MANAGING MARINE LIFE: SCIENTIFIC AND PRACTICAL WORK AT TWO BRITISH MARINE STATIONS, 1884-1902.

by

Gillian Linda Gass

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Managing Marine Life: Scientific and Practical Work at Two British Marine Stations, 1884-1902.

Ph.D. thesis submitted 2006 by Gillian Linda Gass, Institute for the History and Philosophy of Science and Technology, University of Toronto.

Abstract:

The rise of a large-scale trawl-fishing industry in Britain during the second half of the nineteenth century had impacts beyond the nation's economy and food supply. As this industry, and the fish upon which it was based, became indispensable, anxieties grew that the trawlers were overfishing Britain's coastal waters, damaging young fish and sending too-small fish to market. A succession of Royal Commissions and parliamentary inquiries were uniform in their calls for marine fisheries science: scientists, with their privileged access to nature, were thought better placed than either fishermen or Commissioners to find the answers that would guide how Britain's trawl fisheries were managed. British marine naturalists like E. Ray Lankester of the Marine Biological Association (MBA), eager to establish marine research laboratories along their own coastline, saw their opportunity. In 1884, a small marine research laboratory opened at St. Andrews, Scotland, organized and staffed by William Carmichael M'Intosh; four years later, a larger coastal laboratory opened at Plymouth, organized by the MBA and staffed year-round by a Director and a Naturalist, paid workers dedicated to studying economically important food fish species. These stations had been founded in large part on the basis of promises that the scientific work undertaken there would be of practical value to Britain's fisheries. This dissertation examines what keeping those promises looked like. J. T. Cunningham, the Plymouth Laboratory's first staff Naturalist, was the

embodiment of the MBA's promises to government and industry, his work simultaneously scientific and practical in method and outcome. In his studies of the growth, development, and colouration of flatfish like plaice, sole, and flounders, Cunningham had to develop supply chains of specimens coming into the laboratory and inventories of specimens living in the laboratory's tanks. M'Intosh, too, traced the development and migrations of young flatfish while developing sophisticated biological arguments against the possibility of overfishing the sea. Both men, and both laboratories, denied the distinction between scientific and practical work on food fish; the fish themselves figured equally as organisms and as commodities, as objects of study, exchange, and consumption.

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The goodwill of those who keep the archives is necessary to the success of any history dissertation, and I was lucky to have the assistance of the kind staff of the Special Collections departments at Edinburgh University and the University of St. Andrews, Scotland. I wish particularly to thank Ms. Linda Noble and the staff of the National Marine Biological Library in Plymouth, England, keepers of the Marine Biological Association archives. Ms. Noble went out of her way to be helpful and to help me feel at home in the Library and in Plymouth, and I am very grateful for all she did on my behalf. The staff's hard work digitizing the early volumes of the MBA's Journal and posting them to the NMBL website ended up saving this project after the unexpected closure of the Zoology Library at the University of Toronto, not to mention creating a wonderful resource for those interested in the history of marine biology in Britain. I am grateful to all three archives for granting me permission to quote from their manuscript collections. Jennifer Hubbard, IHPST's previous "fish woman," not only took time to chat with me at her office at Ryerson University but also gave me a copy of Gunther's biography of M'Intosh, which proved essential. Roy Pearson, Bonnie Horn, and the staff of the Gerstein Library at the University of Toronto and the late Zoology Library went beyond the call of duty in helping me get my hands on old books and journals without which much would have been missed. Denise Horsley and Muna Salloum at IHPST were sympathetic, organized, and calming presences. Polly Winsor, my supervisor, was always ready with enthusiasm and helpful suggestions, and my other committee members, Paul Thompson, Janis Langins (thesis supervisory committee) and Ellie Larsen (specialist exam committee) were kind and interested and helped things run smoothly. My greatest debts, of course, are to those who had to put up with me during the thesis-writing process: my parents, Charlie and Carol Gass, and my brother Donnie are the best and funniest and most supportive family anyone could want, and they all clocked a lot of hours with me on the phone and on my many visits back home. Finally, my husband, Kendall Shields, helped and encouraged and made me laugh every day. There is no way this thesis could have been written without his constant support and sympathetic good nature, and I dedicate it to him.

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Preface: The Practical and the Scientific

This is a thesis about the intersection of scientific and practical work, and about the practical aspects of scientific work, in the hands of two marine naturalists at two British marine stations in the last decades of the nineteenth century. The naturalists, J. T. Cunningham (1859-1935) and W. C. M'Intosh (1838-1931), unlike the vast majority of the many British naturalists interested in marine organisms, found themselves in the enviable position of working year-round in close proximity to the sea, with unmatched access to the many specimens they needed for their work. Both Cunningham, as staff Naturalist employed at the Marine Biological Association Laboratory in Plymouth, England, and M'Intosh, Professor of Natural History at the University of St. Andrews, Scotland, and founder and head of the St. Andrews Marine Laboratory, made numerous contributions to the study of marine food fish. Their work identifying and studying the development, growth, and habits of the Pleuronectidae – flatfish such as the flounder, plaice, sole, turbot, and brill – occupied the majority of their time and comprised the majority of their published output for scientific and general audiences.

Cunningham's and M'Intosh's flatfish researches did more than satisfy their scientific curiosity about these decidedly odd-looking fish: their articles, books, and lectures were also the fulfillment of public promises made on behalf of the new marine laboratories in which they worked, promises that these laboratories would produce results that were relevant and useful to the nation's booming fishing industry. During the second half of the nineteenth century, fishing in Britain had been transformed from a small-scale, traditional activity providing food and income to coastal villages and the fishing families that lived there, into a large-scale industry dominated by capital-intensive steam powered boats and port city-based corporations that was of paramount economic importance to the nation as a whole. Not only was the fishing industry responsible for a huge number of jobs at the coasts, along the railways that carried the fish inland, and in the urban fish markets where the customers lived, but it was essential to the nation's food supply, which in the absence of affordable meat had come to rely heavily on trawled fish. By the last two decades of the nineteenth century, the enthusiasm for Britain's booming trawling industry had become dampened by anxieties that the fish left in the ocean might not last long. Fishermen and trawler-owners called for government commissions to investigate and legislate; successive government commissions, finding themselves without information on which to base their findings, called for statistics to be collected and objective studies to be made; and certain of Britain's increasing population of marine-oriented biologists, in particular E. Ray Lankester, argued and promised publicly that coastal biological laboratories, similar to those already established in other coastal nations, could provide the practical results needed to properly understand, regulate, and improve the fisheries.

Lankester's promises led to the founding of the Plymouth laboratory in 1887, and to the hiring of J. T. Cunningham to provide a steady stream of scientific and practical results on the biology of food fishes. The laboratory was paid for by a large number of donations from an assortment of sources, including the government, the fishing industry, and the scientific community. The much smaller St. Andrews laboratory, part of the venerable University and expected to be used by faculty and visiting researchers, had been founded three years previously, on the strength of M'Intosh's recent contribution of some preliminary scientific studies of the effects of trawling to a Royal Commission called to investigate the subject, and with the encouragement (and a small sum of money) from another government body, the Fishery Board for Scotland.

The work done by Cunningham and M'Intosh under the auspices of these new stations, then, was expected to be both scientific and practical in character. This thesis explores what the combination of scientific and practical work – what we might today call pure and applied scientific work, or technoscientific work – looked like, at a variety of levels of their scientific practice. I explore the workplace pressures experienced by Cunningham as he pursued and published his studies of economically important fish species; the processes by which and structures through which the naturalists obtained their specimens from the sea; the difficulties of identifying fish embryos and keeping specimens alive in the laboratory; Cunningham's work on two apparently distinct projects that nonetheless shared the same epistemological anxieties and indeed the same population of specimens under study; Cunningham's and M'Intosh's presentation of food fish biology to a popular audience interested, as were the naturalists and their sponsors, in fish "Both as an Organism and as a Commodity";[1] the development of a specimen supply service transporting marine organisms from Plymouth to the many inland laboratories and classrooms, in parallel to the inland transport of food fish from quay to inland market; and M'Intosh's public arguments for the value of food fish biology precisely to show that no government action was necessary. In each of these theatres of scientific activity, I find close links of unexpected kinds between the scientific and the practical, showing what the promised interaction of the two kinds of work actually looked like in

[1] As in the title of Cunningham's first book, *A Treatise on the Common Sole (*Solea vulgaris*), Considered Both as an Organism and as a Commodity*, (Plymouth: Marine Biological Association of the United Kingdom, 1890).

action, and revealing some remarkable features of the practical side of all scientific work, no matter what its stated aims.

The relationship between pure and applied science was a concern to Cunningham, M'Intosh, and many others during this period of new interaction between biology and industry in Britain. That same relationship has long been a significant area of inquiry for historians of science, and in particular historians of technology, who have sought to establish, either once and for all or uniquely for each historical context, what the relationship between these types of work might be. Our contemporary view, which sees a close and probably two-way interaction between, for instance, medicine and biology or chemical engineering and chemistry, is generally thought to have taken shape during the late nineteenth century, although recent scholarship has suggested an earlier origin.[2] This close interaction has been nicely defined recently by Ursula Klein as the "systematic and stable interconnection of scientific and technological practices and institutions into a 'technoscience'."[3]

Klein's paper is the most recent contribution to a long-running discussion about how best to deal with pure and applied science or science and technology as historians. In his 1985 work *Technology's Storytellers*, John Staundenmaier identified the science-technology relationship as an active and promising area of inquiry for historians of

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[2] Ursula Klein, "Technology avant la lettre," *Perspectives on Science* 13 (2005): 226. Klein, however, finds that in her own area of historical expertise, organic chemistry, even in the eighteenth century that field was engaged in "an early form of technoscience" (226). See also Wolfgang Lefèvre, "Science as Labor," *Perspectives on Science* 13 (2005), who has some interesting things to say about what the interaction of science and technology means for our ideas of science as production, especially for those sciences, like modern biotechnology, that produce the objects that they then study.

[3] Klein, 226. The term "technoscience" was coined in Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society*, (Cambridge: Harvard University Press, 1987), 174, and defined somewhat less precisely as "all the elements tied to the scientific contents no matter how dirty, unexpected or foreign them seem," as against our traditional, tidier view of the ideas and inventions of "science and technology."

technology, and devoted a long chapter to chronicling the work done so far.[4] A more recent essay by Sungook Hong expresses frustration at historians' repeated attempts to demonstrate a definitive science-technology relationship using single case studies, only to have another author reinterpret these same case studies as evidence for an entirely different sort of relationship.[5]

Neither Hong nor Staudenmaier give sufficient attention to another historiographic essay, Otto Mayr's 1976 "The Science-Technology Relationship as a Historiographic Problem".[6] In its first presentation as a talk at a 1973 colloquium, Mayr called for more detailed case studies to clarify the nature of the science-technology relationship. Subsequently, Mayr revised his paper to argue that searching history for clues to the science-technology relationship is "illusory and of necessity bound to fail".[7] Instead, historians should seek insights into their own debates by studying how terms and relationships are defined differently in various historical contexts. Ronald Kline, undertaking just such task in his 1995 study of late-nineteenth and twentieth century engineers' and scientists' interpretations of the science-technology relationship, claimed to be one of the few answering Mayr's call.[8]

Any historian attempting to answer Mayr's call faces a tricky task, but it is not really much trickier than the historian of science's usual work of taking past science on

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[4] John M. Staudenmaier, *Technology's Storytellers: Reweaving the Human Fabric.* (Cambridge: Society for the History of Technology and MIT Press, 1985).
[5] Sungook Hong, "Historiographical Layers in the Relationship Between Science and Technology," *History and Technology* 15 (1999).
[6] Otto Mayr, "The Science-Technology Relationship as a Historiographic Problem," *Technology and Culture* 17 (1976).
[7] Mayr, 664. Mayr's major overhaul of his paper "by virtue of the author's reactions to the sessions" is revealed in Nathan Reingold and Arthur Molella's "Introduction", *Technology and Culture* 17 (1976). His original argument is revealed by the discussion transcripts, reprinted on pp. 672-673 following Mayr's article, particularly through Thackray's comments.
[8] Ronald Kline, "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880-1945," *Isis* 86 (1995).

its own terms rather than substituting present ones. In this thesis, I was presented with a wealth of opportunities to demonstrate how the actors themselves construed the relationship between biology and the fisheries as it was developing and becoming closer. Rather than using "technoscience" or even "pure and applied science", both twentieth-century labels for the relationship in question, I have made use of the participants' own terms, which included such pairs as "scientific and economic", "theoretical and practical", and "pure science and practical science". Identifying individual studies of Cunningham's as particularly scientific or particularly practical, as I do in Chapter Four only to then demonstrate that in the doing they were all of a piece, was again done as much on the terms of the participants as possible: for all they argued about the inseparability of scientific and practical work, it was clear to all what counted as practical, especially (again as in Chapter Four) when industry and government were demanding an answer to a particular question.

