

Essays in Natural History and Agriculture eBook

Essays in Natural History and Agriculture

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Contents

Essays in Natural History and Agriculture eBook.....	1
Contents.....	2
Table of Contents.....	7
Page 1.....	8
Page 2.....	10
Page 3.....	11
Page 4.....	12
Page 5.....	13
Page 6.....	14
Page 7.....	15
Page 8.....	16
Page 9.....	17
Page 10.....	18
Page 11.....	19
Page 12.....	20
Page 13.....	21
Page 14.....	22
Page 15.....	23
Page 16.....	24
Page 17.....	26
Page 18.....	27
Page 19.....	29
Page 20.....	30
Page 21.....	31
Page 22.....	32

Page 23.....	34
Page 24.....	35
Page 25.....	36
Page 26.....	37
Page 27.....	38
Page 28.....	39
Page 29.....	40
Page 30.....	42
Page 31.....	43
Page 32.....	44
Page 33.....	46
Page 34.....	47
Page 35.....	48
Page 36.....	50
Page 37.....	52
Page 38.....	54
Page 39.....	55
Page 40.....	56
Page 41.....	58
Page 42.....	59
Page 43.....	60
Page 44.....	62
Page 45.....	63
Page 46.....	64
Page 47.....	65
Page 48.....	67

Page 49.....	68
Page 50.....	70
Page 51.....	71
Page 52.....	72
Page 53.....	74
Page 54.....	75
Page 55.....	77
Page 56.....	79
Page 57.....	80
Page 58.....	81
Page 59.....	82
Page 60.....	83
Page 61.....	84
Page 62.....	85
Page 63.....	86
Page 64.....	87
Page 65.....	88
Page 66.....	89
Page 67.....	91
Page 68.....	93
Page 69.....	94
Page 70.....	96
Page 71.....	98
Page 72.....	99
Page 73.....	100
Page 74.....	101

Page 75.....	103
Page 76.....	104
Page 77.....	106
Page 78.....	108
Page 79.....	109
Page 80.....	110
Page 81.....	112
Page 82.....	114
Page 83.....	115
Page 84.....	117
Page 85.....	118
Page 86.....	119
Page 87.....	120
Page 88.....	122
Page 89.....	123
Page 90.....	124
Page 91.....	125
Page 92.....	126
Page 93.....	127
Page 94.....	128
Page 95.....	129
Page 96.....	131
Page 97.....	132
Page 98.....	133
Page 99.....	135
Page 100.....	136

Page 101.....	138
Page 102.....	139
Page 103.....	141
Page 104.....	142
Page 105.....	143
Page 106.....	144
Page 107.....	145
Page 108.....	146
Page 109.....	148
Page 110.....	150
Page 111.....	152
Page 112.....	153
Page 113.....	154
Page 114.....	155
Page 115.....	157
Page 116.....	158
Page 117.....	159
Page 118.....	160
Page 119.....	161
Page 120.....	163
Page 121.....	165
Page 122.....	166
Page 123.....	167
Page 124.....	169
Page 125.....	171

Table of Contents

Section	Table of Contents	Page
Start of eBook		1
CONTENTS.		1



Page 1

CONTENTS.

Facts and observations on the salmon.

Introductory Observations

The Salmon enters and ascends Rivers for other purposes besides

Propagation

Suggestions for an alteration in the Laws regarding Salmon

Artificial Breeding of Fish

Artificial Propagation of Fish

Remarks on a Proposed Bill for the better Preservation of Salmon

Letters on agricultural subjects.

On the Cultivation of Wheat on the same Land in Successive Years

The Cultivation of Wheat

On the Gravelling of Clay Soils

Cotton

Papers on natural history.

Wrens' Nests

The Long-tailed Titmouse

Identity of the Green with the Wood Sandpiper

The Stoat

The Marsh Titmouse

Creeper

Wrens' Nests

Alarm-note of one Bird understood by other Species of Birds

Dates of the appearance of some Spring Birds in 1832, at Clitheroe

The Rook Serviceable to Man.—Prejudice against it

Sandpipers

On Birds Dressing their Feathers with Oil from a Gland

Mocking powers of the Sedge-warbler

The Water Ouzel

Scolopax, Sabines, Sabine's Snipe

Fish and other River Phenomena

Lampreys

On the Spawning of the Minnow

Eels

On the Possibility of Introducing Salmon into New Zealand and

Australia

On the Formation of Ice at the bottom of Rivers

On the Production of Ice at the bottoms of Rivers

Gossamer

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Facts and observations on the salmon.

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Facts and observations on the salmon.

In the following observations I intend to offer some remarks on the various migratory fish of the *genus Salmo*; and then some facts and opinions which tend to show the importance of some change in the laws which are now in force regarding them.

We have first the Salmon; which, in the Ribble, varies in weight from five to thirty pounds. We never see the fish here before May, and then very rarely; a few come in June, July, and August if there are high floods in the river, and about the latter end of September they become tolerably abundant; as the fisheries near the mouth of the river have then ceased for the season, and the Salmon run very freely up the river from that time to the middle or end of December. They begin to spawn at the latter end of October, but the greater part of those that spawn here do so in December. I believe nearer the source of the river they are earlier, but many fish are seen on the spawning beds in January; and I have even seen a pair so late as March; but this last is of very rare occurrence.

Page 2

Some of the male Kipper (Kelts) come down in December and January, but the greater part of the females remain in the river until April, and they are occasionally seen herding with shoals of Smolts in May. In this state they will take a worm very readily, and are, many of them, caught with the fly in the deeps; but they are unfit to eat, the flesh being white, loose, and insipid; although they have lost the red dingy appearance which they had when about to spawn, and are almost as bright as the fresh fish, their large heads and lank bodies render it sufficiently easy to distinguish them from fish which are only ascending the river, even if the latter were plentiful at this season; but this is unfortunately not the case.

Secondly, we have the Mort. I am not sure whether this fish is what is called the Grilse in Scotland, or whether it is the Sea Trout of that country; it is a handsome fish, weighing from one and a half to three pounds. We first see Morts in June; from that time to the end of September they are plentiful in favourable seasons in the Hodder, a tributary stream of the Ribble, although they are never very numerous in the Ribble above the mouth of that stream. It is the opinion of the fishermen here that this is a distinct species; my own opinion is, that it is a young Salmon, and yet, if I were called upon to give reasons for thinking so, I could not offer any very conclusive ones: the best I have is, that there is no perceptible difference in the fry when going down to sea. It may be said, How do you know that one of the three or four varieties of Smolts which you describe further on, is not the fry of the Mort? To this objection, if made, I say that these varieties exist in the Wharfe, where, owing either to natural or artificial causes, there is never either a Mort or a Sprod (Whitling?) seen.

Thirdly, we have the Sprod, which is, I believe, synonymous with the Whitling, Whiting, or Birling of Scotland. It is a beautiful fish of six or eight ounces in weight, and has more the appearance of the Salmon than the Mort; it seldom ascends the river before July, and, like the Mort, is far more abundant in the Hodder than in the Ribble; this fish sometimes rises pretty freely at the fly, and when it does so, makes a very handsome addition to the angler's basket, but at other times it is difficult to hook, because of its shyness. It disappears in a great measure about September.

Fourthly, we have the Pink, or Par, which is found of two or three sizes in the Ribble; the largest are all males, and in October the milt in them is large; they are small fishes, ranging in weight from about one to three ounces each, and it is well remarked by the author of that delightful book "Wild Sports of the West," they have very much the appearance of Hybrids between the Salmon and the Trout; they rise very freely at the fly and maggot, from July to October, and afford good sport to the angler who is satisfied with catching small fish. I trust I shall be able in the following pages to give some information respecting this fish which will assist in dispelling the mystery in which its natural history has been enveloped.

Page 3

I will now mention a few of the opinions respecting the various species of the Salmon, and also my own, when they are at variance with the generally received ones, and give the facts and reasonings which have induced me to form those opinions, and I shall be very glad, if I am in error on any of these points, if some one of my readers, better acquainted with the subject than I am, will take the trouble to set me right. It seems to be the opinion of many, indeed of most persons, that the Salmon spawns from November to February, that the young fry, or Smolts, go down to the sea in the April or May following; my own opinion is that they stay in the river much longer. The Grilse is by many believed to be a distinct species, whilst others stoutly maintain that it is a young Salmon.

The testimony of the witnesses from the Severn, the Wye, the Lee, near Cork, and the Ness (see the evidence given before the Select Committees of the House of Commons in 1824 and 1825), would lead one to suppose that the fish were in best season from November to March, whilst the evidence of the witnesses from other parts of the kingdom goes to prove that this is the very worst period for catching them.

One maintains that each river has its own variety of fish, which can be distinguished from the fish of any other river; another contends that there is no such difference; a third states that stake nets are exceedingly injurious to the breed of the fish; and a fourth attests that stake nets only catch the fish when they are in the best season, that neither Kelt nor fry are taken in them, and that if they were prohibited it would only be preserving the fish for the grampuses and seals;—in short, the evidence regarding both their habits, and the best mode of catching them, having in view the preservation and increase of the breed, is so completely contradictory as to leave a doubt in the mind of every one who reads it, and has no other means of forming an opinion. I will endeavour to show in some instances which of the testimonies is correct, and it will be for my readers to judge how far I succeed, and I hope they will be so obliging as to correct any error into which I may fall.

First.—It is my opinion that the fry of Salmon are much older when they leave the river than seems to be generally supposed, and that the growth of this fish is by no means so rapid as it is considered to be by those who have written upon the subject. For several years previous to 1816 the Salmon were unable to ascend into the upper parts of the river Wharfe, being prevented either by the high weirs in the lower parts, or by some other cause, and of course there were no Smolts or Par; but in that year either the incessant rains of that summer or rumours of the formation of an association for the protection of fish, or some other unknown cause, enabled some Salmon to ascend the river, thirty or forty miles, and to spawn there. In the next spring, 1817, there were

Page 4

no Smolts, but about September they began to rise at the very small flies which the anglers use in that river—they were then a little larger than Minnows. In the spring of 1818 there were blue Smolts, or what are generally known as Salmon fry, which went down to the sea in the May of that year; but these were only part of the brood, the females only, the males remaining all that summer, being at the period when the females went down very much smaller than they, and what was called at the Wharfe Grey Smolt and Pinks, or Par elsewhere.

I have shown that there were two migrations from the spawn of 1816; but this was not all—there still remained a few Smolts through the summer of 1819, which by that time were from four to six ounces in weight, and which are known by the anglers there as Brambling Smolts. The blue marks on their sides are very distinct, and the fish is a perfect Smolt, except that it is considerably larger. It is quite different from the Whitling, or Sprod, which is not known in the Wharfe, at least not in the upper parts of that river, whilst the Brambling is never seen in the Ribble. [1]

The Brambling is a beautiful fish, and it rises very freely both at the May fly and the artificial fly through the summer; it is occasionally caught by anglers with the worm on the Salmon spawning beds in the autumn, with the milt perfectly developed, and in a fluid state. Although this fish is not found in the Ribble, so far as my observations and inquiries have gone, I believe that it is found in the Tweed, and perhaps also in other rivers running into the German Ocean; for a letter addressed to Mr. Kennedy, who was chairman of the select committee appointed to investigate this subject, by a Mr. George Houy, states that the Smolts are sometimes found there ten inches long, which he attributes to their not being able to get down at the proper period for want of a flood in the river. But I know that in the Ribble Smolts will go down to the sea without there being a flood at all, if that does not come within ten days or a fortnight of the time at which they usually descend to the sea. I also know that Brambling are found in the Wharfe, in years where there has been no deficiency in that respect; yet why they should be common in that river, when they are never met with in the Ribble, which has ten times as many Salmon and Smolts in it, I am unable to comprehend.

It is my opinion that the ova of the Salmon are not hatched before March or April. Two anglers, who were in April wading in the river Wharfe, came upon a spawning bed, which they had the curiosity to examine; they found a number of ova, in which they could see the young fry already alive, and one of them took these eggs home with him. By regularly and frequently supplying them with fresh water, he succeeded in hatching them, and kept some of the young fishes alive for some time; but they died in consequence of neglect, and were even then very diminutive. The opinion

Page 5

generally received in Scotland seems to be, if I may judge from the evidence given before the House of Commons, that the Smolts go down to the sea in the spring after they are spawned, and that they return in the summer and autumn of the same year as Grilse. When they return, and what size they are on their first visit, I have hitherto been unable to ascertain; but I think I have succeeded in proving that they do not go to the sea so soon as is generally believed, nor do any of the witnesses give their reasons for thinking that they do. I should very much like to learn what evidence they have to offer in behalf of this opinion.

I remember seeing an article in the "Scotsman," perhaps about twelve months ago, in which it was stated that Dr. Knox had made some important discoveries in the natural history of the Salmon and Herring, both in their food and propagation, and, if I recollect aright, it stated that he had ascertained that the eggs remained several months in the gravel, and that then, in a few days or weeks after, they (*i.e.* the fish hatched from them) were so much grown as to go down to the sea; but none of the data which enabled him to arrive at this conclusion were given, and since then I have heard nothing about the matter. As it is so long since I read this article, I may have quoted it incorrectly, but I believe its substance was what I have stated.

The only conclusive evidence I can find about the hatching of Salmon fry is that of Mr. George Hogarth (second Parl. Report, p. 92), and his account agrees with my own: he states that he took Salmon spawn from the spawning beds, and by keeping it freely supplied with fresh water, he succeeded in hatching some of the eggs; he gives drawings of the appearance of the fry in three or four different stages, from the egg to the age of eight days (see Appendix to second Parl. Report), that the young fry, by keeping them well supplied with fresh water, were very lively and vigorous for three weeks, but that they after this time appeared to grow languid and uneasy, and as they would eat nothing they died when one inch long. Unfortunately he does not state at what time of the year they were hatched, but if this were in March or April, which I see no reason to doubt, it is sufficient to prove that they would not reach the size that Smolts are when they leave the river for the sea; for supposing them to be hatched the last week in March, and that they lived a month, this would bring us to the time when they are about to migrate, at which time they average more than six inches long; many of them are eight inches, and at this period they are fond of feeding upon worms, flies, maggots, and caddis worms, as is known to every schoolboy living on the banks of a river frequented by Salmon. It is also my opinion that neither Salmon nor Trout spawn every year, [2] for Salmon ascend the river as early as January, in the highest condition, with roe in them no bigger than mustard seed: these

Page 6

could not have spawned that season, as the Kelts, particularly the females, do not return to the sea until March or April, [3] and at that time they are in very bad condition, and do not appear to have a particle of spawn in them; and in the evidence of Mr. Mackenzie (see Parl. Rep., p. 21), we have an account of a Grilse Kelt which was caught and marked in March, 1823, and was again caught as a Salmon on its return to the river in March, 1824. In this case the fish had evidently required a residence of twelve months in the sea before it was in a condition to visit the river a second time, and in the Wharfe it is the constant practice of the angler to catch Trout through the winter with very minute roe in them, and in high condition with the worm and Salmon roe, and also with night lines. In fact, one of the fishermen has frequently remarked to me that he occasionally caught dishes of Trout with the fly in January, and in finer condition, than he has found them in April, which he accounted for by saying that the spawned fish (Kelts) of that season had not begun to rise freely at the fly at the former period, but they had at the latter, so that his pannier contained as many Kelts as fresh fish. Another reason has just occurred to me: it is, that in January the spawned fish will still be in the small brooks in which they are so fond of breeding, and of course the bulk of the fish remaining in the river at that time would be fish in good season.

As it is some years since I acquired this information, or at least a part of it, I felt afraid of giving it incorrectly; and I therefore addressed a letter to a friend living on the banks of the Wharfe, requesting him to send me all the information in his possession on this subject, that derived from his own observations, as well as that collected from others. He has since the above was written sent me the following reply:—"I have seen Robinson (one of the best anglers and fly makers between Cornwall and Caithness), and have had some conversation with him on the subject of Salmon, &c. He is of opinion that the spawn of the Salmon remains five months in the gravel before hatching; he examined the spawn in April, and found the young fry alive in the eggs, and Ingham, another angler, took some home and kept one of the Smolts two or three months. I have subsequently seen Ingham, and he has given me the same account. All the fishermen here are of opinion that the female Smolts remain one year, and the males two years, before they go down to the sea. The Bramblings are supposed to be Smolts which remain a year longer than the usual time; they are few in number, and are generally taken with the May fly. I have no doubt that the above opinions are correct, for we have now three distinct sizes of Smolts in the river exclusive of Bramblings, the largest of which are nearly four ounces in weight, and are all males, as they contain milt in October and November. The next are the females of the present year: I have had one since the receipt of your letter, which weighed half an ounce and measured five inches in length; this was a real blue Smolt; the third are the males of the same age, and are much smaller; these are occasionally taken with the worm, and will rise at the fly all the next summer."

Page 7

"We were for several years, but I do not know the dates, entirely without Salmon, and of course without Smolts; and we invariably found that the Smolts made their appearance the year after the Salmon, but were very small till the second year, when we had what we call blue Smolts, which disappeared in May or June; and what you called Pinks, which remained till the following year; and Brambling Smolts, which remained another year. The fishermen here are also of opinion that neither Salmon nor Trout spawn every year. Robinson says that one day lately (the letter is dated December 13th) he caught seven Trouts, six of which were in good season; and he brought me two the other day, one of which contained roe, and the other was in excellent condition." My friend states, in a subsequent communication, that one of the fishermen had told him that he had caught the male Smolt (Par) more abundantly on the Salmon spawning beds than elsewhere, and my friend adds that the opinion there is, that if a female Salmon gets up to the spawning beds, and if no male accompanies her, yet her eggs are fecundated by the male Smolts; and they allege, in support of this opinion, that a female got up one season and spawned, and though no male was seen near her her eggs were prolific. I mention this, although I apprehend it is evidence which the unbeliever will consider inadmissible, for though no male was seen, still there may have been one, or admitting that one did spawn, without being accompanied by a male, yet another, which contrived to bring her mate along with her, may have spawned in the same place the same season; yet, notwithstanding its liability to these objections, I have no doubt myself that if a female were to come alone her eggs would be impregnated by the Par. It is an excellent maxim, that Nature makes no useless provisions; yet, if we admit that Par are young Salmon, for what purpose is the milt if not to impregnate Salmon roe? and if we deny this to be the fact, we must endeavour to show that there are female Par, but in all my examinations, I have never been able to meet with one that contained roe. That the Grilse are Salmon is proved I think sufficiently by the evidence given before the House of Commons. Mr. Wm. Stephens states (see Rep., p. 52) that he has known Grilse kept in a salt-water pond until they became Salmon, and that fry that had been marked came back that year as Grilse, and the year after as Salmon; and Mr. George Hogarth states that he has often seen a Salmon and a Grilse working together on the spawning beds, as two Salmon, or two Grilse; and Mr. Mackenzie states (page 21) that he, in March, 1823, marked a Grilse Kelt with brass wire, and caught it again in March, 1824, a Salmon of seven pounds weight. The testimony of the witnesses from the Ness, the Severn, the Lee, and some other rivers, is too positive and too well supported to admit of any doubt as to the excellent condition of many of the fish ascending those rivers in November, December, and January—a

Page 8

period when they are out of season, and full of spawn generally, and even when many fish are caught in those rivers in the same unseasonable condition. The fact that there are many fish in fine season in those months may be, I think, accounted for, if we admit that Salmon spawn every other year, which I have I think shown to be very probable; but what it is that induces those fish to ascend rivers so many months before the spawning season, I cannot explain. Probably there may be some quality in the waters of these rivers, all the year, which is congenial to the habits of the fish, while the same quality may only be found during part of the year in others; it is certain that the quality of the waters in rivers generally varies very much with the season: thus the water of the Ribble, after a flood in summer, is always of a dark brown colour, being so coloured by the peat moss over which it passes, while in winter no such tinge can be observed; and there may be other differences with which we are unacquainted; however, whether this is the true reason or not, it certainly cannot be that the fish which spawn in October are impelled by their desire to propagate their species to ascend the river the January before; and if this long residence in fresh water were necessary for the proper development of the ova in one river, we might suppose it would be necessary in all; yet this is not the case, as the red fish which ascend the river in November and December have at that time the spawn in them nearly ready for exclusion.

On one point, about which there is great difference of opinion, viz. whether the fish which are bred in the river generally resort to it again, and whether each river has its own variety of fish, I am not a competent judge, as I am acquainted with too few rivers to pretend to decide. I may, however, just remark that the Hodder, though it is a much smaller river than the Ribble, is always much better stocked with Salmon, Morts, Sprods, Smolts, and Par than is the latter river, which I attribute to the fact that more fish spawn in the river Hodder, which runs for many miles through the Forest of Bowland (the property of the Duke of Buccleuch) and other large estates, and the fish are much better protected there than in the Ribble, where, with one or two exceptions, the properties are very much divided, and few people think it worth their while to trouble themselves on the subject. Dr. Fleming, in his letter to Mr. Kennedy (Appendix to the first Rep., 1825), seems to doubt that Salmon enter rivers for any other purpose than of propagation, but lest I should misrepresent his opinions, I will quote what he has said on the subject:—"In the evidence taken before the Select Committee during the last season of Parliament, and appearing in the report, there are several statements of a somewhat imposing kind, which, as they appear to me to be erroneous and apt to mislead, I shall here take the liberty of opposing." He then enumerates several opinions expressed before the Select Committee, one of which is, that Salmon enter and leave rivers for other purposes than those connected with spawning (see the evidence of Messrs. Little, Halliday, and Johnstone).

Page 9

First, "That they enter rivers to rid themselves of sea lice (*Monoculus piscinus*);" secondly, "That they forsake rivers to save themselves from being exhausted by residence in fresh water, and from having their gills devoured by a maggot (*Lernaea salmonea*)." The whole history of the Salmon contradicts this hypothesis. Another of these errors is, that it is asserted (Rep., 1824, p. 145), "That Salmon always return to the same river;" this is not probable, when we consider the circumstances in which they are placed during their residence in the sea. On the first of these opinions, I am not a competent judge; but I think that the fact that Salmon enter rivers nine or ten months before they are ready to spawn, is of itself sufficient to show that there are other reasons for their entering rivers than those connected with propagation. With respect to the second, I believe that after Salmon have once entered rivers, at least when they have ascended into the upper parts of them, they never offer to descend again until they have spawned. On the third opinion I would remark, that although I do not think that Salmon always come to the same river in which they were bred, yet I think they will do so if they can; and I think that the fact which I have mentioned of the Hodder, a smaller and a tributary stream to the Ribble, containing many more Salmon, as well as more Morts and Sprods, countenances this supposition, for why should the larger number of fish ascend the smaller river except for such a reason?

I am of opinion that Salmon do not grow so fast in the sea as is generally supposed. It is here generally believed that the Smolts, which go down in the spring, come up again in the August or September following, five or six pounds in weight; and George Little, Esq., in his evidence states that as his opinion, but he does not give any other reason for it than this: "That the Grilse that ascend the river in June weigh one and a half or two pounds, and that those which come in September weigh five or six pounds," —but opposed to this supposition is the evidence of Mr. Mackenzie, before referred to (second Parl. Report, p. 21), who states that he caught in March a Grilse Kelt which weighed three and a half pounds, that he marked it with a brass wire, and let it go, and that in the March following he caught it again a Salmon of seven pounds weight. Now a fish which weighed three and a half pounds as a Kelt, would weigh five pounds or six pounds when in high condition the summer before, and if this were so, which I believe all persons who are acquainted with Salmon will admit, the fish would have gained only one pound or two pounds in fifteen or eighteen months. Besides, if Salmon grew as fast as is stated and believed by many persons, the breeds of different years would vary very much in weight, whereas it is known to everybody that we have them of all sizes, from five pounds to forty pounds; and it is contrary to analogy to suppose that a fish

Page 10

which is two or three years in arriving at the weight of as many ounces, should in two or three months acquire as many pounds. There are, however, two or three things about which all persons agree in opinion—one of these is: that the breed of Salmon is decreasing every year, and that the great cause of this decrease is the want of protection, and a consequent destruction in the spawning season. The complaint on this head is universal from north to south; from the Shannon to the Tweed, the cry is—"Protect the breeding fish, or we shall very soon have none to protect." And yet, although the destruction of the spawning fish, and the destruction of the fry in the Spring, are the chief reasons for this alarming falling off, no one seems able to devise a remedy; no one seems inclined to make the necessary sacrifices for so desirable an object, and without these sacrifices it would be absurd to expect the fish to become plentiful; and instead of furnishing an abundant supply of cheap and wholesome food to all classes, which they certainly would do if the fisheries were properly regulated, they will either become wholly extinct, or so rare as to be found only at the tables of the wealthy. James Gillies, in his evidence, states that his brother had in one night killed in the Tweed four hundred Salmon at one landing-place in close time; and all the reports are full of statements showing how unceasing and universal is the persecution the Salmon undergo, not only when in season, but at all times, and most of all when every one should do his utmost to preserve them—I mean when they are spawning. In this neighbourhood the properties generally are so much divided, and so few good fish are allowed to ascend the river, that no one has any interest in protecting them in close time, and the consequence is, as might be expected, that all sorts of contrivances for taking them are resorted to: they are speared and netted in the streams by day and night; they are caught with the fly, they are taken with switch hooks (large hooks fixed to the ends of staves), or with a triple hook fixed to the end of a running line and a salmon rod; if the river becomes low, parties of idle fellows go up each side of it in search of them, and by stoning the deeps, or dragging a horse's skull, or large bone of any kind through them, they compel the fish to *side*, and there they fall an easy prey, in most cases where the pool is of small extent. In a river so small as the Ribble, it will be readily believed that not many fish can deposit their spawn in safety, when practices of this kind are followed almost openly, and when no one feels a sufficient interest in the matter to put a stop to them. A single party of poachers killed four hundred Salmon in one spawning season near the source of the river; the roe of which, when potted, they sold for L20. Need we be surprised, then, if the breed decreases? The only wonder is that they have not been exterminated long ago.

Page 11

I may perhaps be allowed to say what, in my opinion, would remedy this alarming destruction, particularly as no one hitherto seems to have devised an efficient preventive. I believe that in 1826 there was an Act of Parliament passed which either repealed or modified some of the old laws on the subject, and I have also understood that the good effects of this new law are already perceptible in Scotland, to which it is exclusively applied. There was a bill introduced into Parliament in 1825 which was intended to apply to the whole kingdom; but some of the clauses were so very objectionable, that if they had been carried they could not possibly have been enforced without stopping and ruining the manufactories which were carried on by water-power, and the bill was consequently abandoned. The first thing to be done is to give the proprietors on the upper part of the river such an interest in the fisheries as will make them anxious about the preservation of the fish in the spawning season; and to accomplish so desirable an object no one ought to fish or keep a net stretched across a river for more than twelve hours each day, or from sunrise to sunset; and every mill-owner ought to be compelled to facilitate the passage of the fish over his weir by every means consistent with the proper supply of water to his wheels. At present the fisheries at the mouths and lower parts of rivers so completely prevent the access of the fish to the upper parts, that unless there happen to be high floods, which prevent the fishermen below from keeping their nets in, the upper proprietors comparatively seldom see any until the season is at an end. The evidence before the House of Commons on this point is exceedingly amusing. One person thinks the upper proprietors have no right to expect any fish, as they have never paid any consideration for them when they bought their estates; another states that he pays £7,000 a year to the Duke of Gordon, and that if he is compelled to observe a weekly (not a daily) close time, he will lose that proportion of his rent; another observes the weekly close time, and opens a passage for the fish, but places a crocodile, painted in very glaring colours, in the gap to frighten them back again; another says he observes the weekly close time in his cruive fishing, but no one is allowed to inspect the cruives; another sends men to break down the stake nets in the estuary, which reach from high to low water-mark, and at the same time stretches a net completely across the river from March to August, so that a fish cannot pass without his permission. No wonder that fish are scarce in the upper parts of the river, when such samples of *disinterestedness* are manifested by the proprietors of the fisheries below. No wonder that the upper proprietors should be careless about the protection of fish from which they are not allowed to derive any benefit. No wonder that they should connive at, and even encourage, the shameful destruction of fish in close time, since that is the

Page 12

only time they are allowed to have any. Let the fishermen below make it worth the while of the upper proprietors to protect the fish, and they will receive that protection; but it is too much to expect from human nature that these proprietors will take all the odium and trouble of preserving them when others reap all the benefit. There ought to be conservators employed, to see that the fisheries are properly regulated, and these should be paid by an assessment on all the proprietors in proportion to the value of their fisheries.

I should also recommend an extension and uniformity of close time in all the rivers in the kingdom, for although it is an undoubted fact that some clean fish are caught in the river early in the season, yet they are comparatively few in number, and their capture involves that of a far greater number of spawning and Kelt fish, which are not only of no value for the table, but the destruction of which is in effect the destruction of millions of fish which would proceed from them. In the first Parl. Rep., p. 11, Mr. Walter Jamieson says, that in the river Tweed, from January 10th to February 1st, he caught one hundred and twenty-one fish, only one of which had spawned; from February 1st to March 1st he took forty-four fish, twenty-five of which had not spawned—fifteen were Kelts and four were clean fish; from March 1st to March 10th he took seventeen fish, seven of which had not spawned (four of them on the 10th)—six were Kelts and one clean fish. Now the close time varies in almost every river, and some have no close time at all; thus in the Ribble the close time begins on September 15th and ends on December 31st, and in the Hodder there is no legal close time; but there is no practical difference between them in this respect, every one thinking himself entitled to kill all the fish he can, at all times of the year, in both of them. The observance of the weekly close time, that is, opening a passage for the fish from sunset on Saturday night to sunrise on Monday morning, is a mere farce, even if it could not be evaded, as it almost invariably is, for it is well known to every one conversant with the habits of Salmon, that they only ascend the rivers when there are freshes (floods) in them, and in summer the ground is generally so dry, and vegetation absorbs so much moisture, and the evaporation is so great, that it not only requires twice as much rain to produce a flood in the river then as it does in winter, but when the rain does come its effects are only visible in the river for a short time. I have known a strong fresh in the Ribble in the morning, and the river low again in the afternoon of the same day. A fresh coming at the beginning of a week, would disappear long before the close of it, unless the rainy weather continued; and thus the strict observance of the weekly close time would be of little service to the upper proprietors unless the fresh came at the right end of the week.

Page 13

The Smolts and the Par ought to be protected as strictly as the Salmon; and there ought to be a penalty attached to the killing of them, or having them in possession, and conservators of rivers ought to have the power of inspecting all mills and manufactories driven by those rivers, to ascertain that they have no contrivances for taking the fry on their way to the sea, as it appears that in some rivers they are taken in large quantities. There ought also to be a penalty attached to the killing of Kelt fish, which in that state are not only tasteless and insipid, but actually unwholesome; yet they are pursued and destroyed with as much avidity as the fresh fish, and a very small number of the few that spawn in safety ever return to the sea. A penalty ought also to be inflicted for selling, buying, using, or having in possession Salmon roe, either in a fresh or salted state, as its excellence as a bait for Trout and Eels, and the consequent high price at which it sells, are sufficient temptations to poachers to kill the Salmon in the spawning season even if they could not sell or use any other part. Yet destructive as this practice is, there is an extensive trade in this article— a fishing-tackle maker in Liverpool having told a friend of mine that he sold 300 lbs. in a season, which, supposing every egg to hatch, would produce perhaps five times as many Salmon as are caught in one year throughout the whole kingdom. [4]

In concluding this imperfect sketch, I may remark that I have omitted many things concerning the natural history and habits of the Salmon, fearing to trespass too much on the patience of my readers; but I have wished, in addition to communicating some facts in the natural history of this fish, which I believe are not generally known, to call the attention of the public to the present state of the Salmon fisheries in England. Many of the preceding observations are founded on the evidence of persons connected with the fisheries in Scotland, and are perhaps no longer applicable to that part of the kingdom, since there has been an alteration in the laws; whether this is the case or not, I have no present means of ascertaining. I shall be glad if any one having a knowledge of the subject will say what benefit, if any, has been derived from the alteration; however, it is sufficient for my present purpose to show what is the state of things when there are no laws on the subject, or, which is the same thing, when there is no attention paid to them; a state of things which, instead of promoting an abundant supply of these excellent fish, and rendering the Salmon fisheries nationally important, tends by the habitual disregard of the laws by one party, the selfishness of another, and the neglect of a third, to render these fisheries of little and decreasing value; whereas if the lower proprietors would allow a tolerable supply of Salmon to come up the river when they were worth taking, and the upper ones would preserve them during close time, there would be plenty for each and for all.

Page 14

I am aware it will be difficult to legislate upon this subject without injury to what is of infinitely greater importance—I mean the manufactories of the country. The absurd and impracticable clauses which were contained in the bill for the protection of the fisheries, which was introduced into Parliament in 1825, show this; yet notwithstanding this difficulty, I think it is possible to protect the fish without interfering with the interest of the mill-owners, and to make such laws on the subject as will be effectual, without calling forth a single objection from any unprejudiced person. I shall be glad if what I have said on this subject should induce any gentleman to turn his attention to it. There must be many whose opportunities of observation will enable them to determine whatever is doubtful in the natural history of the Salmon tribe; whose experience will teach them the defects and absurdities of the present laws on the fisheries; and whose influence will, if they can be induced to exert it, materially contribute to their amendment.

Clitheroe, January, 1834.

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The salmon enters and ascends rivers for other purposes besides propagation. [5]

In addition to the objections which I have offered to the seeming doubt of Dr. Fleming, whether Salmon enter rivers for any other purpose besides propagation, the following have come to mind; and though they do not apply to the Salmon, they confirm me in the opinion that there are reasons, of which we know nothing, for fish ascending rivers, which are not at all connected with propagation. One is the habit of what is here called streaming. In the winter the fish not engaged in spawning (I speak of Trout, Grayling, Chub, Dace, &c.) leave the streams and go into deep water; either because the water is warmer there, or because they there find more food; and it is well known to fly-fishers that they do not catch many fish in the streams if they begin early, say in February. It is proverbial here that fish begin to stream when the great grey, or what is called in other districts the devil or dule crook, and in March brown or brown drake, comes upon the water; and I have seen Trout by scores leaping at a weir in the beginning of May, whether in search of food or an instinct implanted in them to keep all parts equally stocked with them, I do not know; but it has certainly nothing to do with their spawning. Is it presumptuous to suppose that God in His providence has implanted this instinct in Salmon for our good, that we might have a supply of excellent food, which without this would be in a great measure unattainable? Whether this is the true cause, and the only one, I am unable to determine; but this is the effect produced, and in the absence of other reasons it is, in my opinion, one that ought to be admitted. Another reason why fish ascend rivers is their impatience of heat. I speak now

Page 15

more particularly of Grayling; if the weather is very hot at the end of May or the beginning of June, the Grayling in the Wharfe (they are almost unknown in this part of the Ribble) ascend the mill streams by hundreds, and go up the wheel races as far as they can get, and stay there until the stoppage of the wheels (many a ducking have I had in pursuit of them), when they are obliged to beat a retreat, and this often proves a disastrous one to many of them. The ascent of young Eels by millions, and the ascent of the Flounder, are neither of them connected with the propagation of their kind, and though I cannot say for what purposes they do ascend, I am, I think, justified in doubting assertions which seem to have nothing to support them but the positive manner in which they are made.

The Salmon Par is neither a Hybrid nor a distinct species of the *genus Salmo*, but a state of the common Salmon. The author of "Wild Sports of the West" says of the Par, as I have noted previously, "That it has very much the appearance of a Hybrid between the Salmon and the Trout, and (in a note) that the natural history of this fish is doubtful. Some conjecture that it is a Hybrid between the Salmon and Trout, because it is only found in rivers which are frequented by Salmon. Others think it a cross breed between the sea Trout and river Trout," and then he speaks of this "hybridous diminutive," as if he thought one of these opinions was correct. That the Par is not the result of a cross between a sea Trout and a river Trout, is proved by the fact that there are no sea Trouts in the Wharfe, the Par (admitting it to be a distinct species, which I do not), the Salmon, and common Trout being the only kinds of Salmonidae which are found in that river, at least where I am acquainted with it. If the Par be the result of a cross between the Salmon and the Trout, what becomes of it in the spring, and where are all the Par, which were so abundant in October, gone to in April? Did they migrate to the sea, the shoals would be met with by somebody; and did they stay in the river they would be caught at one time or other. However, as it is well known that neither of these cases is ever realized, we must suppose another, which I have already done in my former communication. In fact, in angling in the beginning of March, fish are often caught which would puzzle the most experienced fisherman to determine whether they are Par or Smolts, especially after they have been caught some time; and in a large number caught at that time there are all the intermediate shades of appearance between the perfect Par and the real blue Smolt.

