than 1 per cent. effective against strongylus. In these same larger doses, with fasts of at least twenty-four hours, the treatment is highly effective. In one case, where the chenopodium and linseed oil were given simultaneously, the treatment was apparently 100 per cent. effective against strongyles; in another case, where the linseed oil was given two hours after the chenopodium, the treatment was 100 per cent. effective against cylicostomum and 96 per cent. effective against strongyles; in another case, where the chenopodium was given in divided doses followed by linseed oil an hour after the last dose, the treatment was 100 per cent. effective against cylicostomum and 95 per cent. against strongylus.

SUMMARY.

Contrary to what has been supposed, the removal of strongyles from the large intestine of the horse presents no great difficulties. The remedy of choice is oil of chenopodium, which displays an efficacy of 95 to 100 per cent. when given to horses fasted thirty-six hours and given in doses of 16 to 18 mls, in one dose or in divided doses, accompanied by a quart or a litre of linseed oil or followed one or two hours later by this amount of linseed oil. The small worms, cylicostomum, are more readily removed than the large, red palisade worms, strongylus, probably due to the fact that strongylus attaches to the mucosa and cylicostomum does not. Turpentine appears to be the second choice of the remedies tested. In the doses used, iron sulphate and tartar emetic gave very poor results and promised little of value in the treatment of strongylidosis.

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SCRAPIE.

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IN the June number of the *Journal of Comparative Pathology and Therapeutics* of this year (p. 102) there appeared an article on "Scrapie" by Sir John M'Fadyean, in which, amongst other things,

my views relative to this disease¹ were subjected to criticism. The object of this paper is to deal with the objections raised therein, while at the same time some new points relative to the disease are submitted.

Sir John's article commences with a discussion of the historical aspect of the subject. This section is borrowed largely from a lecture by Sir Stewart Stockman delivered at St. Boswells and printed in the *Journal of Comparative Pathology and Therapeutics*, Vol. XXVI., 1913, pp. 317-327, as also in the *Scottish Farmer* for 8th November 1913. Readers of my report (which was completely accessible to Sir John) will note that the much fuller treatment of this aspect of the subject given there lends no support whatever to the view expressed by Sir Stewart Stockman, and quoted with approval by Sir John, that the disease first appeared in England about 1799 and that it was introduced by Merinos. Indeed, the disease was definitely known in England as far back as 1732 (*vide* Report, p. 7), while the Merinos charged with the bringing in of the disease were introduced between 1780 and 1790.

My intention here is not to give a systematic discussion of the subject, but rather to deal with points as raised by Sir John. The next point, therefore, which merits consideration occurs on pages 102 and 103 of the article in question. Here Sir John states that "the disease finds the conditions most favourable for its spread in the large hill flocks," and "that the existence of the disease may be concealed for years will be readily understood when it is remembered that it is especially prevalent on the large tracts of hill pasture where throughout their lives the sheep are seldom or never seen except by the owner and his shepherds." This is not the case, as the disease at the present time especially occurs on low ground arable farms in Northumberland and Roxburgh among half-bred stock which could not exist under the conditions specified by Sir John.

In the section which deals with the diagnosis of the disease the following statement is made: "Lastly, it may be said that in actual practice the fact that the disease when once it has appeared in a flock tends to claim an increasing number of victims as time goes on greatly reduces the chance of error in diagnosis. In other words, the succession of cases suggests, if it does not prove, that the disease is contagious." Passing over the alleged equivalence of these two sentences, it may be stated, relative to the question of contagiousness, that other quite different interpretations are possible, and, indeed under the special circumstances here existent, are much more probable. These will be dealt with later.