Sticking with the language of "practical" work, too, allows for another dimension of the thesis to emerge. Several of the areas of practice in which I investigated the interaction and intersection of scientific and practical work were decidedly prosaic in nature, decidedly easy to overlook: the practical aspects of doing scientific work, whatever the purpose. The historiographic focus on laboratory practice, instruments, experiments, and organisms in recent years has called our attention to many of the previously taken-for-granted participants in scientific work. In the process, the work of doing science – never mind the conceptual difficulty of the subject matter – has come to be seen as vastly more complicated than previously thought, involving a surprisingly large cast of characters. In the case of biological work, and especially in the case of laboratory-based study of live or fresh specimens like the work done by Cunningham, M'Intosh, and by a large and growing number of other British naturalists, establishing a reliable and plentiful supply of specimens was essential to making any progress studying marine organisms. In reconstructing the lines of supply, the "specimen supply chains" that kept the desired specimens moving from the sea to the coastal laboratories, and further inland to the schools and universities distant from the shore but in need of marine specimens, I reveal the series of social and working relationships, the connections between the scientific and fishing communities, that made possible the constant traffic of organisms from the sea where they lived to the laboratories where they were studied.

The notion of fish, such as the sole that were the subject of Cunningham's first book, as both "organism and commodity", might seem to map neatly onto the other dyad motivating this thesis, that of scientific and practical work, with fish being considered organisms for the purposes of scientific knowledge and a commodity for the purposes of advising government and industry on the nation's fisheries. But not so: the identity of the fish, for all those who dealt with them, was a complex of organism and commodity. The fishermen caught fish for sale at market, agitated over fishing practices they thought to be destructive, took naturalists out to sea on their boats, and learned to perform artificial fish fertilizations while at sea in order to bring back live embryos to the laboratory. The naturalists struggled to keep fish embryos alive in the laboratory for use in experiments, for display in the public aquarium, and by way of developing techniques that could later be used to establish commercial sea fish hatcheries; they sought out suppliers for the specimens they needed and worried about controlling the quality of the specimens sent inland. Everyone concerned interacted with the fish in complex ways that are barely suggested by the idea of "organism and commodity", but are revealed by paying close attention to the practical aspects of studying fish scientifically.

In Chapter One, I demonstrate the transformation that took place in Britain's fishing industry during the second half of the nineteenth century, which has been well catalogued by fisheries historians such as Robb Robinson. The industrialization of fishing, with the spread of trawling, the expansion of the railway networks, and the rapid growth of inland urban markets for fresh fish, culminated in the acceleration of the process with the advent of steam-powered trawlers, large corporations, and major ports like Hull, Grimsby and Aberdeen, and gave rise to widespread concern within the fishing industry that the fish supply might run out. I illustrate the magnitude of the transformation by following the fortunes of two flatfish species: the sole, which had always been sought after in urban areas as a luxury fish for wealthy consumers; and the plaice, which had once been considered "offal", an unsellable coarse fish not wanted except in the coastal villages in which it was caught, but which became a staple food for the poor in inland urban areas, especially in the form of fried fish and chips. Alongside this changing economy for food fish, I develop an account of the changing economy of British biology, which was developing its own appetite for marine organisms as a site of inquiry into fundamental biological questions like evolution and development. I show how these two growing economies came together to form the context in which two of Britain's marine laboratories were founded, with the public promises of benefits to the fisheries made in the process becoming the background for the researches done by two naturalists, Cunningham as an employee at Plymouth and M'Intosh as a professor, public figure and founder at St. Andrews.

Having established this essential context, in Chapters Two and Three I explore the intersection of scientific and practical work at the level of working conditions and the logistics of obtaining a supply of specimens. Cunningham was hired to embody and carry out the commitments made by the Marine Biological Association, founders of the Plymouth laboratory, to provide practical benefits to the fisheries by supporting scientific work on food fish species. To that end, his work was supervised, and his publications expected to support the Association's new journal. His notebooks include a surprising amount of record-keeping amidst the lab notes: expense reports, a time-clock of sorts recording time in and time out each day, and explanations of absences. Though under the thumb of the Laboratory's series of Directors and the Association Council, Cunningham benefited from unparalleled access to the variety of marine organisms, particularly fish, required for his studies. Like M'Intosh, Cunningham had an assistant, a laboratory fisherman; both naturalists worked to establish connections within the fishing community and among other naturalists to obtain the specimens needed for their studies. In Chapter Three I develop the notion of the "specimen supply chain", and reconstruct several of these multi-element linkages between the naturalist and the fish, between the laboratory and the sea.

In Chapter Four, I follow Cunningham in the course of two simultaneous investigations carried out using the same population of laboratory-dwelling flounders. In one study, situated in the context of traditional scientific interest in the flatfish group as exemplars of the evolution and development of asymmetry and the role played by the "conditions of life" in guiding both processes, Cunningham exposed the lower white side of the flounders to light and was able to induce colouration on the exposed side, in a series of experiments lasting several years. In another, done in the context of calls from the trawling industry for government to establish minimum allowable landing sizes for flatfish species in order to protect young fish from being killed before they could reproduce, Cunningham observed and measured the laboratory flounder population over a number of years. Seeking a relationship between fish length and sexual maturity, Cunningham used the laboratory flounders to stand in for a range of other flatfish species that were not so easy to maintain in the laboratory. I demonstrate that, despite the very different contexts motivating these studies, Cunningham's concerns about each – about the role played by the conditions of life, and the risks of having laboratory fish stand in for wild fish – were remarkably similar, showing the very close relationship, on the level of both theory and practice, of the scientific and practical work undertaken by Cunningham.

Chapter Five maintains its focus on the organisms while moving to yet another area of scientific practice, first showing how Cunningham's and M'Intosh's popular books used the general public interest in food fish – prompted by the trawling anxieties as well as wildly successful Fisheries Exhibitions in London and Edinburgh – to sell another commodity: marine biology, as practiced at Plymouth and St. Andrews. Part of the work of such publications was to show how the work done there was essential to the fisheries and to the scientific communities alike. In this chapter I explore another way in which the Plymouth laboratory made itself essential to British biology; revisiting the notion of the specimen supply chain, I show how the chains connecting the sea to the coastal laboratory were extended further inland as the Marine Biological Association developed a specimen supply service, in the course of a few years becoming an essential source of the marine organisms used in the laboratories and classrooms of Britain's many inland colleges and universities. In this chapter's third section, I examine the recent "animal studies" trend in the history of biology, which has called attention to the ways in which organisms can influence the scientific work through which they are studied, especially through the process of being brought to live inside laboratories. I consider, too, how Cunningham's flounders might fit into the largely twentieth-century category of "model organisms" like the mouse and the fruit fly.

Chapter Six shifts focus into the realm of scientific argument, detailing M'Intosh's remarkable views on the alien nature of the sea. Arguing against contemporary methods of argument by analogy as much as against contemporary fisheries science, M'Intosh maintained that the sea's creatures – that is, the nation's fish supply – were immune to human influence by virtue of being fundamentally different in environment and in biology from the better-known creatures of the land, air, lakes, and streams. The practical value of scientific work on the fisheries, then, was not to specify how the fishing industry's activities should be limited, but instead to establish why no regulation was necessary. This view set M'Intosh against the scientific mainstream, and particularly against the views of workers at the Plymouth laboratory; it also sets him against current views about the influence of humans on food fish populations. In this chapter, I demonstrate the bases on which he built his view of "The Resources of the Sea", and track the hardening of his views, and his criticisms of the actions of the Fishery Board for Scotland, in the years after its publication.

In each arena of scientific practice explored in this thesis, I show how the relationship between scientific and practical work was made manifest in the work done by and around Cunningham's and M'Intosh's studies of flat fish. This relationship was inherent in the fish themselves, in the particular set of economies in which they were found during the closing decades of the nineteenth century in Britain. They were the focus of fishing effort and scientific study; getting them, keeping them, and studying them was a process that involved a range of people, institutions, and technologies in addition to the naturalists who are the main focus of this study. Even in these earliest days of British fish biology done in the context of practical concerns and commitments, the scientific and the practical were intertwined at every level and in every area of scientific practice.

Chapter 1

Introduction:
The Economies of Fish and Fishing in Nineteenth Century Britain

Industrialization, and the economic and social transformations that came along with it, was old hat to the nineteenth century by the time its effects were felt by Britain's sea fisheries, but this lack of novelty did not render the changes that took place during the second half of that century any less rapid or bewildering. The massive scaling-up of fishing efforts, made possible by a series of technological innovations as well as national and local economic conditions, caused a pervasive reorganization of what it meant to "follow the fish", and produced a redefinition of nearly every traditional category related to fishing. Fishermen, formerly owners or apprenticed future owners of their boats, became waged labourers in the employ of limited liability companies owned by merchants and railwaymen; fishing boats, formerly sail-powered craft often small enough to pull up on the shore at day's end, became large, steam-powered, capital-intensive undertakings requiring sophisticated harbour facilities; and trawling, formerly a fishing method of dubious merit and definite drawbacks, became by far the dominant mode of catching fish at the largest English and Scottish ports and the backbone of the nation's supply of white fish, carried inland by rail. At least as dramatic were the redefinitions that took place in categories related to the fish themselves. As what had been a luxury food item, limited to a few species of fish and a small group of consumers, became additionally a staple of the inland urban working class diet, species that had once been classed as unwanted by-catch or "offal" became instead economically important commodities, while the luxury fish went from expensive rarities to attainable treats and back again as their numbers dwindled. The life history of these animals was redefined as well: with the rise of concerns about the impact of trawling on the populations of both marketable and juvenile fish, knowledge of the development, habits and habitats of food fish shifted to become a topic of interest, and indeed some urgency, not only to science but to business and government as well.

All of these transformations played a part in the nineteenth century careers of two flat fish, the sole and the plaice (Figure 1). The sole, along with the flatfish turbot and brill, had for some time been a luxury food item, brought to market in small quantities and destined for the tables of the upper classes; the industrialization of fishing increased its numbers at market, but its status as a food fish was never in doubt, save for fears that, as one observer said in 1904, "a future generation…shall perhaps know the sole only in the museum."[1] Plaice enjoyed a more Dickensian trajectory, its humble standing as by-catch left behind as it became a sought-after food fish in the country's rapidly mutiplying fish and chip shops, which were supplied by the trainloads of fresh white fish brought to port by fleets of steam-powered trawlers. By 1894, the nation's first sea fish hatchery at Dunbar, Scotland was turning out tens of millions of young plaice to re-stock the sea with this essential foodstuff and economically vital commodity.[2]

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[1] F.G. Aflalo, *The Sea-Fishing Industry of England and Wales*, (London: Edward Stanford, 1904), 364.
[2] J. T. Cunningham, *The Natural History of the Marketable Marine Fishes of the British Islands*, (London: Macmillan and Co, 1896), 30.

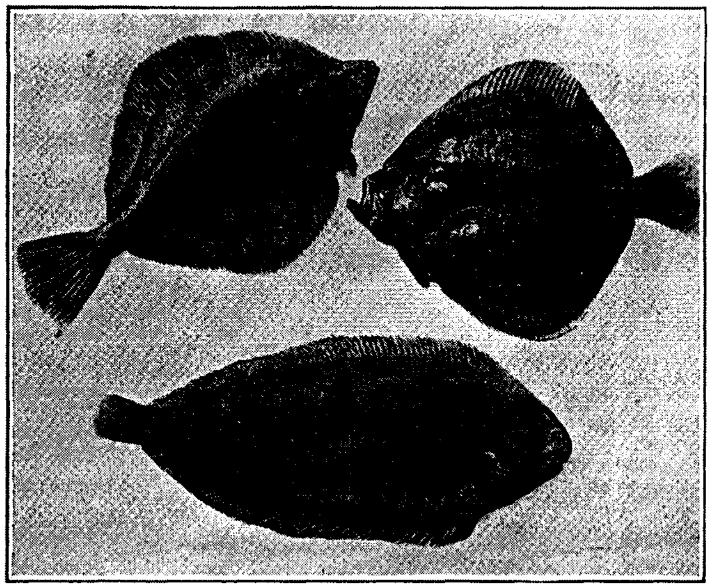

Figure 1. Sole, turbot, and plaice: two luxury fish and a coarse fish. Sole and turbot were sought-after fish throughout the nineteenth century and were carried inland along the roads, while plaice, once not worth transporting, became a staple food for the inland urban poor during the latter half of the century, when it was caught in huge quantities by trawlers, carried inland by railway, and sold in fish and chip shops. Counter-clockwise from bottom: sole, turbot, plaice. Photograph from Aflalo, *The Sea-Fisheries of England and Wales*, 1904.

