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THE



GROUSE DISEASE.

A

STATEMENT OF FACTS TENDING TO PROVE THE
PARASITIC ORIGIN

OF THE

EPIDEMIC.

WITH TWO WOODCUT ILLUSTRATIONS.

BY

T. SPENCER COBBOLD, M.D., F.R.S., F.L.S.,

PROFESSOR AT THE ROYAL VETERINARY COLLEGE, AND LECTURER ON PARASITES AND
PARASITIC DISEASES AT THE MIDDLESEX HOSPITAL.

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THE
GROUSE DISEASE.

THE thorough-going sportsman, excellent fellow as he is deservedly held to be, is not without his prejudices. On the subject of grouse, especially, you will hardly succeed in securing his attention unless you can assure him that you have yourself walked a moor, gun in hand, or, at least, have shot over some of the more cereally-productive acres of our English counties. In the hope, therefore, of ingratiating myself, sufficiently, at least, to secure his kind attention to the facts recorded below, I may state that I have shot over moors in the counties of Selkirk, Peebles, and Kircudbright, and have likewise enjoyed the tamer sport of partridge-shooting in Norfolk, Suffolk, and Yorkshire. Without the slightest title to be called a sportsman, I cannot be said to be altogether unfamiliar with the habits of game.

I have been amused by observing with what amiable patience the sporting man will listen to the well-meaning utterances of his trusty keeper respecting the grouse disease. Of course this uneducated worthy knows all about it. To borrow the actual words of a recent writer in the *Field*, the keeper will probably say, "No doubt, sir, it is due to a blight in the vegetation, caused by the cutting east or

north-east wind." How exquisitely simple! How satisfactory! What logical reasoning! East and north-east winds we have had in abundance; but these, it is clear, will hardly account for the murrain alone, so in comes the old stereotyped phrase about the "blight." The opinion of Farmer McIntosh—who, by the way, has not been on very friendly terms of late with sporting men—is given with equal confidence, and, in *his* estimation, the disease has nothing to do with the weather, but with over-feeding. Another says it arises from under-feeding; a third says it is due to excessive wet; a fourth ascribes it to the late spring; a fifth to vitiated dew; a sixth to the large number of old birds left at the close of the previous season; a seventh to Arbutus berries; an eighth to "battery shooting;" a ninth is positive that the disease is due to over-crowding; a tenth that sheep are indirectly concerned in its production, "by interfering with the natural food" of the birds; an eleventh that it is a blood disease, ending in softening of the liver, or in pyæmia; and a twelfth that it is due to parasites. Nearly all persons who cling to the last-mentioned opinion feel quite sure that parasites are not the primary cause of the epidemic, but merely hasten the death of the birds, already enfeebled by something which has operated to reduce their strength. Like a certain writer in the *Field* (under the signature of "W. C."), they will perhaps say, that "when animals become weakened by disease, the parasites increase in an overwhelming degree, and are mistaken by the ignorant for the disease itself." Exactly so. That is precisely the position which I, for one, after twenty years' diligent study of parasites and parasitism, am prepared to defend against all comers, including the rac writer "W. C." himself.

The opinions above mentioned, however, are by no means all that have been hazarded by honest, practical people. They are here appositely put forward as characteristic

examples of statements confidently made public from time to time. I happen to hold to the opinion that the grouse disease is essentially and exclusively a parasitic disorder; and, as will be seen in the sequel, I ground this view upon a solid basis of fact. So far, however, from seeking to display (as some have done) a contemptuous disregard of the opinions of those who differ from me, I wish to say that my object is not to assert dogmatically that other people are wrong, and that I am right, but rather to call attention to facts, some of which have not received the full consideration which is due to them, whilst others are entirely new to science. My interpretation of the facts may be erroneous; but, whether my view be ultimately adopted or not—and I should be surprised if it were accepted by more than a few persons in the first instance—I may fairly lay claim to the discovery of an internal parasite in the grouse, which had hitherto escaped the notice alike of the sportsman and the naturalist. Formerly, indeed, I denied that the disease was due to parasites; or rather, I regarded the presence of tapeworms as insufficient to account for the epidemic of 1866 and 1867. At that time, however, I was practically unacquainted with more than one form of internal parasite living in the grouse. As remarked elsewhere, the year 1867 will long be remembered by sportsmen on account of the scarcity of grouse, due to an epidemic exclusively affecting these birds. There did not appear to be any essential difference between the phenomena of the disease then presented as compared with the features exhibited by similar outbreaks that had occurred before. However, the epidemic of 1867 being more virulent than usual, gave rise to the notion that we had to deal with a new malady. The outbreak of last year, and its reappearance or continuation during the present, is by most persons regarded as, in all respects, the same as that of 1867.