Sir John now proceeds to deal with the etiology of the disease. He remarks (p. 108) that "one may unhesitatingly put aside the views which ascribes the disease . . . to inbreeding, to special predisposition inherent in particular breeds, or to peculiar delicacy of constitution resulting from what is vaguely termed irrational or unnatural methods of breeding and rearing sheep. All opinions of that kind have been arrived at by following false scents." This

¹ "Investigation into the Disease of Sheep called 'Scrapie,'" by J. P. McGowan, M.A., M.D., B.Sc., M.R.C.P.E. (from Edinburgh and East of Scotland Agricultural College). Wm. Blackwood & Sons, Edinburgh, 1914.

is a dogmatic attitude for one to take up who elsewhere (p. 102) hopes "that properly planned investigations will before long clear up many points which are at present very obscure, especially with regard to the manner in which it is spread." No evidence is submitted on which this statement could be based, and no account is taken of the fact that the present outbreak of the disease exists in the locality of Britain where sheep-breeding for show purposes with consequent inbreeding is carried to its extreme limit. Sir John seems also to be unaware of the fact that the historical outbreaks of the disease in Britain, Germany, and France all synchronised with strenuous efforts on the part of private individuals or governments to develop improved races of sheep by consanguine breeding. The history of the "Electoral" and "Negretti" breeds of Merinos in Germany is eloquent in this connection. The Electoral sheep consanguinely bred for "points" were riddled with the disease, whereas the Negrettis, a commercial sheep, among which inbreeding was not practised, had practically none of the disease (*vide* May, as quoted in my Report, p. 18 and elsewhere).

On p. 108 of his article, Sir John makes the statement that "at the present time in this country the disease does not occur in the majority of breeds, but that is easily explained by the fact that most pure breeds are more or less confined to particular parts of the country and are now, to a large extent, out of the way of infection." This is another dogmatic statement which is contrary to fact, for Border County rams, lambs, breeding ewes, and draft ewes (for breeding purposes)—all from scrapie infected flocks—are sold broadcast over Scotland and England, as the records of the Border auction marts will show. He further avers (p. 109) that "the very fact that the disease is now nearly or quite confined to the Border-Leicester and Cheviot, and the crosses between these, almost entirely removes any risk of its being communicated to the other pure breeds, which are never crossed with any of these." Surely Sir John must be ignorant of the large sheep industry carried on in Scotland, where Black-faced ewes are crossed with Border-Leicester or half-bred rams.

Sir John now goes on to state (p. 109) that, before applying the hypothesis that the disease is parasitic in nature (which he inclines to believe), one must endeavour to obtain assurance about the most important facts, and especially to collect evidence with regard to the circumstances connected with the first appearance of the disease in a flock. He mentions relative to this that unfortunately it is very rarely that any really useful information with regard to that point can be obtained. He congratulates himself that in 1912 he had the opportunity to be brought into touch with two owners whose flocks had been infected recently, and was able to gather from them valuable information while the important facts were still fresh in their memory. Sir John then details the cases of Mr A. and Mr B., who bought ewe lambs from a Mr X. in another county. For our purpose here it is not necessary to give in full the subsequent history of these lambs as entered into by Sir John. Moreover, I could parallel, if space permitted, Sir John's story by the cases of Mr C. and Mr D., who also bought ewe lambs from a Mr Y., also in another county. It is even possible that the

Mr X. of Sir John's tale is the Mr Y. of mine. In my case Mr Y. was blamed throughout the Border counties (with what justification will be seen later) for having introduced and spread broadcast this "new" disease. This in passing. To proceed, Sir John sums up the information derived by him from the cases of Mr A. and Mr B. as follows (p. 111): "Leaving the last-mentioned farm out of account, the facts which have been stated above may be summarised by saying that in two large stocks of sheep the disease was, according to testimony that appears to be quite trustworthy, introduced by the purchase of apparently healthy lambs from an infected flock; that a notable proportion of these lambs developed the disease when they were about two years of age; and that, in spite of the sale of all the animals of this lot and their progeny before the next breeding season, the disease subsequently developed in the other ewes, and in fact established itself on the farm. It will probably be generally conceded, that if the facts were as stated above and none of importance has been omitted, they suggest that scrapie is caused by the multiplication of some parasite in the sheep's body, and is spread by the direct or indirect transference of this parasite from the diseased to the previously healthy. But before engaging in speculation with regard to the nature of this parasite certain other facts besides those stated in connection with the history of the disease on the two particular farms must be taken into consideration." One of these facts is "that according to all the available evidence the disease does not spread in spite of the closest contact between diseased and healthy confined in a house. The results of the experiments recorded at the end of this article are in harmony with that view though it cannot be said that they prove it."¹

Thus Sir John states his case, and it may be mentioned in parenthesis, that the passages just quoted would appear to contain all the positive facts on which his paper is based. The part of the evidence submitted last, and incidentally the more reliable, flatly contradicts what preceded it. It is small wonder, therefore, that Sir John draws, or does not draw, the halting conclusion given above "that scrapie is caused by the multiplication of some parasite in the sheep's body, and is spread by the direct or indirect transference of this parasite from the diseased to the previously healthy."

