have been correlated with hypoplasia incidence in 1943, had almost ceased to be so by 1947. The correlation between structure and caries incidence in 1943 was not, then, mainly one of cause and effect, and thus most of the evidence for the authors' theory disappears.

The orthodox theory, which the authors consider obscure, explains the improvement as due to the change from a high-sugar and white-flour diet to one of whole vegetables, low sugar, and high-extraction flour. The 5-year-olds examined in 1947 had been exposed for longest to the bulkier, more detergent, and low-sugar wartime diet. Apart from the lesser tendency of such diet to stagnate and ferment about any teeth, it also acts by promoting bone growth, and so preventing overcrowding of the teeth, and by removing other potential stagnation areas by attrition. Such factors begin to work almost from the time of weaning.

The correlation between caries and hypoplasia in 1943 and 1945 can only be explained by assuming that both are due to a common cause. The education of the mother might be a factor, but so little is known of the extent or the causes of variation in sugar consumption by young children that it is almost useless to speculate on the possible causes of the correlation. That sweet consumption varied widely before rationing is well known, and I have found that even now sugar and sweet consumption varies much more widely than one would expect.

It would not be worth while pointing out the fallacies in the authors' reasoning if they merely urged that we should consume more calcium, phosphorus, and vitamin D. However, they conclude that such is the *best* available way of attacking caries. Most well-informed students of the subject believe that a lower sugar consumption or higher fluorine intake, or use of a higher extraction flour, would, even separately, be far more effective, and it is even possible that caries could be entirely prevented at all ages by such measures used in combination.—I am, etc.,

London, N.W.6. R. B. D. STOCKER.

Stainless Steel Wire

SIR,—Messrs. A. Lawrence Abel and Alan H. Hunt (Aug. 21, p. 379) are to be congratulated on a splendid series of cases sutured with stainless steel. They have added materially to the evidence that one of the simplest, soundest, and probably the best method of repairing herniae is by darning. Also, they have shown that the use of a suitable non-absorbable suture obviates a preliminary attack on the patient's thigh or the effort to squeeze a last reluctant centimetre out of a strip of the external oblique aponeurosis.

I like their repair for incisional herniae in which they darn across the gap without drawing the edges together, but I shall try that principle only in the very large defects. I believe a continuous darn is unnecessary for femoral herniae, several interrupted sutures serving similarly and being easier to insert in the depths. I endorse their views on the use of a non-irritating, non-absorbable suture for closing abdominal-wall incisions, and particularly is it necessary to use such sutures in infected wounds and in cachectic subjects.

Unfortunately their incidence of sepsis in either clean or infected incisions is not mentioned, there being merely a hint that sepsis may possibly have occurred sometimes. This does not assist the judgment of others. I have tried using wire sutures and have found them rather difficult to manage, especially in the deeper tissues, and if one's assistants vary frequently the entanglements increase. At skin level wire is fairly easy to use. No doubt as the years go by the art, as in the case of these authors, can be successfully mastered at depth. My preference is for a suture which serves the same purpose but neither kinks, fractures, nor grates. In practice I find that nylon sutures are equally non-irritating, and behave well in infected tissues. A double strand of No. 7 is strong enough for any closure or reparative procedure on hernia, and is easy to work with.

My own histological studies are on material from reopened wounds in humans, and for what any such studies are worth they indicate a very low level of tissue disturbance by nylon. My clinical results have shown a very low incidence of sepsis in clean and infected wounds, personal figures for clean darns in inguinal herniae being over 150, with sepsis once (in a scrotal haematoma). Figures for infected wounds are being assessed on other types of cases. Criticisms of nylon are not justified when derived by inference from microscopical studies of various sutures buried in the smaller domestic animals, the only real test being what happens in cases. One cannot help wondering whether before darning with steel it is advisable to be sure that one's patient is neither a mariner nor an air pilot, and so liable to set the compass spinning after treatment! I presume the chances of being struck by lightning are not increased.

We are not yet finished in our hunt for the best suture material. Braided nylon, which I have recently tried, is beautiful to work with. Our present ideas of the virtuous monofilamentous strand may give way to a belief that the good behaviour of nylon and steel in infected wounds depends not only on their low irritation of the tissues but also on the fact that they are, unlike silk and cotton, non-wettable substances. Bacteria hiding between the strands of silk from hungry leucocytes has never strongly appealed to me as a theory. The answer may yet lie in braided plastic or braided metal. Perhaps when one of these industries is nationalized the resulting shortage may decide the issue for the other.—I am, etc.,

Oxford. G. E. MOLONEY.