In 1867, the question having been raised as to whether or

not the disease was of parasitic origin, I undertook to draw up a statement of facts, which I subsequently embodied in a paper that was made public at the Dundee meeting of the British Association. Basing my conclusions on the results of *post mortem* examinations, which had for the most part been conducted by others, I then saw no sufficient proof of the parasitic nature of the malady. The mere fact that in several, or in many, instances tapeworms were found in the intestines of the diseased birds appeared to me insufficient of itself to afford positive proof of injury from this source. In fact, it was only in a certain proportion of the birds that these cestode entozoa were detected; and, moreover, they did not occur in numbers exceeding that which many considered to be about the usual proportion found in healthy birds.

Notwithstanding the view then advocated, I protested against the popular notion that entozoa resulted from a vitiated state of the body of the affected birds. As I said at the time, it is not easy for persons totally unacquainted with the singular and complicated phenomena of parasitism, including the laws regulating their local and geographical distribution, to shake off the very prevalent notion that tapeworms and other entozoa are generated only in diseased or unhealthy animals. This old and erroneous conception of cause and effect has been handed down from age to age, and it will certainly yet prevail amongst us for many years to come. I freely admitted the occasional destruction of game birds by the injurious action of internal parasites; but I could not bring myself to see that the epidemic of 1867 was due to this cause. As will be noticed by what follows, I am now of opinion that the disorder was due to the presence of another kind of internal parasite, which was overlooked at the time, the tapeworms only playing a very insignificant and subordinate part in the destruction of the birds.

As in the present epidemic, so also in the former, many

persons stated that the disease was accompanied by visceral disorganization—the liver being the organ chiefly affected; and yet, as remarked at the time, I never remembered, during my numerous examinations of animals that had died with parasites in their interior, to have witnessed such disorganization as was spoken of in connection with the grouse disease. This seemed rather to favour the notion of a blood affection, for I felt sure that no amount of tapeworms could give rise to gangrene and pyæmia of these organs. Another circumstance which more than any other seemed to tell against the parasitic theory—and which will naturally be still held by many as fatal to my view of the case—arose out of the statement of several persons, to the effect that, in the epidemic of 1867, the birds were frequently quite plump at the period of their death. There was one very remarkable instance recorded by the late Mr. J. K. Lord in the pages of *Land and Water*, in which it was alleged that the lungs, liver, and heart of a grouse were found excessively diseased, although the bird was actually shot on the wing. This bird, when examined after death, was pronounced to be free from tapeworms. I have no wish to seek to disprove the competence of the person who made the *post mortem* examination in question, but I may observe that, except in cases where the tapeworms are abundant, their presence may be readily overlooked unless the investigator be at pains to examine the last twelve or fourteen inches of the small intestine, reckoning upwards from the point of junction with the large intestine and the two enormously elongated cæca. Only by laying out the whole alimentary tube from end to end, and by slitting up the several distinctive portions *seriatim*, is the pathologist in a position to speak with absolute confidence on the point at issue. I presume, therefore, this was done, not only in Mr. Lord's example, but also in the case of other diseased birds, the results of the examination of which were made known at the time.

As regards the soft, pultaceous, black, and gangrenous condition of the liver, *that* is a state of things which was not only observed in 1867, but has been noticed over and over again in the present epidemic. I have myself observed a friability of the gland far exceeding that to be met with in the healthy liver; nevertheless I have remarked that such a condition was in all cases associated either with a more or less emaciated state of the bird, or arising out of the circumstance that the bird had long been dead, its viscera thus becoming unduly softened by decomposition. The emaciation seen in the birds of the present epidemic seems to be much more marked than that which has been spoken of in connection with former outbreaks; and this particular symptom is one which, in my judgment, points very strongly to the parasitic character of the disorder. At all events, its extreme manifestation in the present murrain should not be lost sight of.

One writer of experience compares the condition of the liver in diseased birds to a clot of coagulated blood; and the same observer under the signature of "W. H.," in *Land and Water*, May 17, speaks of the prospects of the grouse shooter for the present season as truly gloomy. Writing from Sutherlandshire, he tells us that the disease is making frightful havoc, and expresses surprise that "so little has been said about it," but accounts for this reticence on the part of those interested, by expressing the belief that this apparent indifference "can only arise from the subject being worn out, without cause or remedy being discovered." In the opinion of "W. H." who has been amongst the grouse for nearly half a century, and who has "seen the disease come and go in its various phases," the fatality of the present epidemic is without parallel. I quote this gentleman's observations not merely because they are valuable in themselves, but as affording me an ample apology for directing further attention to the subject. Apart from this sufficiently adequate reason

for further investigation and publication, I am naturally desirous (on the principle involved in the motto, *Palman qui meruit ferat*) that those of us who first discovered and described the parasites scientifically should not be deprived of the credit which is legitimately due to us.