It is necessary here, in order to make my position in the matter clear, to deal at once with a subject raised by Sir John's formulation as given above. Sir John has referred to the "direct or indirect transference of this parasite from the diseased to the previously healthy." I have no desire to enter into academic and barren disquisitions, as has been attempted, relative to the exact distinction between the terms "hereditary" and "congenitally infectious." To the simple-minded the first term is the more inclusive, and could include the second as a special case. For my purpose I will designate as hereditary in connection with disease, any disease obtained by the offspring through the functioning of paternity or maternity

¹ It is a very curious fact indeed that Sir John in his subsequent argument discards entirely the results of his own experiments and bases his discussion entirely on the evidence derived from Mr A. and Mr B., as given above.

as such. This obviously makes the term hereditary include other diseases than those due to an infectious agent, as also it makes the term include diseases contracted by the offspring subsequent to birth, consequent on the act of its feeding on its mother's milk. The term "congenitally infectious or contagious" would therefore stand in contradistinction to "contagious" as ordinarily used in connection with diseases contracted through other less intimate, ordinary, everyday association. Possibly Sir John's "direct or indirect transference" has this latter meaning, and it certainly is the only meaning that it will bear having regard to its use throughout his paper. One specific instance of its use may be mentioned here. In the above statement of his case, the disease cases on which his whole argument for the contagiousness of the disease turns had no ancestral connection with the diseased animals originally imported. Everywhere it is used by him antithetically to what I have defined above as hereditary. In resuming, therefore, I will use the word hereditary (although it includes contagious as a part of a greater whole), as opposed to contagious in the ordinary sense. This is a usage sanctified by wont, and is a practical one understood by farmers and shepherds; moreover, it is intensely practical in the further sense that the two types of disease designated by the two terms demand very different methods of handling.

To proceed, leaving aside the contradictory nature of the evidence contained in the two halves of Sir John's statement as given above, I will now examine whether he is justified in drawing the conclusions he does from the evidence submitted by Mr A. and Mr B. As has been already mentioned, Sir John's whole argument turns on the question of the disease having arisen in animals that had been bred on the farm, or which had been on it prior to the introduction of the diseased animals. He himself disposes of the possibility of the disease having been introduced through the agency of an infected ram (p. 110). He has, however, left undiscussed the possibility of the disease being already present in the ewe stock on the farm, and unrecognised prior to the introduction of the diseased sheep, in which case heredity, as defined above, would quite suffice to explain the cases in question. That this is not a fanciful argument or mere dialectic based on an unthinkable possibility, namely, the non-recognition of the disease, the following quotation from Sir Stewart Stockman's lecture mentioned above will show. Speaking in reference to the experiences of several buyers of sheep from a heavily-infected stock, he says: "Some time after this dispersal sale, buyers of sheep complained that 'scabby' sheep had been sold as they were rubbing themselves, and it would seem that many of the buyers were unaware of the existence of such a disease as scrapie, and not unnaturally put the symptoms down to scab." I can fully substantiate this statement from my own experience. If, therefore, this non-recognition of the disease and its confusion with sheep-scab could occur when large numbers of sheep were involved, it is easy to see how its occurrence in flocks in small numbers and its non-recognition as such is possible. This possibility has a bearing on the accusation levelled at Mr Y, as mentioned above. Mr Y. was blamed for the introduction of this "new" disease. Now, while it may be true that he sold sheep to certain farms in such a potentially