Clitheroe, May 29th, 1834.

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Clitheroe, March 18th, 1846.

To Mr. Pakington (Lord Hampton).

Page 16

Sir,—Through the polite attention of Mr. Cardwell I have been favoured with a copy of your bill—"For the better preservation of Salmon." As this is a subject to which I have paid some attention, I trust it will not be deemed impertinent if I offer some suggestions for your consideration with regard to the free gap. It appears to me that it will be desirable to specify the width and depth of this free gap, or it may on the one hand degenerate into a mouse-hole, or on the other hand the surveyor, by the provisions of the 13th section of the Act, may insist on such a gap being made that the whole of the water may be diverted through it, which in small rivers, where there are ancient and legal hecks or cruives for the purpose of taking Salmon, will destroy the value of the fishery. Then, with regard to fence time:—In the 6th section of the Act, I presume you do not intend that night fishing shall be allowed at any season of the year; but it appears to me that the expressions in the 6th section would scarcely prevent the owners of cruives from keeping them open, as they need not go near them between sunset and sunrise, and then they will neither lay, draw, nor fish with any net, device, or engine. Would it not be better to expressly insist upon all cruive fisheries being positively closed from sunset to sunrise? or, what would be still better, that the cruive or heck should have a free gap in it, of a specified size, which should be kept constantly open between sunset and sunrise. As this is one of the most important sections of the Act, I may be pardoned for calling your particular attention to it; for unless this section be vigorously enforced, it will be in vain to legislate on the subject;—for the proprietors near the sources of rivers (where most of the fish spawn) will never interest themselves about the preservation of fish which they are not allowed to see when in season, and which has hitherto been the case in this neighbourhood at all events; but if the fish are allowed a free passage everywhere, and at all times, between sunset and sunrise, the upper proprietors will then have some inducement to take care of the fish in the spawning season. Until now, all the good fish have been taken in the fisheries near the mouth of the river.

There is at present a great trade carried on in this neighbourhood in Salmon roe, as a bait for Trout and Eels, and scores of spawning Salmon are now destroyed for little else than the spawn they contain. Cannot this be prevented?

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May 5th, 1846.

H. GEORGE, ESQ.

SIR,—I enclose a letter I had addressed to Mr. Pakington on the subject of the preservation of the breed of Salmon. I had written to him because I perceived that he had introduced the bill into the House of Commons, but since that letter was written I have been favoured with your address through the politeness of Sir Thomas Winnington, to a friend of mine, and as he requests that any suggestion about weirs may be addressed to you, I make no apology for enclosing the letter I had addressed to

Mr. Pakington with some further suggestions, which on looking over my letter I find I have omitted to notice.

Page 17

In one of the clauses of the bill (I do not remember which, and I have not the bill at hand to refer to) you require that a grating, the bars of which shall not be more than three inches distant from each other, and which shall be placed at the junction of the tail-goit with the river, as well as in front of the wheel. This I presume is to prevent any fish being injured by the wheels, but I assure you that during the twenty-two years in which I have had the management of the works here, I never knew an instance of a Salmon being either killed or hurt by the wheels. Indeed, I do not know half-a-dozen instances of Salmon ever ascending the tail-goit to the wheel, and I must have seen many instances if this was a common occurrence. This may, however, happen, and the fish may be occasionally injured where there is much fall lost, and a strong stream running from a wheel constituted in the old way with open float boards. But the objections to such a plan on the part of the manufacturers will be insuperable, in fact, the accumulation of sticks and leaves in the autumn, and ice in the winter, will be so great at the grating in the tail-goit, that the wheels will be thrown into back water and the works stopped, and all this loss and inconvenience will be incurred because of the possibility of a Salmon being killed or hurt by the wheel. There is not much probability of this frequently happening, because, as I said in my other letter, Salmon seldom migrate except where there are freshes in the rivers, and then there is so much water flowing down the usual course of the stream, that the fish have no inducement to leave it to seek for a passage elsewhere. I would, however, suggest that power be given to conservators to go at all times up the tail-goits and into the wheelhouses, to see that there are no illegal contrivances in them for catching the Salmon and Smolts in their migration, as I have certainly heard of such things occurring.

In Sir Thomas Winnington's note to my friend, he says we have difficulty enough in endeavouring to obtain support for one day's clear course; two we could not carry, however desirable. Allow me to suggest, that in endeavouring to carry so little you rouse up your opponents, while there is not enough to stimulate the zeal of your friends, for it will be in vain to look for the zealous co-operation of the proprietors on the upper part of rivers unless you give them some inducement. This one day in the week will not effect, and besides this, you make it illegal to catch Smolts, even with the rod, which is destroying one of the greatest amusements of the anglers, and depriving them of the most delicate of fish, and for no object: because, if the provisions of your bill are carried (without this clause), there will be an abundant supply of fish for all purposes, even after the anglers have enjoyed their sport. I do not see the propriety and utility of prohibiting the killing of Smolts, because if they lived they would become Salmon, any more than I see the propriety of prohibiting the eating of eggs, because if they were hatched and lived long enough they would become barn-door fowls.

Page 18

Let the legislature and the estuary fisheries give the upper proprietors a fair share of Salmon when in season, and they will be glad to see the angling for Smolts abolished; but it is rather too bad for the estuary fisheries to catch all the good Salmon, and then grudge to the upper proprietors the angling for Smolts.

In conclusion, allow me to urge on you the propriety of endeavouring to obtain such a bill as will give the proprietors of land on the upper parts of rivers a strong inducement to support you, and at the same time that it does this will not injure the mill-owners; and, with the modifications I have pointed out, I think this may be accomplished. I speak on this subject as a practical man, having some knowledge of the habits of Salmon, and superintending a mill driven by water-power which employs nearly a thousand people; so that if a bill like yours could be worked in a satisfactory manner here, on so small a stream as the Ribble, it may anywhere in the kingdom. But if you make a tinkering job of it, and ask for too little, you will rouse your opponents and discourage your friends. By all means go for a free passage for the fish every night from sunset to sunrise in all cases where this does not interfere with manufactories, and then there will be some inducement to support you.

I refer you to some papers which I wrote on this subject in the Magazine of Natural History, in the year 1834, and if you think it worth while to ask for further information on the subject, I shall be happy to give you any I may possess.

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LOW MOOR, *July 1st*, 1846.

To the Editor of "The Times."

The attempt which is now making to amend the laws relating to the Salmon fisheries, appears to run such a great risk of failure, from the opposition of interested persons, that I think a short sketch of the defects of the present laws and their effects on the breed of fish, and a comparison of them with the proposed amendment, may be interesting to some of your readers, and may, perhaps, induce some influential gentlemen to throw their influence into the right scale, in the approaching discussion on this subject.

The Salmon fisheries in former times appear to have supplied food for a large portion of the people, as there are still traditions current on the banks of various rivers in the north, that the indentures of apprenticeship always stipulated that the apprentice should not be compelled to eat Salmon more frequently than three days a week, and however exaggerated this story may appear at the present day, I hope to succeed in showing that it is neither improbable that it has been so, nor impossible that it may be so again, —if good laws are made for their protection, and these laws are properly enforced. At present there is no doubt the fisheries are rapidly declining, and in some rivers which

used to have a good many Salmon in them, and which used to swarm with Smolts (or fry)

Page 19

in the spring within my remembrance, they are now rarely seen. To show their scarcity I may mention a circumstance which occurred in the Wharfe, which was formerly one of the finest rivers in Yorkshire for Salmon. A few years ago a pair of Salmon were seen on a spawning bed in the Wharfe, about forty miles from its mouth. This became known at the anglers' club, and it was deemed so important to preserve them, that the club divided themselves into three or four watches, and guarded the spawning bed night and day, whilst the fish were spawning, and this spawning lasted about a week.

Here in the Ribble the Salmon fisheries are not quite so near extinction (though they are rapidly progressing in that direction), for although we are very seldom allowed to see or catch fish in seasonable condition, a good many come up the river to spawn, though very few of them ever do so, and very few of those that do ever reach the sea again. The reason is obvious, no one here has any interest in preserving the spawning fish, and they are openly killed by the poachers, who never dream of being prosecuted for it. I am credibly informed that in a stream not five hundred yards from where I write, sixty spawning fish were killed last winter. Some years ago one gang of poachers killed three hundred Salmon on the spawning beds in one season, and sold potted Salmon roe (which is a most destructive bait for Trout) to the value of £20.

In the Lune the proprietors of the fisheries near Lancaster sent men to protect the spawning fish in the streams above; but these men were warned off by the landed proprietors, who said, If you catch all the good fish you must at least allow us to catch the bad ones. In the Tweed and its tributaries it used to be quite as bad (what the new Scotch law has done I do not know), but a poacher who gave evidence before the Committee of the House of Commons in 1825 said that he had assisted to take four hundred Salmon at one haul in close time in the Tweed.

Sir Walter Scott's vivid description of burning the water, which occurs in "Guy Mannering," shows that he knew how to kill Salmon in close time. In fact, his account, and that of Hogg (the Ettrick Shepherd), show that both were regular black fishers.

There are various devices for killing the fish in close time: they are speared, netted, and hooked on the spawning beds, and when the rivers get low, gangs of idle fellows range up and down on the banks, stoning and beating the water by poles, or, what is more effective still, a large bone, or horse's skull, and by fastening a cord to it, one end of which is passed to each side of the river, they draw this skull up and down in the pools where they know there are Salmon, and the fish are so foolish and timid, that they thrust their heads under any stone or cover they can find, and are taken without trouble; it being common enough in such cases to slip a noose over the tail, then tightening it, and the fish is hauled out immediately.

Page 20

Then again, gentlemen who want to have the reputation of being skilful anglers, employ their game-keepers to find the Kippers (Scottice Kelts) or spawned fish in the pools, which is a very easy matter in low water, and dropping a hook baited with a lob worm before their noses, it is greedily taken, and the poor fish (which are unfit for food) are caught. It is then trumpeted forth to the angling world that Mr. A. B. has had splendid sport—he has caught a dozen Salmon with the rod in a single day, meaning it to be understood that these fish have been caught with the fly. I by no means uphold these practices, neither do I think them very deserving of censure in the present state of the law, for all the good fish are taken near the mouths of the rivers.

This leads me to consider the defects of the present law, which is by no means adapted to protect and increase the breed of Salmon.

In the first place, the close time is too short. It commences in the Ribble nominally (for in reality the fish are openly killed all the year through) on the 15th September, and ends on the 31st of December; whereas it ought to extend to the end of April, for the following reasons. A very large proportion of the fish are spawning in January and February, and I have even seen a spawning fish as late as the 3rd of April. In the evidence given before the House of Commons in 1825, it was proved by a fisherman from the Tweed, that in March for one clean fish that was caught there were ten caught that were not so, as they were either fish that had not spawned, or Kelts, that is, fish which have finished spawning but have not returned to the sea, and are then flabby, unwholesome, and unfit for food. A very large proportion of these Kippers or Kelts do not go to the sea until April, and not then without there is a fresh in the river, for, like the Smolts, they seem disposed to remain in the rivers until they can avail themselves of the assistance of a flood, to enable them more easily to reach the sea.

Another defect in the present law is that it fails to secure a supply of good fish to the upper proprietors. There are no provisions in it (or they are not enforced) for giving the fish a free passage, no prohibition of nets, traps, or devices for stopping them in their progress up the rivers. No daily or weekly close time, but everywhere there is so short-sighted a selfishness, that it is completely realizing the fable of the man who killed the goose which laid the golden egg. The fisheries are declining so rapidly, that unless something is done, and done quickly, the breed of Salmon will be extinct in the rivers in this neighbourhood.

Again, there is no power to appoint or pay conservators, and without their assistance there is no chance of preserving Salmon in the spawning beds. Game-keepers are most certainly not to be depended upon.

Page 21

In pointing out the defects of the present laws I have, in fact, given an opinion how they should be remedied. I would extend the close time from the end of September to the end of April. I would establish a daily close time, allowing no net, device, or engine to be employed in taking Salmon between sunset and sunrise above tideway in any river; and below, I would only allow nets to be set for twelve hours per diem. I would appoint conservators, whom I would pay by a tax on the fisheries on the whole course of the river, which tax should be determined by a valuation of the fisheries, and paid accordingly. I would fine every one who sold, used, or had in his possession any potted or prepared Salmon roe for the purpose of angling, and I would give conservators the power of examining all mill goits and races, for the purpose of seeing that no unfair practices were resorted to for the taking of Salmon or Salmon fry; and I would give the upper proprietors the power of making any alterations in mill weirs and dams which did not impair their stability or the efficiency of the water power. If some such enactments as these were made and properly enforced, there is no doubt Salmon would swarm in every river, for their fecundity is such, that a very few Salmon spawning in a river under favourable circumstances stock it abundantly with Smolts. A large Salmon having not less than 25,000 eggs in it, how soon, with a little forbearance and care, would every river swarm with this delicious fish, even to such a degree as to be a cheap food for the poor! But to obtain such results it must be made the interest of every person to protect them.

In reading over the evidence on the Salmon fisheries, which was given before the House of Commons in 1825, I was exceedingly amused by the reasons given by the tenants of some of the fisheries in Scotland why there should be no weekly close time, and the shifts and evasions practiced by others. One said he paid L7,000 a year rent to the Duke of Gordon for his fishery, and if one day in the week were allowed for close time he would lose L1,000 a year. Another said he kept the close time, but he would allow nobody to go and see whether he kept the free gap open or not. Another proved that he kept open the free passage, but it was also proved that he had a crocodile placed in the gap, painted with very glaring colours, in order to frighten back any fish that attempted to pass. Another sent his boats to break down the stake nets which were set in the estuary, but acknowledged that he kept his own nets set across the river day and night. There would be no difficulty in stocking every suitable river in the kingdom with Salmon, either by putting into them a few pairs of breeding fish, or by artificially fecundating the eggs, and placing them in artificial spawning beds. It is a plan I have frequently adopted, and sometimes successfully; but in other experiments I have failed, from the difficulty of choosing a suitable locality

Page 22

in the river. If too rapid a stream was chosen, the eggs and gravel were all washed away; and if too calm and still a place was selected, the gravel was filled up with sand and mud, and the eggs rotted instead of hatching. I am even of opinion that where there is already a breed of Salmon fry in a river, it is not absolutely necessary that any male Salmon should come up the river in the spawning season, the male Par, or Penks, as we call them in the Ribble, being sufficient to fecundate the eggs. If this is doubted, I would ask how it happens that in the autumn they have fluid milt in them? for as nature makes no unnecessary provisions, for what purpose is this, if not to provide for the possibility of a female Salmon coming alone? These Pars swarm on the Salmon spawning beds.

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SUGGESTIONS FOR AN ALTERATION IN THE LAWS REGARDING SALMON.

CLITHEROE, *October 12th*, 1851.

To the Editor of the "Gardeners' Chronicle."

As the amusement of fly-fishing is one which holds a first place in the opinion of every one who understands it, and as the Trout and the Salmon are the only fish which afford genuine sport to the angler, and as I believe that the latter in some of the southern counties is nearly extinct, whilst the former is far from being abundant, I wish to call the attention of such of your readers as are possessed by the true *piscatorial furor*, to the facility with which these fish can be bred artificially. And as many experiments have been made under my direction, and having witnessed the results, I unhesitatingly say that there is little risk of failure, if due care be taken.

The experiments of Shaw and Agassiz, my own also included, have proved that fish can be bred artificially. The experiments of Boccus I have not yet tried, although he proposes to arrive at the same result in another manner, and acting in the manner recommended by them, Trout and Salmon have been bred by thousands during the last ten years.

As the season for making the experiment will shortly be here, I hope that those who intend to try the plan will lose no time in looking after their supply of breeding fish.

To begin with Trout:—Catch as many as you can conveniently obtain upon the spawning beds, [6] and examine them carefully one by one, to see that the spawn and milt are in a fit state for exclusion; and also to enable you to separate the males from the females. If they are in a fit state to be operated upon (which may be known by the facility with which the milt and the roe run from them on a slight pressure), squeeze the milt of the



males into a little water, and when you have obtained all the milt you can get, add so much water that the mixture remains slightly opalescent—say about equal in colour to a tablespoonful of milk mixed in a quart of water; pour this into a deep dish or bowl, large enough to hold the largest of your female Trouts; take one of

Page 23

these and put it into the water so prepared, and gently squeeze the roe from it whilst the vent is immersed in the water. [7] Do this as quickly as possible, and return the fish into fresh water, and then pour off the water containing the impregnated roe, through a strainer, carefully preserving it for the remaining fish, and immediately return the roe into fresh spring or brook water. Repeat the operation for every female Trout, and you will then have a quantity of impregnated roe, which if properly managed will hatch with great certainty. Have ready as many boxes as you are able to stock with spawn (three feet long, two feet broad, and six inches deep). Fill them to the depth of two inches of river sand, which ought to be previously so well washed that there is not a particle of mud left in it, and upon that put two inches of river gravel, also exceedingly well washed, the pebbles varying in size from a hazel nut to a pigeon or pullet's egg. These boxes must be so placed that the water from a spring will flow into the first, and from the surface of that into the second, and below the whole nest of boxes there ought to be a small reservoir made—say three yards by two and eighteen inches deep, and well gravelled at the bottom. All these matters having been previously arranged, and the water flowing nicely over the gravel, sprinkle the impregnated roe equally over the surface of the gravel, say a quarter of a pint to each box, and it will roll down into the interstices of the gravel and find a bed in which it will remain snugly until the spring, when, about March, if all has been properly managed, you will find, on a careful examination, that the young Trout are coming to life by hundreds. I am very particular in recommending spring rather than brook water, for several reasons. In the first place, brooks are liable to be flooded, and are sometimes so overcharged with sand and mud that the gravel in the spawning-boxes is completely choked with it and the spawn is lost, as I know to my great and frequent disappointments. At other times all is washed away together. In the second place, the gravel of brooks swarms with water-lice (shrimps) and the larvae of aquatic insects, as well as bull-heads and loaches, all of which prey upon the spawn of the Trout and Salmon. In the third place, if you put your spawning-boxes in a brook, you will find it difficult to prevent the escape of the fry when hatched, and you are left in doubt as to the success of your experiment. With spring water all these inconveniences are avoided. But if your watercourse should contain water-lice or aquatic larvae, it is a very easy matter to destroy them before putting in your boxes, with a little salt or quicklime. It is also desirable to cover your spawning-boxes with a wire grating, to exclude the light, and to protect them in severe weather from the chance of being frozen.

Page 24

When they begin to hatch, open a communication between the boxes and the little reservoir below, and if this communicates with a watercourse in which aquatic plants are growing, so much the better. The fry, as soon as they are strong enough, will make their way into this ditch, and will find abundance of food among the water plants; thence they ought to be able to make their way into the brook, river, or lake which it is intended to store with them. All ducks, wild and tame, should be driven from this ditch, or few of the Trout will be allowed to find their way to their final place of destination.

These rules, with some modification, are applicable to the breeding of Salmon as well as Trout; the only difference being in the mode of placing the female fish, when obtaining the roe, and the size of the gravel in which the spawn is deposited in the boxes. The Salmon is too large a fish to put into the vessels in which the diluted milt is placed, but I think that she should be held by an assistant, in such a manner that the tail and lower part of the body up to the vent are immersed in the water containing the milt. And it is also very necessary to hold her firmly, otherwise a large fish, in the struggles which it makes to get free, is apt to upset the vessel containing the milt, and then the experiment is at an end, at least for the time. Being held firmly by the assistant, as above stated, the belly of the fish must be gently pressed by the hands to promote the exclusion of the spawn, which on exclusion must be gently stirred in the diluted milt, to bring every grain into contact with it; but the roe ought not to remain in contact with the milt a minute, if it can sooner be got out, as I have found that if the diluted milt be too strong, or if the ova remain too long in contact with it, they become opaque, and never hatch at all, apparently because they are over-impregnated. In the ordinary way in which Salmon and Trout are bred, the milt must be largely diluted with water, and the contact between the milt and ova can only be momentary, for the streams in which these fish spawn (particularly the Salmon) are so rapid, that the milt on exclusion must be carried away immediately.

There is another method, which is preferred by Ramsbottom, to the one I have been describing, and it is certainly less troublesome. This is to take the ova from the female fish in the first place (taking care to exclude the air from it, by immersing the fish into water up to the vent), and when all the roe has been collected into a large bowl or basin, then mix the milt with it, the same diluted in the proportion which has been before described, namely, until the water which covers the roe becomes lightly opalescent.

Page 25

I am quite aware that there is another theory which assumes that impregnation takes place twelve months before the exclusion of the ova. [8] But a very careful and long continued examination of the spawning of minnows and lampreys (I have never been able closely to examine the spawning of Salmon), convinces me that it is not a correct one. Besides, did any one ever succeed in hatching the ova of a fish which had not been allowed to come in contact with milt after exclusion? If they have, when, where, and how has this been accomplished, and where is it recorded? I know that I could never succeed, although I have often tried the experiment. On the other hand, it is the easiest thing imaginable, with due care and a suitable situation, to hatch those which have been properly impregnated after exclusion. But if, to avoid argument, I admit that this theory is correct, it will not at all interfere with artificial breeding of Trout and Salmon; on the contrary, it will materially facilitate it. It will only be necessary to catch female fish with the ova ready for exclusion, and place these ova in clean gravel in a box, as before described, but there will be no occasion for males. But supposing Trout and Salmon can be bred in this manner, which I by no means believe, there would be no means of breeding hybrids, which I consider a far more important achievement, and to which I will now refer.

Ever since my attention was turned to the artificial breeding of fish, it has always appeared to me exceedingly desirable and important to breed hybrids between the Trout and the Salmon. The fry of the Salmon, which, by-the-bye, is perhaps the most delicately flavoured fish that exists in this country, although it lives and thrives in fresh water for two or three years, if kept in a locality where it cannot escape to the sea, yet, if kept longer than that time, pines away and dies. If, therefore, we could obtain a hybrid fish, bred between the river Trout and the Salmon, we should probably produce a fish which, being a mule, would be always in good condition; being crossed with a river fish, it would probably never require a visit to salt water to keep it in good health. Being crossed with a Salmon, it ought to get to a good size in a comparatively short period; and, if it would rise at the artificial fly, or the minnow, ought to afford first-rate sport to the angler.

There does not appear to be a greater specific difference between the Trout and the Salmon than there is between the horse and the ass, between the mallard and the musk duck, or between a cabbage and a turnip. But hitherto, in all my experiments, I have never succeeded in producing a hybrid between the Trout and the Salmon. [9] Yet I do not despair of doing so, for there was always something to complain of, and to doubt about, in every one I tried, and I still think I shall succeed by perseverance. Even if I shall succeed, the result may not prove quite so favourable as I anticipate, but may turn out as unfortunately

Page 26

as the marriage of the gentleman in the story, which relates that, being good-tempered but ugly himself, he married a handsome ill-tempered wife, hoping that his children would have his good-temper and their mother's good looks; but when they came, they were as ugly as the father and as ill-tempered as the mother. So it may prove with these hybrids—they may not always thrive in fresh water; they may not grow to a good size; they may not rise at the artificial fly; they may be worthless for the table. Nevertheless, it is desirable if possible that this should be ascertained. The progeny of a male Salmon and a female Trout may be much better or much worse fitted for a continual residence in fresh water than the descendants of a male Trout and a female Salmon; but this can only be determined by experiment. Dr. Lindley says, in his introduction to the "Guide to the Orchard," that in the cross fertilization of fruits, the seedlings always partake more of the character of the male than of the female parent. But I believe that in breeding mules it is found more desirable that the father should be an ass than a horse. In my poultry yard I breed hybrids between the musk duck and the common duck, and I find that I have a much better progeny from the musk drake and the common duck than from the common drake and the musk duck. In the latter cross, although the males are fine birds, the females are not larger than a widgeon, and fly about almost like wild ducks. This may not always be the case, but it has proved so with me.

But to return to the fish. If any gentleman who is interested in such matters will do me the honour to read this paper, and wishes for further information on the subject, I shall be happy to give it, so far as I am able. Very sure I am that the sportsman who once fairly starts as a fly-fisher, and is so fortunate as to hook a Salmon or a large Trout, will thenceforward despise or lightly esteem corks and floats, ground-bait and trimmers, punts and Perch fishing, and will fairly wish them all exchanged for a nice stream well stocked with Trout—as a gentleman lately said to me, fly-fishing is a perfect infatuation! He was quite right. The extreme avidity with which it is followed by the thoroughly initiated, can only be explained on that supposition; to the casual observer, there does not appear to be any strong excitement in it. But that is a great mistake. Let me get to the bank of a river well stocked with Trout in a good humour, early in the morning, and I feel neither hunger, thirst, nor fatigue if I fish until dark without tasting of anything. And the excitement of hooking a ten or twelve pound Salmon is not much inferior to that produced by a long run after the hounds.

Page 27

I cannot conclude without calling the attention of all interested, and who are able to render assistance in remedying the evil, to the great falling off in the quantity of fish there is in all the Salmon rivers in England. With those in Scotland and Ireland I am not acquainted, but believe that matters are not in a much better state there. I believe that the unsatisfactory state of the laws has a great deal to do with this decline in the value of the fisheries, and I also believe that it is quite possible so to alter the law as to very greatly improve them, and that without improperly interfering with what is of far more importance—I mean the manufactories of the country. As the law stands at present the proprietors of the upper parts of rivers have not the slightest interest in the preservation of the fish in the breeding season, for, as they are seldom allowed to see a fish when it is fit for the table, why should they look after the poachers in close time? Why should they be put to much expense and trouble, as well as the risk of the lives of their game-keepers, merely to breed fish for the proprietors of stake nets and estuary fisheries, who don't spend a farthing in the preservation of the fish when breeding, and yet reap all the benefit? I had occasion, some years ago, to examine the evidence on this subject given before the House of Commons in 1825, and was exceedingly amused at the schemes resorted to to evade the law, moderate and inefficient as was the law at that time. (Since then the law has been altered both in Scotland and Ireland, but I do not know what are the provisions, nor what has been the effect of the new law.) It required that there should be a free passage for the fish (Salmon) through all the traps, nets, weirs, and devices that were used to catch or detain them, from sunset on Saturday night to sunrise on Monday morning. One man said he paid L7,000 a year for his fishery, and should lose one-seventh of his catch. Another said he allowed a free passage on Sundays, but would not permit anybody to go and examine for themselves. A third proved that he allowed the fish a free passage on Sundays, but his neighbours proved that he placed in the gap a crocodile, painted red. And a fourth was convicted of breaking down the stake nets in the estuary of a river—at the same time he had a net stretched entirely across the river above, both day and night. And so with many others, every one striving with all his might to kill the goose that laid the golden eggs.

This is not the way to improve the Salmon fisheries. To do this effectually the upper proprietors must have a strong interest in the preservation of the breeding of fish, and in order to give them this interest they ought to have an ample supply of fish when they are in the best condition; but to give them this supply the law ought to be altered. At present I believe the law does not require a free passage for the fish (at least in English rivers) except from Saturday

Page 28

night to Monday morning; in many of them I believe this is not insisted upon; whereas the law ought to prohibit fishing for or obstructing the passage of the fish every night from sunset to sunrise, and this regulation ought to be rigorously enforced. This would give the upper proprietors a chance of having good fish, and a corresponding inducement to take care of them. Nobody would be so much benefited as the owners of fisheries at the mouths of rivers; they would be the first takers, and would still get the lion's share of all the fish that ascended the river. If this regulation were enforced, the expenses of conservators might be defrayed by levying a small tax, in the shape of a licence for angling, which all true sportsmen would be glad to pay if it gave a reasonable prospect of a well-stocked river. Now matters are getting worse every day, and notwithstanding the enormous fecundity of the Salmon (a large one producing 25,000 ova in a season), they are now extinct in some rivers where they used to be found in my recollection, and in others where they were once abundant they are now very scarce. No one need to wonder at this, when he is told that gangs of poachers are on the lookout for them all through the spawning season. In one winter, some years ago, I am credibly informed that two hundred Salmon were taken in one stream within five hundred yards of the spot where I am now writing. It is nobody's business and nobody's interest to prevent this, and therefore it goes on openly night and day.

Are there no influential gentlemen in the House of Commons who will take up this matter and endeavour to get an equitable and comprehensive law passed for the preservation and increase of the breed of Salmon? It is a matter of even national importance, and if duly provided for and properly attended to, I see no improbability in the supposition that Salmon would again be as abundant as they were when the apprentices on the banks of the Ribble stipulated that they should not be compelled to eat Salmon oftener than three days in the week. The apathy of country gentlemen in this matter is to me unaccountable. I have some reason to believe, however, that Government have at all times been so far from lending their influence to the promotion of any attempts to amend these laws, that they have obstructed rather than assisted them, most probably from an idea that the preservation of the fish would interfere with manufactories. If I thought that this would be the case, I should not say a word on the subject; but I am very far from holding such an opinion. So far from this being the case, I assert without hesitation that weirs need form no obstruction to the free passage of fish, and that without impairing the efficiency of the water power. With the poisonous and filthy mixtures sent by some manufactories down the rivers, the case is far different, and where this is done the case is hopeless. Salmon and Trout will rapidly disappear from such rivers, never to be seen there again, so long as these noxious contaminations are permitted to flow into them.

Page 29

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ARTIFICIAL BREEDING OF FISH.

CLITHEROE, *December 26th*, 1853.

To the Editor of the "Manchester Guardian."

SIR,—I have read with some interest the letter of your correspondent, Salmo Salar, on the artificial breeding of fish; and knowing, as I do, the great interest which the writer feels in the preservation and increase of his namesakes, I shall be most happy if my humble efforts in the same cause throw any more light on the same subject, and in any degree contribute to the same end.

But Mr. Salmo Salar is quite wrong in saying that, with the exceptions of the experiments made on the banks of the Hodder, by Ramsbottom, no efforts have been made to increase the number of Salmon by providing artificial breeding-places. Passing over my own numerous experiments here for the last fourteen or fifteen years (which you, Sir, are aware of, though the fishing world is not), I may refer to the extensive experiments made by Mr. Fawkes, of Farnley, in 1841 and 1842, and renewed again in 1848 and 1849; and the whole of which (with the exception of a portion of these in 1842) were successful. The experiments of Salmo Salar were not made until 1851 and 1852, and were intended merely to test the accuracy of an assumption that the impregnation of the ova takes place long prior to their exclusion; which experiments terminated in a complete failure. Salmo Salar says that the quantity of Salmon fry in the river is enormous; and that he has caught five pounds of them in a single pool in a single day. I have known three times that quantity caught in the same way. But still this proves nothing at all, for it is well known that almost all migratory animals, however solitary their general habits may be, are gregarious at the time of migration. Witness swallows, fieldfares, and even woodcocks. Witness also the clouds of small Eels ascending the rivers in May and June; and if we are to believe the accounts of travellers, the enormous flocks of antelopes in Africa, and of bison in America, are proofs of the same general law. No doubt Salmo Salar will find, as he says, that the Samlets are exceedingly abundant in some of the pools, when they have flocked together for the purpose of migration; but he may perhaps travel for miles either up or down the river before he will find any more. It is notorious that, in the tributaries of the Hodder, they are walled in, in many places, for the purpose of detaining them, that unscrupulous anglers may get as many of them as possible before they go to the sea. Salmo Salar is in error also when he says that Ramsbottom deposited 40,000 in the ponds of Galway, of which 20,000 are expected to be fruitful. The fact is, that he deposited 40,000 in December, 1852, of which above 20,000 are now alive and in the ponds, varying from four to five inches long to two or three, notwithstanding that experiment was made under very unfavourable circumstances; for there was so much

mud in the stream that supplied the spawning-boxes, that when Ramsbottom left Galway he was afraid all the ova would be choked by it.

Page 30

Salmo Salar seems to think that almost all the ova deposited naturally come to life, and that very few of those deposited artificially do so. This, however, is quite contrary to my experience, and I think that if Salmo Salar will listen to the evidence he will change his opinion. It is well known that Salmon are very fond of particular streams, their instinct no doubt informing them which are suitable to their purpose; and when one pair of fish have finished spawning, another pair will come and occupy the same place. Now, what takes place under such circumstances? The ova which were deposited by the first pair are rooted up by the second, and their specific gravity is so near that of the water, that they roll down out of the loose gravel and are picked up by the Trouts, Par, and other fish that are always lying in wait just below for that purpose. When Ramsbottom was in Galway he caught a large Trout, out of whose throat he squeezed a thousand ova, which were deposited in a spawning-box, many of which came to life notwithstanding the pit they had escaped from. The extraordinary avidity with which Trout take Salmon roe as a bait is also a proof (if that were needed) of their preying upon it in the spawning beds. Yet, in addition to them, are all the Par, Bullheads, Eels, Loaches, and aquatic larvae which may be found swarming in every spawning bed by any one who will look for them. In addition to these enemies, millions of the ova are destroyed by being washed away by heavy floods, and as many more are destroyed by being choked with mud and sand in the spawning beds as well as by being left dry at low water owing to the Salmon spawning in places which frequently become quite dry in early spring. No doubt many of the Salmon fry when they have reached the sea are destroyed by enemies there, of which we know nothing. But still, if 500,000 are bred, in addition to all that are reared naturally, it will represent a larger proportion of the whole than Salmo Salar seems to suppose; otherwise, how is it that in rivers where Salmon are protected, or still more in unsettled countries, the Salmon are so numerous? The Salmon in the Columbia river, on the north-west coast of America, are cast dead upon the shores by myriads after the spawning season, and these are merely the fish dying from exhaustion, as a small portion always do here. How numerous, then, are those which ascend the river to spawn, and go down again to the sea afterwards! No doubt the grand object to be attained is to make Salmon abundant, and the most important step towards the attainment of this object will be to give an efficient protection to the spawning fish, and the only way to do this effectually is to give the upper proprietors of rivers such an interest in the Salmon fisheries as will make them worth attention. At present this is far from being the case. Now the upper proprietors are merely considered as so many clucking hens, whose business and whose duty it is to hatch

Page 31

Salmon for the proprietors of fisheries at the mouths of rivers, who do not in many cases spend a farthing in their protection when spawning, and who grievously begrudge the upper proprietors every fish that is able to pass their nets and other engines of destruction. Let the upper proprietors of Salmon rivers bestir themselves so to amend the law as to give them a chance of having a supply of Salmon when they are in season. They cannot and will not have a more efficient ally than Salmo Salar. Salmo Salar is in my opinion quite right when he says that the fish kept in ponds will not be quite so well able to take care of themselves as fish which have been bred and lived all their lives in the river. Nor do I think that this is necessary for any longer period than until the young fry get rid of the umbilical vessel; after which they are quite able to take care of themselves. Before that time they are scarcely able to move, and thousands of them fall a prey, not only to the other fish, but to the larvae of aquatic insects which prey upon them very greedily. As I happen to know from my own observations, the larva of the stone fly (May fly of Lancashire) and those of all the larger ephemera (drakes), to say nothing of the fresh-water shrimps, swarm in all the spawning beds, and no doubt destroy myriads of the ova. All these would be saved by proper precautions and well formed spawning-boxes, with good supplies of spring water to feed them.