Diabetic Coma

SIR,—The letter by Professor D. M. Dunlop and Dr. J. B. Donald (Aug. 14, p. 352) contains an important warning in connexion with the treatment of diabetic coma with large doses of insulin. This is that hypoglycaemia in an unconscious dehydrated patient is not always easy to recognize. This occurred in a patient, 67 years old, whom I treated recently for diabetic coma.

Using large doses of insulin intramuscularly as recommended by Professor R. H. Micks (July 24, p. 200), but unfortunately without immediate blood-sugar control (the patient was treated during the night), a fatal degree of hypoglycaemia was produced. This was only revealed by subsequent biochemical estimations of samples of blood taken at intervals during the night. The patient was carefully observed at half-hourly or hourly intervals for any change in her condition, and particularly for signs of hypoglycaemia, but none were detected. Continuous intravenous saline was given. Three hours before death a blood sample (estimated later) showed the blood sugar to be 17 mg.%, blood urea 78 mg.%, and blood chlorides 631 mg.%. At the commencement of treatment 14 hours previously the values were blood sugar 408 mg.% and blood urea 101 mg.%.

It is clear from such an experience that it is dangerous to give large doses of insulin to a patient in diabetic coma without facilities for *immediate* blood-sugar control. This particularly applies, as Professor Dunlop mentioned, when the treatment is in the hands of an inexperienced physician such as myself.—I am, etc.,

HOUSE-PHYSICIAN.

Lumbar Sympathectomy for Varicose Ulcers

SIR,—I believe that Messrs. John Borrie and E. Vernon Barling in reporting (July 24, p. 203) successful healing of four chronic varicose ulcers with lumbar sympathectomy have publicized a further remedy of this condition. I learned of it in 1946 from Dr. Marcus Werquin, of Paris, a former assistant of Professor Leriche. He told me that he had used lumbar sympathectomy effectively in these cases, but it was before he knew of the operative treatment of varicose veins.

Since that time I have been looking for an obstinate ulcer in which lumbar sympathectomy seemed to be necessary. I have met two ulcers in my fairly considerable experience in the last three years which resisted the remedies laid down by Thomas Baynton in 1797 and retaught so ably by Mr. Dickson Wright since 1929. The first man was not well enough for a sympathectomy, and I have had to admit failure: he had an ulcer which Baynton found resistant—i.e., below the external malleolus. In the second patient my friend, Dr. Kenneth Wolferstan, of Sunbury-on-Thames, is achieving the healing which I failed to obtain. I mention these because I believe that the patients who require lumbar sympathectomy for the healing of varicose ulcers are very few if the ordinary remedies are faithfully applied.

For instance, in reading the case reports of Messrs. Borrie and Barling's patients I did not think that they had received what could be regarded as an efficient operation for their varicose veins. In my experience no ligature, whether high or low, single or multiple, will permanently clear up varicose veins, ulcer, or eczema. I find that permanent effectiveness is attained only by high ligature, division of all the branches, and the certain destruction of the entire column of the internal or external saphenous vein from their beginnings at the malleoli to their endings. I think it is essential that treatment for varicose veins be thoroughly instituted before resorting to the attractive procedure of lumbar sympathectomy, which carries a mortality and will be followed by recurrence of the ulceration if the varicose veins, which are a progressive condition, continue to exercise their deleterious effects on the scar tissue about the ankle.

I agree with Messrs. Dickson Wright, Rowden Foote, Kenneth Wolferstan, and others that all varicose ulcers can be healed if we will apply the supportive bandaging ourselves frequently and faithfully. I think that, while lumbar sympathectomy has a place in the treatment of ulcerated legs when complicated by arterial and trophic conditions, it is a small one.—I am, etc.,

London, W.1. HAROLD DODD.

Genu Valgum

SIR,—Mr. H. A. Brittain's article on genu valgum (Aug. 21, p. 385) deals with the most common deformity of early childhood. Twenty-eight years ago, when rickets was still a common disease, it was the custom to take children with rachitic knock-knees off their feet or to put them in irons. Bony deformity and loss of muscle tone were the causal factors, and the latter was considered the more important. Immobilization in bed and to a less extent in irons aggravated the muscle weakness. As the orthopaedic surgeon rarely sees these cases until the children are 2 years or older, the aetiology and early treatment as seen and practised at an infant welfare centre may be of interest. These children were allowed to exercise freely, their heels and soles were wedged, and they were given intensive treatment for rickets. The results more than justified the break-away from more orthodox methods.

In the far more common non-rachitic cases of genu valgum I believe the lack of muscle tone is the primary trouble and the stretching of the internal lateral ligament of the knee secondary to it. I have been struck by the fact that I see a larger proportion of children with knock-knees in private practice than I did over 20 years at an infant welfare centre. The explanation would appear to be that the children of the comparatively well-to-do spend more time in their prams and less on their feet than the children of the poorer districts, who run about during most of their waking hours.