diseased condition, and in such numbers as to emphasise the economic importance of the disease, there is no justification whatever for the assertion that he did more than that. Indeed, the following extract from a letter received by me from a prominent Border farmer shows that the disease existed, and was well recognised in the Border district long prior to the sale of the sheep in question. Incidentally, the letter has also bearings on a point already discussed, namely, "inbreeding," in reference to the disease, and is as follows: "Scrapie was in this place forty years ago. About forty years ago there was not so much difference in the value of one man's stock from another. They were bred more for mutton, and not so much for the points which win in a show-yard. They were not specialised to such an extent, and the whole blood of the stocks were not so much drawn from a few flocks as it is at present. Hence, as far as I know, the outbreaks of scrapie were more isolated, and people got quit of it by buying clean stock into their places. The disease was, so to speak, stamped out." Without disrespect, therefore, I submit that Sir John's conclusion, as stated above, relative to the disease on Mr A.'s or Mr B.'s farms is wholly unwarranted.

I now pass to that portion of Sir John's article which more specifically deals with myself. He states (p. 112) that "it may be difficult to frame any theory which will explain what Dr M'Gowan calls the epizootiology of scrapie, but it is easy to show reasons for doubting the one which he has advanced. The first is that the most important of his premises, namely, that the disease never spreads from diseased to healthy sheep on the same pasture, can no longer be maintained. The facts which I have described, in connection with two different farms, show that the sale of all the animals which imported the disease into a previously clean flock is by no means a certain method of eradication." The advance by Sir John from the halting, inconclusive "conclusion" of his statement, dealt with above, to the dogmatic assertion implied here should be noted in passing. With reference to the objection to my views involved in the quotation just made, I have just dealt with it, and there is no need for further reference here.

Sir John goes on to say (p. 112): "The view put forward (by me) that an infected flock can be freed from the disease by the less drastic method of gradually selling off the old ewes and buying in a corresponding number of fresh young ewes from clean flocks is not supported by any evidence, and therefore cannot be admitted." It is difficult to be sure whether Sir John means that he himself has no evidence to support this view, or that I have not submitted any. In either case, it will be news to Sir John to learn that it is a recognised method of efficiently dealing with the disease which has been practised for at least the last forty years. It is, however, hampered by the difficulty, intrinsic and unavoidable, of the flockmaster being sure that the fresh animals are from an absolutely clean flock. Failures arising from this method of dealing with the disease, therefore, should not be attributed to the fresh stock being infected from the old and diseased which it replaces, until unimpeachable evidence has been adduced that the incoming animals were absolutely free from all taint. (In addition, a *de novo* origin of the disease, as dis-

cussed in my paper, would also have to be excluded.) I have already quoted from a letter of a prominent Border farmer anent the existence and prevalence of the disease prior to this outbreak. The continuation of his letter bears on the point under discussion at present, and particular attention is directed to what he says relative to the procuring of "clean animals." He writes as follows: "My father bought in clean (Leicester-Cheviot) ewe lambs. He used clean tups. The ewes and gimmers went mixed all the year except in tupping-time, when the fresh tups were used on the gimmers. There never was a case among the imported ewes. We continued to buy in the ewe lambs. We then began breeding half-bred lambs from the gimmers (half-bred and half-bred). They continued clean. We gave this up after a number of years, as we could buy the ewe lambs cheaper than we could breed them. A second time we began to breed from the gimmers, and continued doing so until we got the disease again. We again fell back on buying ewe lambs, and have continued doing so. It is every year becoming more difficult to secure clean stock." This quotation may be left to speak for itself. It is not an isolated case, but, as stated above, is part of a practice which surely would not be persisted in if things were as Sir John suggests.

