nearer to that of 15 mg. of morphine sulphate (11.4 mg. of anhydrous morphine).

Our own studies¹ have shown that 13.3 mg. of morphine sulphate contains the same amount of anhydrous morphine as 20 mg. of papaveretum, and that the effect of these two are indistinguishable clinically.—We are, etc.,

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REFERENCE

- ¹ Loan, W. B., Dundee, J. W., and Clarke R. S. J. *British Journal of Anaesthesia*, 1966, 38, 891.

Upgrading V.D. Departments

SIR,—The question of changing the name "venereology" to some other such as "genito-urinary medicine" was raised in your leading article (21 February, p. 447), by Dr. J. K. Oates (2 May, p. 296), and now by Dr. J. R. Seale (13 June, p. 670). Dr. Seale accepts the legal definition of venereal disease of 1916 and 1917 and tells us that most of his patients are not suffering from such diseases. Venereal disease is a very old medical term used to describe the various infections commonly transmitted in sexual intercourse. Presumably for legal purposes definitions have to be rigid, and in this instance definition was limited to the diseases of which the causative organisms had been isolated and of which it was possible to make a diagnosis beyond doubt. Dr. Seale must know that the great majority of his patients are suffering or afraid that they may be suffering from sexually communicable disease. Most of these patients have the same kind of problems—sense of shame and guilt, fear of disclosure to their families and others, fear that their spouses and children may be involved, and fear that their marriages may break up in consequence of their indiscretions. This and the need to trace and examine the potentially infected contacts are the justification for making this a separate specialty and keeping it so. Since Venus was the goddess of love, one supposes that the word "venereal" was coined to describe the diseases in a nice way. But because of moral judgments the word soon acquired an unpleasant connotation. If we devise a substitute for this word, as Nelson¹ tried to do in 1936, it will soon acquire the stigma and certainly do nothing to help our patients.

Those who are advocating this change should look more deeply into the matter. Let us suppose that the V.D. department at a major hospital, having built up an efficient organization and considerable resources over the years, has its name changed to the department of urogenital medicine, or genito-urinary medicine, or what you will. While the present incumbent remains the emphasis on venereal diseases and their control will continue. When he departs a fine opportunity presents for an able and ambitious young physician with, say, a special interest in renal diseases. The department is ready made for him; his colleagues will approve and the appointments committee will be impressed with his ability and qualifications. When appointed he will, if true to form, hand over the subject of

V.D. to his junior assistant, who will be left to struggle without guidance and without inspiration. Take also the question of a university chair in the subject. It is overdue and, one supposes, it will not be long delayed. Establish a chair of venereology and the subject should derive great benefit; establish a chair of urogenital medicine and the subject will be relegated to the background.

Dr. Oates, in common with a number of other venereologists of his generation, worked in the best-known V.D. clinic in the world, at the Johns Hopkins Hospital. Dr. Oates should know what happened when the distinguished director of that department decided to broaden the scope of its activities and call it a department of chronic diseases. While he remained all was well, but when he went the subject of venereology was soon eliminated. The almost equally famous clinic directed by Dr. John Stokes at Philadelphia suffered a similar eclipse on the venereological side when he was succeeded by an able dermatologist. The consequence has been that no single clinical venereologist of note has emerged in the United States during the past twelve years or more. The Public Health Service has had to struggle with a near epidemic of venereal diseases without trained clinicians and to place chief reliance on epidemiological methods, which is like trying to fly on one wing.

Dr. Oates, Dr. Seale, and those who think like them can call their departments what they like and can adopt various other devices to ease the unhappy lot of their patients. But they should not deceive themselves and they should remember that they have been appointed to diagnose, treat, and control the spread of venereal diseases. I hope they will be proud to remain venereologists. To a great extent their present status has been achieved by the efforts of their predecessors¹ who were willing to enter a despised and neglected subject and, by giving of their best and leaving the subject better than they found it, earned the respect of their colleagues. Only in this way is respect to be achieved—not by calling ourselves something else and changing labels on doors.—I am, etc.,

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REFERENCE

- ¹ Nelson, N. A., *American Journal of Syphilis, Gonorrhoea and Venereal Diseases*, 1936, 20, 448.

SIR,—I would like to give support to Dr. J. R. Seale's views (13 June, p. 670) regarding a change of name for the two hundred and more V.D. clinics in Britain.

Apart from the greatly increased incidence of gonorrhoea, the other two venereal diseases—syphilis and chancroid—are now numerically in small numbers. The bulk of the work in clinics very largely consists of other sexually transmitted conditions, particularly genito-urinary infections such as trichomoniasis, candidiasis, genital warts, non-specific urethritis, etc. Many general practitioners are loath to refer patients to a department labelled "V.D. clinic" or "special clinic." Even explanations that the venereal diseases are but one facet of the work makes patients refuse to take

the practitioner's advice. There is little doubt that these facts do not assist the control of the dissemination of sexually transmitted diseases.