The fortunes of both plaice and sole as targets of fishing effort and research effort – their status as commodities, as natural populations, and as objects of scientific inquiry – were tied to the significant changes in how the sea's fish were pursued, exploited, and understood from the eighteen forties onward. In the second half of the nineteenth century in Britain, fish became important. They also became, or at least were seen as becoming, threatened. In this context, the scientific study of fish – marine biology – became important, too, because of its privileged access to knowledge about the fish, knowledge that would help such interested parties as the fishermen, the government, and the fish-eating general public understand the seriousness of this threat to the fish. Science promised to protect the fish, and so the nation's food supply and an important industry, from harm. The story of food fish in Victorian Britain takes us from railways to marine research stations, from fish moving inland to biologists going to sea, all in just a few decades.

The Fisheries Before the Railways Came

In his 1988 book *The Provident Sea*, D. H. Cushing wrote that "The mechanization of fishing and the standardization of fisheries products came later in the industrial revolution and the reason is an obvious one, that, however abundant, coal costs money but wind and tide are free."[3] There is certainly some truth to this statement, and we'll see later how several forces combined to bring steam power to the forefront in the fishing industry. What's less obvious, though, is why fishing was something that had to become mechanized and standardized – industrialized – at all. The reasons for the scaling-up of Britain's sea fisheries are linked closely with the rise both of demand for sea fish and the means to supply fish rapidly and cheaply enough to meet and increase that demand. Britain was an island nation, and had long been a seafaring nation, but it was not on the whole a seafish-eating nation until after the middle of the nineteenth century.

Writing about the sea fisheries of the southwest of Britain, John Rule paints a vivid picture of the relative absence of fish from the diet of all but a small segment of the inland population. A *Quarterly Review* commentator in 1813 wrote that despite the great expense of other types of meat, fish, and fresh fish in particular, was "a prohibited article, even to the middling ranks of life."[4] London, home to many of the middling and lower ranks, had Billingsgate fish market, but its purpose was mainly to supply luxury fish such

[3] D. H. Cushing, *The Provident Sea*, (Cambridge: Cambridge University Press, 1988), 102.
[4] *Quarterly Review*, IX (1813): 166, 275-7, quoted in John Rule, "The South-Western Deep-sea Fisheries and their Markets in the Nineteenth Century," *Southern History* 22 (2000): 164.

as sole and turbot to wealthy customers, and the fishmongers were not there to supply fish to the poor: "They [the "mass of the City's inhabitants"[5]] would as soon think of going into a shop to ask the price of a pineapple as ask that of cod, turbot or salmon."[6] Rule points out that, though they did not frequent the fishmongers', the London masses could get fresh fish such as mackerel for at least part of the year from costermongers and hawkers, who sold fresh fish and produce from baskets, rather than from fixed store locations as did the fishmongers, so most people's access to fish was perhaps not exactly "prohibited". However, fully thirty years later the *Quarterly Review* was still decrying the absence of fresh fish from the diet of inland Britons, most of whom "know not the taste of fresh sea fish."[7]

Luxury fish were a different story. Sole and turbot were sought-after delicacies, and could be sold at high prices in London. They could fetch so much that it was worthwhile for fishermen to leave their home ports, such as the Devon ports of Brixham and Plymouth, and pass the season fishing sole and turbot off of Dover, where the soles they caught could be landed and transported by land to Billingsgate in fast fish "vans". Southwest fishermen began this migration in the 1820s.[8] In addition to sole and turbot, they sometimes caught plaice, but plaice had no cachet as a luxury fish, and there was no guarantee that anyone would want their catch: "Usually all the light van transport available was needed for the prime varieties and while good fresh plaice might in some

[5] Rule, "The South-Western Deep-sea Fisheries," 164.
[6] *Quarterly Review* IX, quoted in Rule, "The South-Western Deep-sea Fisheries," 165.
[7] *Quarterly Review*, LXIX (1842): 231-2, quoted in Rule, "The South-Western Deep-sea Fisheries," 165.
[8] Rule, "The South-Western Deep-sea Fisheries," 168-169; Cushing claims that this annual migration began in 1812, driven partly by "increased demand for fish in London in the early nineteenth century" and partly by the discovery of "an abundance of turbots of the Ridge and Varne banks in mid Channel" near Dover (104).

years fetch 5s. the hundred of six score, just as often it could not be sold at all."[9] Plaice, along with such white fish as haddock and coalfish, was considered at that time to be "offal" fish, and Robb Robinson argues that it did not constitute part of the diet of the inland poor because it "could not bear the cost of distant transport…Much fish only came into their price range because its quality had deteriorated making it 'generally half rotten and consequently most unwholesome and disgusting food'."[10] The price that could be gotten for sole and turbot meant that it was worthwhile to pay for overland transport to get these fish to the London fishmongers or to the fishmongers in Bath and Bristol who were also suppliers of sole and turbot to the wealthy outside of London. Hundreds of loads each year of sole and turbot catches made the journey from the coastal town where they were landed, to the nearest larger city, and from there to the major fish markets, such that a fish might come ashore at Brixham and first go to Exeter before going on to London or Bristol.[11] As well, popular resort towns such as Scarborough,Yorkshire were likely places to sell a catch of sole or turbot. Fish smacks originating in Ramsgate, Dover, and Plymouth began landing catches at Scarborough in the 1830s to take advantage of this market, to the chagrin of the local fishermen, who formed mobs, attacked and in one case stabbed these intruding fishermen, before reaching an uneasy peace.[12] In Scotland, fresh white fish caught by boats originating in the coastal villages up and down the shore from Edinburgh could be brought to market in the city.

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[9] Rule, "The South-Western Deep-sea Fisheries," 169.
[10] Robb Robinson, *Trawling: The Rise and Fall of the British Trawl Fishery*, (Exeter: University of Exeter Press, 1996), 11. The quotation within the quotation is from the British Parliamentary Papers, 1817, XIV, 383, Papers Relating to Salt Duties.
[11] Rule, "The South-Western Deep-sea Fisheries," 172.
[12] Robb Robinson, *Trawling*, 21.

The majority of white fish caught during this period in England were caught by lining methods (such as hand-lining, long-lining, and great-lining), in which a fishing line, up to two hundred fathoms long and strung with many baited hooks was set along the sea bed; the fish thus caught would sometimes be carried to London in the flooded holds of the smacks to keep the fish as fresh as possible.[13] Trawlers had been in use in England since the end of the previous century, but did not surpass lining as a method of capture until the 1880s.[14]

In Scotland, the major commercial fishery had long been for herring, which were cured and sold mostly overseas; it was this fishery that the Fishery Board for Scotland was created in 1809 to promote. Beginning in 1820, the Fishery Board's mandate expanded to include a role in promoting and developing white fisheries, in particular the cod fishery.[15] White fish had been caught mainly for subsistence by people living on the coasts, and always by lining from small boats, during the herring off-season.[16] Since white fish were less important commercially than herring and because they "involved less spectacular peaks of activity than did the herring and salmon fisheries, and showed lesser variations in abundance", James Coull has argued, it is tempting to think that white fish

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[13] Robinson, *Trawling*, 7.
[14] Robinson, *Trawling*, 23.
[15] James R. Coull, "The Role of the Fishery Board in the Development of Scottish Fishing Harbours c. 1809-1939," *Scottish Economic and Social History* 15 (1995): 29. Anna Gambles has suggested that the British government's decision to actively encourage economic development via the fisheries in Scotland and Ireland, using "direct public expenditure to encourage a commercialized fishing sector in economically backward coastal areas of the United Kingdom" despite an overall policy of deregulation and *laissez-faire*, was an exercise in nation-building and unification, "a new emphasis on the need to consolidate the 'metropolitan' empire of the British Islands through constructive economic policies that would tend to galvanize the political status quo." Anna Gambles, "Free Trade and State Formation: The Political Economy of Fisheries Policy in Britain and the United Kingdom circa 1780-1850," *Journal of British Studies* 39 (2000): 292-293.
[16] Coull, "The Role of the Fishery Board," 35.

were less important in Scotland generally, but this was not so.[17] Coull argues that well before the nineteenth century, "line fishing for white fish was emphatically the main activity of hundreds of fishing villages",[18] and Scottish dialects were rich in terms describing these fish, with distinct terms for sub-species and for different stages of development of the fish.[19] During the nineteenth century, however, there was "a gathering momentum of expansion in white fisheries".[20] Even before the widespread use of trawling and the advent of the railways, the white fisheries in Scotland were increasing in importance. The port of Peterhead, which had long been of importance in the herring fishery, was by 1840 earning more than twice as much money from white fish.[21]

Though most fishing during the period prior to the 1840s was carried out using baited lines and relatively small boats, a minority of fishermen made use of the beam trawl (Figure 2). In beam trawling, instead of fishing with long lines of baited hooks, the fishing boat uses a single large net, towed behind the boat and held open by a large wooden or metal beam. This mode of fishing had in fact been available for a long time – since as early as 1376 – and as long as it had been around, it had been upsetting people, with commissions being set up and rulings coming down from various levels of government in 1376, 1491, 1622, 1630, and so on.[22]

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[17] James Coull, *The Sea-Fisheries of Scotland: A Historical Geography*, (Edinburgh: John Donald, 1996), 79.
[18] Coull, *The Sea-Fisheries of Scotland*, 3.
[19] Coull, *The Sea-Fisheries of Scotland*, 79. Coull reports that in 1989 a pamphlet was produced presenting some of this rich Scottish fish vocabulary.
[20] Coull, *The Sea-Fisheries of Scotland*, 93.
[21] Coull, *The Sea-Fisheries of Scotland*, 97.
[22] Robinson, *Trawling*, 15-16.

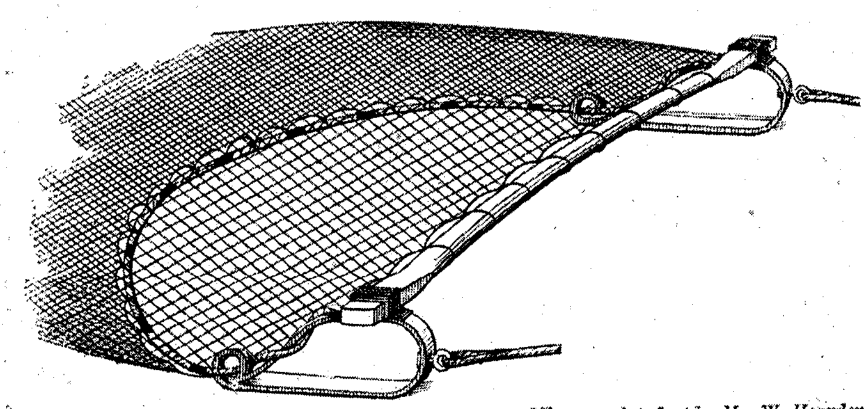

Figure 2. The beam trawl was dragged along the sea floor by boats powered by sail or steam, and used to catch whitefish like plaice and cod. The long, weighted beam kept the net open and submerged; fish were swept into the net and gathered at the tapered, closed "cod-end" (not shown). Illustration from Aflalo, *The Sea-Fisheries of England and Wales*, 1904.

However, trawling did not become an established practice anywhere in Britain until the late eighteenth century, and even then only in Devonshire. There was simply no demand for the number of fish, and the kinds of fish, which included not only sole and turbot but every kind of offal fish as well, that the trawl-net was capable of catching. Recall that the cost of inland transportation was high, and only worthwhile for luxury varieties like sole and turbot; what's more, until the twentieth century Britain had to import much of its ice from Norway, which added yet another expense to the transport of fresh fish.[23] Robinson argues that

> [t]rawling found little favour in many coastal districts because the trawl net caught large quantities of less saleable fish – the so-called offal varieties. On the other hand, long lines and hand lines were popular with fishermen because they mainly took the largest and most sought-after fish – the prime varieties. Contemporary economics dictated that the hook rather than the net was the best way to catch white fish.[24]

[23] Cushing, *The Provident Sea*, 106.
[24] Robinson, *Trawling*, 13.