Two species of entozoa are concerned in the production of the grouse disease; or, if that statement be objected to, I am content to say provisionally, that two kinds of internal parasites are frequently found in birds dying of the so called grouse murrain. One of these parasites is a tapeworm and was first described as such by the late Dr. Baird of the British Museum. He called it the *Tænia calva*. The other is a species of strongle, which is altogether new to science; that is to say, it was only very recently discovered by myself in grouse sent from Nairn Castle, and was described by me in a recent number of *The Veterinarian*. After a careful search of the writings of helminthologists, including Krabbe's recent and beautiful Danish memoir (*Bidrag til Kundskab om Fuglenes Bændelorme*), I have satisfied myself that neither the tapeworm nor the strongle had been recognised as belonging to the parasitic fauna of birds previous to the time when our descriptions were made public. It is true that I had at first some difficulty in believing that Dr. Baird's record (from specimens in the collection of the late Dr. George Johnson, of Berwick) referred to a really new species; but this arose from the circumstance that he stated that the head of the grouse tapeworm was unarmed. This negative character seemed to point to its identity with the *Tænia linea* of the partridge; especially as the celebrated Pastor Goeze had described the latter worm as being totally destitute of cephalic hooks (*Versuch einer Naturgeschichte der Eingeweidewürmer thierischer Körper*, s. 399). In the hope of clearing up discrepancies of statement, and in view of giving a more complete exposition of the character of these two parasites, I respectfully submit the following

technical data to the attention of interested naturalists. I may mention that the tapeworms originally examined by myself in 1867 formed part of a series for which I stood indebted to Professor Flower, F.R.S., of the Hunterian Museum; but I also subsequently received examples from other friends. In this matter, I need only further state that there are no differences of character as between the tapeworms examined in 1867 and those which I have recently obtained by original dissections.

THE GROUSE TAPEWORM.

Order.—CESTODA, Rudolphi.

Family.—TÆNIADÆ, Linnæus.

Species.—*Tænia calva*, Baird.

Characters.—Body flat and white; the strobile consisting of numerous segments, represented by fine striæ above, the joints or proglottides, being twice as broad as they are long in the centre, and nearly square-shaped towards the tail. Head visible to the naked eye, followed by a very narrow neck, the latter rapidly increasing in thickness at about an inch from the upper part; cephalic armature of four suckers, aided by a central disk supporting a double crown of excessively minute hooks. Reproductive papillæ not visible to the naked eye. Eggs developed within capsules, including four or five primitive nucleated cells.

Length of the worm, 4 to 10 inches; width of the head, $\frac{1}{80}$ " ; diameter of the neck, $\frac{1}{250}$ " ; breadth of the mature proglottis or segment, 2" or $\frac{1}{6}$ " ; long diameter of the capsule, $\frac{1}{15}$ " to $\frac{1}{120}$ " , length of the egg, $\frac{11}{450}$ " ; of the embryo, $\frac{1}{680}$ " .

So much for the specific characters of the worm; the

fractions above indicated being those of an inch. Considerable variations in the size of parts are noticeable, but these may be regarded as approximately correct measurements taken from average examples of the worm. At all events, they will be useful in any question of identification which may subsequently arise, when the parasite is compared with other allied forms of cestode entozoa.

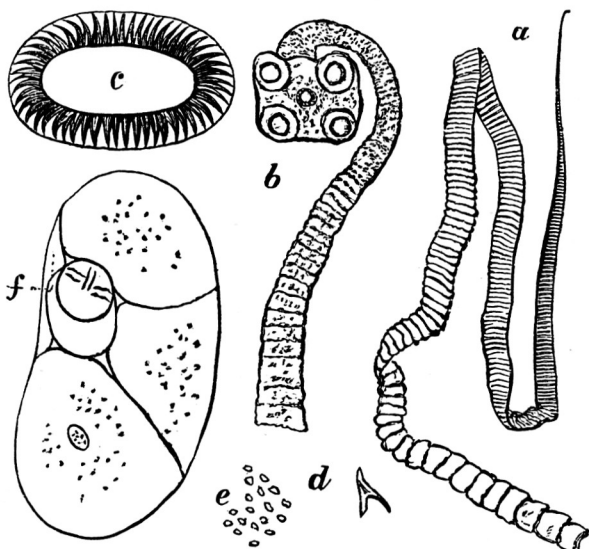


FIG. 1. *a*, Grouse Tapeworm (*Tænia calva*) of the natural size; *b*, head and neck, enlarged about 30 diameters; *c*, crown of hooks (\times about 300); *d*, one of the hooks separated; *e*, the so-called calcareous corpuscles; *f*, egg capsule, containing an egg and six-hooked embryo (\times about 450 diameters).

Without further dwelling on the mere natural history characteristics of the grouse tapeworm, I have now to draw attention to a more precise record of the facts recently observed by myself in examinations of game transmitted by

William Alexander Stables, Esq., the Earl of Cawdor's steward. Part of the remarks immediately following form the substance of my original communication to the *Field* for Nov. 9, 1872 (p. 450). It is necessary to give these earlier details in order to render the subsequent particulars intelligible to those who have not had access to that record.