There is no doubt that a change of name is indicated. Colleagues in all specialties and not least in genito-urinary surgery and nephrology are agreed on this point. The problem is to settle on a name. Of all the suggestions put forward to date, I would have thought that "department of genito-urinary medicine" would be the most accurate and appropriate for these establishments.—I am, etc.,

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Mycobacteria from Aquaria

SIR,—I was interested to read the leading article entitled "Hazard from the Home Aquarium" (23 May, p. 438).

We recently reported an aquarium-borne infection with *Mycobacterium marinum* on the lower arm of an owner of tropical fish.¹ In this connexion we had the opportunity of examining two fishes of the species *Gymnocorymbus ternetzi* from his aquarium. Both fishes had died from a disease causing granulomas of the liver. One of them had a tubercle on the tail, consisting of similar granulomas made up of macrophages and showing central necroses. Staining according to a modified Ziehl-Neelsen technique with a shorter decoloration time revealed numerous acid-fast bacilli in the granulomas mainly in the necrotic foci. Cultures for *Mycobacterium marinum* were positive. Epidemiological investigations showed that the bacteria had been introduced into the aquarium with water fleas. *M. marinum* was recovered from the mud of a pond in which the patient caught water fleas for feeding his fish.

The histological changes in the tubercle and liver of the fish in our case are compatible with those which Aronson found in spontaneous tuberculosis of salt-water fish.² He observed areas of focal necrosis in the liver in which the liver cells were replaced by large mononuclear cells. The cells were to a great extent necrotic and the foci contained numerous clumps of acid-fast bacilli.

As in our case the man and the fish carried the same bacteria and the fish suffered from a disease agreeing with Aronson's fish tuberculosis, there is further reason to consider *Mycobacterium marinum* and *M. balnei* to be identical.—I am, etc.,

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REFERENCES

- ¹ Månsson, T., Brehmer-Andersson, E., Wittbeck, B., and Grubb, R., *Acta Dermato-Venereologica*, 1970, 50, 119.
² Aronson, J. D., *Journal of Infectious Diseases*, 1926, 39, 315.

Respiratory Symptoms and Washing Powders

SIR,—It is of great interest to read Dr. M. Greenberg and others' (13 June, p. 629) account of a survey of workers exposed to derivatives of *B. subtilis*. It would appear

that there was more evidence of obstructive respiratory disease in his sample of workers than among the workers exposed to similar products examined by ourselves.¹ On the other hand, the F.E.V.₁/V.C. ratio is not age standardized and differences in age structure of the two populations might to some extent account for the difference. Also, a higher proportion of the workers in his survey were sensitized to the *B. subtilis* derivatives than in our own series.

Levels of enzyme concentration in air are not given, but exposure to higher levels for longer periods might account for the difference.—I am, etc.,

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REFERENCE

- ¹ Newhouse, M. L., Tagg, B., Pocock, S. J., McEwan, A. C., *Lancet*, 1970, 1, 689.

Hypotension Caused by L-Dopa

SIR,—I have read with interest the report of Dr. D. B. Calne and his colleagues (21 February, p. 474), regarding hypotension caused by L-dopa in patients with Parkinson's disease, and the subsequent comment of Professor J. H. Burn (7 March, p. 629) on the mechanism of this effect. If patients on chronic L-dopa therapy are in fact secreting dopamine from their sympathetic nerve terminals, then a beta-adrenergic blocking agent such as propranolol should antagonize this effect, since dopamine appears to have predominantly beta-adrenergic properties.¹ This line of reasoning led us to attempt the administration of propranolol to a small number of patients on L-dopa treatment who had developed symptomatic hypotension. We observed in several an abrupt cessation of symptoms and a partial correction of the hypotension. One patient had a particularly striking response.

A 46-year-old woman with Parkinson's disease of six years' duration presented as a severe primarily bradykinetic parkinsonian with minimal tremor and only mild rigidity. She required assistance to arise from a sitting position, stood with a markedly stooped posture, and could barely walk with small shuffling steps festinating very severely. Blood pressure was 110/70 sitting or standing. The pulse was 80/minute and regular.

Treatment with laevodopa was begun in August 1968 at a daily dose of 1 g. When the daily dose was increased to 2 g., the patient complained of "dizziness" when standing. Blood pressure fell to 95/70 on one occasion with no increase in pulse rate. Her symptoms were controlled with the use of elastic bandages on the legs and the daily dose of L-dopa was gradually increased to 3, 4, and finally 5 g. At this dose level there was appreciable improvement. Rigidity was abolished to be replaced by a general hypotonia. However, she was still occasionally dizzy and blood pressure fell to 70/50 on standing—despite the use of elastic bandages and an abdominal binder. Continuing symptoms of postural hypotension compelled a further reduction of the dose to 3.5 g. per day, on which she continued for a year without any change in hypotensive symptoms.

