the reason for this peculiar phenomenon appears to be unknown. It seems, however, to be purely transitory.

Lastly I think that, in the interests of the patients and for the sake of the reputation of the operation, fenestration ought only to be performed by surgeons already perfectly trained in aural surgery and with considerable experience of fenestration on the cadaver. It appears to me that professional ethics forbid the performance of this very specialized operation by surgeons who have not the necessary training.—I am, etc.,

Hôpital St. Antoine, Paris. PAUL DUBS.

Cutaneous Cancer

SIR,—Your leading article on the above subject (May 22, p. 986) recalls an experience which may amuse your readers. In the early summer of 1937 the Bill which became subsequently the great Factory Act of 1937 was considered in Standing Committee. The Committee consisted of 70 Members of Parliament drawn from all Parties, and for the greater part of its activities I was the only medical Member. There were 27 sessions of the Committee. The Bill bristled with medical problems, and, being the only Member with medical experience, I was a frequent speaker. Sir John Simon, as Home Secretary, was in charge of the Bill; my seat was directly behind his, and duologues between him and myself were constantly repeated. One of our most serious disagreements came over the schedule of industrial diseases subject to notification.

The Bill was a consolidating Bill, superseding all previous Factory enactments, whose purpose and effect would be to legislate for a generation to come; the previous main Act was dated 1901. In that Act a number of diseases had been scheduled as notifiable the incidence of which had materially diminished in 36 years, and other diseases had assumed greater importance. I pointed out that phosphorus poisoning, notifiable in 1901, had almost ceased to exist in 1937, but cutaneous cancer showed a sharp upward curve of frequency. I contended that phosphorus poisoning should be omitted from the notifiable schedule and cutaneous cancer included, notification of which, I argued, would lead to earlier recognition and treatment and thus prevent the serious consequences which were frequent in overlooked or neglected cases. I was overruled with the argument that it was "administratively convenient" to transfer as many of the items as possible from the 1901 to the 1937 Schedule, and the Schedule of notifiable industrial diseases in the 1937 Act (*vide* Section 66) remains, as in 1901, restricted to "lead, phosphorus, arsenical or mercurial poisoning, or anthrax."—I am, etc.,

House of Commons. E. GRAHAM-LITTLE.

Risks of Dicoumarol Therapy

SIR,—In your annotation on the risks of dicoumarol therapy (May 22, p. 988) the work of Glueck and her colleagues[1] is quoted on combined heparin-dicoumarol therapy in coronary thrombosis, the results in a treated series of 44 patients compared with 44 controls being as follows:

Thrombo-embolic complications

	Present	Absent	Total
Treated	3	41	44
Controls	12	32	44
Total	15	73	88

	Died	Recovered	Total
Treated	9	35	44
Controls	20	24	44
Total	29	59	88

It is then stated, "Though these figures may not satisfy statistical demands, it is only by the accumulation of the results of careful clinical studies that final decision will be reached on this important problem." Readers may be puzzled as to why significant differences are dismissed as not satisfying statistical demands, so it may be helpful to point out how the misconception has arisen, a misconception that is due to the authors of the paper rather than to the writer of the annotation, for he has simply quoted their conclusions.

In the body of the original paper the data referred to 25 treated patients and 25 controls, and among these neither of the differences shown above, though favourable to the treated, is significant, as is pointed out in the conclusions. A short addendum is then appended giving results for a further 19 treated patients and 19 controls, observed after the manuscript had been submitted. The authors, however, do not point out in the addendum that with the addition of these new results the previous conclusion as to the statistical significance of the differences requires modification.

Both the tables shown above give $\chi^2 = 5.143$ (using Yates's correction). Thus in both instances the chance of drawing two such diverse samples from a homogeneous population is no more than 1 in 43, the usual conventional level of statistical significance being 1 in 20. The authors selected patients for the two series by the sound method of choosing alternate subjects for treatment. They remark in their conclusions (on the total of 50), "Although the series of cases is too small, and the variables of the disease itself too wide to make statistical analysis significant . . ." This might be taken to imply that even if the results were significant they would still be doubtful, owing to the variability of the disease, as to whether differences might not be due to some accident in the selection of alternate patients rather than to the results of treatment. They have, however, tested a considerable number of variables, and scrutiny of their tables shows (in the first 50 at least) that none could account for the more favourable experience in the treated group. If then detailed analysis of the 38 additional patients yields the same results as the original series, it would seem that a good, though not of course overwhelming, case had been made out for the beneficial results of anticoagulant therapy.—I am, etc.,

London, W.C.1. J. A. FRASER ROBERTS.