Sir John further remarks (p. 112) "But even if it had to be admitted that scrapie does not spread except from mother to offspring, Dr M'Gowan's conclusions with regard to the nature of the disease (a massive sarcosporidial infection) would have to be rejected, first because they are largely based on other assumptions which are unproved and improbable, and secondly because they leave absolutely unexplained some of the most important and generally admitted facts in connection with the disease." After expressing surprise that any one well acquainted with the facts regarding the past and present distribution of scrapie should suggest that the disease is caused by a parasite universally present in the sheep in all parts of the world, he proceeds to ask the questions "Why has the disease ceased to occur in breeds and localities in which it once was common although the parasite remains, and secondly why does it persist and even extend in the Cheviot and Leicester breeds and the crosses between these in Northumberland and the Border counties of Scotland?" "Dr M'Gowan," he states, "makes no attempt to answer the first of these questions, possibly because he thinks it is inferentially dealt with in his answer to the second. This he disposes of by suggesting that the heaviest infestation with sarcosporidia occurs at about two years of age, that the common or exclusive method of infection in sheep is by the passage of the parasites from the muscles of the pregnant ewe through the wall of the uterus to the lamb; that the lambs of gimmers are therefore more heavily infected at the time of birth than those borne by older ewes, and finally that scrapie is at present common among the British breeds mentioned because in these it is the custom to recruit the ewe stock by means of young ewes bred from gimmers." Sir John then proceeds as follows: "The first defect of this explanation is that it includes a hypothesis that has little probability, namely that sarcosporidia pass from the ewe to the lamb while the latter is still in the womb." Sir John is at perfect liberty to

designate as a hypothesis the results obtained by me relative to the passage of sarcosporidia to the foetus *in utero* and recorded by me at pp. 88 and 89 of my article, but the facts are as there stated. In continuation of this theme I now insert the subsequent history of the four lambs involved in the experiment just referred to, which up to the present has not been made public. One of them died from an acute affection before reaching the age at which scrapie usually discloses itself; one of them developed typical scrapie between the age of two and three years; while the two others did not develop the disease even after the expiration of four years.

Some further experiments, also as yet unpublished, may be usefully inserted here also, as they have a bearing on several of the points already raised. Six lambs born from healthy mothers (Cheviots) obtained from a part of the country remote from the "scrapie" area and in which scrapie was unknown, were "twinned-on" to scrapie mothers in milk dying from the disease, whose own lambs had been killed off in order to accomplish this. They were suckled on these foster-mothers for various periods (depending on the length of time the mother lived) from a week or so to a month or a month and a half. All of them died when between two and two and a half years old from typical scrapie, whilst among healthy lambs born from and suckled on healthy mothers, the mothers themselves, several hogs, etc., in all about thirty or forty sheep of all ages, which pastured and lived in intimate association with the diseased animals just mentioned during the whole time of the experiment, not a case occurred. The nett result of these experiments goes to show that both the sarcocyst and scrapie can be transmitted from the mother to the offspring *in utero*, and that both can be transmitted to healthy lambs through the milk of diseased mothers. They negative the idea of the disease being contagious in the ordinary sense of the term, whilst, as just mentioned, they support the view that the disease is hereditary in the sense that farmers and myself appreciate it, and in a sense that lays a foundation for an intelligent coping with the condition.

As a continuation of the extract from Sir John's article with which I have just dealt, Sir John goes on to say that "the known facts, including the successful infection of mice by feeding, makes it more probable that in the sheep infection occurs by way of the alimentary canal and therefore during extrauterine life." No evidence is adduced in support of the view expressed here except the feeding experiments of Prof. Theobald Smith with mice, which, being omnivorous, are in quite another category from graminivorous animals such as sheep. Moreover in Prof. Smith's experiments even the intra-uterine passage of the sarcocyst was not excluded.

Sir John, in his criticism of my article, continues (p. 113) "A more serious defect of the explanation is that it appears to make two assumptions that are contrary to fact, namely, that all the flocks in which scrapie has ever existed have had the ewe stock recruited by two year old ewes which were themselves borne by gimmers, and that scrapie is bound to develop in sheep that are thus bred. It is disappointing to find that although the whole theory put forward by Dr M'Gowan hangs upon these two points he makes no attempt to substantiate them. Apparently it had not occurred to him that his