Trawling caught on in Devonshire despite all of this because of the access of much of its population to the coast, removing transportation costs from the equation. True, fish landed along the Devon coast had to make a long, expensive road trip if they were to be sold at Billingsgate in London, but this was only an issue for sole and turbot anyway. For all of the other fish, local markets were nearby: Robinson points out that "no centre of population in that county – then the third most populous in Britain – was more than thirty miles from the coast. Many of the cheaper varieties of fish could therefore find markets nearby."[25] The "contemporary economics" of Britain as a whole might not have supported the widespread adoption of trawling, but the local economics operating in Devonshire made trawling an appropriate choice there. When the famously migratory fishermen of the Devon ports (many of whom were among the fishermen who annually fished and eventually settled Newfoundland) saw the opportunity to take advantage of the local economics of other places, they did so.[26] The disruption at Scarborough mentioned above is one such story: during the summer, the population of Scarborough increased greatly, not only with wealthy summer residents but with the households' worth of servants that had been brought along. A market thus existed both for prime and not-so-prime fish, and beginning in 1831 two trawling smacks from Plymouth began landing their catches at Scarborough for the summer tourist season. This action roused the anger of the local Scarborough fishermen not just because the Plymouth fishermen were not locals, but mainly because they were trawlers and their presence upset the local fish economy; Robinson comments that "[s]ometimes, the sheer quantity of fish landed by

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[25] Robinson, *Trawling*, 17.

[26] Margaret Gerrish, "Following the Fish: Nineteenth-Century Migration and the Diffusion of Trawling," in *England's Sea Fisheries: The Commercial Sea Fisheries of England and Wales since 1300*, edited by David J. Starkey, Chris Reid, and Neil Ashcroft, 112-118 (London: Chatham, 2000).

just these two trawlers had a detrimental effect on prices."[27] The market for fish in Scarborough had been accustomed to certain levels of supply and demand, and the arrival of trawling in response to the high demand had the power to noticeably change the supply of fish, even though there were only two trawlers. Needless to say, once peace had been reached with the local liners, more trawlers arrived in Scarborough, from Plymouth and the Kent ports of Dover and Ramsgate – to which the Devon fishermen had first migrated in 1820 in search of soles for the London market. By 1840, two trawling smacks were registered in Scarborough itself, and Devon trawlers had begun landing fish as well at the port of Hull on the Humber River. The technology had become established in Devonshire, and transferred from Devonshire to Yorkshire, by virtue of the favourable local fish economies present at both places.

Trawling was a faster, easier way to catch vastly more whitefish. It did away with the laborious process of baiting the hundreds of hooks on each long line, as well as with the necessity of obtaining and shelling mussels with which to bait the lines. On paper, then, it would seem like trawling should have spread like wildfire and quickly become the sole (so to speak) means of catching fish, rather than lurking around for centuries, eventually finding local acceptance in Devonshire, and only belatedly becoming dominant towards the end of the nineteenth century in Britain. But not so. Robinson's reference to "contemporary economics" is revealing: historians of technology have for some time recognised that the adoption and spread of a particular technology, no matter how obviously advantageous it might appear, is governed by numerous factors that operate in particular places and times. The sum of these factors or forces might usefully be called the "contemporary economics", the economy or environment in which a

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[27] Robinson, *Trawling*, 21.

particular technology (here, the beam trawl) or activity (here, catching fish by trawling) finds itself either ignored or important. The history of trawling in Britain is an excellent example of how error-prone the "inkblot" model of technology spread can be, and the necessity of understanding the role played by the local contexts in which a technology is situated.[28] Erik van der Vleuten has described this traditional model as "the more or less implicit deterministic view...that the superior new...technology [will] spread out like an uncontrollably expanding inkstain on the map of Europe pushing away the inferior traditional technology."[29] The pattern of spread illustrated by trawling in Britain has more in common with a model now more commonly held among practitioners of the history of technology, such that "the basic scheme of a new technology might follow different diffusion patterns at different localities,"[30] due to the particular social, economic, and practical conditions particular to each locality.

By 1840, then, trawling technology, which had been available in various forms for centuries, had found local acceptance in Devonshire and in two other regions, Dover/Ramsgate and Scarborough/Hull, to which the Devon fishermen had migrated in 1820 and 1830 respectively. All three regions had in common the presence of a fish market where many varieties of white fish, not just the prime varieties best caught by

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[28] An article by Robinson subtitled "The Diffusion of an Innovation" seemingly adopts such a model, and certainly the included map showing the gradual northward spread of trawling in the North Sea takes an "inkblot" view of the spread of trawling. However, the article's text, with its description of the Devon fishermen going first to Kent and then to Scarborough in small numbers suggests a more sophisticated account of multiple centres of radiation of this technology – a series of inkblots, at least. (Robb Robinson, "The rise of trawling on the Dogger Bank grounds: The diffusion of an innovation," *Mariner's Mirror* 75 (1989).) Gerrish's article on the "diffusion" of trawling likewise does not commit to a particular model of technological diffusion, but instead traces the movements of people between Britain's trawling ports.
[29] Erik van der Vleuten, "Steam, Styles, and States: Steam technology in the Netherlands and Denmark during the Industrial Revolution," final project, Eindhoven University of Technology, Netherlands, 1992, 1. For a survey of classic literature on technological diffusion, see David J. Jeremy, *Transatlantic Industrial Revolution: The Diffusion of Textile Technologies between Britain and America, 1790-1830s*, (Cambridge: MIT Press, 1981), 2-3.
[30] Van der Vleuten, 1.

lining, could be sold without having to be transported over land for any distance. But its local success was just that: trawling was not the dominant mode of catching these fish, and did not become the dominant mode until the contemporary economics of Britain changed with the coming of the railways. The railways, and the new set of rules for sending fish by train that redefined fish as no longer a luxury product but instead as a mass commodity, formed a link between the huge supply of fish promised by trawling and the huge demand offered by the booming industrial cities of inland Britain.

The Railways and the Great Scaling-Up of the Trawl Fishery

The changes to the British fisheries that took place between 1840 and 1880 have been described by Robinson as "little short of revolutionary," and the railways played an essential role in this change. Railways were a much faster way to move fish around the country than were the roads, even though the building of turnpike roads earlier in the century had somewhat improved the ease of overland transport. This increase in speed meant that fish could arrive to market fresher or from further away, but that improvement alone was not nearly enough to effect big changes in the importance of fish, and the fishing industry that supplied it, to the British economy. The railways became a transformatively important force in the scaling-up of fisheries in Britain only when fish became cheap, or at least relatively cheap, to transport by rail.

Robinson argues that when the first British railways were established, their founders did not have the carriage of fish in mind as a source of freight, and that initially when it came to transporting fish "the railways had done little more than take over part of the traditional boat, cart and pannier pony traffic flowing along their routes."[31]

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[31] Robinson, *Trawling*, 24.

Meanwhile, inland in cities like Manchester, while the demand for fish as such was not particularly surging, food shortages among the working classes were becoming acute, even while coastal fishermen were catching sometimes more fish than they could sell. There was, that is, a "gap that separated fishermen from the mass of the inland consumers";[32] a gap that was not bridged merely by the presence of train tracks connecting, say, Hull and Manchester.[33] "It is too simplistic a standpoint," argues Robinson, "merely to accept that the railways just took over the transport of fish with few problems, and less than accurate to assume that the subsequent development of a mass market was an overnight occurrence."[34] The railway companies' strategy in competing with other forms of overland transport was to try and take away their most profitable business, the high-cost luxury goods, by cutting their rates on just these items. Initially, then, transport by train only made financial sense for the same fish varieties already being transported by road: turbot and sole, not flounder and plaice.[35]

A breakthrough occurred in Manchester through the efforts of James Laws, who was the manager of one of the three railway companies that formed the rail link between Hull and Manchester, the Manchester and Leeds Railway. Laws was able to negotiate cheaper rates for the transport of fish along this route, as well as to establish a fish stall in Manchester. Fish landed by the Hull area trawlers could be brought in overnight to Manchester and sold cheaply at the fish stall; Robinson tells of how "the poorer classes flocked to buy in such numbers that for a time the footpath over the neighboring bridge

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[32] Robinson, *Trawling*, 24.
[33] The port and the city were connected by the Hull and Selby line and the Leeds to Manchester line from 1840 (Robinson, *Trawling*, 24).
[34] Robb Robinson, "The evolution of railway fish traffic policies," *Journal of Transport History* 7 (1986-1987): 33.
[35] Robinson, "The evolution," 35.

was completely blocked. The entire consignment of 3,192 lb was disposed of in under two hours. The story was much the same over the following weeks and fresh fish was soon established in Manchester as an article of cheap mass consumption."[36] The agreement on lower rates for fish transport that Laws had brokered between the Manchester and Leeds, Leeds and Selby, and Hull and Selby railway operators was broadened in 1842 to include railway operators across England under the name of the Railway Clearing House, and railway transport of fish became both cheaper and faster than transport along the roads.[37] In Scotland, as railways reached coastal areas, an enormous market for fresh or very lightly cured white fish arose in Glasgow, which like Manchester was booming and in need of food for the masses of workers living there.[38]

Making fresh white fish available cheaply to the large inland populations by way of rail transport meant a huge surge in demand for these fish, though channels of distribution also took some time to develop within the cities.[39] It had long been true that there was a steady and high demand for sole and turbot, but the numbers of people able to afford these fish was a small fraction of the masses of people who would, and now could, buy fish like plaice that had formerly been considered as offal. The days when a catch of plaice risked not finding a seller at all, and so wasting the cost of transport to market, had ended, and demand for plaice and its humble cousins was high. Under these conditions, trawling, which had always been disadvantaged by the high percentage of by-catch of fish like plaice and for the somewhat squashed condition of the fish after being pulled along by the trawl, was in a position to thrive at any port with a rail connection to an

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[36] Robinson, *Trawling*, 27.
[37] Robinson, *Trawling*, 27-29.
[38] Coull, *The Sea-Fisheries of Scotland*, 97.
[39] Robinson, "The evolution", 37.

urban centre. In particular, the Humber River ports of Hull and Grimsby flourished as major sources supplying coarse white fish to the tenement-dwellers of Manchester and the like. Even as the number of fish caught and landed increased greatly, the surging demand inland meant that fishermen and fish sellers enjoyed the same prices as they had gotten before, but were selling many more fish.[40]

Even in places where trawling had already been relatively thriving, cheap railway carriage made a big difference. In Devonshire, it kicked off something like a mania for trawling in the port of Brixham. Brixham was close to the two railheads at Torquay and Paignton, and could send fish a short way by road to meet the trains. When even this short trip was no longer required with the extension of the railway as far as Brixham itself in 1868, things really took off. John Rule describes the "speculative boom" that ensued:

> Those who had been doing an ordinary living trade in fish, suddenly found themselves in a position to dispose of all they could catch at good round London prices. A fishing fever resulted. Householders sold their properties, tradesmen borrowed all that they could to invest in smacks costing from £800 to £900. The fever lasted around three years before fishing returned to the hands of the practical fishermen and Brixham then settled down to enjoy a prosperous 20 years.[41]

Fish landed at Brixham could be transported rapidly to London or Bristol for sale. The other major Devon port of Plymouth had rail access much earlier, from 1849, and Rule argues that Plymouth fishermen

> found it a means of conveyance ideally suited to the transport in quantity of the coarser varieties in which its trawlers had tended to specialise. If prime varieties were

[40] Robinson, *Trawling*, 29.
[41] Rule, "The South-Western Deep-sea Fisheries," 174-175.

