Bull.* (in the press).

4. Foldes, F. F., Lipschitz, E., Weber, G. M., Swerdlow, M., Pirk, L. A. *J. Amer. med. Ass.* 1956, **160**, 168.

was not different in the ethnic groups, again pointing to dissimilarities between coronary-artery and aortic disease, as discussed by Dr. Elkeles. On the other hand, coronary-artery disease appeared to be more common among the men with aortic calcifications than among those without such lesions; these particular data were insufficient for statistical analysis.

Thus, some degree of association between these two lesions may exist. In this connection, the inadequacy of available methods for the clinical diagnosis of both coronary and aortic disease cannot be overstressed. With Dr. Elkeles' and our data as a base-line, it might be of interest to determine the prevalence of radiologically demonstrable aortic calcifications in older people belonging to populations said to be relatively immune to atherosclerosis; the possible radiological hazard must, however, be kept in mind.

On attempting to explain the greater prevalence of coronary-artery disease among the Jewish men, we found that the dietary patterns of these particular Italian and Jewish groups could not provide more than, perhaps, a very minor clue; caloric and total fat intake was closely similar in the two groups even though the Jews, on an average, consumed about 10 g. more animal fat a day than the Italians, in isocaloric exchange for fat of vegetable origin.^{9 10} Unfortunately, we could not compare the diets of the men with and without coronary-artery disease, since the number of diseased individuals was too small for valid analysis—a frequent consequence of working with random populations rather than sick persons and “controls”. On further analysis, we were surprised to find that elevation of serum-cholesterol, blood-pressure, and weight was associated with a greater frequency of coronary-artery disease only among the Italian men; none of these three factors, alone or in combination, appreciably affected the disease-rate in the Jewish men. The conclusion appeared inevitable that factors other than those investigated in this study predisposed these Jewish men to the development of coronary-artery disease.

These data are somewhat at variance with recent reports on Jewish groups in Israel.^{11 12} Space does not permit discussion of possible reasons for this divergence; but, as Sir Sheldon Dudley pointed out, epidemics of the same disease may be caused by dissimilar combinations of factors, according to time and place.¹³ He referred to transmissible diseases, but his point applies equally to the epidemiology of non-infectious conditions. It goes without saying that we have talked of “Italians” and “Jews,” but refer only to the particular Italian and Jewish Americans in our study. The epidemiology of atherosclerosis no doubt differs among people in these broad groups according to their given ecological setting. Nor do we wish to imply that our findings necessarily suggest that a genetic factor is involved in the observed differences, since there are many environmental variables of possible significance that were not studied in our investigations. We all hope that atherosclerosis and coronary-artery disease will eventually turn out to be largely due to things we do rather than to what we are. Our study is relevant—not so much, perhaps, because it constitutes a small link in the growing chain of epidemiological knowledge of these disorders, but because it demonstrates that such studies, involving statistically valid population samples and the simultaneous testing of multiple variables, are feasible and accurate within the limitations of currently available methods.

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Ann Arbor, Michigan, U.S.A.

9. Epstein, F. H., Carol, R., Simpson, R. *Amer. J. clin. Nutr.* 1956, 4, 1.

10. Epstein, F. H., Simpson, R., Boas, E. P. *ibid.* p. 10.

11. Toor, M., Katchalsky, A., Agmon, J., Allalouf, D. *Lancet*, 1957, i, 1270.

12. Brunner, D., Loebel, K. *ibid.* p. 1300.

13. Dudley, S. F. *Proc. R. Soc. Med.* 1936, 30, 57.

TIME AND THE NURSE

SIR,—How I agree with points raised in your annotation of Nov. 30, particularly the reference to routine temperature recording.

In most cases, pyrexia has subsided within forty-eight hours nowadays and four-hourly temperature charts are a waste of time and paper, with a few obvious exceptions. Often as a busy night-nurse have I cursed the day-sister who demanded four-hourly temperature recordings on as many as 20 out of 30 medical patients, most of whom were admitted for investigation only.

Now as a ward-sister myself, I only wish that student nurses showed the same competence in the use of the sphygmomanometer, when occasion demands, as they do in the use of the thermometer.

I also suspect that many housemen and registrars, already indoctrinated into the orthodox methods, would recoil with horror at the thought of a blank temperature chart for any patient.

Regular temperature recording is just one of many hospital rituals, which also include daily weighing of healthy infants, waking patients at the crack of dawn, and polishing ward floors to mirror-like smoothness.

These customs are handed down to us with our black stockings and “dog collars” as a relic of the Victorian era. We can dispense with much of this and yet maintain our standard of bedside nursing.

Discretion compels me to remain anonymous, lest genteel cries of “Mutiny!” rain down upon my head from members of the nursing hierarchy.

MUTINEER.

PNEUMOCONIOTIC NEUROSIS

SIR,—It is axiomatic that before change is thought necessary in an existing procedure, one should prove beyond reasonable doubt that it is ineffective; it is always advisable to back reforming zeal with a few practical suggestions as to what changes are necessary and why. I submit that Dr. Macdonald (Nov. 23) has not even begun to prove his case; if he will look back through the correspondence on this subject he will see that I never asserted that “all was well” in regard to pneumoconiosis assessment. It is indeed fairly obvious from the letters of Dr. Dünner and Dr. Hardy (Nov. 23) that there are discrepancies between the standard of assessment of pneumoconiosis boards throughout the country; according to them some boards appear to be human enough to make an assessment against the radiographic evidence—which at any rate attests to their pursuit of fairness if to nothing else.

Meanwhile, Dr. Macdonald thumps the tub of destructive criticism and assures us that better things happen in South Africa, where geographical and climatic conditions are different. Great attention has been given by correspondents to the “initial radiograph,” which seems to have been seized upon as the sinister motif to the whole situation. It is only an initial measure done to prevent bias, since any doctor, even a member of a pneumoconiosis panel when acting outside his official capacity, can examine a patient and certify that he has pneumoconiosis on a chit to the local Ministry of Pensions, thus initiating normal procedure. This rather clumsy method of ensuring fairness seems to have given rise to serious misunderstanding. There are in addition four radiographic stages of the disease upon which, together with clinical examination, exercise-tolerance test, &c., compensation is finally based. But the very point I have tried to make is that these radiographs are no more responsible than any other part of the examination for the ultimate assessment.

I will not endeavour to refute Dr. Dünner and Dr. Hardy's assertion that the decisions of boards are based on the “strict legal definitions of pneumoconiosis within a general legal directive”. According to my information the boards are solely responsible for the diagnosis and disablement question. There is, of course, a prescribed list of dusty occupations together with a thoroughly badly worded pamphlet, *Medical Notes on Pneumoconiosis*, issued in 1951 by the Ministry of National