I think Salmo Salar has very greatly over-estimated the quantity of Salmon fry that go down to the sea from the rivers. He speaks of them going down by millions. Now we will take the river Hodder as a river with which both Salmo Salar and myself are well acquainted, and I will venture to say that, so far is this an over-estimate, that if he would take the hundredth part of the number he would be much nearer the truth. The Samlets when they go to the sea may be reckoned to weigh eight to the pound, and two millions would at that rate weigh one hundred and ten tons. Does Salmo Salar think that one ton and a tenth of Smolts go down the river Hodder to the sea on an average of years? I have more favourable means of judging of the quantity that go down the river Ribble than I have of those of the Hodder, and I believe I should very greatly exaggerate their numbers if I estimated them at any such weight as a fourth of that quantity. Again, the Hodder and the Ribble are, in some respects, far more favourable for spawning than many other rivers; for partly owing to the country through which they pass, and partly owing to the rapidity of their streams, the gravel is large and very suitable for spawning in; there is also far less mud and sand in them, and the spawning beds are much less liable to be choked up than they are in many other rivers. No doubt the Salmon will make the best selection in their power, but they can only select from such places as there are; and if those are not suitable the ova must be in a great

Page 32

measure destroyed. Since Ramsbottom returned from Scotland he has visited the river Dee, about forty miles from Chester, and there he found the spawning beds (ridds as *Salmo Salar* calls them) silted up with mud and sand, and the ova buried in them to the depth of eighteen inches. How or when were the newly hatched fish (supposing, which is very improbable, that they ever did hatch) to make their escape from such a heap of filth? It would be quite impossible.

In conclusion, it seems desirable and quite necessary to say a few words as to the priority of discovery of this process of fish propagation. The French claim it; the Irish seem to claim it; the Messrs. Ashworth take great credit for it; and now *Salmo Salar* says he first suggested it. Allow me, as there are so many claimants in the field, to suggest one or two more. In the year 1832, without knowing that such a thing had ever been done or even thought of, I made some experiments on the spawning of fish and the artificial impregnation of their ova, which I communicated to "Loudon's Magazine of Natural History," in which they appeared. After that came the Duke of Buccleuch's game-keeper, Shaw, whose experiments were both satisfactory and conclusive. This was in 1836 or 1838. Then after my experiments at home, I induced Mr. Fawkes to take up the matter in 1841, and they were resumed in 1842, and again in 1848 and 1849, both with Salmon and Trout. It was at this period that Ramsbottom came into the field. At Mr. Fawkes's request I instructed him in the art, and sent him to Farnley, where he was perfectly successful; and since then, I believe he has had more experience and been more successful than any other propagator in the kingdom.

The principle of this system is very easily comprehended; but success depends on many niceties of manipulation, and much experience in judging whether the fish, both male and female, are in the proper condition for operating upon.

This experience is not gained without much practice. This practice Ramsbottom has in great perfection. There is no doubt the artificial breeding of fish will be found exceedingly beneficial, if properly carried out; and I hope to see the time when *Salmo Salar* may catch half-a-dozen of his namesakes at Whitewell, any good day in the season.

I am, Sir,
Yours very truly,
THOMAS GARNETT.

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ARTIFICIAL BREEDING OF FISH—(CONTINUED).

CLITHEROE, *9th January*, 1854.

To the Editor of the "Manchester Guardian."

SIR,—As I believe that Salmo Salar is quite as desirous of increasing the breed of Salmon as myself, the controversy between us may be reduced to very narrow limits. He believes that Trout eat very few of the Salmon ova, and therefore cannot do much harm. I will just mention a few facts which make me think otherwise. When Ramsbottom was in Galway he caught in one night twenty-five Trout on the spawning

Page 33

ground, which had on the average not less than five hundred ova in each of their stomachs; from one of their throats he squeezed a thousand. As the net would not take a fish of less than two pounds, how many had passed through it? When he was at Knowlmore, in sweeping the river for spawning fish he caught nine Par, two Trouts, and a Sprod on the spawning bed, all of which were gorged with Salmon spawn; when he went into the brooks there he never found a pair of Trout spawning without also finding a number of smaller fish behind, some of which he caught, and in all such cases found them gorged with roe up to the throat; the male Trout would occasionally drive them off, but as soon as he returned to the female they were again close in the rear.

In the "Perthshire Courier" of the 22nd December is the following statement: The men employed in taking the breeding fish secured a Whitling on Tuesday about three-quarters of a pound, and as they observed Salmon ova coming out of his mouth he was brought to the office of Mr. Buist for examination; on being opened, upwards of three hundred impregnated Salmon ova were taken from his stomach quite undigested. It may be, therefore, fairly presumed, that this youngster had taken this quantity for his breakfast; if he dined and breakfasted in the same style each day during the breeding season, it is difficult to estimate the expense of his keep. Such is the amount of loss of impregnated roe in one morning from one trifling fish; what must it be throughout the season from the various enemies it has to encounter?

Salmo Salar is facetious about the destruction of the roe by insects, and says, "because an aquatic insect will devour a minnow's egg, which is not as large as a pin's head, we have no right to infer that it will devour that of a Salmon, which is as large as a pea; it would be just as reasonable to suppose that because a wasp feasts upon a cherry, or a strawberry, therefore he will eat a turnip or a mangold wurtzel." As he seems to have made a slip of the pen in naming the two last *fruits*, allow me to supply what I suppose he meant to say, which I presume was that because a wasp eats a cherry or a strawberry, we must not therefore infer that he will either eat a pear or a plum; if that is his meaning, I think I can understand it. If he adheres to his own version, I would merely observe that there is no analogy in the two cases. But the inference does not rest upon mere supposition; the freshwater shrimps at Knowlmore were seen devouring the ova in the spawning-boxes. We have seen above that Par eat ova as well as Trout. Let us suppose that the millions of Smolts (as Par) have only one meal each of Salmon roe, and we will stint them to twenty ova apiece. I fear that very few of the five millions which Salmo Salar says are deposited in the Hodder will be left to grow into Salmon. In addition to these, ducks, both wild and tame, eat them greedily. When Ramsbottom was in Galway he saw that the tame

Page 34

ducks frequented the spawning ford, and the superintendent bought one, and found its crop quite full of Salmon roe. If this had been buried eighteen inches in the gravel (as Salmo Salar suggests), the duck would have had some difficulty in extracting it; but so far as my experience goes, it is not usually one-half that depth, although this varies in different rivers. Then, if one Salmon is able to plough up gravel which is cemented together by sand and long continuance in one place, why should not another be able to do the same when the gravel is loose and easily removed? But there is another enemy whom Salmo Salar has not mentioned, who does more harm than all the rest: that is the poacher, and I fear that many of the Salmon which Salmo Salar saw spawning in the Hodder and its tributaries have since then made a journey overland. At all events, I am credibly informed that in one season a gang of poachers took seventy Salmon in the Hodder. Is he sure they have taken none this season? Salmo Salar seems to think that one pair of Salmon will not spawn on the same ground, which has been previously occupied by another pair; but he has only to watch the same ridd for a week or two to be convinced he is mistaken. As to fish refusing to spawn on new gravel, I may state that when Mr. Fawkes was making his experiments at Farnley he put some new gravel into his brook, and there were sixteen pairs of Trout spawning on it the next morning. Salmo Salar says that if he can have those simple checks which he enumerates to the present practices, he will restore abundance of Salmon to the Ribble; they are all very good in their way, but do not go quite far enough, and they would do very little good without a fourth, namely, protection from the poacher for the fish on the spawning beds. Until this can be given more efficiently than it is at present, all the rest will be unavailing; and until the upper proprietors can have a greater interest in the preservation of Salmon than they now have, they cannot be expected to give themselves much trouble on the subject.

My readers would not be much edified by strong assertion and counter-assertion of what Trout do, and what they cannot do; nor is it probable that where we differ we should convince each other; neither do I see any occasion for personality, when both parties are actuated by the same motives—a desire to see the Salmon fisheries restored to a state of great prosperity. I therefore avoid noticing some of Salmo Salar's remarks, which seem to me a little tinged with this spirit, and hope we shall be able to act in concert for the attainment of that desirable result. Salmo Salar will find that the number of Smolts is not always determined by the quantity of ova deposited: if he will examine the bed of the Hodder the next low water, he will find many of the ridds disturbed by the ice floods of yesterday; and if he doubts this, I shall be happy to examine them along with him, if he will give me previous notice of his intention.

Page 35

Since the above was written I have seen Ramsbottom, who tells me that the stream in the Tay, where he caught the whole of the fish from which he obtained 300,000 to 400,000 ova, was on one side of it one continuous ridd, and that the fish could not avoid ploughing up the gravel which previous fish had spawned in, and at Oughterard, where 300 pairs of fish spawned in the same number of yards, it was the same; and they found thousands of ova buried so deep that they were rotting in great quantities.

With regard to what Salmo Salar says about the infrequency of a veritable spawning bed being washed away by floods, I refer him to what I have said previously; but Ramsbottom tells me the game-keeper at Harden (Haworth) will be able to give him sufficient proof that in the Langden Brook this has occurred, as he found the ova on the dry land by thousands, which had been left there by the flood.

When Ramsbottom was at Perth he found on one of the fords, a space of twenty yards long and fourteen yards wide, filled with ridds, which was entirely left dry. What would become of all the spawn deposited there?

Salmo Salar seems to think nature is quite sufficient to take care of her own interests without our interference, and that without some counter-acting influence to keep the breed of fish in check, the river would not hold all that would be bred. I quite agree with him in this, provided nature had fair play; but she has not, and occasionally needs a little help: else why do we employ game-keepers to trap cats, foxes, and weasels, to shoot hawks, carrion crows, and magpies, and to breed pheasants, as well as to prevent poaching? If these precautions are unnecessary, why go to such expense? and if they are necessary for hares and birds, may they not be also for fish?

I hope Salmo Salar will investigate what I said about walling in of the Smolts in Langden Brook. I fancy he may have seen these enclosures himself; at all events, I have, and although I cannot prove they were erected for that purpose, I do not doubt the accuracy of my information.

I am, Sir,
Yours very truly,
THOMAS GARNETT.

* * * * *

The following letter was sent to me from Chester:—

CHESTER, *3rd February*, 1854.

SIR,—We are about to make application to Parliament for a Commission of Inquiry into the state of laws respecting the fisheries of England and Wales. And Mr. Ashworth, of

Poynton, has been so good as to refer me to you, as able and willing to furnish us with information on the subject.

The annual meeting of the river Dee fishery association will be held on the 20th instant, when I purpose to lay before them the draft of a petition to Parliament for their approval.

I am anxious in the meantime to obtain all the information possible relative to the working of the present laws, their defects, and the alterations to be proposed in them, in order that a condensed statement may be embodied in the petition as the ground of our application.

Page 36

I should be exceedingly obliged for any remarks your experience may suggest, and trust you will accept the cause which dictates my writing as a sufficient apology for troubling you on the subject.

I have had great pleasure in reading your able replies to Salmo Salar's letters. On the appearance of the first, I was strongly prompted to reply to it myself, but rejoiced to find him in much better hands.

I remain, Sir,
Yours very truly,
WILLIAM AYRTON.

* * * * *

CLITHEROE, *4th February*, 1854.

TO WM. AYRTON, ESQ.

DEAR SIR,—I am favoured with your letter of yesterday, and shall be glad to give you any information I may possess on the habits of Salmon, or the requirements of any act of Parliament necessary for the preservation and increase of this valuable fish. Being a mill-owner, I have interests which are supposed to clash with those of fish preservers; but I hope to be able to show that all mill-owners are able to give a passage over their weirs at all times when the fish are inclined to run; that is, when there are freshes in the river. I say this the more confidently, as I believe the works here are the largest in England for the power of the stream they stand upon, and I find it necessary to employ 150 horse-power of steam. Yet I find from a careful register, which has been kept here since the year 1838, that we are able, without interfering with the efficiency of the water power, to give the fish a passage over the weir 181 days, or part of days, annually, and this at times when alone they are disposed to avail themselves of such a passage—that is in floods.

The suggestions that occur to me from time to time I will not fail to send you. At present the following seem to me to be essential, to give efficacy to any Act of Parliament framed for the purpose of preserving and increasing the breed of Salmon, for without some such provisions the gentlemen on the upper parts of rivers will have no inducement to exert themselves in the matter.

First.—No nets or other engines, except rod and line, should be used for taking fish from six o'clock at night to six o'clock in the morning, and all fish should be allowed a free passage up the stream every night when this does not destroy or impair the efficacy of the water power.

Second.—No mill-owner nor his servants, nor any other person, should be allowed to take fish at his weir, or within fifty yards of it.

Third.—Conservators should be allowed to go into all wheel-races, wheel-houses and tail-goits, and also upon all lands on the banks of Salmon rivers, as well as inspect all cruives, weirs, &c., without being deemed guilty of trespass.

Fourth.—All weirs kept solely for fishing purposes, cruives, &c., should be compelled to give a free passage to the fish every night from six o'clock to six o'clock in the morning; and any obstruction placed in the gap calculated to hinder or frighten the fish back, should be deemed breaches of the Act of Parliament and liable to a penalty.

Page 37

Fifth.—All nets and other devices for catching Eels should be prohibited in April, May, and June.

Sixth.—Close time should be altered and extended, as well as made uniform, in all rivers.

Seventh.—The sale and use of Salmon roe should be prohibited.

Eighth.—Justices should be enabled to assist the passage of fish over weirs by any contrivance which did not impair their stability nor the efficiency of the water power.

Ninth.—All cruives should be formed of vertical bars, and should have the intervening spaces to measure not less than three inches.

Tenth.—No nets used in a Salmon river should measure in the mesh less than two inches and a half from knot to knot.

Eleventh.—Any person having no right of fishing found with a net in his possession or a Salmon out of season, should be guilty of misdemeanour.

Twelfth.—A ten shillings' licence for angling for Salmon.

The reasons for most of these suggestions will be obvious to you, but there are some which may not be so; I will therefore give a short comment on such.

Third and fourth.—The conservators shall have the right to inspect all wheel-races, cruives, &c., to see they are properly regulated, and also to see that no contrivance is used to drive the fish back. In the evidence given before the House of Commons in 1825, it was proved that the lessee of a fishery in Scotland used to place a crocodile painted red in the king's gap, which the law compelled him to give from Saturday night till Monday morning.

Fifth.—The prohibition to set Eel nets in April, May, and June is to prevent the destruction of Smolts when going down to the sea.

Seventh.—Salmon are destroyed here when spawning chiefly for the sake of the roe. If a man were fined for selling it or having it in his possession, this inducement would be weakened.

Eleventh.—There is the same reason for seizing the net of the black fisher that there is for seizing the snare of the poacher, and if the latter can be convicted for having hares or snares in his possession, I do not see why the former should not for having nets and Salmon.

A meeting of the gentlemen interested in the fisheries of the Ribble and the Hodder will be held on Friday, the 17th instant, previous to which time I should be glad of your criticism.

I am, dear Sir,
Yours very truly,
THOMAS GARNETT.

P.S.—It occurs to me since closing my letter that I have forgotten one important provision required in any new Act of Parliament—namely, protection to the Smolts in their downward migration. Here the pools are swept with small meshed trammel nets of all the fish that they contain.

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ARTIFICIAL PROPAGATION OF FISH.

CLITHEROE, *23rd April*, 1863.

To the Editors of the "Leeds Mercury."

Page 38

GENTLEMEN,—I am somewhat at a loss to understand the object of Mr. Horsfall's letter on this subject which appears in the "Mercury" of to-day. If he means that fish hatched by this process are as much at the mercy of their natural enemies as they are in their natural spawning beds I differ from him entirely; but if he means that there is no good in breeding migratory fish like Salmon, when the obstacles to their return in the shape of stake nets, impassable weirs, and poisonous waters are so numerous as they are at present in many rivers (the Wharfe and the Aire are examples of both), I entirely agree with him. Let us consider both suppositions, for the more this subject is ventilated the more likely is good to arise from the discussion. I think Mr. Horsfall is entirely wrong in the first supposition, for the following reasons: By artificial propagation the young fish escape all damage from floods, and particularly ice floods, which scoop out all the loose gravel from the spawning beds, which are frequently entirely carried away by these floods. They escape all danger from drought, which in some rivers is almost as bad, there being now several mounds of dry gravel in my length of the Ribble which were spawning grounds last December. They escape being destroyed as ova by Trout, Eels, Bullheads, Loaches, the larva of aquatic insects, ducks (wild and tame), water rats, and water shrews. The last are said to be destructive to the spawn; but this I do not vouch for, as these two last-mentioned animals have not come under my own observation as devourers of spawn.

With regard to the 500 Salmon ova said to have been taken from the stomach of a Trout, Ramsbottom is the authority for it, only he says there were nearer 1,000 than 500, and he took them from the maw of a large lake Trout at Oughterard, when netting the spawning Salmon for his artificial propagation. When Ramsbottom was fish breeding for Mr. Peel the year after he first went to Ireland for that purpose, he went into the brooks at night with a light. He never found a pair of spawning fish without also finding several waiters on Providence in the shape of small Trout, which were picking up the ova that descended the streams towards them. Several of these he caught, and they were perfectly gorged with spawn.

With regard to the ducks, Ramsbottom is again my authority. He found that a flock of tame ducks frequented the spawning beds at Oughterard; he bought one for the purpose of ascertaining whether they eat spawn or not, and he found its crop quite full of spawn. With regard to the aquatic larvae of insects, Mr. Horsfall may easily satisfy himself that they destroy spawn if he will turn some into an artificial spawning bed. One of my friends failed to hatch his Trout ova because he could not keep out the fresh-water shrimps.

Mr. Horsfall seems to think that nature would be sufficient to take care of her own interests if man did not step in to aid her endeavours; but if he is a sportsman he no doubt has a game-keeper, who not only preserves the ground from poachers, but traps cats and weasels, shoots hawks, magpies and carrion crows, breeds tame pheasants, and generally looks to the well being of the game without trusting to the efforts of unassisted nature.

Page 39

Let us take the second supposition, that there is no good in artificial propagation when the fish which are sent to the sea can never come back again by reason of insurmountable obstacles. If Mr. Horsfall means this he is quite right; there is no good in the upper proprietors of Salmon rivers becoming brood hens for the owners of fisheries at the mouths of rivers or the proprietors of impassable weirs, who take all the fish which get to the foot of these weirs. I quite agree with Mr. Horsfall that it is in most cases easy to build practicable fish passes, and at a slight expense, if people were willing to do so; but I wish to show that notwithstanding the boasted effects of the Act of 1861, the upper riparian proprietors have not a sufficient inducement to build fish passes, and will not do so unless the expense can be made very moderate indeed.

I will take the river Ribble to illustrate my meaning. As a general rule we have no fresh run Salmon until May, and the upper proprietors are supposed to have a sufficient share of the fish that ascend the stream if the owners of the fisheries in the estuary and the tidal part of the river cease to net from six o'clock on the Saturday night to six o'clock on the Monday morning. That is a day and a half per week. The fishing for Salmon (except angling) ceases on the 31st of August, and from the 1st of May to the 31st of August there are 123 days. Call the period eighteen weeks, which gives us twenty-seven days during which time the Salmon have liberty to pass to the upper parts of the river. But on the average of seasons, owing to droughts, the rapid absorption of moisture by vegetation, and the great evaporation, there is no fresh water to enable the fish to ascend during two-thirds of that time. Every one who knows anything of the habits of Salmon is aware that they never ascend the rivers from the estuary unless there is a fresh in the river; and, as I said before, on the average of seasons there is no fresh for two-thirds of the time from May to August. This reduces the twenty-seven days (which are supposed to feed the upper proprietors with Salmon to repletion) to nine days, and these nine days are expected to stock the river and its tributaries for one hundred miles. It is true I have not taken into consideration the privileges which the upper proprietors have of angling to the 1st of November; but besides the fact that the fish are then full of spawn, and ought not to be killed at all, very few rise at the fly, and when they are taken they can neither be sold nor used by any one who knows what a fresh Salmon is. It is a greater crime against public polity to kill a spawning Salmon than it is to steal a sheep; for, supposing it produces 10,000 ova, and one in a hundred returns as a Salmon, it returns from a place (the sea) where it has cost nothing in rent, taxes, or superintendence, and, in the finest condition imaginable, it invites us to take it.

Mr. Horsfall and I both wish for the same results (rivers swarming with fish), and although we may somewhat differ as to details, I have no doubt both would be glad to see public attention directed to these matters rather more than it is at present.

Page 40

If Mr. Horsfall will do me the honour to come and see me, I will show him an efficient fish-pass which has been in operation forty years. It may suggest some ideas to him, and he may be able to suggest some improvements in it which I should be glad to receive.

I am, Gentlemen,
Your obedient Servant,
THOMAS GARNETT.

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LOW MOOR, *4th January*, 1865.

DEAR SIR,—As I believe Mr. Eden, the Commissioner of Salmon fisheries, is visiting various districts connected with Salmon rivers in England and Wales, with a view of explaining the proposed alterations and additions to the bill of 1861, and as I think from what I have learnt that the proposed alterations and additions will not be satisfactory to the upper proprietors of Salmon rivers, I wish to call your attention to the matter, that, if he should come into this district, the gentlemen interested may be able to point out to him how far these alterations are from meeting their wishes. Supposing that the new bill (as published in the “Field” newspaper, and explained and commented on by Mr. Eden) is to be understood as a government measure and one in which they will allow of no alterations, I maintain that it is very objectionable both from what it omits and what it purposes to do.

To begin with the former, or, in other words, to take the recommendations of the Worcester meeting as the groundwork of new legislation, it does not touch on several of them; they were, so far as I remember (for I have no memoranda to refer to) an extension of the weekly and annual close time—minimum penalties: —a close time for Trout, and a right of way on the banks of Salmon rivers for all water-bailiffs, duly appointed, without their being deemed guilty of trespass; and a tax on fishery nets and implements, for the purpose of defraying the expenses of protection.

Now, so far as I understand the bill as proposed, the only one of these recommendations included in it is the tax. I am wrong in this—the taxation is not included in the bill, but was suggested by Mr. Eden at the meeting he attended lately at Chester. The bill proposes that the choice of conservators shall be vested in the magistrates at quarter sessions, and the conservators shall have power to expend all the funds raised by voluntary subscriptions for certain purposes mentioned in the act. But Mr. Eden suggested at Chester that if these funds were inadequate the conservators should have the power of supplementing them by a rate on the owners and lessees of fisheries in proportion to their extent. Now one man may have an estate on the banks of a river extending for miles from which he derives little or no revenue; while another may have a fishery not extending more yards than the other does miles,

but from which he derives a revenue of as many pounds as the other does pence. If Mr. Eden's meaning is lineal extent, I feel very sure it will not meet with the approval of the

Page 41

upper riparian proprietors. Again, why should the magistrates in quarter sessions (ninetenths of whom know nothing of Salmon or Salmon rivers) choose the conservators? What, for instance, would the magistrates meeting at Wakefield know of the Ribble or the Hodder? What would they care about the matter? They would choose the men who had power to tax the riparian proprietors and lessees; but as they would not be taxed themselves, they would look on with great composure. No; if we are to be taxed, let us tax ourselves, and not leave it to those who will have no interest in the matter, and who may involve us in litigation and expense over which we shall have no control.

The recommendations of the Worcester committee deserved more consideration on the part of Government. They were suggested by men of great experience, and, moreover, unless they are adopted and legalized by Parliament there can be no permanent prosperity for Salmon rivers. Take the extension of close time as an instance. It cannot be right that the owners or lessees of estuary fisheries shall be allowed to take ninety per cent. of the fish which they have neither bred nor fed, and whose well-being and increase they have done nothing to promote; while the upper proprietors, on whom devolve all the care, trouble, and expense, are to rest satisfied with what the thirty-six hours per week can give them. What did they give the upper proprietors on the Ribble and the Hodder last season? Little or nothing. When the bill of 1861 was before the House of Commons, I had an opportunity of suggesting (indirectly) to the late Sir George Cornwall Lewis the propriety and desirableness of an extension of the weekly close time for the benefit of the upper proprietors. He replied, "You might as well propose to restrict the shooting of partridges to three days a week as to restrict the netting of Salmon." But with all due deference to so great an authority, there is no analogy between the two cases. If partridges had all to migrate and return before they could be legally shot, and had, like the Salmon, all to return by the same road, ninety per cent. of them before reaching the district where they were reared would become the prey of men who had neither bred nor fed them. I fancy sportsmen would want protection for them; and if they were not able to obtain it, they would do what is seriously proposed by many people with regard to the Salmon—they would do all they could to exterminate them, rather than continue to act as brood hens to hatch chickens for other men's eating.

Then take the annual close time and the pretended compensation it offers in the two months' rod-fishing (September and October). After the nets have been withdrawn, what is it worth? Or, what is the value of black fish full of spawn? They cannot be sold; they are not fit to eat; the spawn has nearly arrived at maturity, and the only value the fish has is in the spawn, which is potted and sold in many instances by the poacher who kills the fish. He deserves no other name, whatever may be his rank or station.

Page 42

Again, in the 21st section, regulating the weekly close time, it is enacted "That any person acting in contravention of this section shall forfeit all the fish taken by him, and any net or movable instrument used by him in taking the same, and, in addition thereto, shall incur a penalty of not exceeding five pounds, and a further penalty of not exceeding one pound for each fish." But in the 17th section, which regulates the annual close time, though there is a penalty for the contravention and forfeiture of the Salmon so taken, there is no forfeiture of nets and implements. You will no doubt remember how this worked when the watchers took a net and boat, near Preston, last season, after the setting in of the annual close time. How the owner of the net and boat came to claim them, on the pretence that the net had been stolen from the bank, where it had been left to dry, although his own men were the parties who were so illegally using them.

Minimum penalties.—I see no mention of them in the new bill, although it is notorious that many magistrates have fined convicted poachers in the penalty of a farthing or a shilling. What is this but an encouragement to do so again?

Close time for Trout.—This is greatly needed in Salmon rivers, as it is well known that many a poacher pretends to be fishing for Trout when he is looking after Salmon. This is doubly needed when the Salmon ascend the small tributaries to spawn.

The right of way for water bailiffs.—There is no clause or section in the new bill giving the right of way on the banks of Salmon rivers to duly authorized persons without their being deemed guilty of trespass. But there is one by which they are permitted to examine weirs. There is on my part no objection to this examination, but why are millowners stigmatized by being subjected to exceptional legislation? Are not the gamekeepers of gentlemen who have many miles of river subject to no surveillance on the part of the water bailiffs as likely to act illegally as the servants of the millowners? Let both be watched with equal care, and I do not mind how vigilant the watching may be; but I do object to being made the object of special and exceptional legislation. The tax ought to be upon nets and rods and other implements in proportion to their value. But if a tax is laid on the extent of the fishery, we may bid adieu to voluntary subscriptions.

In conclusion, if Mr. Eden comes into this district, I think it ought to be distinctly intimated to him that no bill would be satisfactory to the upper proprietors which did not give them a greater interest in the increase and improvement of rivers. There are three ways of doing this. The mesh of the Salmon net might be enlarged from eight to twelve inches round. This would allow grilse to pass, and fill the river with breeding fish. Or, secondly, the weekly close time might be extended so as to include Friday as well as Saturday afternoon and Sunday.

Page 43

Or, thirdly, the annual close time for net and rod fishing might commence a month earlier than at present; say net fishing to close on the 1st of August, and rod fishing on the 1st of October. Any of these measures would give the upper proprietors a much better supply of fish than they now have. They all, I think, deserve consideration. One thing at least is certain, that unless the upper proprietors have a better share of the fish than they have at present, they will soon cease to take an interest in their preservation.

To Colonel J. Wilson Patten, M.P.

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LOW MOOR, *10th January*, 1865.

MY DEAR SIR,—I shall be very glad if I can induce you to read my opinions on the Salmon question. It is one which I think may become of even national importance, if properly managed. But the sad tinkering it has hitherto received in the nine hundred and ninety-nine Acts of Parliament wholly or partly devoted to the subject makes me almost hopeless about future legislation. Yet it seems to me that the only way to greatly increase the breed of Salmon is so simple and obvious, that its not having been adopted long since can only be accounted for by supposing that all the parties interested in the matter are like the man in the fable, who killed the goose that laid the golden eggs.

Hitherto the law has never properly recognized the claims of the upper riparian proprietors. These men have all the trouble and expense of rearing and protecting the young fish, whilst the owners of estuary fisheries, men who never lift a hand nor spend a penny in taking care of the brood, take above ninety per cent. of the grown Salmon when in season; and even then think they are hardly used. How can it be expected that the upper proprietors should be very earnest in their protection of fish from which they derive little or no benefit, merely acting the part of brood hens and hatching the chickens for the benefit of other people?

In June, 1769, 3,384 Salmon and Salmon Trout were taken at a single haul of the net in the Ribble, near Penwortham. Now the sea is as wide, and, for anything we know to the contrary, as capable of feeding them as it was a hundred years ago; and the rivers are as capable of breeding and rearing them now as they were at that time; and therefore I do not see why, if proper steps were taken, they should not be as abundant now as they were then.

If we take a sheep or a bullock, and to his first cost add the rent of the land on which he has pastured, and something for insurance and interest on capital, the transaction is not a very profitable one in the long run. But in the case of the Salmon, we send a little fish

down to the sea which is not worth a penny, and he remains there, paying neither rent nor taxes, neither gamekeepers' nor bailiffs' wages, costing nothing to anyone, until he returns to the river, worth ten or twenty shillings, as the case may be. Surely this is a branch of the public wealth that deserves sedulous cultivation.

Page 44

I think with you that the Calder can never become a Salmon river, so long as manufactories flourish on its banks, and it is not desirable that it ever should become so at their expense; but even in the Calder (and its tributaries) a little care would prevent immense mischief. Some people at Church, a few years ago, very carelessly pushed a quantity of poisonous matter into the Hyndburn brook, and the first thunderstorm that followed carried it down the Calder into the Ribble, and poisoned all the fish between Calder foot and Ribchester. Take another instance of carelessness in the Ribble, the emptying of the gas-holder tank at Settle, which when turned into the river killed nearly all the fish between that town and Mitton. Several other instances occur to me, but these two are sufficient to show the great mischief occasioned by avoidable neglect and carelessness. Such mischief should not be perpetrated with impunity.

The act of 1861 was very good as far as it went, notwithstanding some oversights; but it did not go far enough. It did not give to the upper riparian proprietors such an interest in the fish as they are entitled to, nor is the interest they now have sufficient to induce them to exert themselves in the preservation and increase of the Salmon as they might and would do if such additional stimulus were given to them. The law now is, that no nets shall be used in the taking of Salmon between twelve o'clock at noon on Saturdays, and six o'clock on Monday mornings. That is, forty-two hours per week. But in the Ribble, as a rule, we never see seasonable Salmon until May. Now from that time to the 1st of September, is, say sixteen weeks, and at forty-two hours per week (the length of the weekly close time) this gives twenty-eight days during which time the fish may pass up the river without interruption; but this is by no means the true state of the case. Everyone conversant with the habits of Salmon knows that they never ascend rivers except when they are in a state of flood; and in average summers, partly owing to droughts, and partly to the rapid evaporation and absorption of moisture by vegetation, these twenty-eight days may fairly be reduced by two-thirds, to give the true time allowed for the ascent of the fish. But say ten days, which are supposed to give an adequate supply of fish to a hundred miles of river,—the extent of the Ribble and its Salmon-breeding tributaries. Is it surprising that the upper proprietors are not satisfied with this state of things? It would be surprising if they were content with such a cheeseparing allowance.

When the bill of 1861 was before the House of Commons, I had an opportunity (indirectly) of suggesting to the late Sir George Cornwall Lewis the propriety of a considerable extension of the weekly close time. He replied, "You might as well propose to shoot partridges only three days a week, as to restrict the netting of Salmon to only three days." With all due deference to such an authority, there

Page 45

is no analogy between the two cases. But if partridges had all to migrate and return before they could be legally shot, and had, like Salmon, to come by one road, and if, like them, ninety per cent. of them became the prey of men who had neither bred nor fed them, I fancy the sportsman who reared them would want some restrictions placed on their being shot by men who had not spent a farthing in breeding and protecting them, but who took the lion's share in their appropriation.

I saw Lord Derby on the subject last spring. He had, however, so little time at his disposal that he could only give me a few minutes. He said a good deal must be allowed for vested interests. I said, "My Lord, I am a manufacturer. When the Ten Hours Bill was passed, manufacturers were deprived of one-sixth of their fixed capital at a stroke, and had not a farthing allowed for their vested interests; nay, more, that measure involved the destruction of machinery which had cost millions. All this was done on grounds of public policy. And is not the Salmon question one of public policy? If, as I suppose, the measure I advocate produced a great increase in the breed of Salmon, the estuary fisheries would be the first to profit by it. They are the first on the river. Indeed, the stake nets in the estuaries are taking fish daily in times of drought, when fish will not ascend the river at all."

In 1859 we had not a fresh in the river between the 10th of April and the 1st of August. And last year we had only a few days of flood between the beginning of May and the 31st August, when close time (for nets) commences.

I have said above that only ten days per year are allowed for the supply of fish to the upper proprietors. I may be told that they have two months (September and October) in which they are allowed to angle for them. True, but what are they worth? They are not allowed to be sold, they are not fit to eat, the fish are black (or red), the milt and spawn nearly at maturity, and the only temptation they offer is to the poacher (who often pots the roe as a bait for Trout); and he is a poacher, whatever his rank or station, who will kill an October fish when full of spawn.

Last year, at my suggestion, a meeting of gentlemen interested in Salmon fisheries was convened at Worcester, during the meeting there of the Royal Agricultural Society, and a number of suggestions were made, and resolutions were come to, which were intended to serve as a basis for the desired alterations in the Salmon Bill of 1861. I have no memoranda to which I can now refer, but the most important, according to my recollection, were the following:—The extension of the weekly close time; the annual close time to be extended to Trout; a right to be given to all conservators and water-bailiffs, duly appointed, to pass along the banks of Salmon rivers without being deemed guilty of trespass; a tax on fishing-nets, rods, and implements, to defray the expenses of protecting the rivers from poachers.

Page 46

The Commissioner of Salmon Fisheries, Mr. Eden, has been convening meetings of gentlemen interested in Salmon rivers at Chester, Conway, York, and various other places, to explain the provisions of the bill which Government introduced at the end of last session and intend to bring forward again. I have not attended any of these meetings, but expect he will be at Whalley or Preston shortly, when we shall hear what he has got to say. The new bill, as printed last year, does not embody any of the suggestions of the Worcester meeting; but as I learn from private sources, Mr. Eden, at the various meetings he has lately attended, has thrown out various suggestions, some of which are highly objectionable.

For instance, he suggests that the magistrates in quarter sessions assembled shall have the power to appoint conservators, and that the conservators shall have the power to expend all the money raised by subscription in having water-bailiffs to put up fish-ladders, commencing actions at law in certain cases; and if the subscriptions are not adequate to defray all these expenses, that they (the conservators) shall have the power to levy a rate in aid on the riparian proprietors.

I cannot see how this can be made to work equitably. If the rate be laid on the extent of frontage to the river, one man may have a great extent of no value for fishing purposes, another may have only one pool, so conveniently formed and placed for netting that he will be able to catch ten times as many fish as the other. Then how are the fisheries in the estuary and just above tideway to be valued? They probably take ninety per cent. of all the seasonable fish. Will they be willing to pay ninety per cent. of the rate?

Again, the college at Stonyhurst claims a right of *several fishery*, both in the Ribble and the Hodder. That is, they exercise a right to fish in both rivers, where they have no land, and they exercise this right so freely that they take more fish than all the other upper proprietors added together. If, then, the tax is laid on the extent of frontage to the rivers, these reverend gentlemen would escape entirely, so far as the right of *several fishery* extends, and would only pay the rate on their own extent of frontage.

Again, the new bill does not embody the suggestions of the Worcester meeting as to the right of way for the water-bailiffs; but according to Mr. Eden's comment upon it at Chester and elsewhere, a strict surveillance is to be kept on weirs, to which the water-bailiffs are to have free access. Personally I have no objection to this, provided the water-bailiffs are allowed free access to the banks of the river elsewhere; but I have a strong objection to be made the subject of offensive exceptional legislation. Are not gamekeepers as likely to need looking after as mill-owners?

Again, the bill does not touch on minimum penalties. This it ought to do, for in some districts (Wales, for instance) there is a strong animus against all attempts at preserving the Salmon, and notorious poachers, duly convicted of offences against the act of 1861, in some instances have been fined a shilling, in others a farthing.

