

whose first performance is above this limit give no response to vitamin A.

The test is sufficiently simple to be applied to intelligent children of 6 years, so short that it is suitable for serial examinations, and the results are unaffected by practice.

We have pleasure in acknowledging the co-operation of Dr. I. Leitch and Mr. D. Lubbock in organizing and carrying through this investigation.

REFERENCES

- Adams, D. (1929). *Med. Res. Coun. Spec. Rep. Series No. 127*.
 Aykroyd, W. R. (1930). *J. Hyg.*, **30**, 357.
 Berry, G. A. (1886). *Edinb. med. J.*, **31**, 1025.
 de Gouvea (1883). *v. Graefes Arch.*, **29**, Abth. 1, 167.
 Edmund, C., and Clemmesen, Sv. (1936). *Deficiency of Vitamin A and Visual Dysadaptation*. Levin and Munksgaard, Copenhagen; Humphrey Milford, Oxford University Press.
 Forbes, J. (1811). *Edinb. Med. and Surg. J.*, **7**, 417.
 Fridericia L. S., and Holm, E. (1925). *Amer. J. Physiol.*, **73**, 63.
 Holm, E. (1925). *Ibid.*, **73**, 79.
 Jeans, P. C., Blanchard, E., and Zentmire, Z. (1937). *J. Amer. med. Assoc.*, **108**, 451.
 — and Zentmire, Z. (1934). *Ibid.*, **102**, 892.
 — (1936). *Ibid.*, **106**, 996.
 Jeghers, H. (1937). *New Engl. J. Med.*, **216**, 51.
 Lohmann, W. (1913). *Disturbances of the Visual Functions*. John Bale, Sons and Danielsson, London.
 Lythgoe, R. J. (1926). *Med. Res. Coun. Spec. Rep. Series, No. 104*. Privy Council (1863). Fifth Rep. of M.O. for 1862.
 Snell, S. (1876). *Lancet*, **1**, 8.
 Spicer, H. (1892). *Ibid.*, **2**, 1387.
 Tansley, K. (1931). *J. Physiol.*, **71**, 442.

MODERN METHODS OF TREATMENT OF CLUB-FOOT*

BY

DENIS BROWNE, F.R.C.S.

Surgeon, Hospital for Sick Children, Great Ormond Street

It is very flattering but slightly uncomfortable for one who is not an orthopaedic surgeon to be invited to open a discussion at an orthopaedic meeting. However, in the present interesting but undeveloped state of surgery it is probably useful for someone to work at right angles to the accepted lines of specialization, limiting his cases by their age rather than by the system of the body that is affected. So, taking advantage of my position as an amateur orthopaedist, I will start with the platitude that before beginning to treat a condition it is of advantage to decide what that condition actually is. In the case of talipes I believe that this can be done.

The Cause of Talipes

The teaching at present is that the cause of talipes is unknown. I believe, on the contrary, that it is possible to establish beyond doubt the existence of a definite category of congenital deformities due to mechanical causes, and that the classical "talipes equino-varus" is the middle range of a certain series of these deformities. It has been an interesting task working out the limits and possibilities of this category, and I have given elsewhere a summary of the evidence I have collected on the subject (1936). The result has been to convince me that these congenital deformities of the feet are only understood in all their degrees of severity and all their complications and variations when looked at in this light; and that this understanding is a not entirely negligible help to their treatment.

* Read in opening a discussion in the Section of Orthopaedics, including Treatment of Fractures, at the Annual Meeting of the British Medical Association, Belfast, 1937.

It is interesting to examine the causes that prevent the general recognition of what I believe to be an important and definite pathological category. First among the difficulties comes the complication of the argument. Even to state it properly would be a long day's work, and no one could possibly imagine a busy surgeon devoting a day to listening to it. No journal would publish such a statement except in a form compressed to the verge of unintelligibility; a book on the subject might have a sale of a dozen copies; and meetings of this sort are in their hurry a poor substitute for the leisurely courts of argument of the mediaeval schoolmen.

Then the mental process employed is one that is almost entirely neglected in medicine. We have become used to depending on the fruitful and valuable experimental method, in which we produce artificially certain happenings, observe them, and draw conclusions. But there is another method used in science. No one can play experiments on the planets, but yet we know a good deal about them. We have learnt it by reasoning out what would happen were a certain hypothesis true, and comparing these results of abstract thought with what actually does happen.