theory laid him under the obligation of proving that in all other breeds of sheep in which scrapie once occurred, and in all the continental breeds in which it now exists, this system of recruiting by means of gimmers out of gimmers has been practised. So far as regards the British breeds the suggestion is certainly incorrect, and, in the absence of any evidence to the contrary, one may assume that it is equally erroneous in regard to the French and German breeds in which the disease has been observed." Without disrespect to Sir John, I may point out that the first assumption as stated above which he imputes to me and which he makes part of my theory of the disease has nowhere been stated by me, is in no way deducible from anything I have said, and in short has been imagined by Sir John himself for his own purposes. I have, indeed, emphasised the importance that breeding from gimmers has on the origination, continuation, and dissemination of the disease in a flock, but at the same time I have also emphasised the important part which "inbreeding," with all that it connotes, may play in giving rise to similar conditions. There may also be, and probably are, other conditions of a like nature, as yet unrecognised, which all tend in the same direction. From a practical point of view, however, it is a question of laying emphasis on the most important. In arriving at such decisions as these, I confined myself in my investigations to matters as they existed at the present day in this country, and which were capable of being thoroughly investigated at first hand on the spot by cross-examination and otherwise. In this connection it is pertinent to ask Sir John on what authority or on what grounds he avers that my view relative to breeding from the gimmers (and inbreeding, etc.) being capable of giving rise to the disease is incorrect with regard to the British breeds. I have failed to find in Sir John's article any evidence ancient or modern on which such a view could be founded. Further, in order to rebut what is an alleged assumption on my part relative to the existence of the disease in French and German breeds, he makes the assumption that a certain type of breeding is non-existent in France and Germany!

Relative to his second point, "that scrapie is bound to develop in sheep thus bred" (where the ewe stock is recruited from the progeny of the gimmers), as an exercise in dialectics I see no harm in admitting that it is a logical deduction from the position I have taken up with regard to the matter. (It is not of so great importance, however, from the practical point of view, as I have already given it as my opinion that diverse agencies, of which this is one, are at work in originating the disease). The onus, however, of proving this deduction incorrect rests with Sir John, if his views lead him to take up the position that it is incorrect.

In furtherance of his argument Sir John now proceeds to make a quotation from my Report and to comment on it. I state in my Report that "in other parts of Scotland indeed where the practice is to keep up the ewe stock by drawing the *best* lambs from *all* the ages the disease has not appeared." Sir John would have saved himself trouble had he read and understood the sentence which immediately succeeds in my Report the one just quoted. It is as follows: "Where this is done, for one gimmer's lamb drawn for

breeding there are usually five or more drawn from the ewes of the older ages." This would mean that one gimmer's lamb was kept for fifteen or more lambs drawn from the older ages in a case where there were four ages of breeding sheep kept. This proportion as expressed here is a very conservative estimate, for, as a gimmer's lamb is usually a poor lamb, it may happen, and very often does happen, that all the lambs are drawn from the older ages and none at all from the gimmer age. There is abundant facility in this system for selecting the best lambs possible, whereas in the case where the ewe stock is kept up from the gimmers there is no choice whatever, the number of ewe lambs available being usually in defect of the number required to keep up the ewe stock. The result in this last instance is that every ewe lamb whatever its quality must be retained to keep the ewe stock up to strength.

In connection with this question a further point arises to be discussed. Granted that a ewe lamb from a gimmer were selected to keep up the ewe stock on a farm where the ewe stock is kept up from the progeny of all the ages, what are its chances of producing scrapie in endemic proportions in that flock? In the first place such a gimmer will only in a very few cases be a scrapie animal. This scrapie animal then has to run the gauntlet of being diagnosed as scab (*vide supra*) by the shepherd and dealt with as such. The unofficial method of so doing in the case of individual animals is to destroy the animal at once, as this causes in the long run least pecuniary loss. If, on the other hand, owing to the spread of knowledge on the subject, the case is diagnosed as scrapie, then its fate is even more certainly sealed. Suppose, however, that, before giving evidence of the disease, the animal has given birth to lambs and these ewe lambs, which might carry on the disease. Such, however, being gimmer lambs, and especially badly nourished owing to the condition of their mother, would stand a very poor chance of being selected to keep up the ewe stock.