Page 47

To W. H. Hornby, Esq., M.P.

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REMARKS ON A PROPOSED BILL FOR THE BETTER PRESERVATION OF SALMON.

CLITHEROE, *August 27th*, 1860.

HENRY GEORGE, ESQ.

DEAR SIR,—I am favoured by the receipt of your letter of the 25th inst., and the accompanying draft of a proposed bill “for the better Preservation of Salmon,” and proceed at once to offer such remarks and suggestions as occur to me, and shall be glad to learn that they meet with your approval.

In the third clause (section) you give an interpretation of the names under which you wish to include all fish of the Salmon kind. Does not this include common Trout? You specially include Char by name. Would it not be better to limit your intentions to all migratory fish of the Salmon kind, to wit, Salmon, Grilse, &c. &c.? I think also the meaning of a fixed net wants defining more rigorously. As it now stands it appears to me that it would include any net which should be fastened on a root or stone whilst it was being drawn through a pool, if the men employed in doing this were to let go the cords whilst they loosed the net from the obstruction.

Fourth clause.—I quite agree with you on the period allotted to annual close time, but think there ought to be a penalty for buying, selling, or having in possession Salmon roe (save and except for the purpose of artificial propagation).

Seventh.—I do not agree with you at all on the subject of the weekly close time, which in my opinion ought to be for one-half of every day, except Sunday, and the whole of that day. Why should the owners of fisheries at the mouths of rivers, who are at neither trouble nor expense in breeding or preserving the spawning fish, have all the benefit derivable from their increase? Why should the upper proprietors act the part of brood hens for these, hatching and preserving the fish for the benefit of those who take no trouble about these things themselves?

Twelfth.—I do not agree with you as to the size of the mesh: I do not think that a mesh of twelve inches in circumference, or three inches from knot to knot, at all too large; it would permit fish below six pounds to escape, and this being done, there would under any circumstances be a fair supply of breeding fish.

Fifteenth.—I think your leister requires a more rigorous definition. A man in this neighbourhood is reputed to have killed a good many Salmon with a hay or a dung fork. Are either of these leisters?



Your sixteenth section is utterly impracticable. How could such hecks or grates be prevented from choking with leaves in the autumn and ice in the winter, thus stopping the wheels? You might as well require a farmer to hedge out the game. Impose a penalty, if you like, upon any millowner who may kill Salmon in his mill lead; and as you give your conservators power to inspect everywhere, you will readily detect such practices. But it will never do to close the mills by pretexts that the fish may be taken or killed there.

Page 48

Twenty-first.—I do not understand the meaning of this. But taken in its ordinary sense, it seems to me to be very unjust. Many persons have traps in their weirs for the purpose of taking Salmon to which they plead a prescriptive right. Do you mean to do away with these? You may succeed in this, but why should not a man be allowed to fish in the river above the weir where there are no obstructions to the passage of the fish? And why should not a man be allowed to fish with a rod and line below the weir, and as near to it as he chooses? I think weirs might be safely divided into two classes: those used for manufacturing purposes and those for fishery purposes; that a man should be allowed to say in which class his weir should be included. If for manufacturing purposes he should not be allowed to catch Salmon (except with rod and line) within a certain distance below the weir. If he choose to class his weir as one for fishery purposes, he should then be compelled to give a free passage to the fish for twelve hours every day; but he should be compelled to make his election as to the class in which he would include his weir.

Twenty-fifth.—It would never do to allow the commissioners to make bye-laws. Suppose the case of a millowner who got into a dispute with them: he might be utterly ruined by their bye-laws; they might make bye-laws which deprived him of his water-power, under a pretext that they were taking more efficient care of the Salmon.

Thirty-first.—I think the licence to angle should be compulsory, and not at the discretion of the commissioner. That it should be in the nature of a game licence, qualifying and enabling the holder to angle in any river of Great Britain and Ireland, provided he had the consent of the owner of the fishery where he was angling.

(Additional observations). Twelfth.—You say that no double net shall be used. Do you mean to prohibit the trammel, which is usually a treble and not a double net? You also prohibit one net behind another, but you do not specify the distance outside of which a second net would be lawful. If neither a series of Scotch nets nor a single trammel is to be used, by what sort of net do you propose to catch the Salmon?

Nineteenth.—You say the sluices which admit water to wheels or factories shall be kept closed from six o'clock on Saturday night to six o'clock on Monday morning. How, then, are the repairs of shafting and machinery to be made? These are generally done when the workpeople have gone home on Saturdays. Besides, what is your object? If the river is low, the Salmon will not be running up the stream, and if it be in flood there will always be an abundant supply running over the weir in addition to that which would be required to turn the wheel. You add that the water may be allowed to flow freely through the waste-gate, provided the opening of such a waste-gate shall not deprive the mill of the necessary supply of water.

Page 49

Eighteenth.—In this clause you say that in weirs already constructed it shall be lawful for the commissioner, on the application of any two or more persons interested in the fisheries of such river, and at the proper costs and charges of the persons making such application—proof having been first given, &c.—to cause a survey to be made of such dam or weir by a competent engineer, and to direct such alterations to be made therein as shall, in the opinion of the commissioner, be necessary and desirable, &c.

In this clause, which so far as it goes is very desirable, you have omitted a proviso without which it could never pass into a law. You have forgotten to provide for the legal right of the millowner, which would, or might, be taken away by the alteration made in the weir unless there were some provision in the act which prevented this being done. At present there is no such proviso in your act. Here I have offered for years to allow the upper proprietors to make any alteration they liked in the weir, provided such alterations did not affect the milling power, the stability of the weir, or my legal title to the weir as existing at present. And my legal adviser tells me that any alteration made in the weir without a guarantee from the upper proprietors would very probably deprive me of my present title.

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LETTERS ON AGRICULTURAL SUBJECTS.

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ON THE CULTIVATION OF WHEAT ON THE SAME LAND IN SUCCESSIVE YEARS.

To The Editor of the "Manchester Guardian."

CLITHEROE, October 5th, 1843.

SIR,—I PROMISED to send you some details of my attempt to grow wheat on the same soil year after year. These I now forward, and hope they may prove interesting. I was led into these experiments by reading Liebig's book on the "Chemistry of Agriculture;" for, assuming his theory to be true, it appeared to me to be quite possible to grow wheat on the same land year after year; as, according to that theory, the carbon, oxygen, and hydrogen, which constitute the great bulk of all cereal crops (both grain and straw), are supplied in abundance from the soil and atmosphere (or perhaps, to speak more correctly, from the latter), and we have only to supply those inorganic substances, which, however numerous, form but a small part of the whole weight of the crop. With the view of testing this theory, and hoping that I might be able to find out what were the elements which built up and cemented the carbon, oxygen, and hydrogen together—or, in other words, which constituted fertility—I begun, in the autumn of 1841, to experiment on a field which had been exhausted by a succession of crops, and which had just been cleared of one of oats. I chose an exhausted field in preference to any other, as the

only one in which I could test the truth of the theory. It was very foul, being full of couch grass and weeds of all kinds. It was ploughed up and hastily

Page 50

picked over, for the season was so unfavourable for cleaning the land (from the great quantity of rain that fell) that I was almost induced to abandon the experiment. Previously to sowing the seed, one-fourth of the field was manured with a compost of night-soil and coal-ashes, at the rate of forty tons to the customary acre (7840 yards); the remaining three-fourths having the seed put in without any manure whatever. The winter was very unfavourable for the plants in our cold wet soil, and in the unmanured part of the field many of them perished, and those that survived made very little progress, from having no stimulus at the roots. Thinking it desirable to apply my experimental manures in moist weather, I waited until the 6th May, when I treated that part of the field which had *not* been manured (three-fourths of the whole) in the following manner. I applied guano to one-fourth, at the rate of two hundredweight to the statute acre, and the same weight of nitrate of soda over another fourth, leaving one-fourth entirely without manure. The wheat manured with the guano and nitrate of soda grew vigorously, and the ears, more particularly in the part manured with guano, were the finest I had ever seen, but when it came to ripen it shrivelled in the ear, and the sample was very indifferent; the soil being evidently deficient in some property necessary for perfecting the grain. The crop also suffered much from the depredations of the birds.

The portion manured with night-soil produced
to the statute acre 32 bushels of 60 lbs. each.

Guano " " 27 " " "

Nitrate of Soda " " 27 " " "

Unmanured part " " 19 $\frac{2}{3}$ " " "

I give these details to show that the land was in an exhausted state previous to the commencement of the experiment I am now about to detail. After the crop of 1842 was reaped, the land was immediately ploughed up, and the season being very favourable, it was tolerably well cleaned, and the seed was sown (without any manure) about the first week in October. After the wheat came up, it was manured with a dusting of one hundredweight of guano, over the entire field (about one acre, three roods), to keep the plants alive through the winter. In the spring, being divided into three portions, it was manured with the same number of experimental manures, which were furnished to me by Mr. Blyth, of Church, near Accrington, who also analyzed the soil and subsoil for me. These manures were applied about the 10th of April, and the experiment was still further varied by covering a portion of each division with guano a fortnight afterwards, at the rate of two hundredweight to the acre, but all the manure applied to the crop, including the hundredweight of guano put on in the autumn, did not exceed 6 $\frac{1}{2}$ hundredweight. The crop, which was a very thin one in the spring, improved so much by the application of these manures, that when it came

Page 51

into ear, it was allowed by all who saw it to be the best in the neighbourhood; but the heavy rains of July caused it to lodge in the best part of the field, and there it was attacked by rust, and the sample was very indifferent. In addition to this drawback, there being very little wheat grown in the neighbourhood of the town, and this being much earlier than any of the other fields, was attacked by the birds as soon as the grain was formed in the ear. Notwithstanding all the efforts made to prevent them, they continued feeding upon it until it was cut; and it is a very moderate estimate of the damage, to say that they destroyed one-fourth of the crop throughout the field. That part of the field covered with manure (No. 1) being the earliest, suffered most. There were patches of several square yards where there did not appear to be a single grain left; and wherever the birds took a grain from the middle of the ear, when in the milky state, the grains on each side of it appeared to grow no more, but shrivelled up in the ear.

I have little doubt that in this portion of the field one-third of the crop was destroyed. All this seems to reduce the experiment to little more than guess-work; and it will, probably, be very difficult to persuade those who did not see the field when it was cut, to credit this report of the devastation made by the birds; even when they are told that Clitheroe is a town of 7,000 inhabitants, and probably as many sparrows, and that apparently they were all assembled to feed in this field; and they became so accustomed to the good living they found there, that even when our neighbours' wheat was fit to eat, they continued to favour this field with their visits in preference to going elsewhere. I estimate the damage on No. 1 at one-third, No. 2 at one-fourth, No. 3 at one-fifth; this was later than the others, and suffered more from rust than birds.

The following are the results:—From 3,060 yards manured with No. 1, there were obtained 1,042 lbs. of wheat, or 27 $\frac{1}{2}$ bushels of 60 lbs. each to the statute acre; if we add one-half to this, as we assume that one-third was destroyed by the birds, it will give 41 $\frac{1}{4}$ bushels to the statute acre. The weight of straw from this portion was 188 stones 5 lbs., 14 lbs. to the stone. From 2,856 yards manured with No. 2, 962 lbs. of wheat were obtained, and 155 stones 9 lbs. of straw; this is equal to 27 $\frac{1}{4}$ bushels per acre, or with one-third added, for estimated damage, it is equal to 36 bushels per statute acre. From 2610 yards manured with No. 3, there were 1,067 lbs. of wheat, and 211 stones 7 lbs. of straw, or 33 bushels to the statute acre, to which if we add one-fourth, according to the estimate of damage, it will be equal to 41 $\frac{1}{4}$ bushels per acre. It will be observed that this portion yielded a far greater weight of straw per acre than either of the others, and from the sort of manure applied, it was expected that this would be the case.

Page 52

No. 1 yielded straw at the rate of $297 \frac{3}{4}$ stones per acre. " 2 " " " " $246 \frac{3}{4}$ " " " 3 " " " " $392 \frac{1}{3}$ " "

Many people may feel inclined to say, that all these apparent data are mere guesses, and that a crop may be made into anything one likes, if they assume so much for damages; but, fortunately, it is not all guess-work. I have stated previously that I covered a part of each division with guano a fortnight after the application of the manures in April, intending to see what advantage was obtained by the use of it; but, owing to the depredations of the birds, the portions of the first and second divisions manured with guano were not kept separate from those which were left without guano; but the third being later, and, therefore, not so much injured by them, gave me an opportunity of ascertaining the effect. I measured off a land which had been so manured, and reaped and thrashed it out separately. From this land of 100 yards long and 10 feet wide (3,000 square feet), there was obtained 220 lbs. of wheat, or 53 bushels of 60 lbs. per statute acre; and this was far from being the best portion of the field. I don't mean that it was not the best portion of the crop, but I mean that the soil was not so good there as it was in other parts of the field; as I have before stated, in the best part of the field the crop was spoiled by being lodged by the rain, and subsequently attacked by rust.

I communicate this to you, in the hope that the publication of it in your paper maybe the means of stimulating others to try the same experiments. It is not too late yet to try for the next year's crop, and I have no doubt that Mr. Blyth will be happy to supply both material and information to any who may require them from him. It is the duty of everyone to promote the advancement of agriculture; and this is my contribution towards it. I have not yet done, for I have sown the same field with wheat again, and hope, with a favourable season, to reap a still more abundant crop next year.

* * * * *

To the same.

CLITHEROE, *October 12th*, 1844.

SIR,—Last October you published an account of an attempt of mine to grow wheat on the same land year after year; and, as I have repeated the experiment this year, I shall be obliged if you will be kind enough to insert the account of it in the "Guardian," as the subject appears to me to be an important one; and, as many persons who may read this letter may either not have seen the former, or may have forgotten it, I trust that a short summary of the former experiments may not be out of place.

These experiments took place in the autumn of 1841, after the field had been cleared of a crop of oats, which was a very bad one; the land being not only naturally poor, but foul and exhausted by long cropping. As the season was very wet, it was indifferently



cleaned, and one-fourth of it manured with a compost of night-soil and ashes, and then the field was sowed with wheat. Two of the remaining three-fourths were manured on the 6th of May, 1842 (the spring being a very dry one, no rain came until that day), one with guano, the other with nitrate of soda, each at the rate of two hundredweight to the statute acre, and the remaining fourth was left unmanured.

Page 53

The following were the results at harvest:—That manured with night-soil and ashes produced 32 bushels of 60 lbs. per acre; guano, 27 bushels; nitrate of soda, 27 bushels; unmanured, 19 $\frac{2}{3}$ bushels. When the field had been cleared of the crop, it was immediately ploughed up, and, as the season was favourable, the land was well cleaned and sowed with wheat in October, 1842, without any manure except 1 cwt. of guano, which was scattered over it when the wheat was coming up. The field was divided into three portions, and in April, 1843, was manured as follows:—No. 1, with 90 lbs. of sulphate of magnesia, and 2 cwt. nitrate of soda to the statute acre; No. 2, with a compound from a manufacturer of chemical manures; No. 3, with 60 lbs. of silicate of soda and 2 cwt. of nitrate of soda to the acre; and, with the view of still further varying the experiment, a part of each portion was sowed with guano a fortnight after the application of the chemical manures. The crop promised to be a very good one, but it was much plundered by the birds, and as the summer was wet, it suffered also much from rust. Allowing for the destruction occasioned by the birds, the crop was estimated at:

41 $\frac{1}{4}$ bushels in patch No. 1,
36 " " No. 2,
41 $\frac{1}{4}$ " " No. 3,

and in that part of No. 3 which was also covered with guano, it reached by actual weight (not by estimate), 53 bushels of 60 lbs. to the acre. Those patches in Nos. 1 and 2 which had guano put on them, suffered so much from the depredations of the birds, that no account was taken of them separately. The crop was cleared off the land, which was cleaned, and again sowed with wheat on 3rd October, 1843. It was drilled in rows seven inches apart, and at the rate of 2 $\frac{1}{2}$ bushels to the acre. It is to the results of this crop that I now wish to call your attention. Before sowing, the land was subsoiled to the depth of from 14 to 16 inches; except a strip of about 10 feet in width, down the middle of the field, which was left untouched for the purpose of determining what were the advantages derived from subsoiling. If the advantage was merely that of thorough draining (for the field had not been thoroughly drained previous to the subsoiling), it was thought probable that this strip of 10 feet wide would be drained by the subsoiling on each side of it; but if, in addition to this, the wheat plant derived more nourishment by striking its root deeper into the soil, where that was loosened by the subsoil plough, the crop ought to be better in the subsoiled than in the unsubsoiled part. The field runs over the ridge of a hill, and upon that ridge the soil is so poor and thin, that it was deemed expedient to give it a slight dressing of coal-ashes and night-soil, from an idea that the plant would scarcely survive the winter unless some stimulus were applied there; but the ashes contained little manure, and were only applied to the worst part of the field, covering about one-third of its surface. The wheat

Page 54

was Spalding's Prolific; it came up evenly and well all over the field. It was hand-sowed with lime early in February to the extent of about 24 cwt. of dry lime on the acre. In order to ascertain the value of lime, and the proper quantity, I had the field uniformly covered with it, except one land, which was left entirely without, and the headlands, which had one three, the other six times as much lime put upon them as any other part. The field was also dressed with a chemical manure of the following ingredients on the 16th March, costing:—

L. s. d.

1 1/4 cwt. nitrate of soda	0	17	6
1 " impure sulphate of magnesia	0	5	0
3/4 " silicate of soda	0	11	3
3/4 " common salt	0	2	0
1 1/4 " gypsum	0	2	0
Mixing and applying it, say	0	2	3

Total for statute acre L2 0 0

Speculating on the probability of a dry summer, I gave it an extra quantity of manure, and I think where guano is used afterwards, as it is by me, the nitrate of soda might be dispensed with, which would bring the cost to L1 2s. 6d. per acre. I should prefer guano to nitrate of soda, because of the phosphates contained in the former. At the distance we are from the sea (about thirty miles) it would seldom be necessary to apply common salt, as the gales of winter generally bring as much as is needed; but last winter we had no high winds, and I thought that where salt was applied with other chemical manures, the wheat was more luxuriant than where there was none; but owing to a misunderstanding of the instructions to that effect, the produce was not kept separate. When the chemical manure was applied, one land was left without, for the purpose of comparison. Guano was sowed on the land on the 29th March, at the rate of something less than 2 cwt. to the statute acre, one side of the field being covered with Peruvian, the other with African, and the land on which no chemical manure had been sowed was half of it covered with guano, and the other half left without anything except lime; but as it was thought desirable to ascertain the value of the chemical manure without guano, half of this patch was sowed with the chemical manure in April, after the long drought of the last spring had set in. A small patch was left without manure, to show the natural condition of the field, and to serve as a comparison with the manured part alongside it, and also with the condition of the field when the experiment commenced, 1841-2, when the unmanured portion yielded only 19 2/3 bushels to the acre. This part of the experiment, however, was frustrated by the carelessness of the men who thrashed out

the wheat. The crop was a very good one throughout the field, but was evidently shorter and thinner where there was no lime, and also where guano was applied alone. It was best on the headlands where more

Page 55

lime had been applied. The weather was extremely favourable until the wheat was going out of bloom, but it then changed, and the crop was beaten down by the rain, in some places so thoroughly that it never rose again; and from that time to the day it was reaped (21st August), there were not more than six fine warm days. This cold and ungenial weather would, no doubt, materially affect both the quantity and quality of the crop,—the sample only being just fair. On thrashing out the crop, I find the result to be as follows:—Where the guano and chemical manure were applied, but no lime, the yield was $49 \frac{1}{5}$ bushels of 60 lbs. per statute acre; where the land was left unsubsoiled, it was $52 \frac{1}{2}$ bushels; when guano alone was applied, it was $42 \frac{1}{3}$ bushels; where the chemical manure alone was applied, it was $43 \frac{1}{2}$ bushels; where the African guano was applied, it was 45 bushels; where the Peruvian was applied, it was $52 \frac{2}{3}$ bushels; on the headlands, where three times the quantity of lime (or $3 \frac{1}{2}$ tons per acre) was applied, it was nearly 62 bushels; and where six times the quantity of lime (or 7 tons to the acre), it was $49 \frac{2}{3}$ bushels. I give this last result as it was ascertained, but do not consider it conclusive, for the wheat plant on this headland looked quite as well as the other, until it went out of bloom, when from some unknown cause it was partially blighted; an irregular patch from a foot to a yard in width and extending almost from end to end of the headland becoming brown and parched, as if affected by lightning or some atmospheric visitation. With the view of making these results a little clearer to the eye, I subjoin the following tabular statement of the produce per acre in the different parts of the field:—

Bushels of 60 lbs. per statute acre.

Guano alone	$42 \frac{1}{3}$
Chemical manure alone	$43 \frac{1}{2}$
Guano and chemical manure, with 24 cwt. lime to the acre, but land unsubsoiled	$52 \frac{2}{3}$
Guano and chemical manure, but no lime	$49 \frac{1}{5}$
African guano and lime	45
Peruvian " "	$52 \frac{2}{3}$
" " and 3 times as much lime	62
" " and 6 " "	$49 \frac{2}{3}$
Average crop throughout the field	50

It may be as well to observe, that the total expense of manure, and of its application to that portion of the field which produced sixty-two bushels per acre (including the guano and the additional quantity of lime used), was at the rate of 81s. per statute acre. Deducting the cost of the nitrate of soda, the utility of which, under the circumstances, I am inclined to doubt, it would have been 63s. 6d. I consider these to be very favourable results, and as offering strong inducements to continue the experiment. I have

accordingly had the land ploughed up and cleaned; and it was again sowed with wheat on the 9th inst.

Page 56

Having detailed the general results of the experiment, I beg to offer the following remarks upon some points in it, which seem to me to require a little elucidation. I consider the success of this experiment to be in a great measure owing to the use of soluble silica and magnesia; because, although there is an abundance of silica in the soil, my first crop showed very miserable results, the grain being ill-fed and poor, and the straw soft and discoloured, although the year 1842 was, in this district, very favourable for wheat, the month of August being singularly fine and warm; but when I combined the nitrate of soda with sulphate of magnesia, as in experiment No. 1 in 1843, but still more so when I combined it with the silicate of soda, as in No. 3 of that year, the straw became as strong, firm, and bright as need be desired; and this year when both these salts are combined with nitrate of soda, common salt, and gypsum, I have not only good and bright straw, but also an abundant crop of wheat.

With respect to the lime used, it may be as well to state that the field had not been limed for many years, and although in a limestone district, showed a deficiency of lime on analysis. The soil is a strong loam, on a brick clay subsoil, in which there is little or no lime, although the stony clays, which form the subsoil in a great part of the district, abound in it, containing from twenty to thirty per cent. of carbonate of lime. I had always believed that lime was used in great excess in this neighbourhood, and had, in fact, an idea that its good qualities were overrated, inasmuch as it does not enter into the composition of the plant, except in very minute proportion; but last winter I saw a paper (by Mr. Briggs of Overton) on the possibility of growing wheat on the same land year after year, in which the utility of lime in preventing rust was incidentally touched upon. I also saw Liebig's letters explaining the action of quicklime in liberating potash from the clay; and then I considered it very important to ascertain the proper quantity to be applied. The quantity required to decompose the phosphate of iron was not great, and assuming Liebig's theory of its action in liberating the potash to be true, it seemed to me that an excess of lime would permanently impoverish the land; for, supposing that the crop required 100 lbs. of potash, and as much lime was applied as liberated 500 lbs., what became of the 400 lbs. which did not enter into the composition of the plant? was not a large portion of this 400 lbs. washed down the drains by the rain, and so lost for ever? Perhaps the absence of lime in this field accounts for its beneficial action in the experiment just detailed; but if my supposition is correct, that any excess of potash which may be liberated from the clay by the use of quicklime (that is, any more than may be required to perfect the crop), is washed down the drains, and thus the land is permanently impoverished by the excessive use of lime, it behoves

Page 57

landed proprietors to ascertain what is required, and they should take care to apply no more than is necessary. This caution is most particularly needed in this neighbourhood, where lime is cheap, and where the opinion is prevalent that the more there is applied the better it is for the land, and where it is common to apply ten or twelve tons to the acre. I have stated above that chemical manure was applied to a small portion of the field after the setting-in of the drought in April. The action of this manure showed that a good thing may be very injurious if applied at an improper time; for, although it produced a stimulating effect on the plant immediately after its application, there was too little moisture in the land to dissolve it thoroughly, and thus enable the plants to appropriate it, until the rain came, about the end of June, when the wheat had been in flower some time; but the stimulus was then so great that all the plants threw up fresh stalks (from the roots), which were in flower when the wheat was cut, and it was then found that they had not only impoverished the plants, but had prevented the grain from ripening. This was the case not only in the experimental field, but in several others also, where the chemical manure was sowed after the setting-in of the drought. When the field was sowed with guano, it was thought desirable to cover one part of it with the African, and the other with Peruvian, for the sake of comparison; but as the African did not appear to produce the same stimulating effect as the other, fifty per cent. more was applied, that the cost might be equal (the Peruvian cost 10s., the African 7s. per cwt.); but as the latter application of the African was made when the wheat was just shooting into ear, the same objection applies to the experiment which does to the chemical manure applied after the drought had set in—viz., that there was not sufficient moisture in the soil to dissolve it thoroughly until the plant was too far advanced to benefit by it; and therefore its failure would be no proof of the value of the African as compared with the Peruvian, which was the object of the experiment. It is true, no bad effects followed the application similar to those produced by the misapplication of the chemical manure in dry weather, yet if soluble salts like the latter did not find sufficient moisture in the ground when applied in April, there is reason to suppose that the former would not do so when applied in May. I regret the failure of the experiment without any manure, as I think the result would have shown satisfactorily that the land is so far from being impoverished by this system of cropping, that it is improving every year. I think, however, that this is shown by the produce of the land manured with guano alone. In the first year's experiment the produce from guano alone was 27 bushels per acre, and both straw and wheat were very indifferent in quality. This year the produce from guano alone is 42 $\frac{1}{3}$ bushels;

Page 58

and although neither straw nor wheat are so good as upon the adjoining lands, they are both very much better than they were in 1842. It will be observed that the result from the unsubsoiled portion is very good, and if nothing more were said about it, people would be led to conclude that there was no advantage in subsoiling. But this, in my opinion, would be a great mistake; for to say nothing of the advantage which the unsubsoiled portion would derive from the drainage which it received from the subsoiling on each side of it, I found, when the field was ploughed up this autumn, that whilst the unsubsoiled portion was stiff and heavy, the subsoiled part was comparatively friable and loose, like a garden, and will, I expect, show its superiority in the succeeding crops. It must be borne in mind, in reading these experiments, that we have here one of the most unfavourable climates in the kingdom for growing wheat, from the excessive quantity of rain that falls, three times more rain falling annually in the north of Lancashire than at York, and this, no doubt, is very prejudicial to the success of such a series of experiments as I have been detailing. It has been objected to these experiments, that allowing all to have been done which is here detailed, it leads to no important conclusion; for although it may be practicable to grow wheat every year, in a small field like the one experimented on, it is not so on a large scale. But the objectors should remember that there is not the seed of a single weed sowed with the manure; and therefore if the land is thoroughly cleaned, and kept so, by hoeing the crop in the spring, it will require very little labour to fit it for another. But I shall be better able to speak on this head next harvest, having sowed wheat on an oat stubble with once ploughing. It is said there are no weeds in Chinese husbandry, and if they can eradicate them completely, so may we, if we adopt the same methods and follow them up as perseveringly. Again, admitting that it is not practicable to grow wheat on the same land year after year on a large scale, yet if we can double the crop in those years in which we do grow it, by the application of chemical manures (and the same manures are applicable to all cereal crops), will not that be a conclusion worth arriving at? That it is possible to do so, is, I think, sufficiently shown by the results I have obtained. What, then, may we expect when these experiments are infinitely multiplied and varied, under the superintendence of skilful and experienced men, who will devote their whole time and attention to the subject? Will raising the average produce from twenty-five to fifty bushels per acre be the utmost limit to which improvement can be carried? I believe not. In conclusion, I would urge on all owners and occupiers of land, the importance of devoting at least a small field to agricultural experiments, as I think there can be no doubt that, if these are carefully and systematically made and followed up by agriculturists generally, we shall be so far from needing an importation of corn in average years that we shall have a large surplus to spare for our neighbours.

Page 59

NOTE.—In the use of silicates of soda and potash one precaution is very necessary—viz., that you really have a soluble silicate, and not a mere mechanical mixture of ground flint and soda: this is a very different thing, and one, if it be not carefully guarded against, which will lead to nothing but disappointment. Again, the silicate may be properly made in the first place, but in a long exposure to the atmosphere the soda attracts carbonic acid, and the soda is liberated, and this has defeated my expectations more than once. Again, though I consider it desirable to defer the application of it until vegetation has fairly started in the spring, yet, in one instance, I delayed the application of it so long, that there was not moisture to dissolve it until the end of June, and then the wheat began to shoot afresh from the roots and the crop was seriously injured by it: but this was in an exceedingly dry spring, and might not happen again for many years.

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To the same.

LOW MOOR, *18th December*, 1845.

SIR,—I promised to communicate to you the results of my attempt to grow wheat on the same land year after year, this being the fourth crop of wheat (the fifth white crop) grown in successive years on the same soil, and though I consider the crop an indifferent one, I don't think the failure ought in any degree to be attributed to the over-cropping, but to the wetness and coldness of the season, as well as other untoward circumstances hereafter to be mentioned.

In a former letter of mine of the 12th October, 1844—which was published in the "Guardian" a few days after—I gave an account of the crop of 1844, which was a very good one, being fifty bushels to the acre throughout the field, and as much as fifty-two bushels in the best part of it. This I considered so satisfactory that I had the field again ploughed up and sowed with wheat on the 9th October, 1844, and it is to the results of this crop that I wish to call your attention. As remarked in my former letter, the field was subsoil ploughed in the autumn of 1843, and this subsoiling was carried to such a depth that most of the drains in the field were more or less injured by it; and although this did no injury to the crop of 1844, owing to the very dry season, yet when the rain came in the winter of 1844, the want of drainage was found to be very prejudicial, and in the wet places large patches of the young wheat went off altogether, and there was a great deficiency of roots in many parts of the field; the long continuance of frost and after that the ungenial weather which continued so long in the spring (of 1845) were also unfavourable, yet with all these drawbacks the appearance of the plant after the growing weather *did* come, was very promising, and many of my friends predicted that I should have as good a crop as in 1844. On the 24th March I applied chemical manure of the same kind as I had done in 1844, at the rate of about

Page 60

3 1/4 cwt. to the acre (costing 23s. 6d.), and a fortnight after I had it sowed with 2 cwt. of guano to the acre. When the warm weather came, these manurings seemed to help it wonderfully, and it was, as I have before stated, a very promising crop; but the cold, ungenial weather we had through a great part of the summer, and the continued rain we had whilst the wheat was in flower, destroyed all the former promise: and the manuring with guano, so far from being beneficial, was very injurious—so much so, that I believe every shilling's-worth of it applied to my wheat this year, made the crop a shilling worse than if nothing had been applied; and all ammoniacal manures had the same effect. It may be asked how I know it was the guano, and not the chemical manure. In answer to this inquiry, if made, I may observe, that I supplied two of my neighbours with the chemical manure, and they applied it without guano on very poor land, and they both assert they had never such good crops of wheat before; but everywhere in this neighbourhood, the only good samples of wheat that I saw or heard of were grown on exhausted soil. This appears to me to be a strong proof that chemistry has a great deal to learn before it can adapt its measures to all varieties of seasons, particularly as it cannot know beforehand how the season may turn out. If further proof be required of the injurious effect upon grain crops of ammoniacal manures in general, and of guano in particular, I may mention that in another field of wheat, sowed on the 21st December, and which did not come up until the frost broke, in March (the previous crop having been Swedes), the blade was so yellow and the plant altogether so small and sickly in appearance, that I had it manured with a water-cart from a cesspool in April. This appeared to produce a wonderful improvement immediately, as the plant assumed a deep green and grew very fast, but when it ought to have shot, the heads seemed to stick in the sockets, the blade and straw became mildewed and made no progress in ripening. It was not fit to cut for three weeks after the experimental field, although it was an early white wheat, and the result was a miserable crop—far worse than the experimental field. The instance of injury from the use of guano, I had from a neighbour, who told me he had sowed a patch of oats with it, and that they never ripened at all, and that he was compelled to cut them green as fodder for his cattle. I had a striking proof this season of the much lower temperature required by oats than wheat, when strongly stimulated by manuring. I had gathered an ear of wheat and a panicle of oats the previous season, which seemed to me to be superior varieties; and that they might have every chance, I dibbled them alongside each other in my garden, and determined to manure them with every kind of manure I could procure, as I had an idea that it was not easy to over-manure grain crops, if all the elements entering into the composition of the plant were applied

Page 61

in due proportion to each other, and I also wished to ascertain whether wheat and oats would thrive equally well with the same sort of manuring. I accordingly limed the land soon after the wheat came up, and in March I applied silicate of soda, sulphate of magnesia, gypsum, common salt, and nitrate of soda. A fortnight after this I applied guano, then bones dissolved in sulphuric acid, then woollen rags dissolved in potash (the two latter in weak solution); and the consequence was, that I don't think there was a single grain in the whole parcel—at least I could not find one—the straw was no great length, and the blade much discolored with mildew, whilst the oats were seven feet high, and with straws through which I could blow a pea, and large panicles, although the oat was not particularly well-fed. The inference I have drawn from these experiments is, that as far as is practicable the manuring should be adapted to the temperature, but as this is obviously impossible in a climate like ours, the only way is to rather under than over manure, and to apply no ammoniacal manure to the wheat crop, or at all events very little; for although guano was beneficial to wheat when used in conjunction with silicates, &c. &c. in 1844, yet the injury it did in 1845 may very fairly be set against that benefit. I should feel obliged if any of your readers who may have tried the experiment of manuring grain crops with guano, the last season (1845) would publish the result as compared with a similar crop without such manuring. I feel convinced that such result would be against the use of guano for wheat in 1845. I am the more confirmed in the opinion that ammoniacal manures are unfavourable for wheat, by a series of articles in the "Gardener's Chronicle" on the "Geo-Agriculture of Middlesex," in which the writer states that land in that county which in Queen Elizabeth's time produced such good wheat that it was reserved for her especial use, will now scarcely grow wheat at all, and when that grain is sowed upon it, the straw is always mildewed, and the sample very poor; and this is attributed—and no doubt justly so—to the extensive use of London manure. My crop was only 32 bushels to the acre of 60 lbs. to the bushel; last year the crop, as I have said before, was 50 bushels of the same weight.

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To the same.

CLITHEROE, *7th March*, 1848.

On continuing my attempts to grow wheat on the same land year after year, I observed that the crop of 1845 was very seriously injured by the deficient drainage—the old drains having been destroyed by the subsoil plough. It was therefore necessary to replace them: they were accordingly put in four feet deep. This occupied so much time that the season for sowing wheat had gone by, and the ground was cropped with potatoes, which were got up in September, and the wheat might have been got in early in October. But seeing in your paper that sowing too early was not advisable,

Page 62

and also being carried away by the arguments of the thin-seeders, I deferred sowing until the middle of November, and then put in little seed; and the winter proving very unfavourable, when the wheat was coming up, there was not half plant enough in the spring, and I hesitated whether to plough up the ground or drill in barley. I determined to do the latter, which was done on the 18th April, and wheat and barley grew up together, and when cut and threshed, proved to be equal to 48 bushels to the acre.

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LOW MOOR, *31st December*, 1844.

HENRY BRIGGS, ESQ.