> landed they were sent as before to Billingsgate, Bristol and Bath. Other varieties went not only to those centres, but also directly to towns in the south west such as Taunton, Bridgewater, and Weston-super-Mare; as a salesman put it in 1866, 'We send fish to all the towns down the Great Western Line.'[42]

The coming of the railways made it possible for fishermen to get prime fish to market more cheaply and freshly, true, but there had never been any trouble selling soles and turbot. Indeed, turbot for some reason did not respond well to rail travel (though soles did), and continued to be carried over land along the roads in fish vans as before.[43] The railways' transformative effect was that it made fishing for coarse fish worthwhile by connecting the coastal towns where it was landed and had long been used as food to the inland towns and cities where there were eager buyers for varieties like plaice and haddock that had been an uncertain sell in days past. The railways were a link, giving the inland lower classes access to abundant and cheap protein available at the coast, and giving the fishermen at the coast access to the large groups of consumers willing to pay for fish that had not previously been sought after. Once this connection was made, each group came to rely on the other, and both groups relied on the continued service of the railway. In order for the fishermen, by way of the fish wholesalers and fishwife-auctioneers who were stationed at the quays of "all the fishing ports of 'the slightest consequence'",[44] to maintain their link to the inland markets, they had to supply a large and consistent amount of coarse fish. What had previously been the major drawback of trawling – that it resulted in the capture of large numbers of coarse fish, which were often thrown back dead – made it essential in the new economics of fish resulting from cheap

[42] Rule, "The South-Western Deep-sea Fisheries," 175.
[43] Rule, "The South-Western Deep-sea Fisheries," 175.

railway transport of fish inland. Trawlermen in what had become the thriving fishing port of Hull were landing and selling nearly everything they caught by the end of the 1850s.[45]

For every sale, there must be a buyer; in order for the trawlermen to sell their very miscellaneous and mostly coarse catch, there had to be buyers for miscellaneous and mostly coarse fish. Although the number of fishmongers did increase during this period, fresh fish bought uncooked from shops by consumers was not what kept the demand for coarse fish so high and growing. This demand was sustained by a new kind of vendor in nineteenth-century Britain, whose product became synonymous with the nation itself: fish and chips. These fish vendors would buy coarse fish, often day-old from fishmongers' shops, dip it in flour, fry it in oil, and sell it to the eager public from a tray, a stall, or, eventually, from a shop. By 1861, there were about three hundred people selling fried fish in the vicinity of Billingsgate fish market in London.[46] By 1888 there were an estimated 10,000 to 12,000 fried fish shops in Britain; by 1910 there were some 25,000 in the country, 2,000 of those within five miles of the Manchester Town Hall alone.[47] Until the turn of the century, fried fish was almost exclusively a working-class food. The shops were often unhygenic and smelly, and the frequent target of the attentions of the London health officers.[48] Nor was London the epicentre of the fish and chips trade: the many female workers at the textile mills of Lancashire, who had still to

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[44] Rule, "The South-Western Deep-sea Fisheries," 174; the quotation is from E. W. H. Holdsworth, *Sea Fisheries of Great Britain and Ireland* (1883), 6.

[45] E. Gillett and K. A. MacMahon, *A History of Hull* (Hull, 1980): 306-311, cited in Robinson, *Trawling*, 30.

[46] Robinson, *Trawling*, 31.

[47] John K. Walton, *Fish and Chips and the British Working Class, 1870-1940*, (Leicester: Leicester University Press, 1992), 5, 27. Walton argues that these estimates were likely on the low side.

[48] Robinson, *Trawling*, 32.

provide supper for their families after their shifts were through, made for a thriving fish and chips business in that county.[49]

John Walton has chronicled the history of the fish and chips trade in Britain, and argues briefly in his introduction that the trade influenced and sustained the changes that were taking place in Britain's sea fishing industry. The demand for fish and chips "helped materially in opening out the demand for cheaper fish", and it "provided markets for fish species that had long been ignored or wasted, such as plaice and haddock."[50] What's more, fish and chip vendors were not particularly discriminating about the freshness and condition of the fish they used, which matched nicely with the realities of trawling for fish. Trawling yielded a wide variety of fish, mostly coarse varieties, in somewhat squashed condition, and almost always quite dead, a far cry from the carefully line-caught prime fish once brought to Billingsgate alive in the flooded holds of fishing boats: in Walton's memorable words, "the trawl delivered the fish dead on the dock and necessitated new ways of disposing of them."[51] The fish friers and their eager customers would gladly dispose of these fish, tens of thousands of tons of these fish, the price of which remained throughout this period "only a fraction of that for fresh meat."[52] The coming of the railways to the coasts, the Railway Clearing House rules, and the simmering demand for cheap protein in the inland cities had driven the first great scaling-up of the sea fisheries in Britain, firmly establishing trawling as the fishing method of choice. In so doing, fish had become suddenly an important commodity, and worries

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[49] Robinson, *Trawling*, 32. Walton suggests that prior to the advent of fish and chips shops, there was already a strong culture in Lancashire of buying prepared fried or baked potatoes after work and at fairs and races, so that adding fried fish to the menu was a relatively easy transition. (Walton, *Fish and Chips*, 24-25.)

[50] Walton, *Fish and Chips*, 6.

[51] Walton, *Fish and Chips*, 6.

about the future security of this commodity began to build. The fish and chip trade's seemingly inexhaustible demand for coarse fish was a major factor in the second phase of scaling-up of the fisheries, when steam-powered trawlers came to replace sailing smacks in the 1870s and 1880s. These steamers could land ever greater quantities of fish; as their use spread, the social and economic structures of fishing in Britain were transformed, while at the same time anxieties about the future availability of fish reached a fever pitch.

The "Trawling Controversy" Gets Going

Not surprisingly, trawling flourished under economic conditions in which large numbers of coarse fish could be rapidly and cheaply transported to markets and sold at a steady price. The ports where trawling had first taken hold, such as Brixham, Plymouth, Ramsgate, Scarborough and especially Hull, became more and more active in trawling, with rapidly expanding fleets of trawlers. During the 1860s, trawling fleets had also begun to work out of the North Sea ports of Yarmouth, Lowestoft, and in great numbers out of Grimsby, and from the Irish Sea ports of Liverpool, Dublin, and Fleetwood. Taken together, these fleets represented one thousand smacks, five thousand trawlermen, one million pounds of capital, and a daily catching capacity of more than three hundred and fifty tons.[53]

Just as the first two Brixham trawlers had exerted a disruptive influence when they began fishing at Scarborough in 1830, the North Sea trawling fleet in particular

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[52] James R. Coull, "The Trawling Controversy in Scotland in the Late Nineteenth and Early Twentieth Centuries," *International Journal of Maritime History* VI (1994): 109.
[53] Robinson, *Trawling*, 49.

raised hackles as it expanded the range of areas that it exploited for fish.[54] By 1860 the English trawlers were ranging northward to Scottish waters, to the chagrin of the Scottish line-fishermen who were accustomed to laying their lines down without trawlers passing through, dragging their nets along the bottom. In addition to the damage the trawlers inflicted on the liners' gear, the line-fishermen knew how enormous was the catching capacity of the trawl net in comparison to their own methods, and were alarmed that these invading boats might make off with most of the fish. The incursions of the English trawlers did not escape the attention of the Fishery Board for Scotland, a body that had been founded in order to promote the Scottish sea fisheries, but from this point on found itself acting much more as a regulator limiting the activities of trawlers in Scottish waters. In response to the concerns of the liners, the Fishery Board closed one area of the Firth of Forth to trawling for three years during the 1860s.[55]

Although there was almost certainly an element of anti-English sentiment in the conflicts between the English trawlers and the Scottish liners, tension between the traditional line-fishermen and the newly-thriving trawlermen appeared wherever both groups were plying their trades, particularly all along the North Sea coast. Line-fishermen, as well as fishermen who drift-netted for herring, from the fishing communities of north-east England complained bitterly that trawlers were ruining their gear and depleting the area of fish.[56] Those to whom they complained had a hard time deciding whether the liners' and drifters' claims were well-founded or mere jealously and fear of the new; Robinson points out that the "lack of available statistical information or

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[54] D. H. Cushing in *The Provident Sea* (Figure 49, p. 113) reprints a 1911 map illustrating the gradual expansion of British North Sea trawling ground northward and eastward through the nineteenth century.
[55] Coull, *The Sea-Fisheries of Scotland*, 140.
[56] Robinson, *Trawling*, 49.

of any national organization specifically responsible for monitoring the progress of the fisheries made such assertions very difficult to prove or refute."[57] A body called the Board of British Fisheries operated out of Edinburgh, but like the Fishery Board for Scotland their traditional mandate was to promote the (almost entirely Scottish) cured fish trade and regulate the curing process, not to collect fisheries statistics or police the activities of fishermen in Britain as a whole.[58]

The liners and drifters were inshore fishermen, and though the trawlers were also expanding their range further and further from shore into the North Sea, it was the expansion of inshore trawling up and down the coastline from the trawling ports that was the source of the tension between trawlers and traditional fishermen. These tensions developed during 1862 into a series of meetings held in various fishing communities along the North Sea coast, calling on the national government for an end to or limits on trawling. In response, pro-trawling meetings and the attendant lobbying of Members of Parliament took place in trawling ports like Brixham and Hull.[59]

All of this agitating led to the appointment in 1863 of a Royal Commission to investigate the claims and counter-claims made by trawlers and their opponents. This Royal Commission was to assess not only the conflicting claims made by traditional fishermen and trawlermen, but had the much broader mandate of assessing Britain's sea fisheries, and the regulations currently governing the fisheries, in order to make recommendations for future regulation, or, as it turned out, lack thereof. The three members appointed to the commission were James Caird, Thomas Henry Huxley, and George Shaw Lefevre; they were charged with the tasks of determining, "with the view

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[57] Robinson, *Trawling*, 49.
[58] Robinson, *Trawling*, 50.

of increasing the supply of a favourite and nutritious article of food for the benefit of the Public,"[60]

> 1. Whether the supply of fish from such Fisheries is increasing, stationary, or diminishing.
> 2. Whether any of the methods of catching Fish in use in such Fisheries involves a wasteful destruction of Fish or Spawn, and, if so, whether it is probable that any legislative restriction upon such method of fishing would result in an increase of the supply of Fish.
> 3. Whether any existing legislative restrictions operate injuriously upon any of such fisheries...[61]

The Commission's second task, though cautiously worded, was in fact the one responding most directly to the concerns voiced by the traditional fishermen and countered by the trawlermen. The method of catching fish accused of the "wasteful destruction of fish or spawn" was, of course, trawling. The liners' accusations that trawlers damaged their gear were probably less powerful than their accusation that trawling killed and wasted many perfectly good fish, and worse yet that the trawl nets killed many young fish that would otherwise have grown up to be marketable.

Over the next few years, the members of the Royal Commission conducted numerous interviews with fishermen, boat owners, and others associated with the sea fisheries at eighty-six fishing communities all along the British coastline, in England, Wales, Scotland, Ireland, and on the Isle of Man.[62] The Commission did not have access to a supply of fishing statistics, and so in the absence of this kind of data their methods amounted to asking each interviewee his opinion on each of the three topics – were there

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[59] Robinson, *Trawling*, 50.
[60] "Report of the Commissioners Appointed to Inquire into the Sea Fisheries of the United Kingdom. Vol. I. The Report, and Appendix," *House of Commons Parliamentary Papers* XVII (1866): vii.
[61] "Report of the Commissioners," vii.
[62] "Report of the Commissioners," ix.

more fish, less, or about the same? is trawling destructive, and does it need to be limited by government regulations? do any of the existing laws impede fishing? – and their results amounted to presenting the answers to this question. Robinson has pointed out that

> much of this [evidence] was subjective in nature and offered widely differing views, being often in line with the interests of the witnesses. For example, the bulk of the evidence given by the Yorkshire and north-east linemen suggested that the size of landings was on the decline whilst the trawling trade at Hull and Grimsby claimed they were on the increase.[63]

The same stakeholders whose conflict, agitation, and meetings and counter-meetings had given rise to the Royal Commission in the first place had become the source for the information that was to be used to decide on their conflict. It is hard to see how the resulting Report could have done anything but recreate their conflict in miniature, seasoned with the commissioners' own views on the topic. The commissioners' own views, in the case of Shaw Lefevre and Caird, were inclined towards the *laissez-faire* economic policies that had characterized British government policy of the previous sixty years;[64] and in the case of Huxley, inclined towards a view of the sea fish population as enormous and enormously productive, in little danger of being reduced by fishing pressure.[65]

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[63] Robinson, *Trawling*, 51.

[64] Robinson, *Trawling*, 51; Gambles, "Free Trade and State Formation".

[65] Robb Robinson has written of the "profound effect" of Huxley's "considerable influence on the direction taken by the later 19th and 20th century fishing industries." (Robb Robinson, "The Evolution of Some Key Elements of British Fisheries Policy," *International Journal of Maritime History* 9 (1997): 130.) See also the chapter on "Huxley's Red Herring" in Jennifer M. Hubbard, *An Independent Progress: The Development of Marine Biology on the Atlantic Coast of Canada, 1898-1939*. Ph.D. thesis, University of Toronto, 1993.