The preventive treatment of genu valgum is attention to the musculature of the child throughout infancy. I need not detail the necessary measures here, but it is a fact that many infants with poor tone have to be "taught" to use their muscles, and exercises in weight bearing should be practised from the third month onwards. This is particularly important for the heavy child. The first efforts at walking are made on a wide base which throws a strain on the internal lateral ligament of the knee—the better the muscle tone and balance the sooner the walking base narrows. If at 15 to 18 months there is any evidence of genu valgum, the heels of the shoes are tilted $\frac{1}{8}$ in., increased later to 3/16 in., and the "gramophone record" referred to by Mr Brittain is put on. Exercises are prescribed, especially those which strengthen the quadratus femoris. A favourite one is carried out as follows: The child sits on the floor in the crossed-leg tailor position facing his mother, who sits on a chair. She places her right foot as a steadying block against the inverted sole of the right foot of the child and her left to his left. She then clasps his outstretched hands and teaches him to rise slowly to the upright position and then return slowly to the sitting position, keeping the legs crossed and the feet inverted the whole time. At first she takes most of his weight, but less and less, until he can do the exercise without assistance.

At two years or a little later the tricycle (kiddy-car) serves, with one precaution, as good exercise for the leg and thigh muscles. The wooden bar which runs from the seat to below the handle-bars should be well padded, so that the child shoves on the pedals with the knees well apart and the legs and feet inverted. A similar arrangement can be made on a toy motor-car or engine with pedals. The more fortunate child with a pony has the perfect apparatus.

It was surprising to hear that Mr. Brittain has dealt with 8,000 cases of genu valgum in 15 years. Preventive measures or early treatment should reduce the numbers reaching the orthopaedic surgeon almost to zero.—I am, etc.,

Esher, Surrey. ALLAN HAMILTON.

Procaine Metabolism

SIR,—Procaine metabolism has been investigated during the last 15 years with growing intensity. Althoff[1] recently summarized facts and theories in a German publication. Thus, I hope, some findings dealing with the mode of action of procaine will be of general interest.

Recently Burgen and Keele[2] investigated the quantitative metabolism of procaine in the cat by determining procaine and *p*-aminobenzoic acid blood levels. They found rapid and extensive hydrolysis after intravenous injections. Neither Burgen and Keele nor other workers in this field (Hazard and Ravasse,[3] Dunlop[4]) published data on the second product of procaine hydrolysis, diethylaminoethanol. In experiments on frog's muscle (rectus abdominis), using a modification of the Chang and Gaddum technique (Soehring and Wiedow[5]), we confirmed the decrease in the reaction to acetylcholine described by Riesser and Neuschloss[6] when adding procaine to the bath. We found that this effect is followed by a marked increase in the tonus of the muscle, while acetylcholine excitability simultaneously is restored. Using small concentrations of procaine the response to acetylcholine can surpass the initial effect. These phenomena can be produced in the same way by equivalent concentrations of diethylaminoethanol. Hauschild and Landbeck[7] found an important sensibilization to adrenaline when traces of diethylaminoethanol were given.

Modern procaine therapy thus appears as a special form of sensibilization to adrenaline and acetylcholine, due to diethylaminoethanol set free by hydrolysis. The mode of action of this interesting compound cannot be discussed here in full detail. Its activity on surface tension[8] may be considered, as well as chemical composition, in some step of the choline metabolism.[9]—I am, etc.,

University of Hamburg. KLAUS SOEHRING.

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Gynaecological Psychiatry

SIR,—I have read the article by Mr. Linton Snaith and Dr. Brenda Ridley (Aug. 28, p. 418) with great interest. There is, however, one problem fundamental to investigations of this kind, so far as the question of female orgastic ability is concerned, which the authors have left unanswered and unsolved. What is a "normal woman"? All investigators agree that 40–50% of their women patients show lack of orgasm ability, and most authors regard this as a deficiency, physical or, mostly, psychological, that can and must be cured.

But before making this assumption would it not be our duty to prove to ourselves that the "normal woman" is endowed with an orgastic ability as universal and automatic as the man? In my opinion this assumption, so universally made, cannot be proved. As Terman has pointed out in *Psychological Factors in Marital Happiness*, "Woman's susceptibility to orgasm is either an evolving trait not yet fully established in the phylum or a regressive trait on its way out." And since we can know next to nothing about the remote psychological past of the human race our only way to answer this question is to investigate whether or not female orgastic ability could have played any part in facilitating the survival of the human race. It does not seem to have such significance.