I duly received your obliging letter in reply to my pamphlet on the growth of wheat year after year on the same land, and now offer my rejoinder to your remarks. You seem to consider the expense is too great under the system pursued by me; and that it was more than was required by the crop, is proved in my opinion by the fact that the fertility of the land is very much augmented since the commencement of the experiment in 1841: as my first crop with guano alone produced only 27 bushels per acre, whilst this year from guano alone the produce was 42 bushels. But still I think that your allowance of manure is far too little, and not exactly what I should apply, and I shall frankly state my objections and opinions, in the hope that they may elicit a reply from you, as it will be from discussion and the experiments instituted to test the various theories propounded, that agriculture will be most materially benefited. You state that Liebig's present theory is, that plants obtain the necessary oxygen, hydrogen, carbon, and nitrogen from the rain and atmosphere, and that the plants merely require the supply of inorganic constituents, and that you are inclined to agree with him. My copy of his work on the Chemistry of Agriculture is his first edition; and I don't know how far he has since modified or altered the opinions therein expressed, which are in some degree at variance with each other. He states that it may be received as an axiom in agricultural chemistry that the nitrogen of the atmosphere is never assimilated by plants, except in the form of ammonia or nitric acid. He certainly states that plants and animals derive their nitrogen from the atmosphere; but why, if this be true, does he attach so much importance to the excrements (particularly urine), of men and animals being husbanded with so much care? and he states that for every pound of urine wasted, a pound of wheat is thrown away. But even if he said it was utterly worthless, every practical farmer who has tried it knows how exceedingly valuable it is. It may be said there are other ingredients in urine besides ammonia, and these are what make it valuable; and in reply to this I would ask what is it that makes the ammoniacal liquor from gasworks so valuable? There are no phosphates or alkalies there, and yet what a powerful stimulant it is. Again, Liebig states that the carbon is

Page 63

derived from the atmosphere; but to say nothing of the argument which might be deduced from the advantage which is derived by plants from having their soil loosened about their roots, the experiments of Dumas and Boussingault prove that a tree which was cut off below the branches expired a large quantity of carbonic acid. It may be asked how I know this was not precipitated by the rain. I don't know; but if the plant would assimilate this, why should it not assimilate that which arises from the decomposition of the carbonaceous matter in the soil? My idea is that it does both, and that carbon in the soil does good if it offers an abundant supply of carbonic acid to the plant when it is in a condition to appropriate it. Your allowance of lime appears to me to be far too small, for if any reliance can be placed on my experiments, lime can be profitably used to far greater extent than you seem to imagine. And, again, you seem to think that where there is plenty of silex in the soil, the plant will be able to obtain as much as it requires. I think that it is quite necessary that the silex should be in a soluble state, as I think that it is not only desirable that all the elements necessary to fertility should be in the soil, but that they should be in such a form that they can be assimilated by the plant. Some of our compounds for producing fertility may perhaps be as absurd as it would be to give muriatic acid to a man troubled with indigestion, because free muriatic acid is found in the stomach of a healthy person. Let me recommend you to try both silex and magnesia in a soluble state, and I think you will be satisfied with the benefit derived from their use.

Recurring again to the quantity of manure necessary to grow thirty-six bushels of wheat, I would ask, why limit yourself to so small a crop? The difference in the cost of your manuring a field, and my manuring it, is more than made up by the increase of fourteen bushels of wheat and the corresponding increase of straw, even if the land did not improve every year by the application; and as the seed, rent, labour, and liabilities of the land are the same whether you grow a small crop or a large one, why not have it as large as possible? Again, if I applied far more manure than was necessary, I ought to have had the crop equally good throughout the field; but on the ridge of the hill, where the soil was thin and poor, neither straw nor wheat were so good as they were where it was deeper and richer. My own opinion is, that the plant is never able to extract from the soil all the manure, and therefore it ought to be brought up to a good standard before good crops can be expected. I am not satisfied with any analogy that I can think of, but the best that occurs to me is that of a cloth in a dye-copper. You can never get it to absorb either all or half the colouring matter, and if you don't use far more than is taken up by the cloth, you will never obtain the desired results. Besides, in chemical combinations it

Page 64

is desirable to use far more than the chemical equivalents, or the experiments don't succeed. I perceive that you intend to use guano next year, and that you intend to use it along with the seed. I trust it will not be sowed in contact with either the seed or the quicklime, which you proposed to use in some of your land. The best time I have found for applying guano is in wet weather, just when vegetation is making a start in the spring—say the last week in March, or the first week in April—as I fear a large part of the soluble portion of it would be washed away by the rains of winter. It is true we have had none this winter, but when shall we have such another? Did you ever use woollen rags as manure? They ought to be excellent, as they are almost all albumen, and are, I fancy, to be had at a very moderate price, not far from you. Can you inform me what it is that causes the land to be clover-sick? If it is the abstraction of something from the soil, what is that something? Sir Humphrey Davy said that a dressing of gypsum would prevent it; but clover does not succeed here (even when dressed with gypsum), if sowed every four years. One reason why I think so small a quantity of manure will not succeed, is based on the theory of excrementitious secretion. Decandolle proved that this secretion took place, but he did not succeed in proving that it poisoned the land for a similar crop. I can only reason from analogy, and it does not follow that an analogy drawn from animal life will hold good when applied to plants; but if we were to feed an animal with pure gluten and pure starch, with the proper quantity of phosphates, &c., are we to suppose it would have no excrements? Let this be applied to plants: are we to suppose that the plant assimilates all that is absorbed by its roots and leaves? When that which is absorbed is what would enter into the composition of the plant, is it not more rational to suppose that the inorganic and gaseous constituents only combine in fixed proportions, and that although the plant may absorb a much larger proportion of one than is required, the surplus is discharged excrementitiously, and perhaps may be unfitted for entering into the plant until it has undergone a decomposition? In conclusion, I trust you will pardon my frankness in so boldly canvassing your opinions; but it is in this collision of opinion that the truth will be elicited, and if I judge you aright, it is that you wish to discover whether it harmonizes with your preconceived notions or not.

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LOW MOOR, *1st May*, 1845.

HENRY BRIGGS, ESQ.

Page 65

I duly received your pamphlet on the use of lime, for which I am much obliged, and am delighted to perceive that you confirm the idea (expressed in my pamphlet on the growth of wheat every year on the same land) that the excessive use of lime is ultimately injurious to the fertility of the soil to which it is applied. This, coming from a gentleman of your reputation and experience, will, I hope, induce someone capable of performing the experiment to endeavour to ascertain with precision how much lime it is desirable to apply to an acre to give the best results, and with the least waste, assuming that the land contained little or none previous to the experiment; and it would also be desirable to ascertain whether it is better, in an economical point of view, to apply a small quantity every year, or a larger quantity every third or fourth. My own opinion is in favour of the former method, except that it is difficult to get it ploughed in, particularly in wet weather, immediately after spreading (which is essential where you grow wheat on the same land every year) without injuring the feet of the horses. You speak of ten days or a fortnight being necessary to neutralize caustic lime, but our horses had their feet injured by it six weeks after it had been spread on the land, last year, although the weather had been wet almost the whole of the time, say from the beginning of February to the middle of March. You appear to think that lime will replace silica in the wheat plant. Whose authority have you for this? It will be very important to establish this supposition, but I fear it is too good news to be true. On referring to your letter, I find you don't say what I supposed you did, but that the lime liberates the soluble silicates, potash, &c. This may be, and certainly the beneficial effects of lime in growing wheat are not to be explained by any other hypothesis with which I am acquainted. I am this year trying some experiments to ascertain (if I can) the cause of clover-sickness, and I hope to be in a position to say whether your supposition that lime, gypsum, &c. will prevent it, is correct. My experiments so far are opposed to this theory, but it is not very safe or philosophical to draw conclusions from one or two experiments only. I doubt the possibility of making silicate of soda by merely mixing lime, sand, and salt together, as my chemical friends tell me this cannot be accomplished unless the silex and the alkali are fused together. If a soluble silicate of soda can be made in the way you mention, it will be a great saving of expense. Has it been tried? You have no doubt seen a report of the enormous crop of wheat grown in a field in Norfolk last year (90 bushels to the acre), and that the Royal Agricultural Society have determined to have the soil analyzed by Dr. Playfair. This is very desirable, but as Dr. Playfair is more of a lecturing than an analyzing chemist, I think it is very necessary that his analysis should be checked by another, made by

Page 66

the most eminent chemist that Europe can produce, for 90 bushels is so unheard-of a crop, that no expense should be spared which would enable us to ascertain what the soil contained to enable it to produce such a crop, which is the more remarkable as the field seems to have been a good many years under the plough. As your Wakefield Farmers' Club has many wealthy members in it, allow me to hint the desirableness of your undertaking this analysis, which, if properly performed, will be worth a thousand times more than its cost. When you are aware that even Davy missed 16 per cent. of alumina in one of his analyses and that the chemists of the present day don't seem to have detected the potash which exists so abundantly in potato-tops, you will, I think, agree how exceedingly important it is that such analysis should be checked by others, made without any communication between the parties. You speak of an original letter of Liebig's appearing in the "Farmer's Journal." On what subject is it? as I have no means of referring to the periodical in question. Does it throw any light upon the new manure for which he is said to be taking out a patent? You speak of humus and humic acid. What do you understand by humus? as, according to Liebig, humus sometimes means one thing and sometimes another, and he appears to treat it very much as modern chemists treat phlogiston, as something which they don't comprehend, but which they need to explain the phenomena of vegetation. If you are a believer in humus, what is it composed of, and how does it act in forwarding vegetation? I suppose you will reply, By combining with oxygen and forming humic acid. But would not the theory of the decomposition of carbon do quite as well? I don't perceive the injurious effects of quicklime upon grass land which you anticipate in your paper, but the contrary, and the more caustic it is the more beneficial is its action, so far as I can judge from my own experiments; and it is my practice in liming grass land to spread it as soon as I can get it into the state of flour. I shall be glad to hear the result of your electrical experiment—at present I am rather sceptical on the subject.

P.S.—Am I to suppose that you have abandoned the idea of manuring an acre of wheat for thirteen shillings?

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THE CULTIVATION OF WHEAT.

October 1st, 1852.

To the Editor of the "Manchester Guardian."

The increasing quantity of agricultural produce consumed in this country makes it desirable that the cultivation of the land should be carried to the highest point consistent with profit; and the increasing scarcity of agricultural labourers will shortly render it difficult for the farmers in some districts to gather in their crops. It therefore becomes

increasingly desirable that every mechanical contrivance which will facilitate their doing so should be made as perfect as possible; and also that the crops themselves should be so cultivated as to make these mechanical aids to work to the greatest advantage.

Page 67

But it has been a difficult matter (at least in the wet climate of Lancashire) to ascertain how far it is prudent to manure for wheat, for in unfavourable seasons the plant runs so much to straw that it is liable to lodge, and become mildewed; in which cases the manure is not only wasted, but becomes positively injurious, as appears to be the case in the South of England this year, and as was also the case in the North in 1845, when every shilling expended in manuring the wheat crops of that year made the crop at least a shilling worse than if no manure had been applied.

But if we could find a wheat so short in the straw that it would bear heavy manuring without being lodged, wheat-growing would be a far less hazardous occupation than it is at present, and we might confidently calculate on a far greater production than we can now.

The following appear to me to be some of the advantages of growing a short-strawed wheat:—

1st. It will bear highly manuring without lodging, and with much less liability to mildew, than a long-strawed wheat.

2nd. The proportion of grain to straw is greater in short than in long-strawed wheat.

3rd. As it very rarely lodges, it will be far better suited to the reaping-machine than a long-strawed wheat; and no doubt other advantages will occur to the minds of experienced agriculturists.

When making these assertions I ought to state that my experience of wheat-growing does not extend beyond the counties of York and Lancaster, but from what I can learn of the agriculture of more southerly districts, I fancy these opinions of mine will be found correct even there. I may be asked to prove my assertion, and I will endeavour to do so.

I have been experimenting on the growth of wheat for the last ten or eleven years—particularly with reference to the practicability of doing this on the same land year after year; and that I might do it in the most satisfactory manner, I have varied my seed-wheat and my manure very frequently: but I very soon discovered that the advantages of abundance of manure and high cultivation did not insure good crops of wheat, inasmuch as in our moist climate, we had not one summer in five that was favourable, and consequently the crop was generally lodged, and the straw mildewed. I found that the time of sowing, and also of applying the manure, were matters of great importance, and it occurred to me that the remedy would be—a straw so short, that it would not lodge when highly manured. I consequently addressed a query to the “Gardener’s Chronicle,” asking what was the shortest-strawed variety of wheat known, and was told that Piper’s Thickset was so; I therefore got some of this sort from Mr. Piper, which I have cultivated since 1847. It is a coarse red wheat, but the quality has improved with

me every year, and this season *being the third successive crop on the same land*, I have nearly eight quarters to the statute acre from this variety.

Page 68

2. The proportion of wheat in Piper's Thickset is 38 per cent. of the gross weight of the crop; in the Hopetown wheat (I speak of my own crops only), 34 per cent.

3. Not having seen a reaping-machine, it may seem absurd in me to say that short-strawed wheat is better adapted to it than long-strawed; but every report of the working of these machines goes to show that, so far, they are not well adapted to the cutting of laid corn; therefore a variety that always stands upright will be much better suited to the working of them.

I have been trying for the last six years to obtain (by cross-breeding) a wheat of good quality, and with a straw shorter than Piper's, but hitherto with indifferent success; but, thanks to the kindness of Messrs. Brownells, of Liverpool, who furnished me with many samples of Chilian wheat about three years ago, I have now got varieties much shorter in the straw than Piper's, and some which appear to be of much better quality, but these will require to be tested for a year or two before I can speak decisively about them. The Chilian varieties are very difficult to acclimatize. The original samples were beautiful white wheats, very much resembling the Australian, but when grown in Lancashire they resemble rye more than wheat, and three years' sowing has not much improved them. It has, however, enabled me to obtain crosses which seem better adapted to the soil and climate, and so short in the straw that the highest manuring produces no tendency to lodge.

If we could obtain a variety of wheat of good quality, which, instead of two tons of straw and one of wheat to the acre, produced a ton and a half of each, it might be profitably cultivated, and the differences in the chemical composition of grain and straw are not so very great as to make me despair of this being done some time or other. It may be asked, Where can a short-strawed wheat of good quality be procured? To this I am afraid the reply will be, Nowhere at present. But can none of our expert manipulators, who rejoice exceedingly when they cross-breed a geranium or a fuchsia, turn their attention to the cross-breeding of wheat? Cannot the Royal Agricultural Society offer a premium for a short-strawed wheat of good quality? Do none of the great agriculturists themselves see how desirable such a wheat would be for the agriculture of this country? Apparently not; for with the exception of Mr. Raynbird, of Hampshire, I am not aware of one scientific operator who is endeavouring to produce such a wheat. My own attempts at cross-breeding are such as may be tried by anyone who has sufficient perseverance, and (with one or two exceptions, of doubtful success) have been confined to sowing the different varieties I wished to cross in contiguous drills, and then sowing the produce of these. At the second harvest I carefully select such ears as differ from both varieties, and at the same time seem by their quality of grain and the shortness of their straw to be the best suited to my wishes. It has been, no doubt, to the accidental contact of distinct varieties that we owe the numerous kinds now known to agriculturists, and which differ from each other in colour, quality, yield, and comparative value in the various districts in which they are grown.

Page 69

Fully sensible of my inability to do justice to this important subject, I yet hope (if you do me the honour to publish my letter) that my remarks may induce scientific men to consider it; for it appears unaccountable to me that hitherto they seem to have thought it unworthy of their attention.

P.S.—There is still time to try the experiment during the present season. If any gentleman wishes to try the short-strawed Chilian wheat, I shall be glad to give him a sample of it for the purpose of cross-breeding. Samples were sent to Mr. H. Briggs, Mr. Raynbird, and Mr. Stevenson, Stockport.

* * * * *

January 27th, 1848.

To the Editor of the "Agricultural Gazette."

You invite persons who have grown good crops of grain or turnips to forward you the particulars. I therefore enclose you an account of an attempt which I made to grow wheat on the same land year after year, that account reaching to the fourth white crop in 1844. As I still continue the experiment, I shall be in a position to continue the account up to the present time (as I am now threshing out the last year's crop), and will send it to you if you think it worthy of insertion in the "Agricultural Gazette."

If the account I now send is not worth inserting, please to send it to your correspondent A. W., who doubted whether there were authenticated instances of land producing eighty, seventy, or even fifty bushels to the acre.

I attribute my success in growing wheat to the use of silicate of soda, and yet, singularly enough, until now I have been unable to induce anyone else to try it. This season, however, several persons have applied to me to procure it for them. Among them is the talented editor of the "Liverpool Times," whose farm at Barton Moss shows what good management will accomplish on very unpromising soils. If, as I hope will be the case, the silicate of soda should supply to peat its greatest deficiency, no one will more readily discover it than Mr. Baines.

In the use of silicates of soda and potash, one precaution is very necessary, namely, that you really have a soluble silicate, not a mere mechanical mixture of ground flint and alkali. This is a very different thing, and one which, if it be not carefully guarded against, will lead to nothing but disappointment.

Again, the silicate of soda may be properly made, in the first instance, but in a long exposure to the atmosphere, the soda attracts carbonic acid, and is liberated from the silix, and this has disappointed my expectation more than once.



Again, though I consider it desirable to defer the application of soluble silicates until vegetation has made a fair start in the spring, yet in one instance I delayed the application of it so long that there was not moisture to dissolve it until the end of June, and then the plant began to send up suckers from the roots, and the crop was seriously injured by it; but this was in an exceedingly dry spring, and may not happen again for many years.



Page 70

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CLITHEROE, *March 7th*, 1848.

In continuing my attempts to grow wheat on the same land year after year, I observed that the crop of 1845 was very seriously injured by the deficient drainage—the old drains having been destroyed by the subsoil plough. It was therefore necessary to replace them; they were accordingly put in four feet deep. This took up so much time, that the season for sowing wheat had gone by, and the ground was cropped with potatoes, which were dug up in September, and the wheat might have been got in early in October; but seeing in your paper that sowing too early was not advisable, and also being carried away by the arguments of the thin-seeders, I deferred sowing until the middle of November, and also put in little seed, and the weather proving very unfavourable when the wheat was coming up, there was not half plant enough in the spring, and I hesitated whether to plough up the ground or to drill in barley. I determined to do the latter. It was put in on the 18th April, and wheat and barley grew up together, and when cut and threshed, it yielded 48 bushels to the acre.

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ON THE GRAVELLING OF CLAY SOILS.

There is an old story of a man, who, having a very stony field, determined to experiment on the value of these stones in the growth of his crops.

With this view he divided his field into three equal parts. From No. 1 he gathered all the stones, which he spread upon No. 3, leaving No. 2 in its original condition. He then sowed barley over the whole field, and carefully noted the results. The story ends by saying that No. 1 bore a miserably poor crop, No. 2 a tolerable one, and No. 3 a splendid one.

I quote this story as a text on which I wish to speak as to the advantage of gravelling heavy clay soils. Some weeks since I spent a few days at the village of Milnthorpe, in Westmoreland, and during one day with Mr. Hutton, the celebrated bone-setter, I remarked that the land was very stony, being covered with stones (not pebbles) having very much the appearance of road metal. He replied, that these stones were essential to the fertility of the soil, and said that some years before there was a great demand for such material in the neighbourhood of Preston, and the high prices stimulated the farmers to gather these stones from their land, and send them to Preston; but the consequences were so injurious to the growth of their crops, that they were compelled—at least those who had the means of doing so—to lead stones again upon their land before their crops would grow again with the vigour which they had before the stones were abstracted. This brought to mind what had occurred in my own farm practice. A church was built in the neighbourhood, and the stones for it were hewn on the corner of

a field which was afterwards sown with wheat, and I remarked that the straw was much brighter, the ripening was forwarded ten days, and the sample was much better where the stones had been hewn than elsewhere in the field. (The stones of which the church was built were of ordinary sandstone, probably millstone grit.)

Page 71

Borrowing from this hint, I had the field covered with about 400 cartloads of alluvial gravel (from the bed of the river) to the acre, and the land was then ploughed two furrows deep, one plough following the other. Previous to this gravelling, the land was a stiff, obdurate clay nearly to the surface. The subsequent effect was the doubling, or more probably trebling the value of the land, which has now become a nice friable soil.

I was much amused with the criticisms of some of the neighbouring farmers (men of the old school), one of whom remarked that he had seen land tilled (manured) in various ways before my time, but until now he had never seen a field tilled with cobble-stones. I said, "What is your objection to it, John?" "Why, ye see, it makes th' land so poor." I replied, "Making anything or anybody poor, means robbing them of something. If you had twenty shillings in your pocket, and I filled it up with these cobble-stones, how much poorer would you be? Of what have I robbed this field by putting gravel into it?" "Why, of nothing; but it looks so queer." I said, "John, did you never hear of a man gathering the stones off his field, and then having to lead them back again?" "Yes, I have; but then they were *natural* to the soil." I said, "What does manuring land mean, but putting something into it of which it is deficient? You don't till a muck-midden. If in stony land stones are essential for the vigorous growth of the crop, is it not exceedingly probable that they will be still more beneficial on stiff land which has no stones in it?"

This is a doctrine I tried many years since to inculcate upon our friend Mechi, and some of his land (I speak of its condition twenty years since) needed such a gravelling as much as any land I ever saw. Whether he adopted my suggestion, or his land remains in the same condition now as then, I don't know; but if it does, I would just suggest to him and to all landed proprietors who own stiff clay lands, if they are near to gravel-pits, to try a small portion by gravelling it freely, and let us hear the results.

December 2nd, 1871.

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COTTON.

June 1st, 1842.

J. KINDERMANN, ESQ.

I have for some time intended to call your attention to the importance of attempting to grow fine cotton in Peru, but my inability to do justice to the subject, both from my being practically unacquainted with any mode of growing cotton and my general want of information, has hitherto prevented me; but as I made you a promise to that effect yesterday, I have endeavoured to put a few suggestions on paper, and hope that if they be carefully acted upon, some benefit may be derived from the experiments.

Page 72

We have been (as you are aware) consumers of Peruvian cotton to some extent for the last six or eight months, and from the observations I have made on it during that time, I have no hesitation in saying that it possesses many excellences: it is long enough (almost too long), very sound in staple, and where well managed of a very good colour. Its defects are coarseness and harshness of staple, and if these could be removed I don't see what is to prevent its rivalling the Egyptian and Sea Islands cotton, any considerable approximation to which would very materially enhance its value, seeing that the highest quotation for Sea Island, was last week 30d. per lb. (2s. 6d.), whilst the highest for Peruvian was no more than 6 1/2 d.

With the view of improving the quality of the cotton in Peru, I would strongly recommend you to send seeds of various kinds, packed in air-tight boxes, particularly Sea Island and Egyptian, which some of the cotton-brokers would easily procure from the spinners using these descriptions, and, judging from what I hear of the climate of both countries, I should think the Egyptian would go to a very similar atmosphere and mode of cultivation to that of the country where it had been raised, which would probably render it more easy to acclimatize, and, of course, make it more likely to succeed than a sort of cotton which had been grown under dissimilar circumstances of soil, climate, and mode of cultivation.

These seeds when sown, ought (with the exceptions hereafter to be mentioned) to be planted at such a distance from all other cottons as to render it very unlikely for the wind or insects to carry the pollen from the flowers of one kind to those of another; for without this precaution, such is the tendency in many genera of plants to hybridize (and I believe, from what I have heard, there is this tendency in the different varieties of cotton) or cross-breed with each other, that, however good the quality in the first instance, they would all revert to the old variety in a year or two in consequence of the great preponderance of that variety over any newly-introduced ones. So much are the growers of turnip-seed for sale in England aware of the importance of attending to this, that the greatest precautions are taken to remove all *cruciform plants* from the vicinity of the field whilst their turnips are in flower, as there is such a tendency in them all to hybridize that the quality of the seed is often injured by the wild mustard (*Sinapis arvensis*) springing up in the same or the adjoining fields; whilst, on the other hand, by carefully selecting the best bulbs for seed, and by judiciously crossing one variety with another, new sorts are obtained, combining the excellences of both. This leads me to observe, that probably seed of foreign varieties of cotton may not thrive well in the first instance, and I would therefore strongly recommend the gentlemen who may make the experiment carefully to select seed

Page 73

from the plants on their estates which they see are growing the best and finest cotton, and sow them in contact with a few seeds of each of the sorts you may send out, carefully removing them in every instance as far as may be practicable from the vicinity of all other cotton; and then again sowing the seeds which are obtained from the plants thus raised in contiguity to each other, and carefully examining the cotton grown upon each of them, it is more than probable they will find that *some* of the plants will be varieties partaking of the character of both the parent kinds, and by selecting the best of these and sowing them only (still apart from all other cotton), there is little doubt that much benefit will be derived by the persevering and skilful cultivator.

I have heard it stated that the origin of Sea Island cotton is to be traced to something of this kind. An observing and experimental planter, by carefully examining his cotton, and by sowing his seed only from those plants that produced the finest and longest staple, at last arrived at the excellent quality which is now known by that name.

Look, again, at what has been done in Egypt by the introduction of better varieties of cotton. There these improved varieties have by no means had a fair chance of showing what they are capable of becoming, inasmuch as the wretched cultivator has not the slightest inducement to improve their quality—he gets no more per pound for the finest and cleanest cotton than he does for the coarsest and dirtiest, and therefore it is not very likely to improve under his care. But with all this neglect and want of management, we can see by what it is, what it would most probably become in the hands of an enterprising and intelligent man who knew that every improvement he made in its quality would be to his own advantage. Assuming that your Peruvian friends could so far improve the quality of their cotton as to double its value in this market (and I don't think myself too sanguine in expecting more than this), with very little extra labour nearly all the additional price would be profit.

But supposing even that cross-breeding, or hybridizing, as the horticulturists call it, does not frequently occur naturally in cotton, it is well known that it is very easy to effect it artificially by prematurely unfolding the petals and with fine scissors cutting away all the stamens before impregnation takes place. This requires to be carefully done, so as not to injure the petals, and they will then close again of themselves, and when they expand naturally, then impregnate the stigma of the flower with the pollen of the kind you want to cross with. We owe many of our finest varieties of fruits to this practice. The late Mr. Payne Knight was very successful in raising new varieties of many sorts of fruit in this way, and it appears to me from the experiments I have made that the more frequently this cross-breeding takes place, the more easy (within certain limits) is it

Page 74

to extend it until cultivation has so completely changed the character of the plant that it bears very little resemblance to its original stock. There is nothing growing wild like our cabbages, turnips, and cauliflowers; nor even like our carrots, celery, and asparagus. Where are the originals of our wheat, barley, rye, beans, and peas? Many of these appear to be so completely transformed by cultivation that we don't know where to look for the parent stocks from which they originated. But I am forgetting cotton altogether, yet beg to refer to the preceding paragraph to show how much is owing to careful cultivation, and trust that it may not be without its use if my letter induces your friends to make the experiments here suggested, even though their first attempts are unsuccessful.

This letter was translated into Spanish and circulated in Peru, but with what success I do not know. It was also published in the "Gardener's Chronicle," and led to a reply from Dr. Royle, which occasioned the following letter.

* * * * *

August 14th, 1845.

To the Editor of the "Gardener's Chronicle."

I am very glad that my letter and your remarks on the improvement of cotton in India have attracted the attention of so able a correspondent as G. F. R. (Dr. Royle), who appears to be conversant with a good deal of what has been attempted there. No doubt there are, as he states, great diversities of soil and climate in so extensive a country as India; and if so, although there may be some which are not adapted to the growth of either the *Gossypium Barbadosense* or the *Gossypium Peruvianum*, there must be both soil and climate suited to them in various localities in that country.

My chief reason for suspecting that the injury arises from the new kinds hybridizing with the indigenous cotton, is, that very good cotton has been grown from both varieties in the first generation, but when the seed from this first crop is sown again, the quality always deteriorates (at least all the gentlemen say so with whom I have conversed on this subject). I have a sample of Indian-grown cotton of excellent quality from Pernambuco seed, worth twice as much as the best Surat cotton I ever saw; but I cannot learn that anything deserving the name of aught but a sample was ever obtained. We hear of no increase in the quantity of this improved variety; it does not—like cotton in the United States—go on from ten bags to ten thousand, in eight or ten years; on the contrary, so far as I can learn, it dwindles away to nothing. The Tinnivelly cotton brought forward as an example by your correspondent is no exception to this—it is no more like Bourbon cotton, than Bowed cotton is like Sea Island—at least none that I ever saw. Bourbon is a long, silky-stapled cotton, whilst Tinnivelly has the shortness

and inequality of fibre common to most of the cotton of India. It is generally much cleaner than the cotton grown on the western side of India, but this arises from the greater care in picking it.

Page 75

An intelligent friend of mine, now in India, says that the pod of cotton is overhung by a brown leaf (bractea?), and if the cotton is gathered early in the morning, whilst the dew is on the plant, this leaf is tough and does not break, and the cotton is gathered clean; but if it is picked after the dew has evaporated, this leaf is brittle, and gets mixed with the cotton in the picking. But he says that no persuasion can induce the ryots to keep that which is picked in the morning from that which is gathered in the heat of the day. He also suggests that the cotton should be irrigated during its growth, and alleges as a motive for doing this, that in Egypt and Peru no good cotton can be grown without resorting to it. But the cases are not exactly parallel, inasmuch as no rain falls in either of these countries, whilst rain is most abundant in India, eighty or ninety inches of rain sometimes falling at Bombay in three months during the monsoon.

Another intelligent gentleman with whom I have conversed on this subject since my former letter was written, and who has resided at Bombay many years, where he has paid much attention to this subject, tells me that the gentleman entrusted by the East India Company with the management of one of the experimental cotton estates, assures him he has grown excellent Orleans cotton, and that the ryots were so satisfied with its superiority over the indigenous kind that 1,200 begahs (say 300 acres) were planted with it. But this was two years ago, and as the disturbances took place in this very neighbourhood, he fears these plantations have perished, as he heard no more of the matter, and had omitted to inquire of the gentleman entrusted with the management.

I reserved this until I saw the second letter from your correspondent G. F. R., which I have now read, as well as an article on the same subject in the "Manchester Guardian," in which it is stated that 20,000 acres are now under cultivation, planted with this improved cotton. I fear this is too good news to be true. My informant is a gentleman who was in correspondence with Mr. Mercer, the superintendent of these cotton estates, or some of them, and I have again questioned him. He says that the crop which would be gathered in March last, would amount to what I have stated (1,200 begahs), according to Mr. Mercer's letter to him, but he says it is now twelve months since he heard from Mr. Mercer, as he left Bombay for England shortly after. His fear was that none of this cotton would be gathered, as the disturbances which took place in Central India, and which required so long a time to quell them, were in this very district. If your correspondent G. F. R. has got samples of this improved cotton, of the second or third generation, he would confer a great obligation upon me by sending me a small sample of it by post. But this is wandering from what I intended to say, which was most heartily to thank your correspondent for his second

Page 76

communication, which goes far to prove the truth of what I had previously supposed, that the cotton of India is capable of great improvement by being judiciously crossed with suitable foreign varieties. Your correspondent thinks if the old varieties deteriorate the new when growing in proximity to each other, the new ought, for the same reason, to improve the old; and no doubt they will, but to a much smaller extent. It is said that a man leaping up into the air attracts the earth (proportionately) as much as the earth attracts him, and it may be so with the old and new cotton. What I mean to say is, that although some of the old sort of cotton might be hybridized by the new, the improved variety would be in so small a quantity that a thousand to one the cultivator would never observe it; and such is the aversion or indifference to anything new among the natives of India, that if an improved plant were observed, it is again a thousand to one he would take no pains to preserve it; and if he did, it is again perhaps a thousand to one that it would be entirely spoilt in the next generation by being planted among the indigenous sorts.

I trust your correspondent will continue to favour us with his communications whenever he has any fresh information on the subject, which, the more it is considered the more important it seems to be.

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PAPERS ON NATURAL HISTORY.

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WRENS' NESTS.

THE Editor of Loudon's "Magazine of Natural History," and one of his contributors, Mr. Jennings, were of opinion that the common Wren never lined its nest with feathers. The following contribution was sent to the "Magazine of Natural History" in consequence of this, and led to some discussion afterwards:—

April 17th, 1829.

Mr. Jennings and yourself, in opposition to Montagu, are of opinion that the Wren never lines its nest with feathers; like the knights of the gold-and-silver shield, both sides are right. It is true, many Wrens' nests may be found in which there are no feathers; but did you ever find either eggs or young ones in them?

As far as my observations go, the nest in which the Wren lays its eggs is profusely lined with feathers; but during the period of incubation, the male—apparently from a desire to be doing something—constructs several nests in the vicinity of the first, none of which are lined; and whilst the first nest is so artfully concealed as to be found with difficulty,

the last is very often seen. The Wren does not appear to be very careful in the selection of a site for these cock-nests, as they are called in Yorkshire by the schoolboys. I have frequently seen them in the twigs of a thick thorn hedge, under banks, in haystacks, in ivy bushes, in old stumps, in the loopholes of buildings, and in one instance in an old bonnet, which was placed among some peas to frighten away the blackcaps.

* * * * *

August 15th, 1831.

Page 77

TO PROFESSOR RENNIE.

In your edition of Montagu's "Ornithological Dictionary," just published, you say—speaking of the Wren—"An anonymous correspondent of Loudon's 'Magazine of Natural History,' &c. &c.;" and you remark, "There can be no doubt of these supposed 'cock-nests' being nothing more than unfinished structures of paired birds; otherwise, the story would require the support of very strong evidence to render it credible."

As I am the anonymous correspondent alluded to, I forward you a few observations of facts tending, as I think, to confirm my view of the question.

In the first place, these nests are far too abundant for the birds, which are not plentiful—at least, in this neighbourhood. Again, it is at least five to one that any Wren's nest which is found during the summer without a lining of hair or feathers is ever completed, or has any eggs in it. This I have verified in a hundred instances, when, having found Wrens' nests, I have visited them again at intervals, for the purpose of ascertaining whether my opinion of cock-nests was correct.

Farther, in a small wood adjoining my garden, where I was certain there was only one pair of Wrens, I found at least half-a-dozen nests, not one of which was either lined with feathers or ever had eggs in it; although I discovered they were not all deserted, as I found an old bird roosting in one of them. I was induced to be more particular in my remarks in consequence of my seeing Mr. Jennings's remarks in the "Magazine of Natural History;" and I searched, as I supposed, every bank, bush, and stump in the wood two or three times before I could find the breeding-nest, which I at last discovered in the twigs of a willow on the bank of the river, in the centre of a bunch of tangled grass, cotton waste, and straws which had been left there by the floods, and which the bird had apparently excavated and in it formed its nest, which was profusely lined with rooks' feathers.

The fear of being thought tedious prevents my giving other facts which tend, as I think, to prove the correctness of my opinion; however, I will just add that all the persons with whom I have conversed who take an interest in such pursuits, agree with me in opinion in this matter.

The nest I have just spoken of was also a strong proof that Wrens, although they may not always adapt their materials to the locality they have chosen for a nest, frequently do so; and if this is not with the intention of concealing it, but merely because the materials are at hand, it serves the purpose of concealment also, and very effectually. The one I am speaking of was so exactly like the other lumps of rubbish which had been left by the floods in the same bush, that I did not discover that it was a Wren's nest until I had pulled it out of the twigs; and if a Wren builds its nest in a haystack—which it frequently does—the front of the nest is almost invariably composed of the hay from the

stack, which prevents its being seen much more effectually than if the moss of which the body of the nest is composed were visible on the outside.

Page 78

The fact that the long-tailed tits occasionally associate to the number of six or seven, and have a nest in common, which is mentioned in the same page of the "Magazine of Natural History" as the Wrens' nests, I could prove by the testimony of twenty people who saw the nest and young there spoken of. I should be glad to learn whether the same thing has been noticed by other people.

Among the few rare birds which it has been my good fortune to procure is a Woodpecker, which I killed this summer, and which is not mentioned in your edition of Montagu, although spoken of by Bewick as a dubious species, under the name of the Middle Spotted Woodpecker.

A pair of these birds had built their nest, or rather hatched their young (for there was no nest), in a hole in a decayed ash tree about twenty feet from the ground. There were two young ones, which I secured, as well as one of the old ones, and they are all in the possession of a professional friend of mine, who is a collector of ornithological specimens.