I admit that there is a possibility of misinterpreting the sentence which I myself have quoted above from my Report. It undoubtedly would have been better expressed by saying "from the ewes of *each* of the older ages." While, however, this may explain Sir John's attitude to this part of the subject it certainly does not excuse the lack of knowledge of things pastoral which could render possible the arithmetical exercises indulged in by Sir John in criticising me in this connection (p. 114, fourteenth line from bottom *et sequenda*). Again on page 114 (line twenty-three from top), in referring to the method of keeping up the ewe stock from the progeny of all the ages as a possible explanation of why scrapie has not appeared in flocks where this method is practised, Sir John comments in brackets as follows: "and which hardly needs recommendation since it is almost universally practised." Surely Sir John cannot be so far ignorant of the elemental facts in connection with the subject which he affects to discuss as to be unaware that this is precisely the type of breeding which is *not* favoured in the district where scrapie is endemic, for certain reasons very cogent (to farmers at least) which it is not necessary to specify further here.

The next few pages of Sir John's article do not call for much comment from me, consisting as they do of restatements without

further evidence of his opinions (which I have already discussed), barren discussions of subjects lacking any reliable evidence wherewith to discuss them, and statements of results of some of his experiments which he has been carrying out for the last seven years—results which, while I may be disposed to accept them as facts, are capable of other more probable interpretations than those given by Sir John. There is one point, however, to which Sir John refers which demands more detailed consideration, and that is the relation of the ram to the spread of the disease. It would appear (p. 117) that Sir John is disposed to believe that the ram may spread the disease, although his remarks on this subject are not definite nor so extensive as the importance of the subject would appear to demand. While stating it again as my belief that the ram does not spread the disease, I would refer the reader to my remarks on the subject on pp. 103 and 104 of my Report on the subject. As an interesting sidelight, however, on the sort of evidence adduced in support of the view that the ram spreads the disease, I might refer the reader to p. 110 of Sir John's article, where the following remark occurs in connection with the disease on Mr A.'s farm: "Although Mr A. was of opinion that the disease might have been spread in his flock by the agency of the rams, he never had a ram which developed any symptom of scrapie." Instances where scrapie is alleged to have appeared in a flock subsequent to the introduction of rams which did or did not develop the disease, it may be remarked here at once, are useless for the settlement of the point in question, because so many other circumstances, in their very nature difficult of elimination, have to be eliminated before any reliable conclusion can be drawn. It is quite otherwise, however, in those instances where *no* scrapie cases occurred after the introduction and use of a ram which definitely developed scrapie. Such recorded cases are fairly numerous, and the following letter, coming as it does from a prominent Border agriculturist, whose opinions on the subject of scrapie have undergone considerable alteration within the last few years as the result of his experience, will serve as an example. He writes as follows: "For a good few years we hill farmers were very shy of East Border tups [from the scrapie area], but latterly we have been buying them more freely, and, so far as I have heard, no cases of scrapie have occurred. I have never known a case in a genuine hill stock. Some years ago, before scrapie began to be openly talked about, a neighbour of mine bought a Cheviot tup off — and used him. When he came in from the ewes he was very thin, and was put among the tup hoggs for a bit better keep. Instead of getting better he got worse. They could never understand how it came about that every night and morning when the herd went to feed them every turnip box was couped over. He watched them, and found it was the — tup rubbing and scraping on them. Eventually they killed him and put him in a hole. From the description of scrapie, Mr — had no doubt this tup had it, although at the time they attributed his trouble to 'some heat in the blood.' Well, he got lambs all the time, and not one of the ewes he served nor any of the progeny ever developed the disease."

The last section of Sir John's article which has interest for me,

namely that of prevention, now falls to be discussed. The first section of this paragraph, dealing as it does with the gradual elimination of diseased animals at one end and the introduction of fresh young ewes from clean stocks at the other, has been dealt with by me already and needs no further treatment here.

Sir John, however, goes on to criticise my recommendation that the ewe stock should be kept up from the progeny of the older ages of ewes, by stating that it is inconsistent with my statement that the disease has not appeared in other parts of Scotland, "where the practice is to keep up the ewe stock by drawing the best *ewe* lambs from *all* the ages." The inconsistency alleged here is due to Sir John's misconception of the subject. As stated above, in actual practice, under such conditions a gimmer's lamb, being a poor lamb, is in consequence rarely chosen in competition with the better lambs from the older age ewes.

