XXV.—*On the Introduction of Salmon into the Rivers of Tasmania.* [*Read 12th October*, 1854.]

Downing-street, 2nd June, 1853.

SIR,—I have to acknowledge the receipt of your Despatch No. 152, of 13th August last, with its enclosures, reporting the circumstances under which the measures adopted by the Land and Emigration Commissioners for sending out salmon and trout spawn from this country to Van Diemen's Land had failed of success.

The Commissioners to whom I referred those papers proceeded at once to place themselves in communication with Mr. Boccius on the subject; but in consequence of ill-health, that gentleman was for some length of time unable to attend to it. A copy of his Report is now enclosed, containing some explanations relative to the last experiment; from which it will be seen that he attributes its failure to causes of an accidental nature, the recurrence of which are not to be anticipated: the result has been, that I have authorized the Commissioners to take the proper steps for renewing the experiment, under the superintendence of Mr. Boccius; and it is intended to despatch the spawn-tub by a vessel leaving England early in October next.

I have to add that the probable expense is stated at about £300, which, as in the former instance, will be charged to the Land Fund.

I have the honor to be,

Sir,

Your most obedient Servant,

NEWCASTLE.

Lieutenant-Governor

SIR W. DENISON, &c.

Broadway, Hammersmith,
4th April, 1853.

SIR,—I have carefully read through the documents handed me for perusal from His Excellency Sir Wm. Denison, Governor of Van Diemen's Land, and of other parties, relative to the experiment of transmitting salmon spawn to that colony by order and desire of Earl Grey, Secretary of the Colonies in 1851.

As the arrangements for the purpose were left to my directions and instructions, I regret to read in those documents of the failure of the undertaking, over the cause of which I had no control, but experienced much vexation, as hereafter related. I am now, however, quite satisfied that salmon spawn can be conveyed in perfect safety alive to that colony for the desired purpose of propagation. I am much gratified to read that Sir W. Denison, even with this failure, wishes a second experiment to be undertaken; and for that purpose, His Grace the Duke of Newcastle is desirous to have my views and report as to the cause of such failure, and of the cost of a future trial.

The failure I attribute entirely to the following circumstances, which I trust will perfectly exonerate me from all carelessness or want of foresight towards carrying out to perfection the desired undertaking, after and upon my system of artificial spawning, breeding, and rearing of fish.

In referring to my Diary for 1851, I find that, on the 23rd of December, I was desired to call upon Messrs. Hall Brothers, Ship-brokers; who informed me that they had chartered the barque *Columbus,* Captain Daniel Smith, for Hobart Town direct, and that this vessel was positively to sail on the 12th January, 1852; therefore it would be abso-

lutely necessary that I should have all my arrangements for artificial spawning and breeding, &c., in order and on board by the 10th of that month. These I perfected, and left London on the 7th for Worcester, to procure the salmon spawn, which I succeeded in obtaining on the 9th; and upon my arrival in London on the 10th, with my stock of salmon eggs, I found that the *Columbus* had got very little of her cargo on board, and was not likely to start for some days later. It was during the period of delay I was enabled to collect some trout spawn, from some trout out of Mr. Samuel Gurner, junior's, stream in Carshalton, in Surrey, from whom I had obtained permission to place my salmon spawn in the stream, in order to keep it alive. The vessel was detained till the end of the month, when I completed the arrangements and placed the stock on board; but even then the *Columbus* did not leave the Dock Basin until the 3rd of February, and upon arriving in the Downs she was further detained a week by contrary winds; so that the salmon spawn was thirty-three days, and the trout thirteen days, advanced in embryo previous to leaving England, instead of being that length of time on the voyage. This it will be perceived is quite sufficient to account for and show the cause of the failure, and why the brood was bred out on this side of the equator; the spawn being so far advanced, life was perfected much earlier as the vessel neared the warmer latitudes.

In consequence, however, of the detention of the vessel in England, I had prepared for the brood coming into life on the voyage, and for which occurrence I gave written instructions to Captain Smith for their provision.

