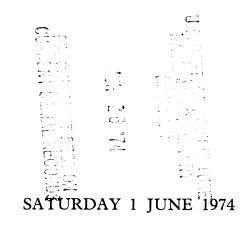
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Hospitals and Developing Countries

SIR,—Having been present at the Jamaica monopolize between 10 and 20% of the Medical Congress I read with interest your report of it (11 May, p. 313). Being concerned with problems of providing medical care in developing countries and being only too aware of the ways in which the achievement of a reasonable level of care in developing countries can be frustrated, I should like to draw attention to two items in the report.

In the symposium on primary health care teams (p. 317) Dr. M. S. Ragbeer, the Dean of the University of the West Indies Medical School, informed us that the present health expenditure in Jamaica was \$20 per person for a population of approximately 2m people—that is, \$40m annual expenditure (£16m). On p. 323 you report the visit to the new Montego Bay Hospital, which serves 60,000 people and whose estimated annual running costs are £1½-3m per year. In other words, 60,000 people (3% of the population) are going to national health budget for the provision of hospital services alone.

This kind of development is an absolute barrier to the provision of the kind of health care which developing countries need. The implications of building an £8m hospital in this area should have been clear from the beginning and the temptation to build such an establishment should have been resisted. I believe passionately in the importance of surgery, being a surgeon, but I have no doubt whatsoever that the priority in developing countries is to provide primary health care. It is the responsibility of the surgical or medical specialist not to demand such a level of facilities that primary health care is denied to the population.—I am, etc.,

A. D. Roy

Department of Surgery, The Queen's University, Belfast

other hand, at any time during the postoperative period can readily be explained by Virchow's hoary old triad in the early postoperative days, followed by detachment of an old clot at a later date when the triad is no longer operative.

The hypothesis I propose is that the clinical manifestations of deep venous thrombosis are caused by emboli from calf veins causing sudden mechanical obstruction at points of confluence with larger veins. In this way a "silent" clot can suddenly move to obstruct a much greater drainage area and so produce swelling of the leg in the absence of renewed activity of the clotting mechanism. If this hypothesis can be upheld its significance in relation to therapy is obvious and far-reaching.-I am, etc.,

JOHN CHARNLEY

Centre for Hip Surgery, Wrightington Hospital, Wigan, Lancs.

Deep Venous Thrombosis: a Hypothesis

SIR,—I would like to put forward for criticism a hypothesis regarding the events leading to the clinical manifestation of deep venous thrombosis.

The feature which to me has always seemed unsatisfactory in current concepts of the aetiology of postoperative deep venous thrombosis is that it is necessary to assume a process of active clot formation persisting for three or four weeks after an operation such as total replacement for hip arthrosis. Operations of this type concern patients who are selected as representing perfectly healthy subjects with a mechanical disorder of the locomotor system and without any known systemic involvement likely to favour thrombosis. Deep venous thrombosis developing "out of the blue" three weeks after total hip replacement occurs at a time when the patient is at the height of rehabilitation and it would be necessary therefore to postulate a very active clotting mechanism to override the prophylactic mechanism of rehabilitation-more active than early in the postoperative period when stagnation will enhance a feeble tendency to clot. The events leading to embolism, on the

Abdominal Decompression in Pregnancy

SIR,—I read your leading article on this subject (4 May, p. 238) and your evaluation of the papers by Coxon et al.¹ and ourselves² with interest. Your evaluation led you to opine that the two papers came to "somewhat contrary conclusions" and to infer that the seemingly beneficial results of decompression which we reported might have been influenced by the fact that our trial "was not a blind one" while that by Coxon et al. who reported essentially negative findings, "was more subtle, larger, and doubleblind with randomization."

I would point out that analysis of our control and treatment groups for various factors which might have influenced our