The old one measures 9 1/2 inches long, and weighed 46 1/2 dwts. an hour after it was killed. The forehead is a dirty buff, the whole crown of the head a bright crimson; the irides a dark lead colour, and it has a white ring round its neck. In other respects it corresponds with your description of the *Picus major*. The sex was not ascertained. The young ones have also the bright crimson head, and differ very materially from the old one.

The Chevy Linnet, as the lesser Redpole is called, is found here throughout the year, and is at no time a scarce bird with us. It frequently builds its nest in the alder and willow bushes, on the banks of the brooks or rivers. It is a late breeder, the nests being often met with containing eggs or young in July. In the winter it feeds upon the seeds of the alder or the cones of the larch, hanging suspended from the twigs like the titmouse.

We have also the Gray Wagtail (*Motacilla sulphurea*) with us the whole year, but it is rather a rare bird at all times and in all localities with which I am acquainted. (1853:—It is more plentiful now than it was in 1831.)

I very strongly suspect Selby is mistaken when he says, "that previous to its departure in September, it assembles in small flocks or families, which haunt the meadows or bare pastures." This does not agree with my observations of this bird, although quite true when applied to the Spring Wagtail (*Motacilla flava*); on the contrary, the Grey Wagtail is solitary throughout the year, except in the breeding season, and never frequents the meadows, but is found in the beds of the rivers, brooks, or ditches, where its shrill note often betrays it to eyes which would otherwise never see it.

Page 79

This bird may be easily distinguished from the Spring Wagtail by its note when flying—yet, notwithstanding the difference is very apparent to a person who hears them both, it is not so easily described. In attempting to do so, therefore, I hope I shall be excused if I don't make the difference so apparent in the description as it is in reality. The latter part of the note of the Grey Wagtail when flying is higher in the musical scale than the former part, and is very staccato, thus: [BAR OF MUSIC] generally being uttered as the bird makes a spring in the air, [10] whilst the latter part of the note of the summer-bird is lower in the scale than the former part, which is more prolonged than in the note of the Grey Wagtail, and is slurred into the latter part, something in the following manner: [BAR OF MUSIC] Of course I don't mean it to be understood that these notes are either of the same pitch, or that they bear the same relation to each other that the notes of the bird do, but as a rude attempt at illustrating what I could not explain in any other way.

A singular habit which I have noticed in several individuals of this species (*M. sulphurea*) has amused me exceedingly. They were in the habit of looking at their own images in the windows and attacking them, uttering their peculiar cry, and pecking and fluttering against the glass as earnestly as if the object they saw was a real rival instead of an imaginary one (a friend who observed it, insisted that, Narcissus-like, it was in an ecstasy of self-admiration). What is more remarkable, two of these instances occurred in the autumn, when one would not suppose the same motives for animosity to exist that would probably actuate them in the spring.

The first of these instances was when I was a boy, and was repeated daily for several weeks, both against the windows of my father's house and those of our neighbour, who, being rather superstitious, was alarmed about it, and came to consult my mother on the subject. She said there was a bird which her brother told her was a barley-bird (*Motacilla flava*), which was continually flying against her windows, and as birds were not in the habit of doing so generally, she thought something serious was portended by it. My mother comforted her as well as she could, and I undertook to rid her of the annoyance, which I did by setting a horsehair-noose on one of the window-ledges which it frequented. I soon caught it, and by plucking out the under-tail coverts, with which I wanted to dress *yellow duns*, I effectually cured it of the propensity—whether, Narcissus-like, it was in an ecstasy of self-admiration, or like the cock which attacked its own image in the boot (which Mr. Robert Warren's poet and painter have immortalized), it would admit of no rival.

It has been suggested, and I think with great probability, that the bird was merely attempting to catch the flies which it saw on the inside of the panes of glass; but certainly it was not so silent about it as these birds generally are when they are feeding.

Page 80

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THE LONG-TAILED TITMOUSE.

To the Editor of "Loudon's Magazine."

Some years ago, when my brother and myself were seekers of birds' nests, we found one of the Long-tailed Titmouse (*Parus caudatus*), about two miles from home, containing young ones half-fledged. Being anxious to rear them, we hit upon the plan of catching the old ones, and giving them the trouble instead of ourselves. We accordingly set lime-twigs near the nest, and caught six old ones out of the seven of which the colony consisted, and brought them away in triumph; but the old ones would not eat in confinement, and all died but one, which we allowed to escape, in the hope that it would come back and rear the young ones. This it did, and by the most unwearied exertion reared the whole brood, sometimes feeding them ten times in a minute.

Never having seen this social habit stated in any ornithological work to which I have access, I am not aware that it is generally known to naturalists; but it is right to state that I have only found one nest of the species since, and this my avocations would not permit me to examine. I am therefore not aware whether the fact I have stated was an exception to the general habit of the bird, or whether such is invariably the case. Some of your correspondents will, no doubt, be able to give an answer to this inquiry.

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IDENTITY OF THE GREEN WITH THE WOOD-SANDPIPER.

To the Editor of the "Magazine of Natural History."

The question whether the Green and the Wood-Sandpiper are the same species seems from Rennie's edition of Montagu's "Ornithological Dictionary" to be undecided; but as a specimen has just come under my notice which appears to me to clear up this difficulty, I shall offer no apology for sending a description of it.

The length from the bill to the tail is 10 inches, to the end of the toes, 11 3/4 inches; breadth, 17 inches; thigh-joint to the toe, 5 1/2 inches. The bill measures 1 5/8 inches from the corner of the mouth, and is very slender; the upper mandible, which is black and slightly curved at the point, is a little longer than the lower one, which is a dark green at the base and black at the point; a dark streak extends from the base of the upper mandible to the corner of the eye, and above it is a patch of dirty white intermixed with minute dusky spots; a small circle of dirty white surrounds the eyes; the chin is white; the cheeks, throat, and forepart of the neck white, spotted with dusky, with which colour a few laminae of each feather are marked their whole length. The breast has a dappled stripe of the same colour as the throat running down the middle of it; with this

exception it is white, as are also the belly, vent, and under tail-coverts. The crown of the head and hinder part of the neck are a dingy brown, which on the neck has a shade of ash colour; the bend of the wing and lesser wing-coverts are a

Page 81

brownish black; the whole upper surface of the plumage is of a glossy brownish-green, which is spotted on the middle wing-coverts with minute white spots, that change to a dingy yellow on the back, scapulars, and tertials, the last of which have twelve spots on the outer margin of the feathers, and six on the inner one; the tertials are very long, the longest of them reaching to within a quarter of an inch of the extreme top of the wing, which reaches to the end of the tail; the quill feathers are wholly black, as are also the secondaries; the upper part of the rump is black, and each feather is slightly tipped with white, which forms small wavy lines on that part of the plumage; the lower part of the rump and upper tail-coverts are pure white; the tail, which is even at the end, consists of twelve feathers, which are barred with black and white alternately.

At the end of Bewick's description of the Green Sandpiper there is a very exact representation of a covert feather of the tail, and an inner-wing covert, which will give a better idea of their appearance than a page of letterpress. The legs are dark green, the outer toe connected with the middle one by a membrane as far as the first joint; toes very slender, middle one 1 1/4 inch long; weight, 2 3/4 oz. Killed on the 17th September, 1831, near Stonyhurst.

I have been thus minute in my description from a wish to clear up the doubt that appears to exist as to the identity of these two birds. The one I have now before me is, undoubtedly, the Green Sandpiper of Bewick, but it corresponds in so many particulars with the Wood Sandpiper of Montagu, and appears to combine so many of the peculiarities of both without exactly agreeing with either, that I think it proves their identity satisfactorily. The glossy green of the upper plumage and the barring of the under wing-coverts and the tail identify this bird with the Green Sandpiper; whilst on the other side the yellowish spots on the scapulars and tertials, the black rump, the length of the leg, and the web between the outer and middle toes are characteristic of the Wood Sandpiper of Montagu.

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THE STOAT.

I. M. (in the "Magazine of Natural History") says that the Stoat is more timid than the weasel, and that it does not change its colour as in the more northern parts of the world. I know not why he calls it timid, even relatively, as I think it is the most fearless wild animal we have in the kingdom, in proof of which I will mention an incident I witnessed myself. I one day saw a Stoat carrying off a large rat it had killed, and I immediately pursued it, but it stuck so tenaciously to its prey (although it was so encumbered with its load as to be scarcely able to run at all) that I was close upon it before it would abandon it; however, it then took refuge in a wall that happened to be

close by. I took up the rat, and the Stoat put its head out of the wall, spitting and chattering with every appearance

Page 82

of the most lively indignation against me for having so unjustly robbed it of a lawful prize. I amused myself with watching it for some time, and then being desirous of seeing how far its evident desire to recapture its booty would overcome its fear of me, I held the rat just before the hole in which it was, when after several attempts, in which its discretion got the better of its valour, it at length screwed up its courage to the sticking-place, came boldly out of the wall, and dragged it out of my hand into the hole.

I know not in what county I. M. lives, nor do I know whether he means to include any part of England in the more northern parts of the world, but I do know that the Stoat is white in the winter in Yorkshire, as I have caught and still more frequently seen specimens of this colour.

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THE MARSH TITMOUSE.

I have been much interested this spring at witnessing in two or three instances the tenacity with which the Marsh Titmouse sits on its nest. Being in a wood near my own house, I perceived a pair of these birds in one of the trees, and having seen them in the same place several times before, and being desirous of finding the nest, I sat down to watch their motions. After examining me on all sides with much chattering and many gesticulations, indicative of dislike and suspicion, the female flew to the root of a tree, three or four yards off, and disappeared, as she had gone to the opposite side of the tree to that on which I sat; and as there were several holes about the root I was at a loss to know in which the nest was built, and began to strike the root with a stick, expecting her to fly out, but nothing appeared. I then examined the holes one by one, and whilst doing so heard her hissing and puffing from within, in such a way that if I had not known she was there I should have thought it was a snake rather than a bird. However, as she would not come out, and the hole was so small that I could not get my hand in, I was obliged to raise the siege until next morning, when I returned armed with a hammer and chisel with which to storm her citadel. As the wood was sound, the hole small, and the nest six or eight inches within the tree, I was five or ten minutes before I could get to it, during which I gave her repeated opportunities of escaping if she chose; but she still sat on her nest, puffing and pecking at the stick that I thrust in in order to drive her off. She at last crept to the further edge of the nest, which I then took out, as I wanted it for one of my friends who is a collector of eggs, but on attempting to blow one I found they had been sat upon too long, and I then felt desirous of seeing whether the old bird would hatch them after having her nest torn from under her, and I turned back to the tree whence I had taken them, and found her still sitting in the hole where I had left her. I regret to add that the humane part of my experiment did not succeed, for although she remained after I had returned the nest to its place, she left it immediately after, and did not return to it again.

Page 83

Another instance which I witnessed was in a nest containing young ones. This was also at the root of a tree, but the situation did not appear to be so well chosen as is usually the case with the Titmouse tribe; for in this instance the hole went quite through the tree, and on one side was large enough to admit the hand. As the young ones were exposed to the weather, and were also liable to be seen by anyone going along the adjoining footpath, I attempted to remedy this defect by covering the larger hole with a sod, which to a casual observer would appear to have grown there. On taking the sod off one day, to see how the nestlings were going on, I perceived that a clod of earth had fallen from the sod upon them, and I took a stick and hooked it out, lest it should smother them. Whilst I was doing this I perceived the old one sat on the further side of the nest, so still and quiet that until I perceived her eye I fancied she was dead; and she also endured several pokings with the stick before she would move, although the hole on the opposite side of the tree enabled her to escape whenever she thought proper.

Perhaps Mr. Rennie, in his next edition of Montagu's Dictionary, will give us a new name for this bird, as the one it has at present is no more applicable to this species than it is to the *Parus caeruleus*, or the *Parus major*, and not half so much so as it would be to the *Parus biarnicus*; and he has changed good names into bad ones with far less reason, witness *Corvus frugilegus* into *Corvus predatorius*. The former name is strictly applicable to that species, and to that alone; and so useful a bird does not deserve the name of a thief. The Chaffinch (which received its name of *Coelebs* from Linnaeus on account of the males alone remaining in Sweden in the winter, which fact is corroborated by White, who found scarcely any but females in Hampshire during that season) has had its name changed by Mr. Rennie into *Spiza*. The old name is characteristic of a remarkable fact in the habits of this bird; why the new one is more appropriate (neither understanding Greek, nor having read Aristotle), I cannot say. Will Mr. Rennie condescend to enlighten me?

Once for all—if we are to have a new nomenclature, let a committee of able naturalists decide upon it, or let us submit to the authority of a master (for instance Linnaeus or Temminck), but don't let every bookmaker who publishes a work on Natural History, rejecting names long established and universally received, give new ones in such a way as serves only to show his own presumption and to confuse what it ought to be his business to elucidate.

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CREEPER.

The Nuthatch does not occur in this, and I doubt if in any part of Lancashire, but the Creeper is very common, and is a bird with the habits and peculiar call of which I have been acquainted from my childhood. Mr. Bree, who combines with accurate and extensive information, an amiable and pleasant manner of communicating it, has not, I perceive, witnessed the Creepers associating with the Titmice in winter, at which I am

rather surprised, and think if they are numerous in his neighbourhood, he will hereafter not fail to perceive them among the small flocks of Titmice which associate through the winter.

Page 84

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WRENS' NESTS.

In Mr. Rennie's edition of Montagu's Dictionary, and also in his "Architecture of Birds," after copying what I have said on the subject of Wrens' nests being lined with feathers, he says:—"There can be no doubt, I apprehend, of these supposed cock-nests being nothing more than the unfinished structures of paired birds; otherwise the story would require the support of strong evidence to render it credible." Mr. Rennie afterwards goes on to say that in two instances he had seen nests which had about half-a-dozen feathers interwoven into the linings with hair; and Mr. Jennings, if I recollect aright, as I have not the work to refer to at present, says that Wrens don't line their nests with anything but moss, and he thinks Montagu is in error when he says they are lined with feathers. Along with this I send you three or four Wrens' nests, which you will perceive have abundance of feathers in the inside; and although the Wren will occasionally use cows' hair along with the feathers, yet I am persuaded from the localities in which I have met with them, that cows' hair has been used because feathers were not to be found; but when the nests are in the vicinity of a rookery, a farm-yard, or any other locality where feathers are abundant, the Wrens will use them exclusively. What the "strong evidence" must be which will convince Mr. Rennie about cock-nests, I don't know; but I know of a dozen of these nests at the present moment, several of which have remained in the state in which they were left in the middle of April. Other nests found about the same time have now young ones in them. I doubt not these nests are occasionally used for breeding in: for instance, if the first nest of a Wren be taken, or if it breed a second time, it will occasionally take possession of a cock-nest; as I have sometimes found that after remaining in the same unfinished state for several weeks, they have afterwards been fitted up with a lining, and bred in.

Mr. Rennie asserts that Montagu is wrong when he says that the Wren always adapts its materials to its locality. Although it certainly is not always the case, yet so very generally is it so, that I think it is not surprising that Montagu made this assertion.

Thus, if a Wren build in a haystack, the front of the nest is generally composed of the hay from the stack; if it be built in a bush by the side of a river, and (which is frequently the case) below flood mark, it is generally covered on the outside with the rubbish which has been left there by the flood; and if it build in a mossy stump, the front of the nest is composed of the dark-coloured moss which grows there. (May 22, 1832.)

Page 85

Along with my last letter, I sent some Wrens' nests lined with feathers, and I could easily have increased them to a dozen of the same sort, only I did not wish to deprive so many of my little favourites of their eggs and young. Every day convinces me more decidedly, that I am right both with regard to the lining of the Wrens' nests, and as to the cock-nests also. The nests I sent you will prove the former, and I know of at least twenty instances of the latter, in nests which I have known of all through the spring, from April to the present time, which have remained in the same unfinished state, although they are not forsaken, as I have found the birds in them, in several instances, when I have examined them. I found one of these nests on the 10th of April, under a bank on the side of the river; and I examined it repeatedly through April and May, and always found it in the same state, although there was always a pair of Wrens about, and I could find no other nest; yet I am sure there was another, for in the beginning of this month (June) there were some young Wrens, which had evidently only just come out of the nest; and there were only two or three bushes grew thereabouts, so that it is not probable they had come from any other quarter, but the bushes were filled with dead leaves, and other rubbish brought down by the flood. However, when I heard them, I looked out for another nest, as I believe (notwithstanding what Montagu says, that there are few birds, if any, that would produce a second lot of eggs if the first were unmolested) that most of the small birds which are early breeders build a second time the same year, even when they succeed in rearing the first brood. I have had proof of this (if anything can be considered proof, except marking the birds), in the Thristle, the Blackbird, the Wren, the Redbreast, and the Hedge Sparrow, whose second nests may be found contiguous to the first; and in point of time, this always happens just when the first brood have left the nest. The cock-bird, too, who had been silent whilst his young were unfledged, begins to sing again, and throwing off the anxious and care-beset manners of a parent, again assumes that of a bridegroom. But to return to Wrens' nests. I found one within ten yards of the one I had known of since the 10th of April, lined, and ready for an egg. As I was anxious to prove what I had so long believed, I pulled out this nest, thinking that the old bird was ready for laying a second lot of eggs; and that when I had destroyed this, as she had no other nest ready, she would probably take up with the cock-nest.

As it was half a mile from my house, I did not visit it again until the 16th of June, and was then delighted to find the old bird sitting on six or seven eggs in the cock-nest, which had remained so long unoccupied. I believe that in this instance there is very little lining (fur, feathers, &c.) in the nest, although I should be sorry to examine it minutely until the young have left it; but I consider it an exception to the general rule, inasmuch as I believe the bird was ready to lay when I pulled out the other nest. As she would have to find another with as little delay as possible, she would not have time to embellish the inside in the same manner as she probably would have done if she had had more time.

Page 86

On examining another Wrens' nest a few evenings ago, I found the young ones had flown, and as there was a cock-nest in some wrack left by the river in a bush a few yards off, I gave it a shake to see if the old ones had taken possession of it for another brood; and I was surprised to see one, and then a second young one come flying out, and a third putting out its head to reconnoitre. Whether the whole brood was there I don't know, as I did not disturb them further. As I had examined this nest only ten days before, when it had not an egg in it, I was at first at a loss to account for these young ones; but I have now no doubt they were the young from the adjoining nest, which had taken up their quarters for the night in the new house. But how had they learnt the way? Young birds generally roost where night finds them, and if I had found only one, I should not have been surprised, but to find at least three, probably six or seven, in a nest where I am certain they were not bred, was something new to me. I went several times in the evening after this, but never found them; I suppose the fright I gave them deterred them from lodging there again.

The editor of "Loudon's Magazine," in a paragraph appended to this article, says: "We have examined the Wrens' nests sent; their staple materials are moss, feathers, and hair. Into the moss on the exterior of the nest are woven a more or less perfect but feeble frond or two, and separate pinnae as well of *Aspidium Filix-Mas*, and leaves of apple, elm, and oak trees. Interiorly cows' hair is not scarce, and is partly inwoven with the moss and laces it together, and partly mingled with the feathers; a horse-hair or two are also observable. The feathers in each nest, apparently those of domestic fowls, are numerous enough to fill the hollow of the hand when the fingers are so folded over as not to much compress the feathers."

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ALARM-NOTE OF ONE BIRD UNDERSTOOD BY OTHER SPECIES OF BIRDS.

In Montagu's "Ornithological Dictionary," under the article "Song of Birds," there is the following remark: "Regarding the note of alarm which birds utter on the approach of their natural enemies, whether a Hawk, an Owl, or a Cat, we consider it to be a general language perfectly understood by all small birds, though each species has a note peculiar to itself." I was last April very much pleased at witnessing an illustration of the truth of this opinion. I found a nest of young Thrustles at the root of a hazel, and although they could scarcely fly, yet as they were near a footpath, and the next day was Sunday, when many idle and mischievous lads would be rambling about, I thought they would be safer out of their nest than in it; and as I knew that when so far fledged, if they were once disturbed they would not continue in the nest, I took one from the nest and made it cry out, and then put it back again; but in one minute, not only it but its three companions had disappeared in the long dry grass which

Page 87

was round about. On hearing the cry of their young one, the parent bird set up such shrieks of alarm as brought all the birds in the wood to see what was the matter. I noticed the Blackbird, the Chaffinch, the Titlark, the Robin, the Oxeye (greater Titmouse), the Blue and Marsh Titmouse, and the Wren all uttering their cries of alarm and apprehension; even the golden-crested Wren, which usually seems to care for nothing, was as forward and persevering as any of them in expressing its fears on this occasion; indeed, the only bird which seemed indifferent to all these manifestations of alarm was the Creeper, which continued its anxious and incessant search for food, as it flitted from one tree to another, examining them from root to branch without ever seeming to understand or to care for what seemed to have so much frightened the others. (June 30th, 1832.)

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DATES OF THE APPEARANCE OF SOME SPRING BIRDS IN 1832, AT CLITHEROE.

Young Rooks heard, 5th April; House Martin seen, 14th; Sandpiper, 14th; Willow Wren, Spring Wagtail, and Redstart, 17th; Wheatear, 19th (this is generally the first spring bird seen); Sand Martin and Swallow, 22nd; Cuckoo heard, 26th; Wood Wren, Blackcap, and Whinchat, 28th; Mocking-bird and Whitethroat, 4th May; Swift, 7th; Flycatcher, 11th; and Fieldfares were not seen until the 2nd of May, which is later than I ever observed them before. (In the parish of Allesby, near Coventry, Fieldfares were observed as late as the 14th of May.)

No doubt many of these birds were in the neighbourhood earlier than the dates I have attached to them, but they are the periods at which I saw or heard them.

The study of Natural History is perhaps as little followed in this neighbourhood as in any part of the kingdom, notwithstanding the facilities which are offered. Our flora is beautiful, varied, and possesses many rare plants, yet I only know of two herbaria; the birds are abundant, yet there is but one collector of them; and as for insects, although I frequently take what I consider rare species, yet I cannot find an entomologist in the whole district, or I would send them to him.

In conclusion, allow me to say, that the leisure hours a somewhat busy life has enabled me to spend in these pursuits, have been some of the happiest of my existence, and have awakened and cherished such an admiration of nature and such a love for the country and its scenes, as I think can never be appreciated by the inhabitants of large towns, and which I cannot describe so well as in the words of one of my friends in a beautiful apostrophe to England, when leaving it—never to return: [11]—



“To thee Whose fields first fed my childish fantasy, Whose mountains were my boyhood’s wild delight, Whose rocks, and woods, and torrents were to me The food of my soul’s youthful appetite; Were music to my ear—a blessing to my sight.”

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THE ROOK SERVICEABLE TO MAN.—PREJUDICE AGAINST IT.

Page 88

A strong prejudice is felt by many persons against Rooks, on account of their destroying grain and potatoes, and so far is this prejudice carried, that I know persons who offer a reward for every Rook that is killed on their land; yet so mistaken do I deem them as to consider that no living creature is so serviceable to the farmer as the Rook, except his own live stock.

In the neighbourhood of my native place is a rookery belonging to William Vavasour Esq., of Weston in Wharfedale, in which it is estimated there are 10,000 Rooks, that 1 lb. of food a week is a very moderate allowance for each bird, and that nine-tenths of such food consists of worms, insects, and their larvae: for although they do considerable damage to the crops for a few weeks in seed-time and harvest, particularly in backward seasons, yet a very large proportion of their food, even at these times, consists of insects and worms, which (if we except a few acorns, walnuts, and potatoes in autumn) at all other times form the whole of their subsistence.

Here, then, if my data be correct, there is the enormous quantity of 468,000 lbs., or 209 tons of worms, insects, and their larvae destroyed by the birds of a single rookery, and to everyone who knows how very destructive to vegetation are the larvae of the tribes of insects (as well as worms) fed upon by Rooks, some slight idea may be formed of the devastation which Rooks are the means of preventing. I have understood that in Suffolk and in some of the southern counties, the larvae of the cockchafer are so exceedingly abundant that the crops of corn are almost destroyed by them, and that their ravages do not cease even when they have become perfect insects. Various plans have been proposed to put a stop to their ravages, but I have little doubt that their abundance is to be attributed to the scarcity of Rooks, as I have somewhere seen an account that these birds are not numerous in those counties (I have never been there), either from the trees being felled in which they nested, or from their having been destroyed by the prejudiced farmer. I am the more inclined to be of this opinion, because we have many Rooks in this neighbourhood where the cockchafer is not known as a destructive insect, and I know that insects of that class and their larvae are the most favourite food of the Rook, which may be seen in the twilight catching both cockchafers and the large blackbeetles which are flying at that time in the evening.

I will mention another instance of the utility of the Rook which occurred in this neighbourhood. Many years ago a flight of locusts visited Craven, and they were so numerous as to create considerable alarm among the farmers of the district. They were, however, soon relieved from their anxiety, for the Rooks flocked in from all quarters by thousands and tens of thousands, and devoured the locusts so greedily that they were all destroyed in a short time. Such, at least, is the account given, and I have heard it repeatedly mentioned as the reason why the late Lord Ribblesdale was so partial to Rooks. But I have no means of ascertaining how far this is true.

Page 89

It was stated in the newspapers a year or two back that there was such an enormous quantity of caterpillars upon Skiddaw, that they devoured all the vegetation on the mountain, and people were apprehensive they would attack the crops in the enclosed lands; but the Rooks (which are fond of high ground in the summer) having discovered them, put a stop to their ravages in a very short time. (June 30th, 1832.)

These remarks are confirmed by a writer in the "Essex Herald" and by Mr. Waterton. The former says:—"An extensive experiment appears to have been made in some of the agricultural districts on the Continent, the result of which has been the opinion that farmers do wrong in destroying Rooks, Jays, Sparrows, and, indeed, birds in general on their farms, particularly where there are orchards."

That birds do mischief occasionally among ripe corn there can be no doubt; but the harm they do in autumn is amply compensated by the good they do in spring by the havoc they make among the insect tribes. The quantity of grubs destroyed by Rooks and of caterpillars and grubs by the various small birds, must be annually immense. Other tribes of birds which feed on the wing—as Swifts, Swallows, and Martins—destroy millions of winged insects which would otherwise infest the air and become insupportably troublesome. Even the Titmouse and the Bullfinch, usually supposed to be so mischievous in gardens, have actually been proved only to destroy those buds which contain a destructive insect. Ornithologists have of late determined these facts to be true, and parish officers would do well to consider them before they waste the public money in paying rewards to idle boys and girls for the heads of dead birds, which only encourages children and other idle persons in the mischievous employment of fowling instead of minding their work or their schooling. But to return to the experiment alluded to. On some very large farms in Devonshire the proprietors determined a few summers ago to try the result of offering a great reward for the heads of Rooks, but the issue proved destructive to the farms, for nearly the whole of the crops failed for three succeeding years, and they have since been forced to import Rooks and other birds wherewith to re-stock their farms.

Of late years the extensive destruction of the foliage and young fruit in orchards by a species of caterpillar has excited the attention of the naturalist, and it has been found to have arisen from the habit of destroying those small birds about orchards which if left unmolested would have destroyed or kept down those rapacious insects.

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SANDPIPERS.

Page 90

Sandpipers breed about Clitheroe. I this year (1832) started an old one from her nest at the root of a Weymouth pine. She screamed out, and rolled about in such a manner, and seemed so completely disabled, that, although perfectly aware that her intention was to allure me from her nest, I could not resist my inclination to pursue her, and in consequence I had great difficulty in finding the nest again. It was built of a few dried leaves of the Weymouth pine, and contained three young ones just hatched, and an egg through which the bill of a young one was making its way. Yet, young as they were, on my taking out the egg to examine it, the little things, which could not have been out of the shell more than an hour or two, set off out of the nest with as much celerity as if they had been running about a fortnight. As I thought the old one would abandon the egg if the young ones left the nest, I caught them again and covering them up with my hand for some time, they settled down again. Next day all four had disappeared.

Montagu says: "It is probable many of the Sandpipers are capable of swimming if by accident they waded out of their depth. Having shot and winged one of this species as it was flying across a piece of water, it fell, and floated towards the side, and as we reached to take it up, the bird instantly dived, and we never saw it rise again to the surface; possibly it got entangled in the weeds and was drowned." I quote this remark because the same thing has happened to myself. I winged a Sandpiper, and on going to take it up, it fluttered into the water and dived, but never rose again to the surface that I could perceive, although I watched long and attentively for it. In this instance the bird could not have been entangled by the weeds, inasmuch as the bottom of the river was covered with gravel and not a weed was growing there. Whether the Sandpiper laid hold of the gravel at the bottom with its feet, or how it managed, I cannot tell, nor have I ever been able to account for it. (June 30th, 1832.)

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ON BIRDS DRESSING THEIR FEATHERS WITH OIL FROM A GLAND.

Mr. Waterton doubts ("Mag. of Nat. History," vol. v. p. 413) if the small nipple on the rump of birds is an oil-gland, or that birds ever oil their feathers with matter obtained from it; and he asks if any naturalist will say that he has ever witnessed this process, and if so how it is that the bird contrives to take this oil in its bill and how it manages to oil its head and neck? I will therefore state what I think I have witnessed, and trust to Mr. Waterton's forbearance if I am in error; yet I cannot help suspecting that Mr. Waterton's queries are (like those of Charles the Second to the Royal Society) more for the purpose of laughing at our ignorance than from any wish he has to obtain information, for I can scarcely suppose that so acute an observer can have failed to perceive everything perceptible on the point at issue.

Page 91

I have just watched a Muscovy Duck go through the operation of preening and dressing its feathers, and it certainly appears obvious enough to me that this bird uses the gland on the rump for the purpose for which birds are generally supposed to use it. The bird erected the feathers on the rump so as to exhibit the gland very distinctly, and then, after pressing it with the bill, rubbed the under mandible and chin down to the throat upon it, and then, after drawing some of the feathers through the bill, rubbed the lower mandible and chin upon the back and scapulars, apparently to apply the oil which adhered to them, and then, turning its head back, it rubbed the crown and sides of the head and neck upon those parts which it had previously rubbed with the chin and under mandible. By this rubbing of the head and neck it is easy to perceive how birds can oil these parts if it be allowed that birds oil themselves at all.

I cannot see how we can explain this action of birds in relation to any other object. It certainly does not seem calculated to expel or disturb any vermin lodged there, and I remarked that it never occurred except when the bird had been applying its bill to the gland as above mentioned. However, Mr. Waterton, and anyone who doubts this oiling, may readily judge for themselves. Let them take a common duck, and shut it up for two or three days, so that it can have no access to water except for drinking, and at the end of that time let them turn it out, and allow it to go to a brook or pond; it will give itself a thorough ablution—ducking, diving, and splashing with its wings—and on coming out, will begin to dress and arrange its feathers, very frequently applying its bill to the gland on its rump. If this application is not for the purpose of procuring a supply of oil, perhaps Mr. Waterton will have the goodness to inform us what it is for, and what end this gland answers in the economy of the feathered tribes if not that which has hitherto been supposed. (June 30, 1832.)

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MOCKING POWERS OF THE SEDGE-WARBLER.

In the article "Sedge Bird," in Montagu's "Dictionary of Ornithology" (Rennie's edition, p. 455), the writer says: "It has a variety of notes, which partake of those of the Skylark and the Swallow, as well as the chatter of the House-Sparrow." According to my observation, it has a much greater variety than this. I have heard it imitate in succession (intermixed with its own note, *chur, chur*), the Swallow, the House-Martin, the Greenfinch, the Chaffinch, the Lesser-Redpole, the House-Sparrow, the Redstart, the Willow-Wren, the Whinchat, the Pied-Wagtail, and the Spring-Wagtail; yet its imitations are chiefly confined to the notes of alarm (the fretting-notes as they are called here) of those birds, and so exactly does it imitate them in tone and modulation, that if it were to confine itself to one (no matter which), and not interlard the wailings of the little Redpole

Page 92

and the shrieks of the Martin with the *curses* of the House-Sparrow, the *twink, twink* of the Chaffinch, and its own *care-for-naught* chatters, the most practiced ear would not detect the difference. After being silent for awhile, it often begins with the *chue, chue* of the House-Sparrow, so exactly imitated in every respect that were it not for what follows, no one would suppose it to be any other bird. It is called a Mocking-Bird here, and it well deserves the name, for it is a real scoffer at the sorrows of other birds, which it laughs to scorn and turns into ridicule by parodying them so exactly. I never heard it attempt to imitate any of the Larks or Thrushes, although I have listened to it for hours.

This bird was very plentifully met with in Wharfedale ten years ago, and is also found in this neighbourhood, but I am not aware that anybody in either of these districts ever attempted to keep one in confinement, although from their powers of imitation, I think the experiment well worth trying; probably the idea that it would be difficult to supply them with proper food has prevented the experiment being made. (May 2nd, 1832.)

I am surprised that no other writer on Natural History has noticed the wonderful imitative power of this bird. So far is the above notice from overstating this bird's powers of imitation, that I have scarcely enumerated half the notes which it hits off with such wonderful exactness.

In listening to one the other day for about a quarter of an hour, I heard it give three notes of the Swallow, two of the Martin, and two of the Spring-Wagtail; and in addition, notes of the House-Sparrow, Whinchat, Starling, Chaffinch, Whitethroat, Greenfinch, Little Redpole, and Whin-Linnet, besides the notes of half-a-dozen birds which I did not know; at least, a reasoning from analogy would induce me to think them imitations, and I have no right to suppose they were not because I did not happen to recognize them. I am not strictly correct when I say that it only imitates the alarm-notes (called here fretting-notes) of other birds, for although this is generally the case, it is not invariably so. For instance, in addition to the alarm-note of the Swallow, *chizzic, chizzic*, it also had the *whit, whit*, which the Swallow uses when flying about, and the chatter of self-satisfaction (not the song) which one often hears in a barn when two Swallows are arranging their plan of operations in the spring. Again, in addition to the shriek of the Martin, there was the note which it utters when on the wing in pursuit of its food. There was also the chirrup of the Greenfinch, and the *whee, whee, whee* which is the climax of the Linnet's song, by which it is so irresistible as a call-bird, and which appears to bring down the flock in spite of themselves.

Page 93

Although the Sedge-Bird imitated all I have mentioned, it made much more frequent use of the notes of some than of others—the Sparrow, the Whinchat, the Swallow, and the Starling appeared to be its chief favourites, whilst it only touched once or twice on the notes of the Greenfinch and the Linnet. It had been very sparing also in its use of the Chaffinch's note, until one in the neighbourhood had begun to *twink, twink, twink*; then the Mocking-Bird took it up, and *twinked* away for fifty times together. Next morning the Linnet's note was much more frequent in request, and it also made more use of notes with which I was not acquainted. On neither day did it touch upon the notes of the Redstart, or Pied-Wagtail, both of which I had heard frequently used by the Mocking-Bird before. On the other hand, I had not previously observed the notes of the Starling and Whin-Linnet, and therefore, although I have said that I have never heard it make use of the notes of any of the Larks or the Thrushes, I would not be understood to say that this never happens. It is, perhaps, difficult to say whether it has a note which is not an imitation of some other bird, but there is one which it always makes use of when any person approaches its nest (intermixed, however, with the notes of the Swallow, Whinchat, and Whitethroat). This is something like *chur-r-r, chur-r-r*, prolonging the sound of the *r* very considerably, and in a style which would be quite an acquisition to the Northumbrians if they could attain it. (May 29th, 1834.)

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THE WATER OUZEL.

The Water Ouzel sings very frequently, and as much in winter as at any time. Perched on a stone or a piece of ice, it chirps away at a famous rate, but its song consists almost entirely of its note *zeet, zeet*, which it hashes up in all sorts of ways, lengthening and shortening—now a crotchet, then a semiquaver, rising an octave or so, and then descending again. It makes as much of it as can be made, but with all its efforts its song is a very so-so affair, all its syllables beginning with *z*, and almost ending with it too. Yet, although it is not much of a songster, it is almost a sacred bird with me, in consequence of the associations connected with it. A pair had built for forty years, according to tradition, in a wheel-race near to where I was born, and had never been molested by anybody until a gentleman in the neighbourhood, who was a great ornithologist, employed his gamekeeper to shoot this pair. I think the natives of Calcutta were not more indignant when an unlucky Englishman got one of their sacred bulls into his compound and baited him, than was our little community at what we considered so great an outrage. The gamekeeper narrowly escaped being stoned by myself and some more lads, any one of whom would have shot fifty Blackbirds or Fieldfares without any misgivings.