Regarding the efficacy of this method of dealing with the disease, the following results obtained by one stockowner, who acted in the matter on his own responsibility on the general recommendation contained in my Report will speak for itself. The facts as given throw interesting sidelights on the whole question of the disease, especially with regard to its possible etiology and epidemiology. Space, however, does not permit of these points being gone into here. The following are the facts in connection with this case :—

The average number of scrapie cases per year for a long period of years prior to the introduction of the method of breeding advised by me in my report was 120. Two and a half years after the adoption of the measures suggested by me, when the effect of these measures might be expected to show themselves, the number of scrapie cases for the year had fallen to three.

As regards my second recommendation, relative to the killing out of diseased animals to prevent any possibility of their being used as breeding stock, Sir John remarks that "it is superfluous for the declared object, first, because the sale of visibly affected ewes is practically impossible, and secondly, because, according to all published experience, no such ewe ever survives long enough to be bred from again." With regard to Sir John's first objection, it may be stated that a regular and recognised trade in such animals takes place every week to a large manufacturing town in the North of England, while with regard to the second, it is pertinent to ask Sir John whether the recommendation is not applicable in the case of an in-lamb gimmer, showing evident signs of scrapie a month or two before she was due to lamb.

With regard to "in-breeding" and its relation to the disease, and Sir John's remarks thereon, I have already discussed this subject in this article, and would refer the reader also to my statement of the case as it appears on page 104 of my original Report. With reference to Sir John's remark relative to the point under discussion, where he states that "it is a notorious fact that the disease has often played havoc in half-bred flocks," implying by this that in-breeding cannot, and does not, take place in such flocks, it may interest Sir John to know that the closest in-breeding can, and does, take place in the "half-bred" flocks as they exist in the Border districts of Scotland. Apart from this, the closest "in-breeding"

can, and does, take place in the pure Cheviot and Border-Leicester breeds from which the "half-breds" are derived.

Again, Sir John, whilst making little of my recommendation relative to the breeding from the older ages of ewes, has found it incumbent upon himself to recommend that rams should not be used until they are in their third year!

Sir John makes some recommendations on his own responsibility. These are detailed in the remaining part of this section. None of these recommendations are original, and his most important one, relative to getting rid of the disease by killing out the whole flock, young and old, has been tried and rejected time and again, owing to its impracticability from a financial point of view, as also from the point of view of obtaining the desired end.

SARCOSPORIDIA AS THE CAUSE OF SCRAPIE.

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THE view that the ovine disease termed scrapie is caused by the common *Sarcocystis tenella* of the sheep's muscles was discussed in an article by me which appears at an earlier part of this volume of the Journal (p. 102). Dr M'Gowan, the originator and, as yet, the sole upholder of this view, appears to think that in the article in question I did not deal fairly with the subject as expounded by him, and I therefore return to it, in order to make such amends as appear to be necessary, and to restate and criticise his views regarding the etiology of the disease, with the explanations and additions which he now offers (*see pp. 278-290*).

The disease is not contagious, or, in Dr M'Gowan's own words ". . . all the evidence points to the non-contagiousness of the disease." I will not imitate the author by stigmatising this as simply a dogmatic assertion, but will content myself with saying that all the evidence on this point that Dr M'Gowan made public in his report was so meagre that no person accustomed to weighing evidence could for a moment think of accepting it as proof that the disease is never spread by contagion. Here is the substance of it:—

"Had it been contagious, with the sale broadcast of lambs from scrapie flocks, and of such ewes from the flocks as had survived till they were of the age to be cast, the disease would now be widely spread. Such, however, is not the case. Again, the usual way of combating the disease, consisting in selling off the old ewes and buying in young sheep from clean flocks, renders such a view untenable; for here the diseased and healthy mingle freely with one another; and yet when the last of the diseased stock has been sold the disease has been stamped out."

It is almost amazing to find anyone acquainted with the present great hold which scrapie has on the sheep stock in the north of England and the south-east of Scotland, and who knows the extent to which it prevailed formerly in other parts of the country, not merely denying that the facts in this connection constitute pre-