After concluding their many interviews, the Royal Commission reached a verdict on all three of the questions that they had been charged to answer, and issued a report in 1866 that found little to worry about. On the first question: "The total supply of fish obtained upon the coasts of the United Kingdom has not diminished of late years, but has increased,"[66] and could stand to increase still further. This increase coincided with a larger set of positive developments: more people were working in the fishing industry, doing better financially than they had before, and supplying a large amount of fish to the population at less than one-eighth the cost of beef.[67] As to the second question: "beam trawling in the open sea" was not found to be "a wastefully destructive mode of fishing, but is one of the most copious and regular sources of supply of eminently wholesome and nutritious fish. Any restriction upon this mode of fishing would be equivalent to a diminution of the supply of food to the people; while there is no reason to expect present, or future, benefit from that restriction."[68] What was more, trawled fish was not landed in a "damaged and unwholesome" condition, nor had it damaged fished populations in areas where it was practiced, nor was it particularly widespread, not being used at all along much of the coast.[69] The disputes among "fishermen of different classes" that had prompted the inquiry had been "carefully considered," but almost none were found to be "of sufficient gravity to render special legislation necessary or desirable."[70] And with regard to the third question, about the restrictiveness of current regulations governing fishing, the Commission judged "the laws relating to sea fisheries to be complicated,

[66] "Report of the Commissioners," ciii.
[67] "Report of the Commissioners," summarized in Table of Contents, iii.
[68] "Report of the Commissioners," ciii.
[69] "Report of the Commissioners," summarized in Table of Contents, iii-iv
[70] "Report of the Commissioners," ciii.

confused, and unsatisfactory,"[71] not to mention seldom enforced. They recommended that "all Acts of Parliament which profess to regulate, or restrict, the modes of fishing pursued in the open sea be repealed; and that unrestricted freedom of fishing be permitted hereafter."[72] Inshore areas were not yet showing signs of damage, but the Commissioners granted "that by the use of improved engines, the destruction of fry might reach such a pitch, as to bear a large, instead of, as at present, an insignificant ratio to the destruction effected by the natural enemies of fish, and by conditions unfavourable to their existence."[73] The only way to detect when the influence of fishing became significant was through collecting "trustworthy statistics...extending over a considerable number of years."[74] Setting a minimum allowable catching size, rather than "interfering with the implements of fishermen, or with their methods of fishing,"[75] was the way to deal with such an eventuality. Among the rules to be repealed were those restricting foreign fishermen from landing catches in British ports, in hopes that other nations would reciprocate the arrangement.[76] The recommendations of the Royal Commission report were brought into law two years later with the Sea Fisheries Act of 1868.

In their clash with the traditional line-fishermen, the trawlermen had more than won: they'd received official recognition that their mode of fishing was neither wasteful nor destructive, nor was it harming the fish supply; indeed, trawling had been recognised as essential to satisfying the nation's appetite for a "copious and regular" supply of food fish. Moreover, the regulations previously in place governing trawling – which were

[71] "Report of the Commissioners," ciii.
[72] "Report of the Commissioners," cvi.
[73] "Report of the Commissioners," cvi.
[74] "Report of the Commissioners," cvi.
[75] "Report of the Commissioners," cvi.
[76] Robinson, *Trawling*, 53.

rarely enforced, but included such things as prohibiting Sunday fishing, maximum length restrictions on the size of the trawl beam, and minimum size limits on net mesh[77] – were to be done away with, and trawlers could go about their business in an environment where there was nearly complete "freedom of fishing".[78] Even should trawling be found destructive in future, the practice itself was not to be the focus of blame, but instead the size of fish allowed to be landed.

In discussing the 1866 report issued by the Royal Commission, it is traditional to engage in some disbelief and clucking, not just at the dead-wrongness of their conclusions but at the primitive methods used to reach such confidently stated conclusions which then became the basis for laws. It is, in fact, very difficult to resist the temptation to cluck at this point, for instance at the Commission's interpretation of the question of "whether the supply of fish…is increasing, stationary, or diminishing" to mean instead whether "the supply of fish obtained upon the coasts of the United Kingdom of late years" was increasing or not – that is, that the number of fish being landed during a period of insatiable demand and the rapid scaling-up of fishing effort could be taken as an indicator of how many fish there were in the sea, which seems to have been the spirit of the original question, and the concern of the traditional fishermen. Robinson says diplomatically on this point that what "the Commissioners were unable to examine effectively in the absence of much hard data was whether the continued increase, in terms of boats and gear deployed, was accompanied by a commensurate increase in the amount of fish actually caught."[79] The Commissioners themselves recognised the limits of their methodology, particularly when they found themselves with

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[77] Robinson, *Trawling*, 50.
[78] Coull, "The Trawling Controversy in Scotland," 110.

no data to rely on except the general impressions given by the people interviewed. Their report, while certainly calling for the government to let the fishermen fish however they liked, did not envision a future of zero government involvement in fisheries affairs of the kind that is invoked by calling the report, as one fisheries historian did back in 1966, "the true and final apotheosis of classical *laissez faire*".[80] The government's role was not to limit the fisheries, but the Commission recommended that the government begin to do a much better job of monitoring the fisheries, and in particular of collecting statistics in a regular, organized fashion, by way of avoiding the "constant recurrence to panics to which the sea fishery interest has hitherto been subjected,"[81] and presumably by way of making life easier for any future Royal Commission appointed to report and make recommendations on the industry and the commodity upon which the industry depended. If the line-fishermen had failed utterly in their attempt to convince the government to limit the trawl fishery, they had at least succeeded in bringing the fishing industry, and trawling in particular, into the spotlight of a government inquiry – and the spotlight had revealed the government's almost complete ignorance of what had become a major industry in Britain, and a crucial part of the nation's food supply to boot.

Whether the government was monitoring them or not, trawling in the decades following the Royal Commission report must have *felt* a lot like "the true and final apotheosis of classical *laissez faire*", if not described in exactly those words by the thousands of boys from the British underclasses pulled into the trawling trade to work as indentured "apprentices" as the trawling industry struggled to deal with a labour shortage

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[79] Robinson, *Trawling*, 52.
[80] R. H. Barback, *The Political Economy of the Fisheries* (Hull, 1966), 18-19; quoted in Robinson, *Trawling*, 53.
[81] "Report of the Commissioners," quoted in Robinson, *Trawling*, 53.

that threatened to limit its spectacular expansion. As the trawler fleets of Hull and Grimsby came to number almost one thousand smacks by the late 1870s,[82] trawler owners were increasingly wealthy men, but they faced a real problem: the traditional fishing communities had nowhere near enough men and boys to crew all of the trawlers that could profitably fish. The solution was to be found in the workhouses, orphanages, reformatories, and back alleys of Britain: a host of "wanderers, waifs and strays"[83] who could be made to work on board trawlers until the age of twenty-one for little more than shelter or a small allowance, and a share of the smack's by-catch to sell for bait or oil. Stories of the mistreatment of some such apprentices, and the suicides and desertions of others, began appearing in newspapers during this period.[84]

Even before receiving the stamp of approval from the government that put no limits on its further expansion, trawling was proving to be socially transformative. Commercial fishing was becoming, as the arrival of the apprentice army shows, far less a family affair than it had previously been. And as it scaled-up, fishing was becoming a much more centralized activity than previously. The trawlers were still relatively small craft, with a crew of five making voyages of up to eight weeks in duration, but even this was a shift from the daily trips made by a father and sons in an open long-lining boat. Increasingly, the crew did not own the boat from which they fished, and one owner might have several profitable boats. Hull and Grimsby grew rapidly along with trawling, and while trawlers operated out of many port cities, the twin ports on the Humber became the capital cities of English trawling.

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[82] Robinson, *Trawling*, 53.
[83] Robinson, *Trawling*, 55.
[84] Robinson, *Trawling*, 57-59.

But amid all of this triumphant growth, the traditional fishermen's concerns did not go away. The more vigorously the inshore waters frequented by the liners and drifters were gone over by the growing trawling fleet, the more the liners' and drifters' concerns about damage to their gear and to the fish population increased. In 1878, these concerns prompted another government inquiry into the impact of trawling on the fish and fishermen of England and Wales. The Commissioners were Frank Buckland and Spencer Walpole, who already worked for the government as Inspectors of Fisheries for England and Wales; Buckland was also a well-known popularizer of the natural history of fishes.[85] They were charged with finding answers to largely the same questions put to the 1866 Royal Commission. Was trawling destructive of young fish and of the gear of traditional line fishermen? Was the fish supply decreasing, and if so, was trawling to blame? Was it "probable that any Legislative Regulations would tend to promote the Welfare of the Fishermen engaged in such Fisheries, and to increase the Supply of Fish for the Benefit of the Public"?[86] Buckland and Walpole presented their findings on these questions in 1879, and like the Commissioners before them, reported that no, it did not appear that trawling was wastefully destroying young fish or causing the fish supply generally to decrease.[87] This Commission, though no more receptive than the previous Commission had been to the liners' claims that trawlers were damaging the fish population, was more sympathetic to the liners' complaints about the trawlers inflicting damage to their lines and drift nets, albeit accidentally. The Commission's report concluded that there was "considerable injury" being done by trawlers to the gear of other fishermen, and that

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[85] On Buckland, see Roger E. Jester, *Fisheries and the State: A Study of Some Aspects of Resource Conservation and Government Policy in England, 1860-1902*, (Ph.D. thesis, New York University, 1971).
[86] "Report on Sea Fisheries of England and Wales," *House of Commons Parliamentary Papers* XVII (1878-1879): iii.

trawlers needed to display better lights when they were stationary or moving. The 1883 Sea Fisheries Act resulting from this report stated that trawlers had to avoid inflicting such damage to others' gear and boats – though, as James Coull points out, this pronouncement was not backed up with "any sanctions for failure to comply"[88] – and also banned the "manufacture and sale of instruments for destroying fishing implements."[89]

Regulating the Freshwater Fisheries

Despite the impression given by the reports of the 1863 and 1878 Commissions, it is important to realize that the British government's approach to the fisheries during this period was not entirely or mindlessly *laissez-faire*. In addition to their readiness to set up official inquiries at fairly regular intervals – there was to be another one in 1883, of which more later, causing one observer to describe the government inquiries into trawling and the fish supply as "incessant"[90] – there was, in fact, some readiness to step in and apply restrictions when a particular natural resource was seen to be endangered. The conservation movement in Britain got its start in mid-century with some of the numerous natural history clubs and societies (which by 1890 blanketed the United Kingdom), which set among their goals the protection of Britain's wildlife, and birds in particular, from overhunting, overcollection, and cruelty.[91] As well, organisations like the Royal Society for the Prevention of Cruelty to Animals and the Royal Society for the Protection of Birds campaigned actively against wasteful killing of birds for their plumage.[92] The

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87 "Report on Sea Fisheries of England and Wales," xxxviii.
88 Coull, "The Trawling Controversy in Scotland," 110.
89 "Bill, intituled, Act to carry into effect International Convention concerning Fisheries in North Sea, and to amend Laws relating to British Sea Fisheries," *House of Commons Parliamentary Papers* IX (1883): i.
90 A1, newspaper clipping [mid-May 1885], "Sir Lyon Playfair on Sea Fisheries," [74].
91 John Sheail, *Nature in Trust: The History of Nature Conservation in Britain* (Glasgow: Blackie and Son, 1977), 2-6.
92 Sheail, 9-12.

agitations of these groups led to government action, and several pieces of protective legislation made it into law: the Sea Birds Protection Act passed in 1869 and expanded in 1880, the Wild Birds Protection Act passed in 1872 and expanded in 1881, and the Wild Fowl Protection Act of 1876.[93]

During the same period that few restrictions were being imposed – and, indeed, some existing restrictions being lifted – on the sea fisheries, several pieces of legislation were passed to protect Britain's freshwater fisheries through the imposition of closed seasons and minimum landing sizes for species considered under threat. The impetus for this legislation, Peter Bartrip has argued, came chiefly from the powerful anglers' lobby who feared the decline of their sport along with freshwater fish stocks, and also perhaps, but to a far lesser extent, from concerns about the decline of freshwater fish as a natural resource and food item.[94]

A private members' "Bill for the Better Preservation of Fish in England and Wales", seeking a closed season for fishing of species breeding in fresh water, size limits prohibiting the capture of small fish, and stopping the use of certain nets, was introduced to Parliament in 1864, but withdrawn after its first reading. However, the bill's claim that "the Fresh-water Fisheries of England have of late Years been greatly injured…"[95] was taken up again the following decade when a large and important commercial and recreational freshwater fishing area, the Norfolk and Suffolk Broads, was perceived by

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[93] Sheail, 22-27.