On Nov. 2, 1872, I received from Dr. John Millar a batch of grouse, several of which had died of the "disease." They had been transmitted from Cawdor Castle, Nairn, along with a communication, from which I was permitted to gather some highly interesting facts. Thus, Mr. Stables is of opinion that when an outbreak of the epidemic takes place, it "follows a season in which the grouse have been unusually numerous and healthy," and he supports this *post hoc propter hoc* principle by a reference to the fact that a remarkable quantity of sound game was obtained from the Cawdor moors during the previous year. He adds:—"They showed well this year up to the time the young were beginning to fly, when, all at once, numbers of old birds were observed sickly," and those which were found dead lay generally "along the edges of small burns—the number of diseased and dead birds increasing as the season advanced."

In a letter quite recently communicated to the *Field* (May 17, 1873), the same gentleman states that in 1871 the grouse were unusually abundant on the moors in question, and in fine condition. The disease "appeared to a small extent in the autumn of 1872; but about a month ago it broke out with great virulence, the birds dying in greater numbers than the keepers ever before observed."

At the date already mentioned I examined four birds sent by Dr. Millar, and, as Mr. Stables spoke only of three as "victims of the mysterious disease," I supposed one of the four was accounted healthy. Certainly the odd one was in fair condition, the other three being very thin—two of them reduced, in fact, to mere skeletons. I examined the skin,

muscles, and blood of one of the birds with much care, whilst the intestinal contents of all four were more or less completely searched. The evidences of pyæmia, with softening and disorganisation of the lungs and liver, which were so much spoken of in connection with the outbreak of 1867, were here totally wanting. In this view it was, as well as for other purposes, that I subjected the blood and muscular tissue to microscopic scrutiny. The skins of all were beset with lice—the external parasites being dead, and therefore readily dislodged by disturbing the feathers. Only one imperfect tapeworm was detected ; and I failed to obtain any evidence of the existence of *Ascarides*, two species of which genus have been described as infesting the Grouse, as well as other birds of the same family (*Tetraonidæ*).

But for one circumstance, I might have pronounced these birds as remarkably free from entozoa. They were infested by minute nematodes of a species which I had not hitherto encountered ; and the worm being apparently new to science, I ventured to call the species, provisionally, *Strongylus pergracilis*. Examples of this new parasite occupied the whole length of both of the intestinal cæca. They were present in greater or less abundance in all four of the birds. The male parasite gave an average of one third of an inch in length ; the females extending up to three eighths of an inch, or rather more. The latter had their oviducts crowded with eggs arranged in single file, displaying various stages of yolk segmentation ; but I did not notice any fully formed embryos. It may afford some notion of the extraordinary abundance of these nematode entozoa, when I state that from less than a teaspoonful of the cæcal contents I obtained many hundreds of specimens. I am confident that no one of the four grouse contained less than a thousand examples, and I believe that one of the more diseased and emaciated birds contained fully ten times that number.

That such a multitude of active nematodes could reside in the intestines of any bird without producing suffering or serious inconvenience to the bearer is hard to believe; and therefore, in the total absence of other evidence, it is perhaps not too much to assert that the murrain of the past season was attributable to the presence of multitudes of very fine strongles. I am perfectly well aware that birds can fly about with all sorts of strange entozoa in their bodies without noticeably suffering in the least degree, and we have lately become acquainted with a singular instance of this apparent immunity. Professor Wyman found *Eustrongyli* surrounding the cerebellum of seventeen out of nineteen snake birds or water turkeys shot in Florida; and he remarks that the presence of these threadworms "in the cranial cavity might be called the normal condition of this bird." Dr. Wyman's parasite is a viviparous form of nematode. (See Proceed. Bost. Nat. Hist. Soc., for 1868.) These and other like data do not, however, disprove the generally admitted conclusion, which I have over and over again had occasion to verify, that internal parasites are liable to occasion inconvenience, suffering, disease, and not unfrequently death itself, to their bearers, more particularly when they occur in excessive numbers. A parasite-bearer, like any given territory, can only support a certain number of occupants. In the case of entozoa an unusually wet season following a mild winter is eminently favourable to the excessive multiplication of certain forms of these creatures.

Shortly after dissecting the four grouse above referred to, I had an opportunity of examining with still greater care the intestinal contents of a fine healthy bird from the same locality. In this case also a large number of the strongles occupied the cæca, though not in such prodigious numbers as in the emaciated bird above mentioned. There were several examples of the *Tænia calva*—these tapeworms being limited to a narrow portion of the small intestine situated

from five to ten inches above the point of junction of the cæca with the large intestine. No other forms of entozoa were present. I may likewise add that the more cursory examination of the contents of yet another grouse—the sixth under my observation thus far—also yielded a quantity of strongles.