This bird very much resembles the Wren in its habits and motions, its nods and curtsies, and cocks its tail in exactly the same manner. Its nest is also similar in form to that of the Wren.

Page 94

Some persons seem to think that it is impossible for the Water Ouzel to walk at the bottom of the water, owing to its body being of less specific gravity. I will not argue the point with them, but disbelieving my own eyes, I will endeavour to submit with a good grace; otherwise I should have said that I have repeatedly seen it doing so, from a situation where I had an excellent opportunity of observing it, the window of a building directly over the place where it was feeding. It walked into the water and began to turn over the pebbles with its bill, rooting almost like a pig, and it seemed to have no difficulty whatever in keeping at the bottom at all depths where I could see it; and I have frequently observed it when the water just covered it, and its head appeared above the water every time it lifted it up, which it did incessantly, turning over a pebble or two, then lifting its head, and again dipping it below to seize the creepers (*aquatic larvae*) it had disturbed from their hiding-places. Besides, its speed was too slow for diving. Every aquatic bird with which I am acquainted moves much faster when diving than when it is swimming or walking, and its course is generally in a straight line, or nearly so; but the Water Ouzel, when feeding, turns to the right or left, or back again to where it started, stops and goes on again, just as it does when out of the water. Yet when it wished, it seemed to have the power of altering its own gravity, as after wading about two, or perhaps five minutes, where it could just get its head out, it would suddenly rise to the surface and begin to swim, which it does quite as well as the Water-hen. The awkward, tumbling, shuffling wriggle which it appears to have, is occasioned by the incessant motion of its head as it turns over the gravel in search of creepers, which appear to me to form the bulk of its food.

Sir George Mackenzie seems to think that these birds destroy salmon spawn, and this opinion is prevalent in Scotland. If it is correct, it would go far towards putting an end to my partiality for them; but I rather think that they are unjustly accused, and believe they are catching creepers when they are supposed to be eating spawn. If this is the fact (and it is well worth ascertaining) they are rendering an essential service to the fisheries instead of injuring them, because these creepers (the larvae of the stone-fly, bank-fly, and all the drakes) are exceedingly destructive to spawning-beds, and as the Water Ouzel feeds on them at all other times, and as they are more abundant in the winter than at any other season, I think this is the more probable supposition. Of course, if Sir George Mackenzie has shot the bird, and speaks from his own knowledge, after dissecting it, there can be no doubt of the fact that it destroys spawn; but if he merely supposes so because the Water Ouzel feeds in the same streams where the salmon are spawning, it is very probable he is mistaken, for the reasons before mentioned. (May 29th, 1834).

Page 95

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SCOLOPAX, SABINES, SABINE'S-SNIPE.

Some years ago I killed what I am now persuaded was a Sabine's-Snipe, but unfortunately it was not preserved, for hanging it up in the larder with the other birds I had killed, I found to my great mortification that the cook had stripped it of every feather before I was aware, and before I had noted down the markings of the plumage.

The dry weather of August, 1820, had driven a flock of the Golden Plover from the moors to the banks of the river Wharfe, and on the 14th of that month I had been out with my gun, endeavouring to shoot some of them. On my return I sprung this Snipe from a pond near home, and killed it. When I picked it up, I was astonished to find a Snipe with the plumage of a Woodcock, and showed it to a friend of mine, who is a good practical ornithologist, but he was as much puzzled as myself to give it a name; so not being able to find a description of it in any books to which we had access, we jumped to the conclusion that it was a hybrid between the Snipe and the Woodcock, and called it a bastard Woodcock.

According to the recollection I have of it, it was as large as the solitary Snipe, and the bill was a little longer; the general appearance of the plumage on the wings and back resembled a dark-coloured Woodcock; but under the wings the fine blue inner coverts exactly resembled those of the Snipe. In those days I had no idea of the value attached to rare birds, nor did I know anything of the art of preserving birds, or of bird-preservers, and no doubt some of these gentlemen will pronounce me a great Goth when I tell them that what I regretted most, when I found that the bird was plucked, was the loss of the wings, the feathers of which I wanted to dress artificial flies with. Three days after I had killed this, I saw another in a ditch adjoining Sir Henry Ibbetson's park, at Denton, but being in his preserve I had no opportunity of procuring it. I had never seen one since, and until I had seen the sixth edition of Bewick's "Birds," I was unable to make out its name, about which I may still be mistaken. (May 29th, 1834.)

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FISH AND OTHER RIVER PHENOMENA.

A writer in the "British Naturalist" says, that "fish don't feed, and therefore we may conclude they don't discern in sunny weather." If the author had ever been a May-fly-fisher he would have known that bright weather and clear water were essentially necessary to his success.

This fly is one of the best baits I know for large Trout, and is much used by the anglers in some of the rivers in Yorkshire (perhaps in other counties also), where two methods of fishing it are practised. The one is bobbing, which with one sort of bait or another is

universal, and therefore needs no description. However, it is always practised in bright weather. In the other method (which I believe is peculiar to the North of England) the May-fly (stone-fly) is fished

Page 96

with a long line in rapid streams, in the same way as the artificial fly, except that it is fished up the stream; that is, the angler throws his line into the stream above where he stands, and allows it to float down opposite to him, when he makes another throw; by this means he always keeps his line slack, and the May-fly floats on the surface, which is essential to his success. I mention these two methods of angling because both are practised in bright weather, and therefore prove that fish both discern and feed in such days. I believe the fact is, that at such times they frequently see too well for the angler, and are consequently aware that his artificial flies are not what they seem to be. Fishes, particularly Par and Grayling, may be seen rising by dozens at the small flies (midges) which abound in sunny weather, yet the angler is unable to hook a single fish. First-rate anglers are well aware of this, and abandon their larger flies as the summer advances, use smaller hooks, dress their flies much finer, and substitute horsehair for the fishing-gut, when they can procure it of good quality.

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LAMPREYS.

Lampreys abound in the Ribble. Some of them, of the large species (*Petromyzon marinus*), weigh three and four pounds each, [12] but owing to a prejudice against them (I suppose on account of their ugliness) they are seldom eaten. I will illustrate this prejudice by giving the remark of a keen fisherman to myself, on my saying that I should eat a large one I had just caught. "Well," said he, "if you can manage to eat such a thing as that, you would not stick at devouring a child in the small-pox." This, if not an elegant, was at least a forcible expression of his opinion on the subject, and this dislike of them is almost universal in this neighbourhood. (Jan. 17th 1832.)

"An Old Angler," in the "Magazine of Natural History," having questioned the assertion of Sir Everard Home that the Lamprey was hermaphrodite—in fact, that all were spawners and emitted eggs—the following was addressed to the "Magazine of Natural History":

When I had the pleasure of writing to you before, I had either overlooked or forgotten the queries of "An Old Angler" respecting the Lamprey. However, your remarks have induced me to pay a little more attention to the subject. I can now confirm in the strongest and most conclusive manner the supposition of "An Old Angler" that the sexes are as distinct in the Lamprey as they are in the Cod or Herring. How so distinguished an anatomist as Sir Everard Home fell into such a mistake, it is not for me to say; but I am as certain that the sexes are distinct in the Lamprey as that they are so in any other animal, and I will now give my reasons for making this positive assertion.

Page 97

On the 8th of May, chancing to cross a small stream, I saw a number of Lampreys in the act of spawning, and remembering the queries of your correspondent, I stood to watch their motions. After observing them for some time, I observed one twist its tail round another in such a manner, and they both stirred up the sand and small gravel from the bottom in such a way, as convinced me it was a conjunction of the sexes. However, there were so many of them together, and they kept so continually moving about, that I could not single out the two individuals, and thus ascertain whether they were male and female; but I felt so desirous of being able to set this question at rest, that I went again next morning, and was fortunate enough to find only two, a male and a female. I then witnessed several sexual conjunctions, during which the sand and small gravel was stirred up by them, and each of which was followed by the ejection of a jet of eggs from the female. I then caught them both, and dissected them. The sexual organ in the male was projected above a quarter of an inch, and the body filled with milt; the female, although she seemed to have shed a considerable quantity of her spawn, had still a tolerable stock remaining.

I frequently afterwards witnessed the same thing, and always found the same difference of sexes; in fact, there was generally no difficulty in distinguishing the male from the female, without taking them out of the water: the latter might be readily known by the enlargement of her body, and the former by a still more incontestable token. I have been induced to describe this more minutely than I otherwise should have done, in consequence of the mystery in which the propagation of fish has been wrapped hitherto; and I am not aware that what I have described has been witnessed by anyone before—at least I don't know that it has been recorded.

I caught half-a-dozen Lampreys, four males and two females, and preserved them in spirits, and these I now forward to you.

I am unable to give the same information concerning the large Lamprey, having never seen it in the act of spawning; but I have repeatedly caught both milters and spawners of species with the milt and roe as distinctly visible in them as it is in the Salmon or any other fish.

I am of opinion that the *P. marinus* and the *P. fluviatilis* are distinct species, for the following reasons:—1st. Because the latter stays with us the whole year, while the former only ascends the rivers to spawn, and then returns to the sea immediately. 2nd. Because fish that are in the habit of descending to the sea, never (unless the small Lamprey be an exception to the general rule) arrive at maturity [13] until they have visited it; and, 3rdly, because there are no intermediate sizes (at least in the Ribble) between the one which, although only six or seven inches long, and an ounce in weight, is yet capable of propagation, and the one of a pound. Not having one of the larger kind to refer to, I am unable to point out any specific difference of form. (May 2nd, 1832.)

Page 98

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ON THE SPAWNING OF THE MINNOW.

As I had been so fortunate in observing the Lampreys, I felt desirous of ascertaining whether the same thing could be seen in other fish (as in Natural History it is not always safe to reason from analogy), and as there was a large shoal of Minnows spawning near the place where I had seen the Lampreys, I determined to watch their motions. They happened to have chosen a very convenient place for being observed, being between two large stones in the river, which lay about three feet from each other; so that by cautiously approaching them from behind one of the stones, I got close to without disturbing them, but after watching them carefully and repeatedly within the distance of two feet, I can only speak doubtfully of their operations, for they were so numerous, and their motions were so incessant; and when a female was about to shed her spawn, the males (which were ten times more numerous than the females) crowded round her in such a manner as to render it very difficult, if not impossible, to speak with certainty on the subject. I will state what steps I took to satisfy myself, and perhaps the history of my failure may be of use to future observers.

It occurred to me from what I observed, that it was probable the males had the power of absorbing the eggs after their exclusion by the female, and impregnating them within their own bodies; and I caught a dozen males at different times, when they were attending on females, and opened them, but I could discover nothing like an egg. I then caught a female, and scattered the spawn (which was expelled by the slightest pressure) in a place frequented by a number of males, but they took no notice of it whatever. I after this caught a female when she was surrounded by a number of males, and apparently in the act of shedding her spawn, and examined whether the spawn which I pressed from her body was impregnated; but it appeared perfectly homogeneous, and so delicate in its texture that it burst with the slightest touch, whilst in that which I picked up from among the gravel where it was scattered abundantly, the impregnation was visible with the assistance of a microscope, and it was so much tougher in its covering as to bear rolling about in my hand without injury. I then tried to impregnate the eggs *mechanically*, and applied a drop of the spermatic fluid to the egg at the moment of exclusion, and it certainly seemed, in one instance, both to increase the size and to alter the colour of the ova it was applied to; but I was not able to produce the same effect so decidedly in any of my subsequent attempts.

My observations, which were often repeated, induce me to believe that the egg is impregnated at the moment of exclusion, and that two males have (almost invariably) access to the female at the same time; for I frequently remarked, that when a female came among a number of males, they immediately pursued her: if she was not ready for shedding her spawn, she made a precipitate retreat; but if she was, she came boldly in among them, and was immediately pressed closely by a male on each side, who when they had been in that situation a short time, were superseded by other two, who

wedged themselves in between them and the female, who appeared to treat all her lovers with the same kindness.

Page 99

One difficulty is, that the spermatic fluid mixes very readily with water; and I cannot imagine how its virtue is preserved, [14] if (as I suppose must be the case) the egg is impregnated after exclusion; but I also think it probable that the ventral fins of the female serve to conduct this fluid to the place where it is needed, and the chemical affinity between it and the egg may be sufficient for impregnation.

P.S. July 27th. I tried to hatch some of the eggs I had endeavoured to fecundate. The attempt was unsuccessful. I placed the eggs, which I had put into some clean-washed gravel, in a shallow vessel (open at the top, and with holes drilled through the sides) in a small stream of water, but I found to my great mortification on looking for them a day or two after that there was not one left, but that in their stead were many aquatic insects, which had no doubt feasted on them as long as they lasted, and after this I was not able to meet with another shoal of Minnows in the act of spawning.

The head of the Minnow in the spawning season is spotted over with small white knobs, apparently osseous in their structure, which make their appearance immediately before the fish begins to spawn, and which disappear again as shortly after, and I think they are intended as a protection to the head of the fish during the spawning; as I remarked that they generally thrust their heads in between two pebbles, and had their tails sticking up almost perpendicularly. Yet this was not always the case, as they sometimes ran nearly out of the water, and it was in this situation that I observed what I have before mentioned, as I found it impossible to discover anything that was done by those in deeper water; for when a female went into such a situation, there was such a crowd of males rushed to the place that I lost sight of her in a moment.

I was astonished to find how quickly the eggs were hatched. I discovered a large shoal spawning on the 11th of May; on the 12th they were diminished to one-tenth of the number, and on the 14th (the 13th being Sunday) there was not one left. As I had by no means satisfied myself on the subject, I felt disappointed that they had so soon finished their operations, and I took up a handful of the gravel where they had been spawning, and examined it with the microscope, to see whether I could discover any ova, and how they were going on, when to my great surprise I found them already hatching and some of them already excluded from the egg. One of them, which I took on the point of a knife, swam briskly away, and another was the means of pointing out an enemy to me which I had not previously suspected, and that I had always hitherto believed to be the prey of and not the prey upon fish. The poor Minnow had somehow got fast to the point of the knife, and in its struggles to free itself it attracted the attention of a creeper—the larva of one of the aquatic flies called drakes (*Ephemerae*)—which pounced upon it as fiercely as the water staphylinus does on the luckless tadpole, but, fortunately for the Minnow, either the glittering of the knife-blade or the motion of my hand, scared it away again without its prey.

Page 100

The young Minnows in this state were quite transparent, except the eyes, which were disproportionately large; and they seemed to be perfectly aware that they owed their safety to concealment, as those that I saw immediately buried themselves in the gravel when they were set at liberty. (July 27th, 1832.)

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EELS.

To the Editor of the "Gardener's Chronicle."

My attention has been called to a paragraph in a Worcester paper giving an account of a (so-called) discovery by Mr. Boccius, that Eels are propagated by spawn, like other fish, and that they are not brought forth alive, as had hitherto been supposed. This may be true, but before I can give an unqualified belief to the assertion, I should like to have a few questions answered by Mr. Boccius. Who saw the fish from which those thousands of eggs were extracted at the time this dissection was made? Are the parties who saw these eggs quite certain that the fish was an Eel and not a Lamprey? Who saw the eggs from which Mr. Boccius produced living Eels? Who beside Mr. Boccius ever saw Eel-fry in a pond which had no communication with a river? Will Mr. Allees and Mr. Reed (the gentlemen to whom the spawn was exhibited) say whether the ovary which was shown to them was pretty much of the same form as that of the Lamprey? and if not, in what respect did it differ?

I am induced to ask these questions, both because by inference they show my own opinions on the subject, and because I am led on undoubted authority to believe that Mr. Boccius is inclined to claim at *least* all that belongs to him; and also because I have my doubts about the scientific attainments of Mr. Boccius in the Natural History of Fishes.

It is difficult to prove a negative. My never having seen the strange things above mentioned certainly does not prove that other people with better eyes and more discrimination have likewise failed to do so; but I can't help doubting, and I publish my doubts in the hope that the subject may be further inquired into. A true naturalist ought only to wish for the truth, without reference to his own preconceived notions; but so far as my examinations have gone, I have failed altogether to detect spawn in the fringes which I have fancied were the ovaria of the fish, or elsewhere, and I don't believe that Eels are bred in fresh water at all. I see the fry ascending from the sea in May and June by thousands and millions, but I never met with one of these in a pond having no communication with a river. I have little doubt that I shall be pronounced in error touching this matter, except perhaps by those who know how perseveringly these little Eels make their way up every stream, ditch, and dribble of water into which they can gain access. They penetrate into the water-pipes and pumps; they climb up the

perpendicular faces of the rocks and weirs which obstruct the course of the rivers, even when they are only moist—adhering to the moss and stones like snails.

Page 101

The downward migration of Eels is observed here from July to the middle of September, but in the Manchester market I find them up to this time (the end of November), and am informed that they are caught at the foot of Windermere in their downward migration.

Would a dissection of the Conger at various seasons throw any light on the propagation of Eels? One would think that in such large fish the ovaria would be much more easily distinguished than in smaller specimens. (November, 1850.)

The above elicited the following reply:—

T. G. denies the possibility of Eels breeding in fresh water. We have a pond here covering three or four acres which swarms with Eels of all sizes. I have caught them from the size of my little finger up to the weight of five pounds. The supply of water is from nothing else than land springs—there being no communication between the pond and any river. When much rain occurs I am obliged to put up a sluice-board, in order to prevent the banks from overflowing. I have taken from one to two hundredweight at a time from a box which the water flows through at the bottom of the sluice-board. The large quantity that has been taken out of this pond leaves no doubt that they breed there to a great extent, but whether they are propagated by spawn or brought forth alive I am unable to say.—G. H., *Finedon Hall*.

Reply to the foregoing.

Your correspondent G. H. says T. G. denies the possibility of Eels breeding in fresh water. This is rather too strong. I don't deny the *possibility* of Eels being bred in fresh water, I only deny the *probability*. The expression I used was that I did not believe they were bred in fresh water at all, and I distinctly stated that my not having seen these things (Eel spawn, &c.), did not prove that other people had not done so. But to the question. G. H. says that he has caught them of all sizes, from the thickness of his little finger to five pounds weight. No doubt he may have done so, but did he catch them of the thickness of a crow's quill, and three inches long? because that is the size at which they usually ascend rivers. He says his pond does not communicate with any river. Is there no escape of water from it? I mean, is the evaporation from its surface equal to the supply of water? If not, where does the surplus go to? Does it not directly or indirectly flow into a river or the sea? I am the more inclined to think that this is the case, because G. H. says he caught a hundredweight at a time from a box which the water flows through at the bottom of the sluice-board. This is exceedingly like what is done here and elsewhere from July to the end of November, when the Eels are on their downward migration. Will G. H. be kind enough to say whether he does not catch his about the same time? will he also say whether the Eels he catches are not Silver Eels? and will he also state whether he does not catch

Page 102

them principally after heavy rains have increased the flow of water out of the pond? If he answers these questions in the affirmative, I shall still think I am right, and request him to keep a sharp look-out after rains in May and June, when I think he would probably see the grigs passing through his box into the pond. If, on the other hand, there is no escape of water from the pond at any time, I must admit that I am wrong, but at present I don't know how to reconcile the impounding the water so completely with what he says about the flow of the water through the box at the bottom of the sill. Where does the water flow to, and for what is this sill?

G. H. replied as follows.

T. G. asks if I have caught Eels of the size of a crow's quill. I have caught them of the size of a tobacco-pipe, and from three to four inches in length.

Our surplus water flows indirectly into the river Nene from our sluice. It supplies some stews where we have been in the habit of keeping reserve fish, and passing over several waterfalls, it enters into a ditch which is about three-quarters of a mile long, and then reaches the river I have just named.

The greatest take of Eels I have had was on the 23rd of December, but the time of the year is of little consequence with us, provided the water is thick and muddy and the weather rather warm, which, of course, only occurs during very heavy rains. If I were to draw all the water out of the pond when in a clear state, I should not catch a fish. The variety is the Silver Eel. Our pond is upwards of fifty miles from the sea; therefore how is it that those little Eels had got no larger during their long journey, interrupted as it is by numerous and almost insurmountable obstacles, before they could reach the little ditch, three-quarters of a mile long, that would conduct them to our pond? And, last of all, after this long and tedious journey, within a hundred yards of their destination they would have to climb four waterfalls and a perpendicular sluice-board. It appears to me they should have grown much larger than a common tobacco-pipe and longer than three or four inches in that time, but I will leave this point for T. G. to explain.—G. H., *Finedon Hall*.

Reply to the foregoing.

Many thanks to G. H. for his second letter on this subject. It appears to me that we think very much alike about Eels.

He says his pond is fifty miles from the sea; "therefore, how is it that these little Eels get no larger in their long and tedious journey? interrupted as it is by numerous and almost insurmountable obstacles, before they could reach the little ditch, three-quarters of a mile long, which would conduct them to our pond? and last of all, after this long and



tedious journey, within a hundred yards of their destination, they would have to climb four waterfalls and a perpendicular sluice-board. It appears to me they should have grown much larger than a common tobacco-pipe during that time; but I will leave that point to T. G. to explain.”

Page 103

This is so fairly put, that I will tell what I have seen, hoping that this will be a sufficient explanation.

In June, 1850, I chanced to go down to the bank of the Ribble, and there I saw a column of small Eels steadily making their way up the stream. I should suppose there might be fifty in every lineal yard, for they kept pretty close to the bank, apparently because they met with less resistance from the stream, and without pretending to accuracy I supposed they travelled at the rate of a mile an hour. This was about five o'clock in the afternoon, and I went to look for them about nine in the evening—they were still going in one unbroken column. How long they had been going when I first saw them, and how long they continued to go after my second visit, I don't know, but many thousands—perhaps millions—must have passed that day. At this rate (of a mile an hour) they would have required little more than two days to reach G. H.'s pond, fifty miles from the sea; but he says they had to pass over three or four waterfalls and a perpendicular sluice-board. If these waterfalls and the sluice-board were covered with moss, they would climb them as readily as a cat does a ladder. I have seen them in swarms at a perpendicular weir here, winding their way through the damp moss with which the stones are covered; but this was not all: where there was no moss, the little things seemed to have the power of adhering to the perpendicular face of the stones, like so many snails. I must not omit to remark, that although they seemed to choose the margin of the stream for the sake of easier travelling, yet they took care to keep in the stream, as I had a nice opportunity of observing.

At the point where I first saw them, the tail goit of a water-wheel had its junction with the river, but being Sunday there was no current there—not a single Eel took its course up the goit, although the water was deeper there than where they went. The water being low and perfectly clear, I could trace their course both above and below the place where I stood without any difficulty.

If we allowed that they travelled a mile in the hour, and that the obstructions of the waterfalls and sluice-board took as long to get over as all the rest of the journey, they would be able to reach G. H.'s pond in four days from the sea; and from what I have seen of their ability to surmount such obstructions, I am quite convinced that they would travel that distance in the time. But say they were a week—they would not grow much in that time, particularly if they had been travelling without food the whole of the distance, and that they must have done so, is proved to my mind by their keeping in column; for if they had dispersed to seek for food, by what contrivance were they marshalled into line again, to enable them to proceed? Now the place I saw them is forty miles from the sea, although not that distance from salt-water. T. says it is no proof that Eels

Page 104

are bred in fresh water because they may be found in ponds having no connection with a river—the proof required is *ab ovo*. If we wait for this proof I fear we will have to wait for some time, for I fancy that no one but Mr. Boccius ever saw the ova of Eels, and he will not condescend to enlighten us on the subject. At the same time I admit that finding them there is no proof that they were bred there, inasmuch as I have myself stocked such ponds for my friends, and what I have done may be done by others.

T. says further there is also room for inquiry into another curious subject—do Eels return to fresh water after having gone to the sea for spawning? In reply to this, I can only say, that no trace of such a migration is ever seen here, and I think if it existed at all, I should have observed it, for the following reasons.

The Ribble here supplies a large mill, the water-wheels of which are 150 horse-power; therefore, when they are at work in the daytime, the whole force of the river is often passing through the mill-lead (goit) and the bed of the river between the weir, and the tail goit in such times is left dry, except in a few pools. If there was a shoal of Eels between these two points it would have been seen at one time or another, and this has never happened, so far as I know. It may be said that they migrate singly, but they don't do so in their first migration, and, so far as I am aware, it is not the habit of any animal to do so. Herrings, Pilchards, Smelts, Flounders, Sturgeon, Bisons, Antelopes, Woodcocks, Swallows, Fieldfares, Locusts, and even Butterflies congregate together previous to migration.

NOTE.—The last paragraph requires some modification, as I have since proved that Eels migrate singly when going to the sea, as I have had occasion to know in a hundred cases when watching my Eel-trap, where every Eel may be seen as it descends into the trap.

On the same subject.

I [Jeremiah Garnett, brother of the writer, and editor of the “Manchester Guardian,”] having noticed the communications on this subject which have recently appeared in your columns, am desirous of mentioning a fact which appears to me to throw some light upon the localities in which Eels are bred, though it leaves the question of the mode of generation precisely where it stood before.

Like your correspondent T. G., I have many times seen columns of small Eels ascending the Ribble and other rivers in the months of May and June, at considerable distances from the sea, but only on one occasion have I seen them under circumstances which evidently brought them near the place of their nativity.

Page 105

I happened to be attending the Lancaster Spring Assizes in the month of March in the year 1826, and learning that there was a remarkably high tide in the estuary of the Lune, I walked down to the riverside about the time of high water, and found that the tide had covered the grass in many places; and as it began to ebb, I observed something moving in every small hollow which had been overflowed, and in which a little water had been left behind. On examination I found that the moving bodies were exceedingly diminutive Eels, rather less, to the best of my recollection, than three-quarters of an inch long, and almost transparent, but exhibiting in every respect the true form of the mature Eel. They had evidently followed the water to its extreme verge, where it could not have been more than an inch deep, and that they must have been very numerous was clear from the large numbers which were left behind and had perished—for that they did perish I found on the following day, when they were lying dead on the grass by hundreds. Some of your correspondents who reside in localities favourable for making observations on this subject may be induced to pay attention to it; the exact appearance may be ascertained, with probably other facts calculated to throw light on the obscure question of their generation.

* * * * *

ON THE POSSIBILITY OF INTRODUCING SALMON INTO NEW ZEALAND AND AUSTRALIA.

October, 1859.

The colonists of Australia, Tasmania, and New Zealand appear to wish for the introduction of Salmon and Trout into the rivers of these colonies, and one of them, Tasmania, is said to have offered the reward of £500 for the first pair of live Salmon which reaches that colony. If this is true it is a liberal offer, and one that is likely to induce various persons, both in England and France, to make the attempt.

I should be sorry to say anything to check so laudable an endeavour, but I greatly fear that Van Diemen's Land (to say nothing of the Australian colonies) is too near the tropics to offer a reasonable chance of success. I think it is practicable to take these fish there (or at least fertilized ova), but I don't think they would live and thrive in the rivers of that colony. Never having been there, I can, of course, only reason from European experience, but the best inquiries I can make lead me to suppose that there are no Salmon in France (south of Brittany), Spain, or any of the countries washed by the Mediterranean Sea; and in America (although I confess I am not so well informed on that country) I have never heard of Salmon being seen to the south of the tributaries of the St. Lawrence. Supposing this to be so, I think that we may fairly infer that if Salmon are not found south of a certain latitude in Europe and America, it must be that the climate of these southern countries is not congenial to the habits of this fish. I believe, however, that the Trout lives and thrives much further south than

Page 106

the Salmon; for instance, it is found in the Pyrenees and in the lakes of Northern Italy (Lady M. W. Montagu). It is also found in Northern Turkey, and probably Albania also (Spencer); and therefore I think it is quite probable that it might live in Tasmania—that is, if the streams are never dried up and the rivers reduced to a number of water-holes, which appears to be the case in Australia. Should this be the case in Tasmania also, I doubt whether even Trout would thrive, for here in Lancashire I have known the Trout to die in great numbers from the heat, when, owing to the water-wheels of the mill diverting the river from its usual channel, there was no stream, but merely a series of detached pools or water-holes; and the Grayling seem to be more incommoded by heat than the Trout, and it was one of the diversions of my boyhood to wait until the wheels of my father's mill were stopped in the hot weather, and then go up the covered wheel-races in search of the Grayling that had gone there to get out of the sunshine. I used to catch them there in great numbers. However, this has nothing to do with the matter, except to suggest that although Grayling are very desirable fish to introduce into the colonies, I fear they would be too impatient of heat to thrive there. But my object in addressing you is to ask whether it is true that the legislature of Tasmania has offered the prize of £500 for the first pair of live Salmon taken there?

Secondly, whether they offer a prize for the introduction of Salmon fry; and if so, what is the amount offered?

Thirdly, whether they offer a prize for the introduction of fertilized ova of Salmon or Trout, and what is the amount?

I ask these questions because I happen to know a good deal on such matters, and I have been applied to this day by James Birch, the head water-bailiff of our river (Ribble), to obtain some information for him on the subject, as he seems seriously bent on making the experiment, provided the reward be an adequate one; for, to be successful, it would involve the necessity of his making the voyage himself, and it would be a cruel thing to induce him to do so, and in the end to find that he was entitled to no reward.

I'll say this for him, that if he tries he will succeed, if success be possible; but his pecuniary resources are too limited for him to undertake such a risk.

I have reason to believe that he has been applied to by Ramsbottom to go to Tasmania, but this he declines to do under Ramsbottom's auspices. As he (R.) professes to be in communication with the authorities of Tasmania (or at all events with influential persons there) let him make the first attempt, and if he succeed, there will be no necessity to apply to me on the subject; but if he should fail—as I think he will—why, then the persons interested in the matter may, if they wish to try again, let me know their wishes and the amount of remuneration they mean to give.

Page 107

I should certainly suggest that both Salmon and Salmon Trout (as well as the common Trout) should be included in their list of desiderata, and although for reasons previously given I have no great hopes of success with the two former, I think it quite probable that the common Trout would succeed better. Of course I know nothing of the fish already in the rivers of Tasmania; for aught I know there may be fish in all those rivers quite as voracious and destructive as the Pike are here. If this is the case, the chances of success would be materially lessened, as Trout and Salmon fry are rare in all rivers stocked with Pike. However, those who are making the attempt ought to know what they are about, and will, no doubt, have considered such obstacles, if there are any such in the way. Will you, therefore, be kind enough to answer the questions I have asked above, at your earliest convenience, and if your replies offer any inducement to Birch to make the attempt, I have no doubt that he will be quite ready to do so.

For various reasons he can only start from here in the autumn or winter, and he should, if he reaches Tasmania with either live fish or fertilized ova, have someone to render him prompt and cordial assistance to enable him to deposit the fish or ova, or fish and ova, in suitable places for spawning and hatching; and therefore if this letter be replied to, the answer ought to say to whom Birch should apply on his arrival in Tasmania.

It may be asked, who is the man who obtrudes his opinions on the colony unasked, and what can be his motives? As I am not aware that I know a single person in Tasmania, I cannot refer to anyone there; but I happen to know one or two gentlemen in Melbourne, and if you will take the trouble to refer there to Messrs. W. and B. Hick, or to W. Bailey, the corn merchant, they will be able to satisfy all inquiries.

If it be asked what I know of the habits of fish, and Salmon in particular, I beg to refer the inquirers to Loudon's "Magazine of Natural History" for 1834 (if there is a copy of that work in the colony), and they will there find two papers (signed "T.G.," Clitheroe) which will show that I then knew all that has since been proved by the elaborate experiments made at Perth by Ramsbottom, and moreover I taught Ramsbottom himself the art of propagating fish artificially.

I want no compensation: the honour of being the first man who succeeded in introducing these valuable fish into the colonies would be a sufficient reward to me. But with Birch the case is different: he is a working man, and L500 would be a fortune to him. On the other hand, he could not afford to come to Hobart Town from England at his own expense, as he has not the means.

Would the colony, if other attempts failed, be willing to pay Birch's passage out and home if he failed also, and would he receive the L500 if he succeeded?

By success I mean that he would either bring live fish or ova that would hatch into live fish. Either of these objects being accomplished, he ought, in my opinion, to receive the reward; for although he would attempt both, he would probably fail in the former.

Page 108

Should he attempt this under my advice, I should not only send Salmon and Salmon Trout and their ova, but the common brown Trout and its ova also, for the reason previously given in this letter; and although I am by no means sanguine of success, on account of the temperature, the experiment is too important to be abandoned for a mere theoretical objection which may be erroneous.

I think New Zealand offers far greater chances of success. It is not only further removed from the tropics, but, if I am rightly informed, the streams are more abundant and constant than those of Australia and Tasmania—in fact, I believe it is as well watered as this country; and if the authorities there are as much alive to the importance of introducing these fish into their rivers, I would undertake to do this with much greater confidence of ultimate success than I should have if I undertook to introduce them into Tasmania or the sister colonies.

Some time since (it may be eighteen months or two years ago) there was a very intelligent correspondent of the “Field” newspaper, whose *nom de plume* was the Maori one, “Wetariki no te wai Herekeke,” or a similar one; and I having written something in the “Field” on this subject, the New Zealander asked for my address, which, for some private reason of his own, the Editor declined to give until so long a time had elapsed that Wetariki Herekeke had returned to the colony—this I learnt from an indirect source — otherwise I should have tried to induce him to undertake the experiment of introducing all the various species of the genus *Salmo* which are to be found in our rivers.

If the colonists of New Zealand wish to make the attempt, I shall be most happy to render them all the assistance in my power, and I know no one so qualified as Birch to undertake the management of such an experiment; for he is exceedingly intelligent, has a perfect knowledge of the habits of both Trout and Salmon, and thoroughly understands the feeding of fish, both in their natural haunts and artificially, and would consequently be able to select suitable localities for conducting such an experiment to a successful issue.

NOTE.—No reply was given to this by the authorities of Tasmania, but a similar communication, addressed to the Governor of New Zealand, elicited a very polite reply from his secretary, in which he said that there were no funds available for such a purpose, but that the subject would be brought before the legislature on their assembling, and would no doubt meet with their favourable consideration; but the Maori troubles broke out immediately after, and I heard no more about it.

* * * * *

CLITHEROE, *October 14th*, 1859.

To the Editor of the “Field.”

In the "Field" of some weeks since, it was stated that the colonists of Tasmania were offering a large reward for the introduction of live Salmon, Salmon fry, or the fertilized ova of Salmon.

Page 109

Will you have the kindness to say what was the amount offered? who were the parties who made themselves responsible for the payment? and what time did they give within which they would pay for a successful attempt?

I am the more anxious to have this information, because I have been applied to for advice by an exceedingly likely person, as the reward (L500) which he understood to be offered is to him so tempting a sum, that he would need very little encouragement to undertake the management of the experiment; and from what I know of him I will venture to assert that he will succeed, if success be practicable.

But before I speak confidently of success, I would like a little more information, and will thank any of your readers who are able to do so, to give me replies to the following questions:—

Are there any Salmon in the rivers of Spain, or in France, south of the Loire, or even in that river? If not, why not?

Are there any Salmon in North America, in any river (not a tributary of the St. Lawrence), south of that river? If there are, what rivers in the States contain Salmon.

Do any of the rivers on the west coast of America below the latitude of 40 degrees N. contain Salmon?

Do any of the rivers of China (not Chinese Tartary) contain Salmon?

If I am right in supposing that the rivers I have pointed out have no Salmon in them, is it not exceedingly probable that the high temperature of these southern countries is unsuited to the habits and uncongenial to the health of these fish? Or how is it when they are on the same seaboard further north, they don't ascend these rivers, unless there are some such objections to their doing so? And if these objections really exist, then do they not equally exist in the rivers of Australia and Tasmania?

But there may be other objections equally fatal: there may be fish in their rivers as voracious and destructive as our Pike; there may be Sharks and other fish in their seas and estuaries, which would snap up every Salmon that entered them. There may be Seals, Porpoises, Albatrosses, Man-of-War birds, and Cormorants, as well as fifty other nameless enemies, all combining their efforts to defeat so desirable a consummation; and, after all, there may be no one willing to make himself responsible for a repayment of the necessary expenses, for corporations and public bodies are proverbially untrustworthy.