The usual period for incubation of salmon spawn is one hundred and of trout fifty days in a temperature of fifty-four degrees; so that the spawn sent out in the *Columbus* was already one-third hatched, or in life, previous to leaving our

coast. Had this delay not occurred, combined with the unfortunate detention under the equator, as Captain Smith states in his report—the weather throughout being intensely hot, with a vertical sun for many days—that was from the 1st to the 14th March,—I have not the slightest doubt that this first experiment would have been perfectly successful: and although it proved a failure, the experiment produced the positive fact, that salmon and trout were bred out on shipboard and at sea, which hitherto has not only never been achieved, but negatives all the theory of every former authority upon the habits of the Salmonidæ. I am, therefore, quite certain of success in a second undertaking. In this first experiment I had great difficulties to contend with, owing to many circumstances, but chiefly produced from the deception practised by the ship-brokers, in fixing a date for the vessel sailing, when they must have been cognizant that such could not take place; in fact, if I may use the expression, it perfectly dislocated the whole arrangement: and this prolonged detention I consider the only cause of the failure.

I trust, therefore, that this truthful statement will be sufficient to satisfy His Grace the Duke of Newcastle that the system I have advanced with such general success can be put into practical operation for the requirements of the colony.

In reference to Captain Daniel Smith, when I placed the spawn in his charge, on the 31st January, I felt quite assured that he would give the strictest attention to it, and do all in his power to carry out the experiment. He is, in my opinion, a careful, upright and industrious man, and deserves every praise and recommendation for his close attention to his instructions.

I have also perused with great care and interest the able and excellent report of Mr. J. L. Burnett, assisted by Dr. Milligan, of Hobart Town, to His Excellency the Governor,

offering their opinion for the future success of a second trial, and which I will reply to seriatim. It is also with unfeigned pleasure I read that so much interest had been created in the colony, from the belief that henceforth salmon can be transmitted to stock the rivers of those regions, and form a grand acquisition for food and mercantile speculation.

In Mr. J. L. Burnett's report, at the desire of Sir William Denison, upon the introduction of salmon and trout into Van Diemen's Land, that gentleman has given a very correct statement of my arrangements on board the *Columbus*, and of the written instructions handed to Captain Smith.

Now, as regards the cause of the failure of the experiment, I believe I have gone so fully into the subject in the foregoing remarks, that it will not be necessary for me again to repeat them in detail.

But upon the subject of the change of the quality of the water, I must entirely differ with that gentleman, Mr. J. L. Burnett, and offer a few words in explanation, as I am convinced, from the practical knowledge and experience I have obtained of the water sent out, that from its own properties it could not go over to putrescence; but owing to many of the eggs of the salmon and trout becoming addled, and which remained for some time in a state of *adipocere*—then decomposition from that state taking place—as the vessel remained under the line, numbers of the brood, if not all, ceased to exist in consequence of the great heat; and thus putrescence was produced—and not only fetidness, but also the thick, slimy state of the water in the spawn-tub, as described by Captain Smith. It was therefore decomposition of animal matter, and not putrescence of the water, in which previously no animal or vegetable matter had existed.

The water sent out was taken from the purest source in

England, and has been proved so to be by men of first-rate science before a committee of the House of Commons when deliberating on the water question for the supply of London; and personally I have tested its purity. I am therefore convinced that by the time the vessel had left the equator, and had reached the cooler latitudes, the putrescence was completely washed out of the spawn-tub by the regulated supply of new water taken from the store tanks to replenish as previously, and which retained its purity and soundness to Van Diemen's Land.

As regards the water in the spawn-tub appearing strongly to be impregnated with oxide of iron, I must say I have strong reasons to believe to the contrary; and I have much to regret that Mr. J. L. Burnett and Dr. Milligan did not test the residuum, as the water sent out to replenish the spawn-tub was taken from a spring of 55 degrees temperature, rising from a depth of several hundred feet from the surface of the earth, and flowing out of the chalk hills of Surrey; consequently little impurity could exist, save carbonic acid and lime slightly in excess, which would only tend to keep the water in a purer state.

I further conceive that the discoloration of the stones and gravel in the spawn-tub must have been produced by carbonate of lime of the water, through the evaporation caused by the great heat of the tropics.