REFERENCE

1 *Amer. Heart J.*, 1948, **35**, 269.

Peptic Ulceration

SIR,—I am very glad to hear Mr. C. Jennings Marshall's approval of the analogy between the pains of chronic peptic ulcer and of chronic ulcer of the leg (March 13, p. 522), but am sorry that he is dissatisfied and has reservations, seen in his question, "Surely the production of pain in the peripheral nervous system has a mechanism different from that in the viscera?" Then again, it appears that he is dissatisfied with the secretory function as a middle term in the pain mechanism, so he selects the motor function. This is not very strong positive evidence in favour of his middle term and I am sure he is dissatisfied with this position also. He rightly indicts the physicians (I would have added the physiologists) for not properly correlating the various factors. Mr. Jennings Marshall has shown a tendency to advance beyond old hypotheses. If he would only move a little further in the same direction he could rid himself of his dissatisfactions.

His first step must be to answer his own question. The nervous system is (to use the words which he applies to pathology) "one and indivisible." Surely, therefore, the production of pain in the peripheral nervous system has a mechanism no different from that in the visceral. There may be a quantitative difference because pain fibres are more sparse and less efficient in the viscera, but surely there is no essential difference.

Next, he insists that "food relationship of ulcer pain inevitably introduces gastric function as a middle term." Food relationship inevitably introduces gastric function, but not necessarily as a middle term. It only becomes necessary to introduce the awkward middle term if the stomach functions are conceived in terms of only a secretory and a motor system. But, if it be remembered that the stomach has in addition a functioning circulatory system, gastric function will be found already included in the first term. In the leg, the circumulcerous cellulitis and circulatory changes directly explain the pain, without the interposing of directly painful motor behaviour of the leg.

A further advantage of these forward steps is that Mr. Jennings Marshall will find that his own favourite "most

potent cause," the ebb and flow of inflammation, will be found adequate to explain the symptoms without the interposition of direct motor causes. Moreover, he will no longer fear the Greeks, even when bearing a question about pain-relief from haemorrhage, for he will be able to discard "muscular atony from acute anaemia" as an unsatisfactory explanation, either in peptic ulcer or congestive dysmenorrhoea. Such prostration, as is implied, does not necessarily accompany pain-relief.

Finally, if he leaves out the myogenic middle term, he will not be called upon to explain how nebulous motor disturbances can directly cause pain in ulcer patients, while definite and violent motor disturbances fail to cause pain in obstructive or regurgitant vomiting after gastro-enterostomy, or in obstruction at the duodeno-jejunal flexure due to carcinoma. Nor will he be asked to explain why, in pyloric obstruction, "the very powerful peristaltic waves which can be seen through the abdominal wall are rarely accompanied by any discomfort" (Hurst and Stewart).—I am, etc.,

Sydney, Australia. V. J. KINSELLA.

Acute Intussusception in Childhood

SIR,—I read the article on acute intussusception in childhood by Drs. Brenda Morrison and Donald Court (April 24, p. 776) and letters in subsequent issues. The authors of the article state that it is very difficult to satisfactorily examine a healthy baby. With this I cordially agree, and it is particularly difficult during the contractions of the intussusception (when it is hard and most palpable) because then the baby yells most lustily. It is at this stage that, barring accidents, 100% of the babies should be saved.

There is only one way by which one can make absolutely certain that an intussusception is or is not present—that is by a bimanual exploration with the right forefinger in the rectum, under anaesthesia. Quite light anaesthesia is sufficient and chloroform the best anaesthetic. If the case is in hospital the child can be examined in the theatre and either the abdomen opened or the surgeon retire with a perfectly easy mind. The authors of the article and subsequent correspondents make no mention of this method. By carrying it out one can explore every part of the abdomen in a child under 2 years.—I am, etc.,

Great Yarmouth. LEONARD LEY.

SIR,—May I add a further point to Drs. Brenda Morrison and Donald Court's timely article (April 24, p. 776) calling for earlier diagnosis in acute intussusception in children? Under the heading "Signs in the Abdomen" insufficient stress is given to palpation of the abdomen. In the initial stage of the condition, and before obstruction and peritonitis have occurred, the mass is only palpable during active peristalsis, and it may be necessary to palpate the abdomen several times before a clear and unmistakable cylindrical tumour is felt. Usually colic occurs at that instant, but it may not be severe. Only by *repeated palpation in the early stages,* when vomiting and flexion of the thighs or the abdomen are the presenting signs, will the initial and pre-inflammatory stage of intussusception be diagnosed.