It is on this second plan that I have tried to investigate the group of congenital deformities to which I consider talipes to belong; and it is because of its unfamiliarity in medicine that very few of my colleagues seem to see the line of argument or admit its strength. The reason for using it is that it is notoriously difficult to interfere in the processes of gestation without wrecking it. I can think of certain experiments that would confirm my results, but to me they appear simply unnecessary cruelty. And, after all, the Chinese have done some fairly relevant experiments on the female human foot. Shortly, the argument is that if one works out in the abstract what would be the consequences of certain things going wrong in the delicate correlation between the growing foetus and its expanding surroundings, one can deduce a series of deformities exactly duplicated over a period of years in the outpatient department of any children's hospital.

Certain stock objections to this theory are made, usually with a kind of hint that the objector could easily think of more, and stronger ones, if it were worth his while. They all seem to me to show a failure to grasp what the hypothesis attacked actually is. For instance, Middleton (1934) says that the negative is proved once for all by the occurrence of talipes in foetuses 3 months old. I believe that compression can begin its work when the foetus is two millimetres long. Burns and Ellis (1937), remarking that there are many cogent arguments against it, give as a sample of them that in the early days of pregnancy the foetus can move freely within the uterus. I have been unable to find any work illuminating this point, but granted the freedom of movement to be as great as it is usually thought to be, this freedom seems a curious argument against the possibility of displacement. And, anyhow, it is no part of the hypothesis that the displaced feet have ever been in normal position; the probability is all the other way. If any one really wants to attack my hypothesis, here are some of the main lines of argument which have to be explained away.

1. The position of the normal foetus before birth.—I believe the drawings of the foetus in textbooks of obstetrics to be inaccurate in certain very important points.

2. The position and range of movement of the feet in the newborn infant.

3. The occurrence of "pressure dimples"; a most interesting line of argument.—Their variations according to degrees of pressure, and normal or abnormal positions of the limbs, seem

to me to be a conclusive proof of the argument. And finally there is their absence in arthrogryposis, a condition I believe to be due to increased hydraulic pressure.

4. Mutual deformities, where two deformed parts that have been pressed together fit into each other like the die and the coin stamped by it.

5. The correspondence of shape of the feet with the position of the joints of the legs, in the different types of talipes.—A variation on this argument is the difference between the feet in double talipes, which I believe to be due to compression (not necessarily abnormally high compression) in a cross-legged position. The invariable difference between the degree of deformity in the feet is just what would correspond to the greater pressure on the outer leg.

6. The variation of the states of the joints and muscles with the degree of pressure as shown by the severity of the deformity and the extent of the dimpling.—The compression theory does not call for any new pathological process of spontaneous muscular degeneration to explain this; it merely assumes that the tissues affected behave as they are known to behave after birth in similar circumstances of pressure and immobility.

7. The much greater frequency and severity of moulding of the feet as compared with moulding of the hands.—This would be expected, as the feet are so much less sheltered.

8. The very complicated but to me satisfactory explanation of arthrogryposis in terms of hydraulic pressure, with its strong confirmation from a disease of sheep.

9. A new confirmation from brain surgery that I have just thought of.—In that syndrome of lumbar spina bifida and talipes which I believe to arise from pressure before the spinal cord has formed, it is well known that hydrocephalus very often occurs. All the cases which develop it have a peculiar deformity of the medulla and upper part of the spinal cord, exactly as if the brain had been squeezed out of the foramen magnum like tooth-paste out of a tube (Russell and Donald 1935). This interferes with the circulation of the cerebro-spinal fluid, and so causes hydrocephalus. It is hardly necessary to point out that, given mechanical pressure on the foetal skull, one would expect this very effect.

Treatment

MANIPULATION

It is curious how much of the present surgical practice can be upset by a simple statement of the aims of treatment. If one states the platitude that one is trying to obtain the normal range of movement of an infant's foot it means that the talipes foot is not corrected until it will go up with the slightest pressure into full calcaneo-valgus—that is to say, till the back of the little toe touches the outer side of the leg. Anything less means imperfect reduction of the deformity, though I have before pointed out that all the classic methods of treatment stop when a range of movement rather less than that of the adult foot has been obtained.

The second crucial point about manipulation is that this full correction (note that it is not an "over-correction") must be obtained at the first manipulation, and that it is gained by a final compression of the whole tarsus as the foot is squeezed against the leg. This primary violent compression of the tarsus has an effect totally different from that of the gradual "coaxing" pulling round of the foot that is at present taught. When the foot is slowly pulled round it gives at its weakest point, the junction of the tarsus and the metatarsus, leaving almost unaffected the centre of the deformity, where the strong bony block below the tibia is left bent and tilted to the curve of the inner concavity of the uterus that has originally moulded it.

