off does not, in some cases, lead ultimately to the dying out of the root. At all events, if we can rob epilepsy of its most terrible features, as Dr. Marshall Hall suggested, by the operation of tracheotomy; or relieve spasm of the glottis by the inhalation of chloroform; or assuage the agony of angina pectoris, and arrest impending death, by a few whiffs of nitrite of amyl, as seems to have been experimentally proved by Dr. Brunton; or cut short the neuralgic spasm by the application of galvanism; although we may still be unable in any degree to cure the morbid conditions on which these symptoms severally depend, it must be admitted that we are very far indeed from being powerless for good. At the same time, there are many of these diseases over which we have no kind of influence, and many in respect of which our curative power is illusory. Among these latter I may name chorea. How far this is a functional disease, may of course be disputed. We know its almost, if not quite, invariable association with cardiac valve disease, and that there are plausible arguments which have been especially dwelt upon by Dr. Hughlings Jackson in favour of the dependence of the choreic symptoms on embolism of the smaller arteries of the brain, probably those of the corpus striatum. But there are difficulties in the way of accepting this explanation, and it undoubtedly needs the confirmation of morbid anatomy. Provisionally, therefore, I think it may still be looked upon as a functional disease.

Now chorea, as we all know, is essentially a malady of childhood, which ends as a rule in recovery in the course of a few weeks, yet leaves the patient with a liability to relapse. There is no disease for which so many remedies have been vaunted as specifics; and there is no remedy, probably, which has been employed in it, whose use has not been attended with what may seem to have been satisfactory results; indeed, the equality of results from the most opposite plans of treatment is a remarkable feature in its history, and has struck many physicians besides myself. Sydenham treated his patients with alternate bleedings from the arm to eight ounces (more or less), and purgings (each being repeated three or four times), followed by a course of purgatives and alteratives; and his patients recovered. Iron, if not first recommended, was, at least I believe, first brought into prominence as a remedial agent in chorea by the late Dr. Elliotson, who administered it in the form of the insoluble sesquioxide, and in large doses; and his plan of treatment was long followed, and found successful. But, curiously, iron has fallen into comparative disuse as an antichoreic, since physicians have replaced in their prescriptions that almost inert powder by other ferruginous preparations of greater solubility and potency. Zinc, again, is a remedy which of late years has enjoyed a wide reputation; and, like the sesquioxide of iron, has been given in large doses; but the mode of administration which has been adopted in regard to it, is one which (except in an occasional fatal or incurable case) could not avoid leading on to a favourable result; it is to commence with small doses, and to increase them day by day as the stomach becomes tolerant of the drug, until a certain effect appears to have been produced upon the patient as indicated by the amelioration of the choreic symptoms, and then the dose is diminished. It is scarcely possible to conceive a plan of treatment more calculated to deceive the physician. Few remedies have found more favour than that last hope for all intractable diseases, arsenic; and it can at least be said of it, that it has proved as successful as any other medicine that has been tried. Those which have been enumerated are among the chief, but they form a very small proportion of the total number of remedies for chorea, which have been from time to time recommended, adopted, and fallen into more or less disrepute. I, like others who have been some years in practice, have tried many of the plans of treatment which have from time to time cropped up; and have especially employed largely the ferruginous, the zincic, and the arsenical methods, and I am free to admit that, for a long time, I had nearly perfect faith in their efficacy; and it was only when I began to compare my own results, and to study the experience of other people, that I began to doubt. Some years since I made some experiments with regard to the treatment of this disease, a summary of which I published in the second volume of the *Practitioner*. I adopted the plan of first allowing my patients to remain a week under observation in the hospital, in order to ascertain, by personal observation, if their disease were undergoing abatement, or if it were stationary, or becoming worse, and at the end of that time of selecting those only for experiment who were obviously not convalescent. "In the second place, I gave each remedy on which I wished to experimentalise alone, and for successive periods of a week or ten days or more, considering that, by the end of such a period, the patient would be fairly under the influence of the drug which he had been taking during it; and that, if that drug possessed any specific influence over the choreic movements, that influence might be expected then to have manifested itself in some appreciable degree. In the third place, when decided signs of convalescence showed themselves, I discontinued either at once, or at the end of the allotted

period of seven or ten days, the medicine, during the administration of which convalescence had been established, and gave in its place either a little compound infusion of gentian or a mere placebo, assuming that, if that medicine had brought convalescence about, its discontinuance ought to be followed, if not by some exacerbation of the disease, at all events by impeded convalescence."

The results of these experiments I summed up as follows.

"They all point more or less clearly to the same conclusions: the first and most obvious being that, so soon as convalescence has begun, it makes little or no difference as regards the progress of the case, whether the specific treatment which had been employed be continued or disregarded; the second, and less obvious one, perhaps (at least, if only the cases there quoted be considered), being that, the association of any specific remedy with the commencement of convalescence is a purely accidental coincidence"; and I added "that, to the best of my belief, my experimental cases, if judged by the event, may be regarded as cases of fairly average success." Since that time I have rarely used, or at all events have rarely used persistently, any specific plan of treatment, but have contented myself, for the most part, with the administration of some simple tonic and dietetic, and hygienic measures calculated, as I have believed, to restore the patient to, or maintain him in, good bodily health, and I cannot perceive that I have been in any degree less successful than I was formerly.

As regards *Diseases due to the Action of Poisons, and those originating in Excess, Deficiency, or Improper Quality of Nourishment*, I need say little here. The principles of their treatment are for the most part well understood; although unfortunately, for many reasons, we fail in a large number of cases to bring them into successful operation. It may, however, be worth while to remark, that often, when poisonous substances introduced from without are acting within the system, we are able, before we actually get rid of the poison, before we actually cure the disease, to obviate some of the more serious symptoms which it produces, and thus not only to bring relief, but to prolong life, and it may be save it. Thus, there is no doubt, that with the aid of opium we may relieve the colic due to lead; and thus also, by various means, we may to some extent neutralise the specific influences of the more potent poisons.

[To be concluded.]

PROPOSITIONS IN ANIMAL MECHANICS.

By the Rev. SAMUEL HAUGHTON, M.D., F.R.S.,
Medical Registrar in Trinity College, Dublin.

THE complete mathematical proofs of the following propositions will appear in my forthcoming work on *Animal Mechanics*.

1. Each muscle is constructed in relation to its joint, in such a manner as to perform one kind of work only; and it performs that work to maximum advantage.
2. The number of muscles employed is determined by the number of distinct actions required from the limb.
3. The shape and form of the bones employed are the necessary consequence of the shape and power of the muscles in action.
4. The smallest muscle in the combination is as carefully adapted to its conditions of maximum work as the largest muscle.

From these propositions, supposed to be extended to the action of every muscle and joint, it appears to me to follow as necessary consequences—

1. That a foreseeing mind planned the type of the limb and of its actions.
2. That, the idea of the limb and of its necessary actions being given, the number, shape, and arrangement of the necessary muscles can be calculated and predicted with as much certainty as an astronomer can predict an eclipse.
3. That the shape and arrangement of the bones follow, of necessity, from the necessary arrangement of the muscles.
4. That any alteration, however slight, in any part of the combination of bones, muscles, and joints, would entail a loss of work, and lead to a less perfect mechanism.
5. That, hence, the permanence and stability of each species (so far as relates to bones, muscles, and joints) is absolutely secured, on the principles so admirably laid down by Mr. Darwin.
6. That the profound study of the mechanism of joints lends no support to the postulate that the similarities in the bones, muscles, and joints of animals, may be explained by common descent from a supposed common ancestor.