Mention is made in the article to the similarity in description in most British textbooks, and I would refer the authors of the article to that given in the 4th and 5th editions of the textbook on paediatrics published by Messrs. E. and S. Livingstone.—I am, etc.,

London, W.1. BRUCE WILLIAMSON.

Pain in Phantom Limbs

SIR,—I was very interested in Dr. J. Donaldson Craig's letter (May 8, p. 904) on "Pain in Phantom Limbs." The subject of painful stumps is a difficult one. As a medical student I do not remember a single instance of my teachers telling me anything about the subject; and as to the question of phantom limbs the paucity of the literature concerning this distressing condition speaks for itself. Space does not permit of my dealing with painful stumps and I will therefore only briefly discuss my views on phantom limbs.

In the intact body there is a continuous stream of impulses bombarding the cortex from the sensory nerve endings in the muscles of the body. This bombardment is evenly balanced on both sides and in a state of equilibrium. When a limb is amputated a neuromuscular imbalance immediately results (see diagram). This will play a great part in future motor activity of the individual and will influence his phantom limb.

It is the readjustment of the remaining muscles of the body to the altered state which now throws additional strain on the sensitized cerebrum and makes the previously existing state of equilibrium so liable to be upset. It is the constant effort of the central activity to maintain this equilibrium between the continuous stream of impulses from the remaining part of the body which makes a phantom limb an ever-present possibility and brings the awareness of the absent member to consciousness.

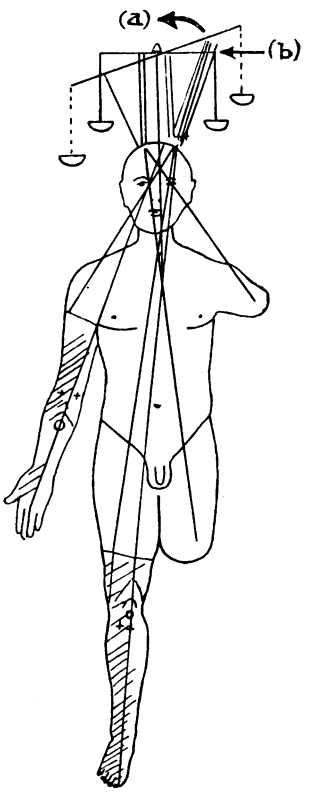

Diagram showing (*a*) imbalance produced by amputation, and (*b*) normal state of equilibrium.

The capacity of the individual's sensorium to readjust itself to the new conditions of his body and its new environment after an amputation may or may not give rise to the varied manifestations of a phantom limb. The physical alterations and mental fluctuations in his subsequent life will act as stimuli and as predisposing factors to cause a central release which will result in pain in the phantom. All amputations are associated in the mind of the individual with a varying degree of pain. This is not dependent on whether pain has preceded the amputation. Further, the individual will react to this pain according to the degree of subconscious pain which already exists in his sensorium, and this phantom pain will be further modified according to his level in the phylogenetic scale. It has been noticed that highly intelligent, sensitive patients are more prone to phantom pain than the more plethoric, unimaginative individuals.

It has been observed that wearing an artificial limb is often a comfort to the patient with a phantom pain, and tight bandaging at night, when his phantom pain becomes aggravated, relieves this pain. In fact, many individuals often hold on firmly to their stumps with both hands in order to relieve the pain. This pressure has the effect of restoring temporarily the balance of the impulses which impinge on the sensorium and so relieves the pain. These observations influence treatment. The patient himself often doubts the reality of his own sensations. Fear of accusation of insanity makes him reticent to talk about his symptoms and he prefers to hide them until he can no longer bear them. This may result in gross mental disturbance, and the sufferer of a phantom limb is often regarded as psychotic.

The patient who shows the slightest indication of phantom pains should be told that we believe he gets these pains—that they are very real to him; and, since he has lost his limb, from his point of view there is no foundation for the stimulus now. Eventually the pains will cease, which indeed they often do.

I agree with the statements that all the operative measures on these conditions usually fail, and I would like to point out that local injections are of very fleeting value. A simple and effective method of treatment by firm bandaging of the stump bears out the hypothesis which I have postulated above: "Sensation removed by amputation must be replaced by further sensation." I would further like to say that phantom limbs are not analogous to causalgia. Amputations of the fingers are not infrequently accompanied by causalgic pains, while phantom pains are rare in fingers because there has been no loss of bulk stimuli. Causalgic pains are not relieved by pressure: phantom pains are.—I am, etc.,

Queen Mary's Hospital, Roehampton, London, S.W.15. LEON GILLIS.