Various amphetamines including dextroamphetamine, ephedrine, propylhexedrine, and hydroxyamphetamine were tried with negative results. Propranolol was then begun in November 1969 at a dose of 10 mg. t.i.d. There was a prompt and striking subjective response. The patient had no further syncopal episodes

and no longer experienced light-headedness on standing up. The daily dosage of laevodopa was increased to 4.5 g. without a recrudescence of these symptoms.

The patient was then readmitted and after a period of observation, propranolol was discontinued. Blood pressure readings were consistently lower without the propranolol. Faintness and light-headedness and several brief syncopal episodes occurred despite the use of elastic bandages in the lower extremities. These symptoms were so severe that the patient was unable to assume the upright position. After a three day hiatus, propranolol was resumed. Blood pressure readings returned to the levels previously observed and symptoms of orthostatic hypotension became milder and less frequent. No further syncopal episodes occurred. Propranolol was withdrawn a second time for several days with similar results. Advantage was taken of the apparent protection afforded by propranolol to increase the dosage of laevodopa by gradual increments ultimately to a total daily dose of 8 g. per day. The dose of propranolol was also gradually increased to 30 mg. given four times daily. On this regimen the patient still had some occasional low blood pressure readings, circa 90/60 or 80/50, but rarely experienced faintness or light-headedness and had no syncopal reactions. The pulse remained slow at 50–60/min. The increased dose of L-dopa yielded some additional significant improvements. The patient has maintained these improvements for the past six months.

While further experience is necessary to assess the clinical value of propranolol in treating L-dopa-induced hypotension, our preliminary experience indicating that it can at least partially antagonize this effect of L-dopa gives some support to the views advanced by Dr. Calne and colleagues, and by Professor Burn.—I am, etc.,

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REFERENCE

- ¹ Goldberg, L. J., Talley, R. C., and McNay, J. L., *Progress in Cardiovascular Disease*, 1969, 12, 40.

Pseudo-obstruction of the Large Bowel

SIR,—The article by Mr. P. K. Caves and Dr. H. A. Crookard (6 June, p. 583) on pseudo-obstruction of the large bowel was very interesting. I was surprised, however, that in no case was the simple procedure of a diagnostic enema applied, where there was no contraindication such as evidence of peritonitis. It is possible on occasion to avoid laparotomy in these circumstances by its use.—I am, etc.,

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Smallpox Vaccination

SIR,—I am delighted by your leading article (9 May, p. 311) supporting our position on curtailing routine smallpox vaccination. This same conclusion was reached by Dick,¹ when reviewing data from Great Britain.

I am concerned that you support the thesis that a safer vaccine may solve most of our problems in smallpox control. The death rate from smallpox vaccination is in the order of one per million primary vaccinations, and the incidence of severe complications such as post-vaccinal encephalitis is only about five per million primary vaccinations. It is evident that only a massive trial involving several millions of vaccinees could convincingly demonstrate greater safety of a new strain. If greater safety of a new strain will be difficult to prove, acceptable efficacy will be equally so. At present there is so little smallpox in the world, even in India and Pakistan, that it would be exceedingly difficult to carry out a field trial of a new vaccine to prove its efficacy.

While I am delighted to see the academic interest in attenuated strains, I feel we should not expect them to be introduced for routine use. The only way to reduce the current toll of vaccination complications in non-endemic nations, then, is reduction of the number of vaccinations performed.—I am, etc.,

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REFERENCE

- ¹ Dick, G., *Progress in Medical Virology*, 1966, 8, 1.

Epidemic Malaise

SIR,—Dr. D. C. Poskanzer (16 May, p. 420) rightly draws attention to the need to apply the principles of epidemiology to the study of epidemics of benign myalgic encephalomyelitis (epidemic neuromyesthenia).

The basic features of the condition as seen in a general practice in North London between 1964 and 1966 by Dr. Betty D. Scott (17 January, p. 170) were identical to those described by Wallis^{1 2} in a general practice at Dalston, Cumberland, between January and August 1955. Extensive virological studies carried out at the Public Health Laboratories in Carlisle and at Colindale were negative. Apart from the frequent finding of cortical depression, objective neurological changes were found in 20% of cases and indicated a widespread involvement. Upper motor neurone lesions with extensor plantar responses were recorded in two cases; ataxia, nystagmus, positive Romberg test, and a patchy motor weakness with brisk tendon reflexes and hypotonic muscles were found in others, and muscle atrophy with ulnar nerve lesions occurred in eight patients.

The latter complication was probably responsible for my first contact with the epidemic, as the number of ulnar nerve lesions referred to my electrodiagnostic clinic in the neighbouring county of Durham early in July 1955 suddenly increased five-fold (none was from Dalston). Ulnar nerve involvement had been reported in previous epidemics in Iceland³ (1948) and New York State⁴ (1950). Later in 1955 and in 1956 a few cases with more generalized motor weakness were seen in Co. Durham. On electromyography the characteristic grouping illustrated in Drs. C. P. McEvedy and A. W. Beard's article (3 January, p. 7) was not