A question will here naturally suggest itself to the mind of the practical man. Does not the fact of the occurrence of these parasites in the intestines of a so-called healthy grouse destroy the notion of disease from this source? My reply is, "Certainly not." A large experience with the symptoms produced by entozoa, both in human and animal bearers, enables me to assert with confidence that the measure of feebleness, disease, or suffering usually corresponds with the degree of parasitism; but not exclusively so, since another factor concerned in the welfare or destruction of the bearer, as the case may be, arises out of the constitutional vigour of the bearer. A strong bird will overcome or resist the irritation set up by the presence of hundreds of entozoa; while a feeble bird, or one attacked before it is perfectly grown, will more or less rapidly succumb to the invasion. It is the same with mankind. One person will have convulsions from a single parasite, while another will play the part of "host" or bearer to the same species of parasite without the slightest discomfort. On these and other grounds, therefore, I do not hesitate to express the opinion that the present grouse murrain is due to parasites. The irritation, probable distress, and subsequent emaciation of the birds are readily explained by the presence of hundreds and thousands of strongles; and the mere circumstance that these parasites are very small, is quite sufficient to account for the fact that investigators have hitherto overlooked them. At this point it will be as well, perhaps, to subjoin a technical description of the new entozoon. The contrast between the characters of this fine nematode worm and those of the

tapeworm will be sufficiently obvious to the most unscientific of observers, whilst the illustrations, drawn from nature in either case, can hardly fail to help in the diagnosis. As in the former case, the fractions of measurement employed are those of the inch.

THE GROUSE STRONGLE.

Order.—NEMATODA, Rudolphi.

Family.—STRONGYLIDÆ, Dujardin.

Species.—*Strongylus pergracilis*, Cobbold.

Characters.—Body filiform, finely striated, gradually diminishing in front, uniform in thickness below; head bluntly pointed, with a simple oral aperture; tail of the male furnished with a bilobed bursa, each half supporting four pointed rays; spicules two, thick, and slightly divergent; tail of the female slightly swollen above the subterminal anal orifice, rather sharply pointed at the tip; vaginal opening situated at the upper part of the inferior sixth of the body.

Length of male, $\frac{1}{3}$ " to $\frac{3}{8}$ "; body, $\frac{1}{400}$ " in diameter, tapering anteriorly to $\frac{1}{2000}$ " at the head; greatest breadth immediately above the bursa, $\frac{1}{350}$ ".

Length of female mostly $\frac{3}{8}$ ", sometimes very nearly $\frac{1}{2}$ "; breadth above the tail, $\frac{1}{270}$ " to $\frac{1}{250}$ ", narrowing at the extreme point to $\frac{1}{7000}$ "; longitudinal diameter of the eggs, $\frac{1}{250}$ ", their breadth being $\frac{1}{650}$ ".

These measurements, taken with a Wasserlein microscope and micrometer, were carefully made by my son, Mr. Charles S. W. Cobbold, to whom also I am indebted for numerous illustrations of the worm, from which the accompanying figures have been selected and drawn on a reduced scale.

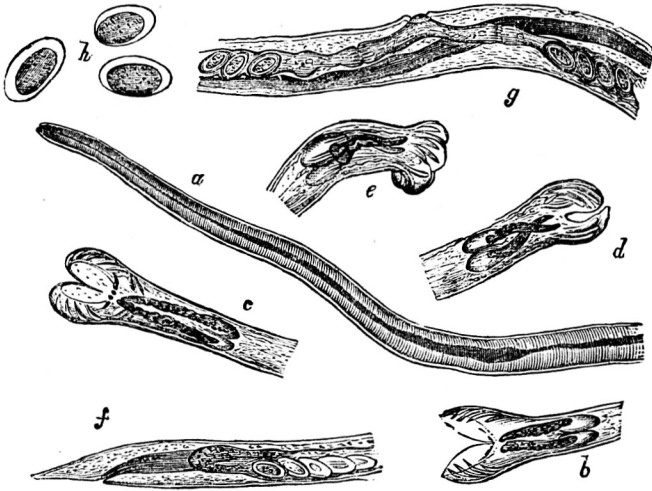


FIG. 2. *The Grouse Strongyle (Strongylus pergacilis)*.—*a*, head and neck; *b*, tail of the male, viewed from the front; *c*, another view, showing the expanded bilobed hood, the two spicules, the glandular organs above, and the bursal rays; *d*, *e*, lateral and oblique views of the tail of the male; *f*, tail of female, with anal outlet and fold of the inferior uterine tube; *g*, portion of the body of the female, showing the reproductive papilla, vaginal passages, and constrictions, with lower portions of each uterine tube, also the digestive canal; *h*, three of the ova (highly magnified).

During the latter part of the month of November, 1872, I received a second batch of grouse from the Cawdor Moors. There were six brace in all, three of the birds being described by Mr. Stables as diseased, but whether picked up dead, or how otherwise obtained, he was not informed. The remaining nine were shot by the keepers, and were "believed to be in good condition." In accordance with Mr. Stables's instructions, the three birds in question were purposely selected from "the most diseased" which Lord Cawdor's

keepers came across. It will, therefore, be seen that I had by this time received eighteen birds from the Cawdor demesnes; and the results afforded by their subsequent examination, if not generally accepted as decisive of the parasitic nature of the disease, will nevertheless be found of considerable interest.