Yet, notwithstanding all these doubts of success, I think the experiment ought to be made; for its success would confer so great a boon on the colony in which it was made, that they (the colonists) ought to incur considerable risk and outlay for the chance of

success, however small. I don't think there will be much difficulty in carrying fertilized ova there, but when hatched I fear they would not thrive.

Page 110

I think New Zealand offers far better chances of success: it is further from the tropics, it abounds in suitable rivers, the climate and temperature are more like England, and I believe the rivers never degenerate into mere water-holes, as they seem to do in Australia; and I think the residents of that colony ought to make a vigorous attempt to introduce Salmon, Salmon Trout, and the common brown Trout into their rivers immediately; and I should be delighted to render all the assistance in my power to accomplish so desirable an object.

* * * * *

ON THE FORMATION OF ICE AT THE BOTTOM OF RIVERS.

Anchor Frosts.

A correspondent of the "Magazine of Natural History," in endeavouring to explain the causes why water freezes at the bottom in rapid streams, says this unusual phenomenon may be rationally accounted for by anyone who has attended to it; that the streams in which anchor frosts occur generally are those which contain water of different temperatures—viz., surface-drainage and land springs and main springs, the first being always colder than the latter, in winter these never being less than 40 degrees, even in severe frosts.

These colder globules being first frozen, float on the surface of the water individually, being prevented from coalescing by the intermediate main-spring-water, and where the water passes in a shallow stream over the pebbles the crystals are intercepted by the interstices of the stones, and then become heaped together in thick beds.

The fact of the crystals of ice (which are specifically lighter than the water) sinking below the surface, is a circumstance requiring explanation. They do not sink from their specific gravity, but in the commotion of the current they are occasionally submerged, and while so are stopped by any obstruction, when they commence and compose the aggregation.

Thinking this was an erroneous view of the matter, I replied as follows:—

J. M., in his remarks on anchor frosts, appears to me to have fallen into several errors in endeavouring to account for them (they are called bottom frosts in Yorkshire); for, admitting that main springs are of the temperature stated (40 degrees) when they issue from the earth, I am by no means prepared to believe that they keep that temperature long, or that the water issuing from them does not mingle intimately and immediately with the water of the river into which it flows; especially in the situations where anchor frosts are most common, which are rough and rapid streams.



From J. M.'s statement it would appear that globules of water of different temperatures mix together without the one imparting its excess of caloric to the other, which is contrary to the experience of everyone; it is true, that in still places there will be different temperatures in the same body of water, but it is not owing to the main springs of which J. M. speaks, but to the peculiar way in which water

Page 111

is affected by cold. It is well known that water increases in density down to 40 degrees, below which temperature it begins to expand, and this expansion continues until it reaches the freezing-point, so that in severe frosts there will be strata of different temperatures from 32 degrees to 40 degrees. Again, he says that "the crystals of ice are intercepted by the interstices of the stones, and then become heaped together in thick beds;" but if my observations are correct, these depositions begin first round the large stones, which are not likely to stop small spiculae any more than are the water-gates of mills, where, he says, the accumulations also take place.

Anchor frosts are most common in the rapid streams occurring below deeps in rivers, and I have seen a weir on the river Wharfe which had a wall of ice four feet high formed upon it in a single night by a sharp north wind. In my opinion a sufficient explanation of this freezing at the bottom of rivers is to be found in the fact that water when kept still may be cooled down below the freezing-point without being congealed; but if the vessel in which it is kept be shaken, a portion of it will be converted into a porous, spongy ice, and the temperature immediately rises to 32 degrees. In the deeps of rivers the same cooling below the freezing-point takes place without congelation, but as soon as this water reaches the stream below, the agitation immediately converts a portion of it into ice, which collects round the large stones at the bottom in the same way that crystallization commences in a solution of salt or sugar around a piece of thread or other substance which may be suspended in it. If a severe frost is followed by a bright day, thousands of these detached pieces of spongy ice may be seen rising from the stones which have served as nuclei for them; which proves that the detention of them is not merely mechanical, but that precipitation (if I may be allowed to call it so) takes place in the first instance, the stone serving as a nucleus, and that this adhesion is destroyed by the action of the sun's rays.

I have never seen any attempt to explain the phenomenon of bottom-frosts before this of J. M.'s, and I am not philosopher enough to speak positively on the subject; but the above is the way in which I have always endeavoured to account for it. Perhaps some of your scientific readers may be able to give much better reasons for it than have been offered either by J. M. or myself. (January 17th, 1832.)

Another writer (J. Carr, of Alnwick,) says that anchor frosts are merely long and severe ones where long masses of ice are frozen to the stones at the bottom of rapid streams, and this is simply owing to these stones acquiring a degree of cold far below the freezing-point, and the water in contact with them freezing and spreading into large sheets of ice, which are sometimes torn up and carry away the gravel adhering to the under surface.

Page 112

Thinking that this was an error, I again wrote to the “Magazine of Natural History” as below:—

I perceive that others beside myself have endeavoured to account for anchor frosts. Mr. Carr says they never occur except in long and severe frosts, and that the adhesion of the ice to the stones at the bottom is owing to their acquiring a degree of cold far below the freezing-point. He is in error when he says they never occur except in long-continued frosts, as the walls of ice which are sometimes raised on the crowns of weirs are invariably (so far as my observations have extended) deposited there *before* the water in the reservoir above is frozen over, which proves that the frost has not been of long continuance, although it may have been severe. As to what he says about the stones acquiring a degree of cold far below the freezing-point, and imparting that coldness to the water, I would just ask how it is that a stone at the bottom of a river acquires this excess of cold, and if it is not more probable that the stones impart warmth to the surrounding water? I can easily conceive how the stones may, by the action of the sun’s rays upon them, warm the surrounding water; but I do not see how they can impart cold, or, in other words, how their temperature can be reduced below that of the water by which they are surrounded. Stones certainly impart warmth to the water they are in, in bright weather, as the rays of the sun do not give much warmth in passing through any transparent medium; but on coming in contact with any opaque bodies, the heat is absorbed or reflected as the case may be, and in this way transparent media such as air and water acquire a warmth by contact which they would not otherwise possess. Thus, if an anchor frost is followed by a bright day, the rays of the sun impart so much warmth to the stones at the bottom of the river as is sufficient to liberate the ice from them, and on such days thousands of pieces of ice may be seen rising from the bottom and floating down the streams.

Since my former observations were written I have had the satisfaction of finding my views on the subject confirmed by a very eminent chemist, [15] and if the discussions in your Magazine were to be settled by authority, and not by argument (which I trust will never be the case), he is one to whom many would be inclined to appeal, and to whom few would refuse to submit. (May 2nd, 1832.)

* * * * *

To the Editor of the “Agricultural Gazette.”

In a leading article of the 10th of January, 1852, after an account of the effects produced on water by radiation and the protection afforded to plants by the ice with which ponds are covered in winter, you go on to say that there are some circumstances under which water-plants suffer greatly, and from a singular cause, but one which when looked into is sufficiently simple and intelligible. As you do not appear to have hit upon the true reason, allow me to quote a little further, and then give my reason for this singular effect.

Page 113

You say that on a very fine but still night, water is cooled less rapidly than the earth: under such circumstances the bottom of the pond cools more rapidly than the surface, the plants become colder—in fact, some degrees below the freezing-point, &c. &c.

I submit that such reasons are inadmissible, for there would be an immediate upward current, which, as water is such an excellent conductor of heat, would immediately equalize the temperature of all the water above 40 degrees Fahrenheit, and stratified (if I may use the expression) above the water of this temperature there would be another layer of water of equal but gradually decreasing temperature until it fell below 32 degrees Fahrenheit.

The explanation I offer is this. It is well known that if water is kept perfectly still it may be cooled down considerably, or at least some degrees below 32 degrees, without freezing; but the moment it is shaken a portion of it is converted into a spongy, porous ice, and the temperature rises to 32 degrees.

What may be the case in the rivers of the South of England I do not know, but in the rapid streams of the North this process may be seen on a very extensive scale in severe frosts. The water in the still pools (before they are frozen over) is cooled down to below 32 degrees, and so soon as this cooled water reaches the next stream, precipitation (if I may so call it) takes place, and the spongy ice lays hold of every projecting pebble, which serves as a nucleus in the same way as threads and bits of stick serve in the crystallization of salts. After a severe frost, when followed by bright sunshine the next morning, I have seen thousands of these bits of spongy ice rising from the stones to which they had been attached to the surface of the water. I have seen after long-continued frost the course of a stream completely altered by this bottom-ice (as it is called here), and I have also seen a weir with a wall of ice on it three feet high (raised in a single night) by the same cause. Now apply this to the bottom-ice in ponds (which however I must confess I never saw). The night being calm, the water gets cool below 32 degrees, but then a breeze springing up the water becomes agitated, precipitation takes place, and the plants serving as nuclei become immediately clothed with this spongy ice, and the sun shining next morning imparts so much warmth to the plants that the ice thaws which is in contact with them, and rises to the surface. Of course if the sun does not shine next morning, and the frost continues, the plants may be clothed with ice for a long time.

To the foregoing the Editor of the “Agricultural Gazette” replied as follows:—

We cannot admit the soundness of our correspondent’s explanation of the formation of bottom-ice or ground *gore*. We are well acquainted with the statements of Arago and other writers as to the cause of this curious phenomenon, and after a careful consideration of the subject believe that it is due to radiation and not to any other cause. Bottom-ice has been observed in ponds on perfectly still nights when there was no breeze to agitate the surface of the water.

Page 114

The waters in the pools between the rapids of rivers can hardly ever be still enough for the water to fall below the freezing-point and yet remain fluid; the temperature of water in such situations is not below 33 degrees.

The following was my rejoinder:—

You say at the end of remarks about bottom-ice that you cannot admit the soundness of my explanation, and that you are well aware of what is said by Arago and others on this curious phenomenon, and that bottom-ice has been observed in ponds when there was no breeze, and that the water in pools between the rapids of weirs can hardly ever be still enough to fall below the freezing-point, and yet remain fluid.

I was not aware before seeing your remarks that either Arago or any other philosopher had ever written about bottom-ice, and even now I do not know what are their opinions on the subject, and if the discussions in your paper are to be settled by authority and not by argument, I can only make my bow and withdraw; but if it meets your views to allow your correspondents to state their opinions temperately, and support them by such arguments as occur to them, I do not yet feel inclined to give up my notions about bottom-ice. Will you allow me to ask whether you ever personally saw ice at the bottom of a pond when there was none on the surface? and if so, under what circumstances? I have heard of such an occurrence, but never witnessed it, and feel inclined to doubt the fact unless you will vouch for it; for it appears to me that the moment the water at the bottom falls below 40 degrees it will begin to rise to the surface, and it is so excellent a conductor that it will instantly equalize the temperature of the mud at the bottom with that of its own temperature.

I am neither chemist nor meteorologist, and therefore I am not able to say much about radiation; but my idea of it is, that its effects in water would be much greater in still pools than in rapid streams, and that, therefore, if radiation was the cause of bottom-ice, there ought to be more of it in the pools than in the rapid streams. But the contrary is the fact, for after a severe night's frost, I can frequently find the streams filled with this bottom-ice, when none can be observed in the pools.

Again, can the fact of the weir which had a wall of this bottom-ice three feet high in a single night, be accounted for by radiation? It appears to me to be very easily accounted for by supposing that the water in the deep above was so quietly cooled down as to retain its fluidity until the shaking it got on flowing over the weir suddenly produced congelation. I think that radiation would not go on at the crown of the weir alone.

Why do you think that the water in pools is never still enough to allow it to get below 32 degrees without freezing on still clear nights? In long deep pools, where the body of the water is perhaps a hundred times as great as the current flowing into it, the motion is so

extremely slow that I cannot for a moment doubt that it gets below 32 degrees without congelation, but when it arrives at a rapid, this ice is immediately formed.

Page 115

The Editor closed the discussion at this point by saying that the subject was not of sufficient agricultural importance to be continued further.

The following is my brother Richard Garnett's [16] account of his observations on bottom-frosts. (The paper was written in 1818, and published in the "Journal of the Royal Institution.")

* * * * *

ON THE PRODUCTION OF ICE AT THE BOTTOMS OF RIVERS.

The phenomenon of the production of ice at the bottoms of rivers has been repeatedly noticed, but I am not aware that any satisfactory solution of the cause has hitherto been given. In Nicholson's "Dictionary of Chemistry," several different hypotheses are enumerated, which I shall not stop now to examine, since it may be safely asserted that they neither accord with the established principles of chemistry, nor with the facts for which they endeavour to account. The most recent theory with which I am acquainted is that of Mr. A. Knight, who in a paper lately published in the "Philosophical Transactions," seems to consider the particles of ice as originally formed at the surface, and afterwards absorbed by the eddies of streams to the bottom. He states, in support of this idea, that he did not observe any similar phenomenon in still water. I shall advert to this hypothesis in the sequel, and at present it may suffice to remark of it and all others which I have hitherto seen, that supposing any of them to be correct, the same effects ought regularly to be produced whenever the atmosphere is at a similar temperature, or in other words, that whenever the frost is so intense as materially to affect the water of a river, we may then expect to find ice at the bottom. Now this is certainly not the case, since the appearance we are treating of never occurs but under peculiar *atmospherical* circumstances, and rivers are frequently frozen over, and remain so for a length of time without a particle of ice being visible at the bottom of their streams. I do not now profess to have developed this mystery, but merely intend to state the circumstances under which the phenomenon takes place, as well as a few particulars connected with it, which are perhaps not generally known, and which may hereafter be serviceable as data for investigating the cause.

It is well known to meteorologists that a severe frost in winter does not always commence in a uniform manner. Sometimes it begins with a gentle wind from the E. or N.E., and is at first comparatively mild in its operations, but afterwards gradually increases in intensity. Frosts of this kind are generally more lasting than others, and during such, I have not observed that any ice is generated at the bottoms of streams; though the deep and still parts of rivers are often frozen over to a considerable extent. At other times, during the continuance of the violent south-westerly gales which are so prevalent in this country in the winter months, the wind frequently shifts on a sudden

Page 116

from S.W. to N.W., commonly about an hour before sunset, and blows with great impetuosity in the latter direction, attended with a severe frost, and sometimes with a heavy fall of snow. The effects of this frost, in places exposed to the wind, are extremely rapid, so as to render the ground impenetrably hard in about a couple of hours from its commencement. Situations that are not so much exposed seem comparatively little affected—at least, I have repeatedly observed that a small sheltered pond in a field was nearly free from ice, while the current of a large and rapid river at no great distance was nearly choked up by it. I believe that the phenomenon under consideration seldom occurs except during such frosts as these, and the following are the principal circumstances connected with it which I am able to state from my own observation.

It may here be premised that ice of this description is seldom seen adhering to anything beside rock, stone, or gravel, and that it is more abundantly produced in proportion to the greater magnitude and number of the stones composing the bed of the river, combined (as will be further noticed) with the velocity of the current. I have been informed by a friend that he has occasionally seen it attached to solid wooden piles at a considerable depth below the surface of the water, but I never saw or heard of any on earth, mud, or clay. It is not easy to ascertain the precise time at which the process begins to take place. It appears, however, almost invariably to commence during the first night of the frost, and probably within a few hours after sunset. On the ensuing morning the first thing which strikes an observer is an immense quantity of detached plates of ice floating down the stream. Mr. Knight naturally enough supposed these to have been formed at the surface by the influence of the freezing atmosphere, and afterwards absorbed by the current; but I think that a minute inspection would have led him to form a different conclusion—*viz.*, that they are first formed in the bed of the river, and afterwards rise to the surface. It is true that none are to be seen in situations where there is no sensible current, and that they abound most in rough and rapid places; but on closely examining any stream of moderate velocity, yet smooth, equable, and free from all appearance of eddy or rippling, a great number of these plates of ice will be found adhering to the rock, stone, or gravel at the bottom. If they are watched with attention, they will be observed to rapidly increase in bulk, till at last, on account of their inferior specific gravity, aided, perhaps, by the action of the current, they detach themselves from the substances to which they first adhered, and rise to the surface of the water. The form of these pieces of ice is very irregular, depending in a great measure on the size and shape of the stones or other substances to which they were originally attached. Most of them seem to be of an oblong

Page 117

or circular figure; they are generally convex on the upper surface, and have a number of laminae and spiculae shooting from them in various directions, especially from their circumference. Sometimes when those floating pieces or plates meet with any obstruction in the channel of the river, they accumulate in such quantities as to cover the surface of the water, and become frozen together in one large sheet, but this kind of ice may be always readily distinguished from that produced in the usual way by the action of the cold air on the surface, which is smooth, transparent, and of an uniform texture; on the contrary, one of these conglomerated fields or sheets is opaque, uneven, full of asperities, and the form of each separate plate composing it may be distinctly traced. In this situation, they generally assume the shape of irregular polygons, with angles somewhat rounded; a form apparently caused by the lateral pressure of the contiguous pieces.

On the river Wharfe, near Otley, in the West Riding of Yorkshire, is a weir or milldam where this phenomenon is sometimes manifested in a striking manner. This structure is of hewn stone, forming a plane inclined at an angle of from 35 degrees to 50 degrees, fronting the north and extending from west to east, to the length of 250 or 300 yards. When one of the above-mentioned frosts occurs, the stone which composes the weir soon becomes incrustated with ice, which increases so rapidly in thickness as in a short time to impede the course of the stream, which falls over it in a tolerably uniform sheet, and with considerable velocity; at the same time, the wind blowing strongly from the north-west, contributes to repel the water and freeze such as adheres to the crest of the weir when its surface comes nearly in contact with the air. The consequence is that in a short time the current is entirely obstructed, and the superincumbent water forced to a higher level. But as the above-mentioned causes continue to act, the ice is also elevated by a perpetual aggregation of particles, till by a series of similar operations an icy mound or barrier is formed, so high as to force the water over the opposite bank, and thus produce an apparent inundation. But in a short time the accumulated weight of a great many thousand cubic feet of water presses so strongly against the barrier as to burst a passage through some weak part, through which the water escapes and subsides to its former level, leaving the singular appearance of a wall or rampart of ice three or four feet high, and about two feet in thickness, along the greatest part of the upper edge of the weir. The ice composing this barrier where it adheres to the stone, is of solid consistency, but the upper part consists of a multitude of thin laminae or layers resting upon each other in a confused manner, and at different degrees of inclination, their interstices being occupied by innumerable icy spiculae, diverging and crossing each other in all directions. The whole mass much resembles the white and porous ice which may be seen at the edge of a pond or small rill where the water has subsided during a frost.

Page 118

It may be further observed that a frost of this kind is very limited in its duration, seldom lasting more than thirty-six or forty hours. On the morning of the second day after its commencement, a visible relaxation takes place in the temperature of the atmosphere. Usually before noon, the wind on a sudden shifts to the south-west, and a rapid thaw comes on, frequently attended with rain. What appears somewhat remarkable is, that during several hours after the commencement of the thaw, the production of ice at the bottom of rivers seems to go on without abatement, and upon examining a rapid stream, the stones over which it flows will be found at this period completely incrustated with the above description of icy plates. It seems evident from this that the bed of the river, which has been reduced below the freezing temperature, is not for some time affected by the change of the atmosphere. This may be in some measure illustrated by the well-known fact, that rain which falls upon a rock or stone wall, is frequently converted into ice, though the air and the ground are evidently in a state of thaw. Before the following morning, the ice of which we have been speaking generally disappears, being carried away by the current or dissolved by the thaw.

The last time that I remarked this phenomenon, was in a stream of the river Aire, near Bradford, in Yorkshire, on the 1st of January, 1814. This instance did not precisely accord with what I have stated to be the usual circumstances of the case, as the frost then had existed several days without any previous appearance of this kind; but there were several indications of approaching change of temperature, and the day following there was a partial thaw attended with rain, the wind having veered from north-west to south-west. This thaw, however, did not continue long, and was succeeded by a frost which surpassed all within my recollection in severity and duration. Yet during the whole of the period, though the thermometer often stood below 18 degrees Fahrenheit, and the estuary of the Tees several miles below Stockton, where the spring-tides rise from twelve to eighteen feet, was for two months frozen over, so as to allow the passage of a loaded waggon, I could never perceive a particle of ice adhering to the rock or gravel, in the bed of the small and rapid river Leven in Cleveland, where I then resided. This circumstance seems decisively to prove that the phenomenon does not merely depend on an intensity of cold.

I confess I am unable to frame any hypotheses respecting the above-mentioned facts which would not be liable to numerous and formidable objections. The immediate cause of the formation of the ice seems to be a rapid diminution of the temperature in the stone or gravel in the bed of the river, connected with the sudden changes in the state of the atmosphere, but it does not seem very easy to explain the precise nature of this connection.

Page 119

We may easily conceive that by a sudden change from a state of thaw to an intense frost attended by a strong wind, the whole body of water in a river may become quickly cooled, and consequently diminish the temperature of the stone or gravel over which it flows; but to suppose that water which is not itself at freezing-point is capable of reducing the substances in contact with it by means of a continual application of successive particles so far *beneath* that temperature as in process of time to convert the contiguous water to ice, seems not to accord very well with the usually received theory of the equilibrium of caloric. However, the fact that the quantity of ice thus produced is always greater in proportion to the superior velocity of the stream, little or none being found where there is no sensible current, seems in some degree to countenance the above idea.

I cannot learn that any experiments have ever been instituted on this subject, though it seems that they might easily be made by a person conveniently situated and possessed of the necessary instruments. A careful examination by properly contrived thermometers of the relative temperatures of the air, the water, and the bed of the river and of the changes undergone by them during the above process, would probably go a great way towards solving the problem. I know no one better qualified for this undertaking than Mr. Knight, if he should at any future time have leisure and opportunity to direct towards it the same acuteness of observation and accuracy of investigation which have enabled him to make such important discoveries in the economy of the vegetable kingdom, and if the explanation of this phenomenon should ever lead to results of any importance to the cause of science, I shall feel sufficiently satisfied if it be deemed that I have been of any service in pointing out the way.

RICHARD GARNETT.

BLACKBURN, *May 16th*, 1818.

* * * * *

GOSSAMER.

CLITHEROE, *October 20th*, 1859.

To the Editor of the "Field."

"A Young Inquirer" asks what is the cause of that appearance so often met with in the autumn, resembling spider-webs. He says, if it be the production of that insect, how do you account for their hanging apparently unsuspended in the air, as it is seen fifty or sixty feet high, without a tree or any other object near to which it could be attached?

I suppose you have not time to give to such questions minutely, as your reply would lead one to infer that Gossamer proceeded from spiders in general; and if it be meant

that all true spiders spin, it is no doubt correct; but the Gossamer which “A Young Inquirer” asks about is the production of a small black spider about the size of a flea, which was a true aeronaut long before Montgolfier or Lunardi, and if “A Young Inquirer” has access to either the “Linnean Transactions” or the first series of Loudon’s “Magazine of Natural History,” he will

Page 120

find particulars in the latter, showing that a violent controversy raged through the three first volumes between Mr. Blackwall and Dr. Murray on the question whether the ascent of this spider (*A. Aeronautica*) was electric, or whether it merely travelled in the direction of the wind. But if “A Young Inquirer” would deserve his name, let him begin with these spiders and observe for himself; he will find the inquiry highly interesting.

He has no doubt frequently seen a small black spider creeping on his hat or clothes (if he lives in the country this must have occurred to him many times); this is the aeronautic spider. Let him take this upon his hand, and if he be in the house let him carry it to the open door or window, and allow it to creep up to the tip of his finger, which he must then hold in a horizontal position. When the spider finds it can proceed no further by creeping, it generally drops a few inches, where it remains suspended for a short time, apparently quite still, but if very closely observed another thread (Gossamer) may be seen proceeding from its vent, and when this has reached the length which the spider’s instinct tells it is sufficient for the purpose, it cuts off the connection till then existing between it and the thread by which it has hitherto been suspended from the finger, and floats away into space. Very often it rises almost vertically, sometimes its course is nearly horizontal, and sometimes it is oblique.

I cannot say, as Mr. Murray does, that I have seen the spider go *against* the wind, neither can I confirm Mr. Blackwall’s assertions that he always goes right before the wind, for I have seen him go apparently across the current, so far as I could judge of the direction of the wind at the time.

If “A Young Inquirer” makes the experiment I have suggested, let him not be discouraged if the first he tries does not go off at all, as I have sometimes found this to be the case, which I accounted for by supposing that possibly the supply of materials might be exhausted at the time.

I do not remember that I ever saw one of these aeronautic spiders preying upon any insect, yet it must be for some such purpose that they ascend to great altitudes, sometimes in countless numbers, and the way they come down again is quite as curious as the manner in which they ascend.

Many years since, as I was walking over the hills in the neighbourhood of Blackburn, on a bright, still morning in September, thousands of small locks of what looked like cotton wool were slowly descending to the ground from various altitudes— some as high as I could see—and tens of thousands of similar locks were lying on the ground on both sides of the path by which I was travelling; and on examination I found that all these locks were Gossamer, some with the spider still with them, but generally deserted. The spiders when they wanted to come down, finding there was no descending current of air, or perhaps, as Mr. Murray says, no electricity, determined to descend in *parachutes*;

they therefore had drawn up their cables hand over hand (as they may often be seen to do when they wish to ascend their own lines) until they accumulated a mass heavy enough to fall by its own weight, and carry them along with it.

Page 121

I have seen Gossamer in this form at other times before and since, but in the likeness of a snow-shower I never saw it except on that occasion, and, if I recollect aright, the same enormous shower of Gossamer was observed to extend as far as Liverpool.

What induced these millions of spiders to go up at the same time, of course I do not know, and can only suppose that they went up to feed; but, as I have said previously, I never saw one of this species preying upon anything. The idea that they go aloft to kill the *Furia Infernalis* is too fanciful to deserve credit. Who knows whether the *Furia Infernalis* is anything else than a murderous Mrs. Harris—at all events, who has seen one, and what was it like?

I suppose they are true sportsmen, and disdaining to take their fish in nets, they, like thorough brothers of the angle, fish only *with fine gut*.

Gilbert White noticed one of these showers of Gossamer, and as his account is very interesting, I quote it. He says that on the 21st of September, 1741, intent upon field diversions, he rose before daybreak, but on going out he found the whole face of the country covered with a thick coat of cobweb drenched with dew, as if two or three setting-nets had been drawn one over the other. When his dogs attempted to hunt, their eyes were blinded and hoodwinked, so much that they were obliged to lie down and scrape themselves. This appearance was followed by a most lovely day. About 9 A.M. a shower of these webs (formed not of single threads, but of perfect flakes, some near an inch broad and five or six long) was observed falling from very elevated regions, which continued without interruption during the whole of the day, and they fell with a velocity which showed they were considerably heavier than the atmosphere. When the most elevated station in the country where this was observed was ascended, the webs were still to be seen descending from above, and twinkling like stars in the sun, so as to draw the attention of the most incurious. The flakes of the web on this occasion hung so thick upon the hedges and trees, that basketsful might have been collected. No one doubts (he observes) but that these webs are the production of small spiders.

These aerial spiders are of two sizes, although of the same colour and general appearance; they are probably male and female. At all events they do not vary in size more than other species of spiders when the sexes differ.

Has it been observed by naturalists that spiders eat their own webs? A large one that I used to feed when I was a lad with wasps, humble bees, and flesh-flies, used to do so occasionally. These insects were so strong that they often ruined the web in their efforts to escape, and the spider, quite aware of the rough customers it had to deal with, would often coil a cable of many folds round them before venturing to seize them with its mandibles. It would, if the web was ruined by the struggles of the insect, deliberately gorge it, which I accounted for by supposing that unless it did so it would not be able to secrete a sufficient supply of material to enable it to spin another.

Page 122

The leaping spiders are another curious species, which construct no webs, although they spin threads. This spider may be seen frequently on the walls of houses, and if carefully watched it will be seen to range up and down in quest of small gnats and other insects; when it observes one it creeps to within about two inches of it, and backing slightly, it appears to hesitate for a moment, and then springs upon the fly, but always before doing so it fixes a thread to the spot from whence it springs, so that if the fly happens to be too strong for it, and is able to detach itself from the wall, they both remain suspended from the thread which has been previously fixed by the spider. This I have seen more than once.

They sometimes venture on larger game than the small gnats. One I was watching one day came upon one of the large *Ephemera* (the Browndrake), an insect ten times as large as the spider, but after many points (for the setting of the spider before it springs is very similar in manner to that of a thoroughbred pointer [17]), in which it kept varying its position, apparently to gain some advantage, it gave up the attempt, discretion proving the better part of valour.

When botanizing on Erris Begh (in Connemara), this summer, I passed through many spider-lines so strong as to offer a very sensible resistance before breaking. I don't remember to have ever before met with them so strong and tenacious, and the makers of optical instruments might there have found abundance of threads which I am told are valuable as *cross-wires* for transit-instruments and theodolites. I did not meet with any of the spiders that had thrown out these lines, but judging of them by their works I suppose they must have been large ones.

One of your correspondents was inquiring a few weeks since how it was that a spider could throw out a long line between two trees or buildings at a considerable distance from each other. This seems to me to be very easily explained, if we reason from the analogy of the flying spider. The spider seems to throw out a line, trusting it will catch somewhere or other, and it is able to ascertain it has done so by pulling at it, and when it finds that it is firmly fixed it starts off to travel upon it, as I have occasionally noticed.

Everyone has noticed how carefully the spider carries her cocoon of eggs attached to the vent, and how disconsolate she appears to be when deprived of them; but I don't think it is so generally known that some of the spiders carry their young on their backs for some time after they are hatched. I remember seeing an instance of this one day when on the Moors, grouse-shooting. I saw what seemed to be a very curious insect travelling on the ling (heather), and on stooping down to examine it I found it was a large spider, upon the back of which (in fact, all over it) were clustered some dozens of young ones, about the size of pins' heads; she also seemed to guard them with great care, and seemed much afraid of losing them.

Page 123

FINIS.

NOTES.

[1] There is a fish somewhat resembling the Brambling in the Dunsop, a tributary of the Hodder, where it is known by the name of the Bull Penk.

[2] My opinion that neither Trout nor Salmon spawn every year is I think strongly corroborated by the fact, that previous to the Act of 1861 the London fish market was supplied with Salmon of the largest size, and of the best quality, in October, November, and December. When these fish were examined, it was found that the ovaries were but small, and the individual ova were not larger than mustard seed. These fish could not have spawned that season, nor would they have done so if left alive, if the growth of the ova in the ovaries is uniform—I mean if the growth of the ova is as great in one month as another—because in May and June the ova in a female Salmon is four times as large as these were in November.

Again, when the gas tank at Settle was emptied into the Ribble, in September, 1861, all the fish so far as was known were killed between that place and Mitton, Salmon as well as Par and Trout. Supposing that Salmon spawn every year, and that the Smolts come up the river, as Grilse in the summer of the same year in which they have gone to the sea in the spring, there ought to have been a great scarcity of both Grilse and Salmon in the Ribble in the year 1862, but so far was this from being the case, that both Grilse and Salmon were more abundant that season than they had been for some years previously, but there was a scarcity of both in 1863.

Again, when the Smolts were turned out of the breeding ponds at Dohulla, Galway, the experiment was looked upon as a failure because no Grilse returned the same season, not one having showed itself, but many came the summer after, proving pretty conclusively that in some rivers, at all events, the Smolt requires a year's residence in the sea before it returns as Grilse.

[3] In the evidence of Mr. George Hogarth, it is stated that he saw upwards of ninety Kelt fish in the mill lead at Grandholme, on the Don, May 6th.

[4] Salmon are said to produce 18,000 or 20,000 eggs each, and I have no doubt that a large Salmon will produce more, as one I examined a year or two ago, of about ten pounds weight, had a roe which weighed two pounds nine ounces, and the skin in which the eggs were enveloped (they were not in the loose state in which they are found just before exclusion) weighed three ounces, after all the eggs were washed from it; so that there were thirty-eight ounces of eggs. I weighed fifty of them, and found they weighed sixty-five grains. At that rate, thirty-eight ounces would give 12,788, and 300 lbs. 1,615,000; but as they would be much lighter when dried and potted than when taken from the belly of the fish, we may safely estimate that the 300 lbs. would contain

2,000,000, a prodigious number to pass through the hands of one tackle maker in a season.

Page 124

[5] From "Loudon's Magazine of Natural History."

[6] I have frequently found, when catching Trout for this purpose, that the milt and roe were not ready for exclusion; when this was the case, I put them into a wire cage, which I sunk in the water, examining the fish every week, until I found they were in a fit state for the experiment.

[7] I fancy that if the ova come in contact with the air on exclusion, they are not so readily impregnated as if they are always covered with the water, and therefore I have laid some stress on the desirableness of keeping the air excluded from the ova as much as possible.

[8] There is, however, one fact which must lead a casual observer to suppose that the ova are impregnated twelve months before exclusion. It is this: the male Par (Salmon fry) are at this season, October, full of milt, almost ready for exclusion; whilst, in the female, the ova are so small that they require a microscope to see them individually, and the whole ovary is merely like a thread, leading to the conclusion that either the milt of the male is not required for the female Par, or the ova are impregnated twelve months before exclusion. The fact is, that the milt of the Par is used to impregnate the ova of the Salmon on the spawning beds.

[9] When I commenced this paper I had no doubt that hybrids had been produced between the Sprod (sea Trout) and the common Trout; since then, having seen the fry said to be so produced, and on making some further inquiries, I find there is some doubt whether the female was a *Sprod*, or merely a white Trout, and therefore I cannot confidently assert (as some time ago I believed I could) that hybrid fish had already been produced. As some of my readers may not know what a *Sprod* is, it may be necessary to explain. In the Ribble we have a fish ascending from the sea in July and August, weighing from six to ten ounces, which, in appearance at least, is a miniature Salmon. I believe the same fish is called a Whitling in Scotland. Besides this, we have a similar but larger fish, which begins to come a little earlier, and which weighs from one to three pounds; this, in the Ribble, is called a Mort (in Scotland a sea Trout). Both these fish (if they are two species) afford splendid sport to the angler, who must never consider them beaten until he has them in the landing-net. They are also delicate eating.

Note on cross-breeding of Fish.

Since the above paper was published, the breeding of Hybrids has been successfully accomplished. I have had fish sent from two different gentlemen living on the banks of the reservoirs belonging to the Liverpool Waterworks; these were beautiful fish (three in number), more like the sea Trout than the Salmon, and the largest of them weighing two pounds. I had put them into the brooks running into the reservoirs three years before.

I also learn from a friend that a beautiful specimen of the *ombre Chevalier* (French Char) was taken out of the Rivington reservoir. About a thousand had been put there by me two years before.

Page 125

[10] Persons conversant with the habits of birds will readily comprehend me; for the sake of those who do not, I will just observe that the flight of all the Wagtails is very peculiar, being a succession of great leaps in the air (if I may be allowed the expression), which form a series of curves, the bird rising considerably at the commencement of each effort, and sinking again at the close.

[11] The intrepid and unfortunate traveller Joseph Ritchie, who accompanied Captain Lyon's expedition to Fezzan, and died there in 1819. Mr. Ritchie was a native of Otley, and an intimate friend of Mr. Garnett and his brothers. The beautiful poem from which the quotation is taken is printed in Alaric Watts's "Poetical Album."

[12] 1853.—I regret that in 1853, and for some years previous, we have not seen one. I fear they are extinct. The smaller kind are still numerous.

[13] The male Par is an exception to this rule.

[14] It appears to be a beautiful provision of Nature that mixture with water should increase the sphere of its action. Spallanzani found by actual experiment that three grains of the seed of a male frog might be diluted with a pint of water without destroying its stimulating power. See "Dissertations," vol. ii. p. 142, chap. 3, Ed. "Mag. Nat. History."

[15] Mr. Thomson, of Primrose.

[16] Assistant Keeper of Printed Books in the British Museum. Author of "Philological Essays," &c.

[17] The toad, when going to take a bee, points for a second or two as beautifully as the best-trained pointer before it strikes with its tongue.