I know from experience that of these two principles of manipulation the ambition of gaining full calcaneo-valgus will be accepted by very few people, and the gaining of it

at the first manipulation by fewer still. But I am convinced that in them is the essence of the matter.

Finally, it is this same forcing up of the foot into calcaneo-valgus that is the one and only manipulation necessary in the after-treatment. The more intelligent mothers can be taught to do it, but it is extremely difficult to get trained masseuses to carry it out. Without exception they explain that it will "spoil the arch of the foot," and persist in a curious variety of manoeuvres which they have been taught, and which seem to me completely useless. After eighteen months or so it usually becomes impossible to push the foot up completely, but a range of movement up to 45 degrees above the horizontal should be kept permanently.

SPLINTING

Once more I beg leave to indulge in a platitude: it is that function is more important in any disability than structure. By correcting the position of the foot and keeping it at rest structure alone is treated. To correct function, the two most important divisions of which are the muscle balance of the foot and the mental impression of how it should be used, it is necessary to allow (and even to stimulate) movement in the corrected position. The classic methods of splinting talipes ignore function. But even in dealing with structure they appear to be inefficient: none of them hold the foot corrected in relation to the body as a whole, though it is on that relation that its proper use depends.

My present model of splint, about the twelfth of its line, is fairly well known. It depends on the mechanical principle that it is possible to control the position of one foot by means of the other. I could easily design one that would be more efficient mechanically, but it would be much more expensive, and would be even less efficiently used. To get the full valgus position that is essential I pack up under the anterior and outer part of the foot with layers of thick adhesive felt. The elastic pressure gained by this is very efficient in moulding the foot; but I have never seen it applied in my way by anyone else. Points in applying it are:

1. Bandage each foot separately. I have been rather depressed by seeing an illustration in a recent book showing a foot being bandaged in with the cross-bar in position (Burns and Ellis, 1937).

2. Fasten the sole-plate first, with the foot packed up in such a manner that the leg-piece projects outward away from the leg. Then when this in its turn is bandaged, it pulls the foot into valgus. I believe it is impossible to overdo this valgus position.

3. If there is a normal foot arrange it in the normal position, pointing outwards from the sagittal plane about 20 degrees. The talipes feet should be arranged to point more outwards, into the "Chaplin position" (here, for the first time, is a true over-correction).

4. Do not be alarmed by a fair amount of reactionary swelling after the first violent manipulation. I have never seen any trouble from this.

5. Never cease the splinting because of pressure sores or skin irritation. Both are rare, neither is important; but if a half-corrected foot is set free it will become far more obstinate than if it had never been touched. (Compare the results of putting on plaster-of-Paris at irregular intervals.) If a baby with simple talipes is put in splints in the first few weeks, full correction and subsidence of reactionary swelling should be attained by the age of 4 or 5 months. Then the sticking-plaster can be left off, and the child supplied with night boots of the same pattern. If the mother carries out instructions and pushes the feet up three times a day to full

calcaneo-valgus there should be no further trouble. I have often found that children treated in this way learn to walk earlier than their normal brothers.

OPERATIONS

The correction of the deformity by moulding with the splint is only possible while the bones are so soft that they will yield before an amount of pressure that the skin can bear without sloughing. On cases which have passed this stage I have for some time been operating according to principles that go flatly against the usually accepted teaching. Sir Robert Jones, for instance, says that as much correction as possible should be gained by wrenching and plaster (that is to say, by stretching the soft parts) before touching the bones; and then that as little as possible should be removed. Now it seems to me that a foot that has been wrenched, or one in which the soft parts have been cut or shifted, is in a state of tension that tends towards relapse. Stretched fibrous tissue and, even more, scar tissue produced by cutting tendons and ligaments, when once left free contract with force and persistency, reproducing the original deformity in a more intractable form. But bone, when once its shape is changed, has no such elastic recoil. In consequence I have been acting on the principle that the soft parts should be left untouched and unstretched, while enough bone is removed to allow the foot to be brought into about the degree of calcaneo-valgus possible to the civilized adult. The situation of the bone removed depends on seeing the deformity as I have described it; it is in a quite different place from the ordinary wedge resection of the tarsus, which has such a deservedly bad name. A most important and usually overlooked part of the deformity is that the external malleolus is too far from the ground, and bone must be removed to let this down to its proper position as well as to allow the foot to be straightened. It might be called a "crescent resection" of the tarsus, the base of the wedge removed being of this shape.