[94] Peter Bartrip, "Food for the Body and Food for the Mind: The Regulation of Freshwater Fisheries in the 1870s," *Victorian Studies* 28 (1985). Part of the power of the angling lobby came from the sport's across-class appeal: angling clubs were leisure organizations that drew their membership from both the middle and working classes, and was regarded as "a 'healthy' and 'innocent amusement'" (297) for men of the working class in particular, who might otherwise, it was worried, spend their leisure time and money in the pursuit of less healthy and innocent entertainment. Bartrip argues that "[s]uch an element of social cohesion identified angling as worthy of patronage by politicians and others interested in the potential of leisure as a means of enhancing social solidarity; this implied preservation of the prey." (297)

local anglers to be declining. Their agitation prompted a government inquiry, undertaken in 1875 by Frank Buckland.[96] Buckland's methodology in conducting his inquiry was much like that used by the Royal Commission of 1863 (and later used again by Buckland and Walpole in 1878): he traveled around to rivers and lakes where fishing was practiced, and interviewed as many people as he could, hearing tales of tons of fish being caught only to be sold as manure, the mass destruction of young fish in small-mesh nets, and similar reports. His recommendations that net-fishing be restricted with a closed season and minimum mesh sizes were made law in the Norfolk and Suffolk Fisheries (Preserving) Act of 1877.[97] Buckland's report, and the ensuing legislation, which applied only locally to the area which Buckland had studied, prompted anglers across Britain to believe that such restrictions should be applied nation-wide.[98]

The next year, the Sheffield Liberal MP A. J. Mundella brought forward a Freshwater Fisheries Bill that included provisions for a closed season for the capture or sale of coarse freshwater fish.[99] "The movement was launched in Sheffield," Bartrip argues, "probably because that city could plausibly claim to be England's angling capital, possessing some eighty clubs whose combined membership ran to some 8,000. But the movement rapidly attracted support in other regions."[100] Mundella's bill passed in August 1878 as the Freshwater Fisheries Act, in fact after having been made considerably more extensive during its readings and in its evaluation by a Select Committee that

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[95] *Parliamentary Papers* 1864, II, 99-104; quoted in Bartrip, "Food for the Body," 287.
[96] Bartrip, "Food for the Body," 288.
[97] Bartrip, "Food for the Body," 288.
[98] Bartrip, "Food for the Body," 290.
[99] Freshwater fishing had its prime and coarse fish just as the sea fisheries did. Salmon was the prime fish, generally angled for on private property by gentlemen of the upper class, and had been protected by legislation and dedicated inspectors (of whom Buckland was the shining example and T. H. Huxley the uninterested embarrassment) for years; the coarse fish sought by the members of the angling clubs could include such species as chub, dace, roach, perch, pike, and eels. (Bartrip, 297 and 299).

consisted of Mundella himself and fourteen others, many of whom were known as men "likely to be predisposed to the protection of freshwater fish".[101] In addition to establishing a closed season for coarse fish – and penalties for fishing in the closed season including both fines and the confiscation of the caught fish and the gear used to catch them with – the Act also "adopted a narrower definition of private waters, thereby sanctioning greater interference with owners' freedom and rights in respect of the closed season."[102]

The British government, then, was not necessarily indisposed to stepping in to limit the activities of fishermen in order to protect the fish stocks. The very same kind of subjective, anecdotal data was used to reach conclusions about the abundance of freshwater fish in 1875 as it had been ten years earlier for the sea fish. But unlike the sea fisheries, the freshwater fisheries did not play a major role in keeping the British population supplied with cheap, protein-rich food; nor were the freshwater fisheries undergoing any kind of transformation that would increase their importance, which was happening almost daily with the sea fisheries. Bartrip's research shows that freshwater fish were important as an object of recreation for the British population. Sea fish, as I hope has been made clear so far, had become important – more than important: essential – as an object of consumption, and as the basis for an industry that was only getting larger and more profitable. The advent of steam-driven trawlers, the subject of the next section, further accelerated the growth of the trawling industry and its social and economic impacts. The bigger the trawling industry became, the more economically damaging any legislative restrictions imposed upon it threatened to be.

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[100] Bartrip, "Food for the Body," 287.
[101] Bartrip, "Food for the Body," 291.

Steam Trawling Appears in England

By the 1870s, the ownership patterns in the trawling industry had become transformed. Where once each family might have owned a boat to use for lining, or a few families might together own and operate a trawler, in large ports like Hull or Grimsby it had become the norm for several trawlers to be owned by one man, who hired other men to skipper and crew the mini-fleet. This change in the structure of ownership had several effects. One of these was that it became much more common for the owner to be a man of some means and a fine house, and as such it became much less common for the owner to accommodate in his own home the many apprentice boys who worked on the smacks. Instead, the apprentices (who were really becoming apprentices in name only) were given an allowance and expected to find their own shelter and food.[103] This shift from the traditional "indoor" apprentice system to the "outdoor" system was already well underway by the end of the 1860s,[104] and was indicative of how the change in ownership structure brought about an increasing separation between the jobs of owning and of operating a trawling smack.

Such a separation led to large changes in the conditions of labour for those who worked aboard the smacks. More and more owners adopted the practice of fleeting, in which a group of trawlers would stay at sea for very long periods of time, and a fast cutter would carry the loads of fish they caught back to shore. For the men on board the smacks, this meant that since the smack didn't have to return to shore to keep the fish fresh, the crew spent far longer periods of time away from home and living on board ship

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[102] Bartrip, "Food for the Body," 292.
[103] Robinson, *Trawling*, 55.
[104] Robinson, *Trawling*, 55.

than had been the case previously. Fleeting was not a new practice, but it became much more widespread during the last few decades of the nineteenth century for a few reasons. As trawlers explored new fishing grounds farther from shore, the voyages to and from shore to deliver the fish to market became very time-consuming. As well, ice continued to be a costly commodity, usually imported from Norway, and the longer the trip back to shore, the more ice was required in order to keep the fish fresh.[105] And aside from all of these concerns, under the new ownership patterns characteristic of the big English trawling ports, fleeting had ceased to become a necessarily collective activity. In previous years, the money earned by the fish carried back to market by the cutters was divided up equally among the owners of the several trawlers in the fleet, regardless of how many fish each individual smack had landed. If one man owned every smack in the fleet, however, he no longer had to fear that his boat's catch was subsidising the income of another owner's less productive smack. Moreover, a new system, introduced in 1878, of packaging the fish in boxes to be delivered to the cutter made it possible to know how many fish had been produced by each boat; Robinson argues that this new system "restored incentive, for hard work and good catches were clearly rewarded", not to mention that fish shipped in boxes reached market in better condition, and so fetched better prices, than fish transported *en masse*.[106]

When the fast sailing cutters were replaced, beginning in the late 1870s, with even faster steam-powered cutters, fleeting was the solidly favoured option at Hull and Grimsby, despite the resistance of the crews to spent such long periods away at sea.[107] These steam cutters were very pricey, but they were also very fast, and the first fish to

[105] Robinson, *Trawling*, 70.
[106] Robinson, *Trawling*, 72.

market usually fetched the best price, so to many trawler owners they were worth the added investment. The capital required in order to add steam cutters to a fleet of sailing trawlers was such that it further scaled-up the ownership structure in the industry: to get into this new technology, argues Robinson, "smackowners had to form companies to finance their construction and operation."[108] The introduction of steam power to the fishing industry had moved it even further away from the owner-operated fishing boat: ownership had itself become a job.

The introduction of steam-powered cutters into the trawling industry was only the beginning. Although they required huge capital investments, a small number of cutters could service a large fleet of sailing trawlers. Powering the trawlers themselves with steam meant accepting new expenses: coal, engineers to run the steam engine, and the up-front costs of the vessels themselves. The use of fleeting, especially when serviced by steam cutters, created an economics which did not favour using steam to power the trawlers themselves, given the going prices for coal and the ongoing availability of wind for no extra cost. Yet something suddenly changed, and within a relatively short span of time steam trawlers came to almost entirely replace sailing trawlers as the main mode of catching fish. Robinson captures the startling suddenness of the change:

> In 1876 no commercial steam trawlers were working from British ports but over the next five years a large number of paddle tugs were adapted for fishing. Shortly afterwards, purpose-built steam-screw trawlers began to appear. By the end of the 1880s Hull and Grimsby had embarked upon a large-scale replacement of smacks by steam trawlers whilst the new trawling centres of North Shields and

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[107] Robinson, *Trawling*, 73.
[108] Robinson, *Trawling*, 73.

> Aberdeen based their development almost completely on steam.[109]

The numbers provided by F. G. Aflalo in his 1904 popular book *The Sea-Fishing Industry of England and Wales* bear out this account. The trawling port of Hull had a fleet of three hundred and thirteen sailing smacks in 1872; eleven years later there were three hundred sailing and one hundred and fifty steam trawlers; and by the time the book was published Hull had four hundred to five hundred steam trawlers, and only a few sailing trawlers remained (see Figure 3a). Grimsby had two steam trawlers in 1882 and eight hundred and twenty sailing trawlers in 1886; by 1892 the sailing trawlers had dwindled somewhat to number six hundred and eighty-six while one hundred and thirteen steam trawlers had joined the fleet; by 1902 there were only twenty-nine sailing trawlers, but four hundred and twenty-four steam trawlers (see Figure 3b).[110]

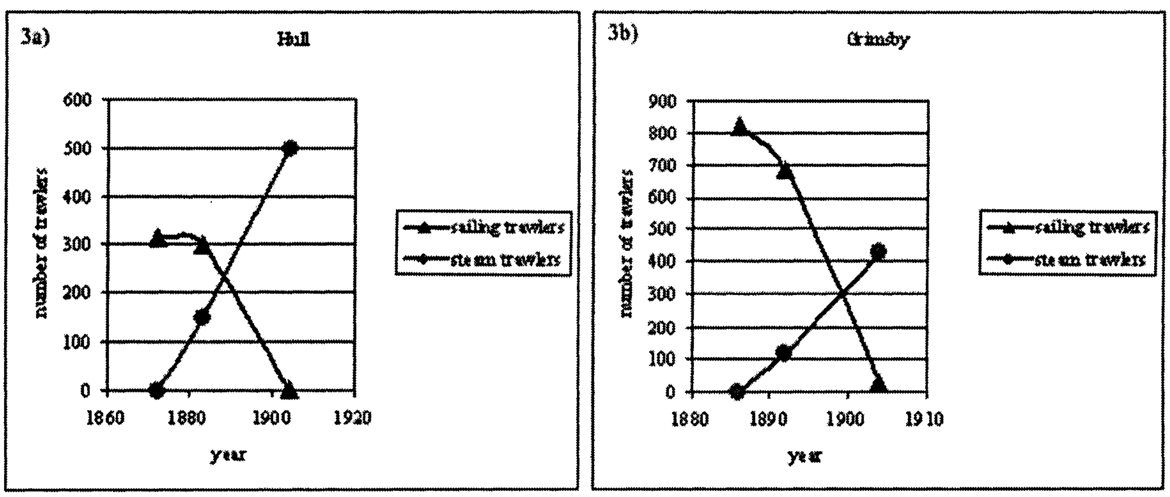

Figure 3. The rise of steam trawling and the decline of sailing trawlers at two major English trawling ports, Hull (3a) and Grimsby (3b). During the 1880s and 1890s, there was a rapid switchover to steam trawling. Data from Aflalo, *The Sea-Fisheries of England and Wales*, 1904.

What had changed was the cost of coal and the availability of steam ships to become involved on a trial basis in the business of fishing. Steam ships had been around

[109] Robinson, *Trawling*, 83.

since early in the nineteenth century, and since the 1860s many had been equipped with more efficient high-pressure, multiple cylinder engines, but the few experiments in using steam ships to catch fish had not succeeded or caught on. Like the adoption of trawling itself, it was local contemporary economics that set the conditions under which steam trawling became worthwhile: in this case, an 1877 trade depression affecting north-eastern England. The Tyne and Wear rivers, normally busy with ship traffic towed in and out by tug boats, were instead a sad scene, with "[s]cores of tug boats...laid up along the Tyne with little obvious prospect of employment."[111] The inactivity of the tug boats and cargo steamers also slackened demand for coal, and the price fell, making the cost of operating a steam-driven vessel, should any work be found worth operating it for, less than ever before. Attempting to supplement their income, several tug masters at the Tyne port of North Shields refitted their tugs with beam trawls and used them to fish.[112]

Over the next couple of years, trawling using refitted steam tugs caught on at North Shields and spread to Hartlepool and Grimsby. History repeated itself: the steam paddle trawlers worked their way up the shore to Scarborough, further infuriating the harried liners and drifters by trawling close inshore (and upsetting the sailing trawlermen too, by bringing tons of fish to market early, getting the best prices, and flooding the market with too many landings), and from Scarborough they ventured northwards into Scottish waters.[113]

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[110] Aflalo, *The Sea-Fishing Industry of England and Wales*, 239 (Hull) and 242 (Grimsby).
[111] Robinson, *Trawling*, 85.
[112] Robinson, *Trawling*, 86. Robinson tells of how William Purdy, master of the tug *Messenger* and one of the first North Shields men to try steam trawling, refitted his tug "by adapting whatever gear was available locally but had to send down to Grimsby for a beam trawl." (86)
[113] Robinson, *Trawling*, 85-86.