Incidentally I may mention, that whilst shooting over the Kirkconnell Moor, in Kircudbrightshire, last autumn, my host, Richard J. Strong, Esq., then residing at Barcaple House, and myself bagged several unusually thin grouse. One of these birds, which rose to my gun at the edge of a burn, was even more emaciated than some of the worst of those received from Mr. Stables; at least, I judge so, in the absence of means of actual comparison. By inspection I ascertained that there were no external evidences of previous gunshot injury; and, in short, to use a phrase which has now become famous, the poor bird had evidently been reduced, by a slow process, to "a mere ruckle of bones." I have no doubt it had the "disease."

My second special investigation of the Cawdor game was conducted with the most painstaking care, and it will save a great many words if I tabulate the results in the following simple manner:—

Number of specimen.	Condition of bird.	State of viscera.	Tapeworms present.	Strongles present.
1	Good.	Full.	None.	Abundant.
2	Emaciated.	Putrid.	Two.	Very numerous.
3	Good.	Full.	None.	Abundant.
4	Good.	Full.	None.	Abundant.
5	Good.	Full.	One.	Abundant.
6	Good.	Shrunken.	Two.	Very numerous.
7	Emaciated.	Much shrunken.	None.	Very numerous.
8	Thin.	Shrunken.	None.	Very numerous.
9	Thin.	Shrunken.	None.	Very numerous.
10	Emaciated.	Distended.	Many.	Very numerous.
11	Thin.	Rather full.	Several.	Abundant.
12	Thin.	Rather full.	None.	Very numerous.

As, notwithstanding my carefulness, there is a possible inaccuracy in this list, it is right that I should indicate it, though the error itself is of no great moment. The grouse were eviscerated by my cook, and when she laid out the entrails of the last six—all of which were more or less impoverished birds—it was rendered doubtful to myself which of these referred to the numbers 7 and 10 respectively, two of the six belonging to the three thoroughly diseased birds. The doubt rested chiefly as regards No. 10. The bird marked No. 2 in the table was extremely emaciated. I had selected this as the most diseased of all. The intestinal canal and cæca of the birds were thoroughly and exhaustively examined from end to end in every case. The cavity of the proventriculus was also inspected in one or two instances, but, as regards entozoa, it only yielded negative results.

On looking to the additional facts thus established, it will be seen at a glance that whilst, on the one hand, the presence of tapeworms was only occasional, the presence of strongles was on the other hand, invariable. The only difference, as between impoverished and healthy grouse in this latter respect, appears to have been that, in the case of the birds out of condition, we encountered a relatively larger number of these nematode worms. It is merely the difference between thousands and tens of thousands; but this disparity, if considered in association with the varying strength of constitution of individual avian bearers, will be amply sufficient, in my opinion, to account for either impoverishment or retention of health, as the case may be. That in some seasons the tapeworms may acquire ascendancy, and thus become the sole cause of mortality amongst the grouse is quite possible; and, under any circumstances, their presence would be likely to aggravate a disorder, whether the latter be proven to be due to another form of parasitism, or to disease arising from causes altogether independent of entozoal infection. In the

present epidemic, I believe the disease to be entirely due to parasites.

Amongst human parasite-bearers the same phenomena display themselves. There is a nematode entozoon which people often carry about with them without suffering the slightest inconvenience; nevertheless a solitary example of this same parasite has been known to occasion lunacy. It is likewise, not unfrequently, the cause of death. Smaller forms of the same tribe of parasites are capable of reducing the strength of their human bearers, precisely after the fashion of *Strongylus pergracilis*. A most distressing instance of this kind has come before me whilst writing these pages. I shall not dwell upon it; but I believe I have thus made out a strong case against my newly discovered parasites; and have little doubt, from other collateral evidence, that avian hosts, generally, are just as liable as human bearers to be afflicted by the presence of entozoa, or to be, as it were, "eaten up by worms."

Lest any persons, after perusing the above remarks, should feel hesitation as to the propriety of partaking of the next specimens of grouse sent to their table, I may add that I have alike consumed portions of the so-called healthy and diseased grouse from the Cawdor Moors. Putting aside the truly wretched appearance which the emaciated birds displayed on the dish, there was really little to object to. The diseased bird marked No. 2 had a rather stronger flavour and was decidedly softer, without being watery, than the fresh and plump bird marked No. 1. This difference, however, was not due to the diseased condition, but from the fact of its having been longer dead. One of the other diseased birds was comparatively dry and insipid. Both were eatable, there being no new or disagreeable flavour attached to either. Several of the good birds were as fine to look at, and as satisfactory in their culinary qualities as any of the many grouse I partook of during the last season.