The results I have obtained with this operation have modified my opinion, gained from experience of other methods, that the use of open operations in this condition was to repair the effects of other open operations. I am not as sure of its value as I am of that of the splint I have described, but I am inclined to think the principle sound. If anyone should wish to try it the main points to remember are to go up as high as possible in front of the ankle, and not to be afraid of overdoing the valgus by removing the outer part of the os calcis.

BIBLIOGRAPHY

- Brockman, E. P. (1930). *Congenital Club Foot*, Wright, Bristol.
 Browne, Denis (1936). *Proc. roy. Soc. Med.*, **29**, 1409.
 Burns, B. H., and Ellis, V. H. (1937). *Recent Advances in Orthopaedic Surgery*, Churchill, London.
 Middleton, D. S. (1934). *Edinb. med. J.*, **41**, 401.
 Russell, D. S., and Donald, C. (1935). *Brain*, **58**, 203.

N. Fastuline (*J. Medical (Ukraine)*, 1937, **7**, 1, p. 75) has investigated the bacterial flora of the air at different altitudes in the Caucasian Mountains. He used Mickel's tubes fitted with Janet's syringes. Each test was carried out with the filtrate of 200 litres of air. The mountain air was found to contain saprophytes and no bacteriae of faecal origin. Above 10,000 feet there was a predominance of mycobacteriae over bacteriae. Among the mycobacteriae the author found penicillium and mucor, among the pigmented cocci *Sarcina lutea*, *Micrococcus roseus*, *Micrococcus citreus*, and *Sarcina rosea*, and among the bacilli *Bacterium mycoides*, *Bacterium subtilis*, and *Bacterium mesentericus*.

MODERN METHODS OF TREATMENT OF CLUB-FOOT*

BY

E. P. BROCKMAN, F.R.C.S.

Orthopaedic Surgeon, Westminster Hospital

In the past the treatment of congenital club-foot has undergone many changes, and in the future it will probably experience others. No generation is ever satisfied with what its forbears have done; this is a right and proper attitude to adopt, otherwise progress or improvement would cease.

Whatever our views as to the pathology or treatment of this deformity, and they are many and varied, it still remains a condition which, however satisfied the surgeon may be, is capable sooner or later of making him appreciate his own limitations; for the orthopaedic surgeon who has treated this deformity over any number of years must time and again have had patients to whose parents he has said, "This child's feet will not relapse," and yet perhaps one, two, or even several years afterwards the patient has been brought to him with the story that the feet began slowly to turn inwards again. You have already listened to an account of the deformity of congenital club-foot in all its aspects. I therefore propose to confine my remarks almost entirely to its treatment.

Some years ago I spent a considerable time upon an investigation of congenital club-foot, and I have not changed my views as to its pathology and aetiology, except that in regard to the former I have realized that the changes in shape of the individual bones of the foot are more marked than I thought at that time.

You probably all know my hypothesis that congenital club-foot is due to a congenital dislocation of the head of the astragalus, that this dislocation is due to an atresia of the socket into which the head of the astragalus normally fits, and that therefore the deformity results from an intrinsic and not an extrinsic cause. I believe this hypothesis is as good as or better than any other, and that it explains more satisfactorily the difficulty we experience in our treatment of the deformity. If the deformity were due entirely to the effects of outside pressure, as is still commonly believed, it should be a simple matter, by exerting a similar pressure in the reverse direction, to correct the deformity and hold it corrected until such time as it was possible for the child to maintain the corrected position by its own muscular power. This I think we must all admit is not in practice what does happen.

From the methods of treatment usually advocated there cannot be any question that in a great many cases the condition is not cured by simple means. We all get cases in which the feet respond to treatment, but we have plenty in which the results are unsatisfactory.

Treatment in the Infant

After a long experience of the method of treatment upon which I was brought up—namely, repeated forcible manipulation of the deformed foot and fixation of the foot in the corrected position with plaster strapping—I have come to the conclusion that it is a poor method in every way when compared with the one advocated by Mr. Denis Browne. It needs frequent repetition several

* Read in opening a discussion in the Section of Orthopaedics, including Treatment of Fractures, at the Annual Meeting of the British Medical Association, Belfast, 1937.