Being in doubt, therefore, on the subject, I exceedingly regret that these intelligent gentlemen overlooked this interesting test, or the examination of the tanks in which the store water had been conveyed, more especially as the tanks had been filled with Thames water many weeks previous to the refilling for the voyage, in order to season them, during which time no oxide had formed or oozed through the var-

nish with which they were covered inside. I have further, many proofs of the insulating properties of the varnish for the purpose.

I have carefully read Mr. J. L. Burnett's report, giving his views as to the defective state of the plans adopted by me. To the

1st. I must beg to remark, that never having crossed the equator, I could only make strict enquiries of parties who had for information as regarded the heat against which I had to contend, and which was the only difficulty I had to fear in transmitting the embryo fish. I learned from Captain Smith and many other parties, I might calculate upon the temperature of the atmosphere between decks at from 85 to 90 degrees under the line. Not contented with nor trusting to my own ideas of being able to keep the water at a lower temperature in the spawn-tub than that of the surrounding atmosphere, I applied to Professor Owen, our great philosopher in natural history, as well as to other scientific gentlemen. The former gentleman also thought that ice would be of service placed in the spawn-tub, to keep the temperature of the water low; but upon discussing the merits and consequences of ice to the spawn and brood, and also explaining my views, he agreed with me that my plan of evaporation, produced from the surface of the water by a supply of new water to the bottom of the spawn-tub, and arranged as I had determined upon, was likely to prove the best method.

2nd. I perfectly agree with Mr. J. L. Burnett, that if arrangements could have been made to have the spawn-tub safely housed on deck, a far greater chance would have been given to the spawn, in consequence of the greater circulation of air over the surface of the water; and I further

agree that two spawn-tubs would have been advisable; namely, one for the breeding and the other for the brood, whereby a greater scope would be obtained for the brood, and a greater facility for the removing of any that might perchance die.

3rd. The quantity of spawn was not particularly in excess, if at all, as I have kept double the quantity of brood alive for many months perfectly healthy in less space than one-half of the spawn-tub, merely delivering to them a plentiful supply of pure water; by which arrangement they remained stunted in growth, until I turned them into water with a regular supply of food.

4th. The *Columbus* made a protracted voyage of 136 days' duration to Hobart Town; so that, had the brood arrived safe, it being then late in the season, the winter floods might have caused some mischief to them—but not to the extent believed, as it is not the brood that the fresh or new water affects so extensively in the fisheries, but the spawn beds, from the earthy deposits brought down by the floods. The period of the year best suited for the transmission of the salmon spawn from England to Van Diemen's Land is October, when, from the usual run of a sailing vessel, it might be expected that it or the brood would arrive out in the summer months of those regions. An arrangement to that effect was entertained in 1851, at that period of the year, but from circumstances became deferred, and the experiment was not carried out until January 1852, as previously detailed. As regards Mr. J. L. Burnett's views of sending out smolts, I fear that they would meet with the same fate, under the equator, as those bred out on board the *Columbus*, and from the same cause, viz., the great heat of the tropics affecting decomposition of the water so

extremely rapidly, or chemically separating, by evaporation, those two gases, which combined form water, and from which cause the fish became suffocated.

5th. I do not doubt that the white salmon, so termed by Mr. J. L. Burnett, might have been conveyed to Van Diemen's Land from the Chinese waters alive, but those fish had not to pass the tropics, therefore might be conveyed safely from a hot climate to a cooler one, and subject to no prostration from excessive heat.

6th. Upon the view taken I think all the points have been examined; and, for my part, I beg to offer my humble thanks to His Excellency Sir William Denison for the kind manner in which he has viewed the experiment; also for his further recommendation to have a second trial. To Mr. J. L. Burnett I have to offer my grateful acknowledgments for the interest that he has taken in the subject, and for his very lucid report thereon.

As regards the cost of a second experiment, that would depend greatly upon the arrangements, although, I believe, if economically undertaken, as far as I should have to execute, it would not exceed £105, and a further £105 for my professional services and attendance. The cost would not include the charges of freight and attendance, &c., on shipboard, and of which I can form no estimate myself, not being connected in trading affairs. I feel, however, the necessity that every economy should be effected in such an undertaking, although I consider the value of the success to be beyond all calculation of pecuniary remuneration.

I remain, &c.,

GOTTLIEB BOCCIUS.