In the month of December, 1872, I received a third batch of grouse from Mr. Stables, and also at the same time two white hares, one of the latter being a remarkably fine animal, whilst the other was so reduced in condition that its appearance when alive could only have excited commiseration, not to say disgust. The coat was woolly, thin, and scanty; its spinous and transverse vertebral processes projecting along the whole length of the back, and presenting the well-known razor-backed character which is met with in sheep far advanced with the fluke disease. The condition of this hare was perhaps even worse than that of the most emaciated bird.

In regard to this diseased hare, I may remark, in passing, that it harboured one large fluke (*Fasciola hepatica*), and also a solitary tapeworm (*Tania pectinata*). The last-mentioned entozoon was much broken up, but I judged it to have measured ten inches in length.

That the fluke had occasioned severe irritation, and had produced much suffering to the hare during life was proved by the *post-mortem* evidence. The liver ducts were much thickened, and in several situations were completely blocked by the formation of hard calcareous plugs. This gland was diminished in size, very dark in colour, and readily broke up when pressed between the fingers. It also imparted to the touch a gritty sensation when sliced with the scalpel.

It is, perhaps, not altogether surprising, that one fluke should be capable of producing so much mischief. When we consider the small size of the bearer and the narrowness of the common liver duct, as contrasted with the width of the entozoon, it is quite clear, indeed, that only a few flukes are capable of destroying hares and rabbits.

To return to the consideration of the third batch of grouse, I have to state that a careful examination of eleven of the birds, yielded results of the same general character as those above tabulated in connection with the second batch. Some of them contained tapeworms, whilst all harboured

strongles more or less abundantly. Thus far, therefore, I had examined altogether twenty-nine grouse from the Earl of Cawdor's moors, and every bird was found to be more or less infested with this new species of nematode entozoon. On the other hand, only about one third of the birds harboured tapeworms, and of this comparatively small section only some three or four of the bearers contained the tapeworms in any considerable quantity.

Without wishing to be dogmatic in any assertion, I must once more protest against the commonly received opinion that "when animals are weakened by disease [then it is that] parasites increase in an overwhelming degree, and are mistaken by the ignorant for the cause of the disease itself." So far from this being true, the very reverse is the case. Entozoa, when in large numbers, and sometimes when in small numbers only, are capable of giving rise to fatal maladies of various kinds amongst mankind, beasts, birds and even fishes; whilst the emaciation consequent upon their invasion of these hosts may be, and usually is, solely attributable to their presence. It will be a long time, I fear, before the old and erroneous conception of cause and effect is thoroughly got rid of. To almost every unscientific observer it appears quite certain that the emaciation of a badly infested host is the cause of the abundance of internal parasites; whereas, in all such cases (where other maladies do not happen to concur), the impoverishment of the blood and tissues is entirely due to the presence of the entozoa.

In the opinion of the Earl of Cawdor's steward, it seemed likely that yet a fourth special investigation of diseased game might aid us further in arriving at a satisfactory conclusion respecting the true nature of the murrain; especially if the examinations were made after the close of the shooting season. In this view, therefore, he kindly forwarded from Nairn Castle another six brace of birds, which arrived on the 25th of last April. In a letter received by me a few days pre-

viously, Mr. Stables remarks that "at the end of last season, about November, the keepers thought that the disease was gradually disappearing." He heard nothing further from them about it, until a few weeks previous to the date of his letter, "when they reported its re-appearance with more virulence than ever—the birds dying all over the moors."

The statements of Mr. Stables respecting the fatality of the epidemic during the present spring, are in perfect harmony with the general testimony offered by correspondents in the *Field*, in *Land and Water*, and in other sporting papers. A writer in the *Daily News* says:—"The murrain amongst the grouse has set in with unusual violence this year. The reports from both the Scotch and Yorkshire moors state that hundreds of the birds are picked up dead or dying in all directions. The disease appears to be as little understood as the Black Death of the Middle Ages, but, of course, theories without end have been broached to account for the epidemic. Although we are as far off from a scientific explanation of the grouse disease as ever, there can be no doubt of the absolute reality of it at present; and should it continue at the rate of which we read, it will be absolutely necessary to close up a great many moors altogether for, perhaps, more than one season."

The opinion thus set forth in the *Daily News* in reference to our ignorance of the true nature of the disorder may possibly be true; but, whether it be so or not, I have all along resolved that no discouraging remarks of this sort should deter me from thoroughly investigating the matter; and, therefore, in the hope of contributing something that might, at least, extend our knowledge of the surroundings of the disease, even if it did not serve to set the question finally at rest, I have undertaken the inquiry, and I rely upon the good sense of practical men to approve of my efforts.

