Family Doctor Team

SIR,—Mr. R. Crossman commences his recent Green Paper on the future of the National Health Service by stating that "the service should be centred on the family doctor team." Is this just another piece of political hypocrisy, employing a sentimental term which the public will like, or can Mr. Crossman really be so misinformed as to be unaware that the family doctor concept will soon have disappeared entirely from British medicine, and the centre will therefore have fallen out of his wonderful future health service.

Already in the industrial areas one's only chance of seeing the doctor of one's choice—presumably a member of this "family doctor team"—is to make sure of confining one's illnesses to office hours. Unfortunately most serious illnesses seem to have a bad habit of occurring outside these hours, when one will be seen by some totally disinterested stranger, who can hardly be regarded as a member of the team, even as a reserve. The Monday morning ward round in hospital, such as I have just done, consists very largely in discharging quite unnecessary admissions sent in by these doctors during the weekend.

Am I criticizing the doctor? Not at all. I am criticizing a system which encourages real or imaginary illness in every possible way. Not only does it cost nothing to visit the doctor but to be "off sick" is often a considerable financial advantage. Add to this the fact that satisfaction in a day's work well done seems to be a dying philosophy, and one can see why a doctor in an industrial area can retain his sanity only by farming out his patients for a large part of the time. The Welfare State is gradually strangling itself with cords of its own making, and Mr. Crossman's new deal will, in my opinion, have no effect but to draw these cords tighter.—I am, etc.,

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Drugs for Dysentery

SIR,—I should like to comment on the article on drugs for dysentery in the series "Today's Drugs" (4 April, p. 36). This article presumably advises specific drug therapy in every case of bacillary dysentery. There is, however, a school of thought, with an increasing number of adherents, which considers specific, as opposed to symptomatic, treatment quite unnecessary in a high proportion of cases.

Sonne dysentery, at least as seen in Britain, is generally a mild and self-limiting disease, and probably any favourite non-specific remedy such as chalk and opium mixture or a drug like Lomotil (diphenoxylate hydrochloride and atropine), though I agree not bactericidal, comforts the patient if required. Indeed in such cases the administration of sulphonamides or antibiotics may be quite useless therapeutically, may even prolong excretion of the causative pathogen as in salmonella infections, and may well stimulate the production of more and more multiply drug-resistant enterobacteria. In the severe cases, particularly in young babies and elderly patients, it would

seem wise to have in vitro drug sensitivity tests done as soon as possible as a guide to the drug of choice.

Here is a brief summary of our findings in in vitro sensitivity tests of *Shigella sonnei* strains of British origin in 1969. Of 509 strains tested, 59 (11.6%) were resistant to sulphonamides alone, 39 (7.7%) resistant to sulphonamides and streptomycin, 102 (20.0%) resistant to sulphonamides, streptomycin, tetracycline, and ampicillin, 75 (14.7%) resistant to sulphonamides, streptomycin, and ampicillin. Other drug-resistant combinations were found.

The drug resistance pattern does vary from area to area and from time to time within an area. Strains in other countries behave similarly and this applies equally to *Sh. flexneri*. These findings would emphasize to me that in vitro tests with representative cultures in any geographical area should be the main guide to specific drug therapy. I personally find it increasingly difficult to recommend any one drug as a panacea for bacillary dysentery.

So far as supportive treatment is concerned, in moderate dehydration when the patient does not require the intravenous control of electrolyte balance, I wonder if giving dilute normal saline and potassium chloride has any advantages over simply increasing the quantity of any palatable thirst-quenching fluid. In the tropics, if oral potassium is considered necessary, a very suitable acceptable substitute is coconut milk, as has been shown for the supportive treatment of cholera.

I would ask two questions arising from the article. It is stated that sulphonamides are widely and *successfully* (the italics are mine) used in the tropics as their cheapness is an advantage. What are the criteria of success? Is nalidixic acid an antibiotic—I am, etc.,

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Compression and Limb Blood Flow

SIR,—We read with interest the study by Mr. M. Spiro and others (21 March, p. 719) on the effects of externally applied pressure on femoral venous blood flow.

Their findings are of relevance to those concerned with postoperative management, especially in relation to the role of stasis in the development of deep vein thrombosis. There is however, a technical inaccuracy in their introduction which is probably the result of trying to condense into a few words the published findings of others. The authors state "... with a 15° head-down tilt on the table stasis was largely eliminated. Venous flow velocity (Wright and Osborn, 1952) and possibly total limb blood flow were increased (Beaconsfield and Ginsburg, 1955) by this manoeuvre." We did not say this in the article quoted. In our study¹ the body was maintained at the horizontal; when one limb was raised to 15° peripheral flow increased, but when the leg was further raised to 45° flow decreased. This reaction is the response of vessels to an alteration in limb posture, and is a local phenomenon since the flow in the contralateral limb is unaltered. Such local reactions must be distinguished from circulatory reflexes which

occur on both sides and are induced by change in body posture. These changes are secondary to baroreceptor activity, as instanced by the reduction in limb flow which occurs when the upright posture is assumed, or the increase in peripheral blood flow which is observed when the body is tilted from the vertical to the horizontal position.^{2,3}

While the article by Mr. Spiro and his colleagues is an interesting piece of investigation its application to clinical practice is, at the best, of extremely limited value. The authors themselves state: "... that if external compression is to continue to be used in the treatment and prevention of deep vein thrombosis its application must be carefully controlled. The optimum pressure usually lies between 5 and 12 mm. Hg but may well vary from patient to patient. . . ." We can affirm from years of experience that in a busy surgical unit this type of control, while possibly desirable, is certainly not practical, and the prophylaxis against deep vein thrombosis depends much more on good preoperative preparation, avoidance of inadvertent peri-operative trauma, gentle surgery, immediate postoperative ambulation, and, of course, "a little bit of luck."—We are, etc.,

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- 2 Bridgen, W., Howarth, S., and Sharpey-Schafer, E. P., *Clinical Science*, 1950, 9, 79.
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SIR,—I read with interest the article on femoral vein blood flow by R. M. Spiro and others (21 March, p. 719). I would be interested to know whether the authors had considered the effect of posture on bandaging and venous flow, for it may well be that their conclusions about bandaging are only valid for the horizontal posture. Venous flow is affected by venous resistance. This, in turn, is affected by the calibre of the veins, which is itself a function of the transmural pressure. The addition of a further hydrostatic pressure within the veins (when assuming an upright position) to the normal dynamic pressure tends to dilate the veins and therefore to lower the resistance they offer to flow. It is interesting to note that in their results maximum flows occur when the bandage and hydrostatic intravascular pressures are approximately equal; the fall off in flow, with increased pressure, is presumably due to compression of the veins and therefore a rise in venous resistance. One would expect, theoretically, in the author's experimental model that this fall in flow would be delayed in the vertical position until the bandage pressure equals the new (now increased) venous pressure.

This prediction is supported by my own research involving the clearance of ²⁴Na from the subcutaneous tissues of the foot. In these experiments external bandaging pressure was applied in a similar fashion to that described by your authors. Figure 1 demonstrates that in dependent posture the