In a paragraph in the *Times* of May 28, I see it stated that "the general opinion is, that the disease is of a different type from that which prevailed in the moors some years ago." It may be so. Let us look more closely to our present evidences. On comparing the results obtained by an examination of the fourth set of birds with those received last year, I could not but be struck with the differences observable as to the degree of parasitism. In the November series, tabulated above, seven of the birds were entirely free of tapeworms; and of the five which actually harboured them, two birds only contained more than two tapeworms apiece. The April birds, on the other hand, all contained tapeworms in more or less considerable numbers; three of the avian hosts being infested to a frightful extent. I sought to ascertain the number of tapeworms in one of these birds, and found that it was not far short of one hundred. The difficulty of being precise as to the number, arose from the quantity of detached heads and necks of the worms, and the fragmentary character of many of the bodies, or strobiles themselves. This, after all, was of no great moment. As to the strongles, they were present in large numbers in all; but, to my reckoning, and speaking generally, scarcely so numerous as in the November birds. At all events, the strongles appeared to me sufficiently numerous to weaken, or even destroy the avian bearers without the assistance of a second kind of parasite; and it, therefore, becomes highly probable that the concurrence of numerous tapeworms sufficiently explained the greater destructiveness of the present outbreak as compared with that of the previous season. In one extreme case I particularly noticed a remarkably gorged or distended condition of the cæcal villi, such as would result from continual irritation set up by parasites in overwhelming numbers. Careful microscopic investigation showed that there was no rupture of the capillaries, and, consequently, no extravasation

in the cæca or in any part of the intestinal canal. That this congested state of the villi was due to the strongles appeared the more certain, since the turgidity was only marked in that part of the cæcum where the strongles were crowded together. With these data before me, I see nothing irrational in the belief that the grouse epidemics of recent years have been due to parasites. To say that these entozoa are merely the result of some cachectic or otherwise impoverished state of the birds, is merely offering a gratuitous assumption resting on no solid basis of fact, and totally opposed to all the experiences of helminthologists. I cannot allow that these tapeworms are merely coincidently or coordinately associated with the disease ; but I apprehend that the phenomena thus set up are of the same order as those which I almost daily witness in the practice of my profession amongst human bearers. I know, also, that similar diseased phenomena display themselves amongst quadrupeds, especially when the entozoa occur in unusual numbers and happen to occupy sensitive or vital organs.

The presence of entozoa, it is true, is no proof of disease. Like other creatures, they have a given territory, which it is their just prerogative to occupy ; but when the territory is beset with more occupants than it can comfortably support, then the indications of incapacity duly manifest themselves. Impoverishment, distress, and, finally, destruction of the territory will ensue. This is what sometimes takes place in the human host, and still more frequently in the mammalian bearers ; and, philosophically speaking, the bodies of men and animals constitute the invaded territories. In this view, further, the intelligent reader will not fail to perceive that these parasitic phenomena afford another curious illustration of the reality of the "struggle for existence," so prominently put forward in Mr. Darwin's work on the "Origin of Species ;" for here we see, as it were, a multitude of Lili-putian creatures actively battling for their own existence at

the expense of the avian territory. For more or less prolonged residence in this territory they are admirably adapted; their organisation being expressly fitted for the parasitic mode of life. Further on this phase of the question I will not now dwell, but I may add the reflection that since more robust hosts than grouse succumb to parasitism, there need be neither surprise nor doubt as to the fact that game birds perish under parasitic influences.

The parasitic theory of the grouse disease has several supporters amongst practical men, who make no pretension to science; and in this connection I would especially refer to Mr. Stables's letter, which appeared in the *Field* of May 17th. (under the signature "W. A. S."). I may also refer to the letter of a previous correspondent (signed "M. C.") in the same periodical for May 3rd. This careful observer has taken the trouble to verify the truth of my discovery. He "placed a small portion of fæcal matter from the cæcum under the microscope, and was very much surprised to find about a dozen specimens of the entozoa" which I had described.

In regard to the game prospects of the forthcoming season the majority of correspondents (like D. G. F. Macdonald, in *Land and Water* for May 17), write despondingly. This gentleman, notwithstanding that he has "shot thousands upon thousands," and also "dissected many," of these birds, is enabled to assert that the disease prevails "in a form hitherto unknown," a circumstance which, in his judgment, "heightens the mystery, and renders it more incomprehensible than ever." After reading his letter, followed as it is by two gloomy epistles signed "B. T. R." and "W. H." respectively, besides several others, it is a relief to turn to the communication of "Aberdonian" in the *Field* of May 24th, 1873, who says that the disease "is on the decrease," and who adduces evidence tending to show that the printed reports have been "greatly exaggerated." On this matter, however, I will only further remark that extremely

opposite statements may come from various quarters, and yet all be equally free from error, intentional or otherwise. Such discrepancies are just what I should expect on the supposition that the disorder is entirely parasitic.

Provisionally accepting this view, there yet remains for our consideration, the important question whether or not anything can be done, calculated either to lessen or eradicate the disease. In my opinion, there is room for further special investigation, the results of which might prove useful both practically and scientifically. Be that as it may, it will be time enough for me to declare my sentiments more fully on this part of the subject when these first steps in the inquiry shall at least have been acknowledged and discussed on their own individual merits.

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