The innovation-through-tinkering of the North Shields tug masters, driven by the contemporary economics operating there during the late 1870s, established North Shields as a significant steam trawling port, which by 1890 had the largest steam trawler fleet anywhere in the world.[114] Hull and Grimsby took the lead, beginning in the early 1880s, in building steam ships expressly designed for trawling (Figure 4).

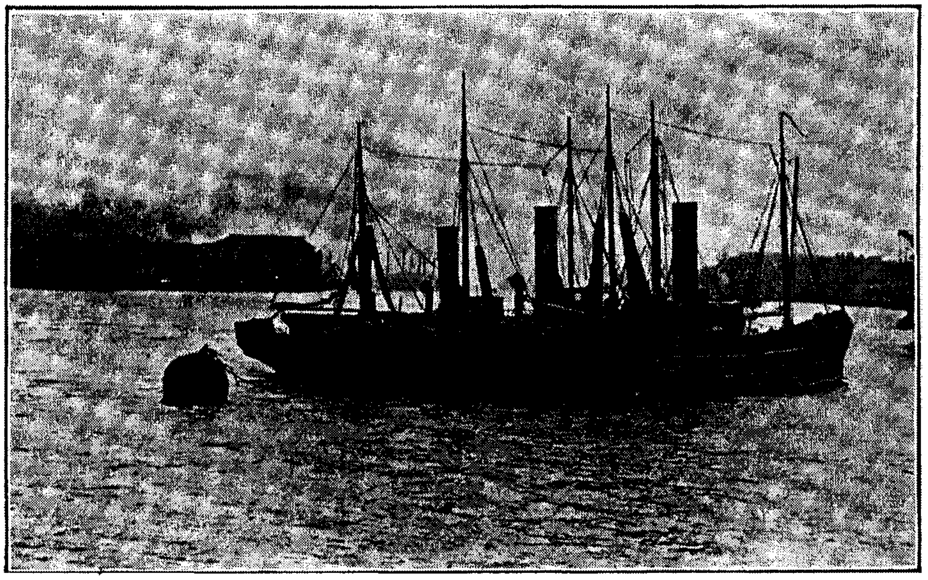

Figure 4. A purpose-built steam trawler, of the type that became the chief mode of catching whitefish by the 1890s. Such trawlers were usually owned by large limited-liability companies and based out of a small number of fishing ports, chiefly Hull, Grimsby, and North Shields in England and Aberdeen in Scotland. Photograph from F. G. Aflalo, *The Sea-Fisheries of England and Wales*, 1904.

Late in 1881 the Hull shipyard produced what Robinson identifies as "the first purpose-built steam trawler":[115] the *Pioneer* was ninety-four feet long with an iron hull and thirty-five horsepower of steam engine.[116] In the first five years of the 1880s, steam trawlers were first being built and launched with an enthusiasm that carried on through the next decades as the British fishing industry continued to boom. And they were catching and selling an awful lot of fish: Aflalo gives another astonishing set of numbers

[114] Robinson, *Trawling*, 93. North Shields had eighty-one steam trawlers in 1890, while Hull had sixty-one. More of Hull's steam trawlers were purpose-built, however.
[115] Robinson, *Trawling*, 91.

to this effect. From an annual catch of four hundred and fifty-three tons of fish in 1854, the number had risen by a factor of more than a hundred to reach 56,000 in 1882, then 78, 223 in 1892, and 165, 510 by the time Aflalo's book appeared (see Figure 5).[117]

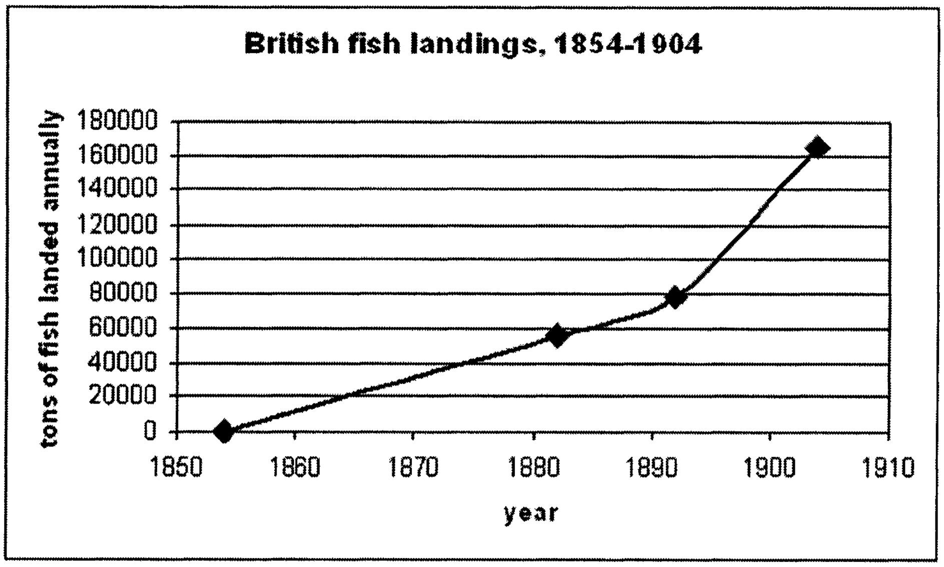

Figure 5. Annual landings of fish in Britain during the period of the transformation of the whitefish fisheries by the advent of trawling and steam trawlers. Data from Aflalo, *The Sea-Fisheries of England and Wales*, 1904.

All of this growth was profitable, of course, but it didn't come cheaply. Even a refitted paddle tug cost up to £3,000, twice what a smackowner would pay for a new sailing trawler, which was itself far more than an individual fisherman could manage. Having steam trawlers built to spec meant that smackowners had to band together in a way even more formalized than their previous cooperation in purchasing steam cutters. Out of this necessity came large, share-issuing limited liability corporations like the Humber Steam Trawling Company, founded in 1886, and the British Steam Trawling Company Limited, established in 1889; both of these corporations raised tens of thousands of pounds of

[116] Robinson, *Trawling*, 91.
[117] Aflalo, 84.

capital by selling shares.[118] Over the course of just a couple of decades, fishing in Britain had been almost unrecognizably transformed in its technologies, ownership structures, geographical distribution, and patterns of labour. This change happened with particular swiftness in Scotland, where the fishing industry seemingly sublimated, passing directly from distributed, small-craft lining to centralized steam trawling within a few years.

When the first English sailing trawlers had shown up on Scottish fishing grounds, the Scottish fishermen had reacted with annoyance and distress, but had not taken up trawling themselves to any significant extent. Indeed, while the English fishing industry was being transformed from one in which fish were caught by liners at fishing villages all along the coast to one in which fish were caught by fleets of trawlers operating out of several large ports, and becoming increasingly centralized at Hull and Grimsby, the Scottish white-fishing industry continued to be based on lining. It grew, certainly, and many fresh fish were sent down the rail lines linking the coastal towns to burgeoning Glasgow, but trawling simply did not become the fishing mode of choice. As of 1875, the port of Aberdeen had only two sailing trawlers registered; a few more began fishing out of Aberdeen at decade's end. In 1881 and 1882, a few refitted paddle steamers began trawling out of Aberdeen, and the *Pioneer* of Scarborough took up registration in Aberdeen the next year.[119] Aberdeen was on its way to quite quickly becoming what James Coull describes as "one of the half-dozen main trawl ports delivering fish to the British market via the railway network."[120] The Aberdeen shipyards began turning out metal-hulled steam trawlers for fishing work in Aberdeen and elsewhere in Britain,

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[118] Robinson, *Trawling*, 93.
[119] Robinson, *Trawling*, 94.
[120] J. R. Coull, "The Development of the Fishery Districts of Scotland," *Northern Scotland* 12 (1992): 126. See also Coull, *The Sea Fisheries of Scotland*, 143-144.

including such lesser trawling ports as Granton, Dundee, Fraserburgh, and Peterhead.[121] Steam trawling, spurred on by the unceasingly high demand for white fish, had become the way Britain kept its people supplied with cheap protein, not to mention a remarkably profitable and fast-growing industry, employing many people both directly and indirectly. While carpenters, sailmakers, and blockmakers were hurt by the decline of sailing trawlers – which were being sold off for a pittance or even merely junked in the glutted smack market – such groups as blacksmiths, engineers, and riveters were reaping the rewards of the steam trawler building boom.[122]

Concerns Emerge from Within the Trawling Community

Amidst all the ebullience of the large-scale, industrialized, far-ranging fishing industry that had become established in Britain by the 1880s, however, concerns grew that the industry might be experiencing a kind of bubble. All of the employment, boats, corporations, and the money that all of this represented relied upon the continued availability of the white fish being trawled. It was not clear that the fish themselves could persist in the face of such an energetic fishing industry, and indeed the phenomenon of fleets of fishing boats voyaging ever further afield in order to keep bringing more fish to market was not lost on those who owned and operated the boats. There had been, in the 1866 and 1879 Commission reports, a willingness to re-examine the influence of the trawlers and limit their activities (or at least the size of fish they could catch) if at some point they were deemed too destructive; as well, closed seasons had been imposed on freshwater fishing throughout Britain in the same year. Perhaps, some thought, the sea fish could benefit from a closed season as well; these fish, after all,

[121] Robinson, *Trawling*, 95; Coull, *The Sea Fisheries of Scotland*, 140.

supported the sea fisheries, which would also perhaps stand to benefit by having to back off from their current froth of fishing activity. Other nations around the North Sea had already looked askance at the formidable and insatiable British steam trawling fleet, and had banned inshore trawling along their own coasts, and perhaps it was time for Britain to consider doing likewise.

The nation's liners certainly thought so: their irritation with the trawlers, which had decreased not at all following their defeat in the 1863 Royal Commission inquiry, had itself become frothy when steam trawlers appeared on the scene. Steam trawlers had more precise control than sailing trawlers, and so could work much closer to the shore, greatly increasing the chances that one might steam right through a carefully laid long line or drift net.[123] The liners had always had two objections to trawling: trawlers carelessly destroyed their fishing gear, and trawlers took all of the fish from the area over which they passed, leaving none to be caught by others.

It was this second fear that came to be shared by some of the trawlermen as well, who in 1882 formed the National Sea Fisheries Protection Association, which met yearly to discuss a range of topics, but always the threat to fish stocks, which was a threat to their livelihoods as well. In a move that would have been unimaginable a decade or two earlier, at the 1888 meeting the NSFPA passed a resolution "calling for the fisheries authorities to be given power to suspend or regulate trawling and certain other methods of fishing whenever expedient."[124] The trawlermen were, in effect, asking the government to limit their activities, probably on the reasoning that if only some of the trawling companies, and not others restricted their fishing activities in order to protect the fish

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[122] Robinson, *Trawling*, 114.
[123] Coull, *The Sea Fisheries of Scotland*, 140.

stocks, others would keep on trawling and the result would be a loss to the forbearing trawlers and no gain to the fish. Two years later, however, they tried to do just that, passing a "self denying ordinance" that established a closed season to trawling by NSFPA members while calling for the government to limit landing size and make international agreements on fisheries regulations. The next year's conference was international in character, and industry delegates from several European fishing nations called for international cooperation on protecting the fisheries. They also called on science to help assess and counter the threat to fish stocks.[125]

This call for science to play a role in supporting the fisheries by understanding and improving the fish stocks was not new. The 1866 Royal Commission report, and the 1879 Buckland and Walpole report after that, had both called for – indeed, demanded – the regular collection of fisheries statistics. The spread of steam trawling, a fresh wave of complaint from the traditional fishermen, and concerns within the trawling community that led to the founding of the NSFPA all led to another Royal Commission on Beam Trawling to be appointed in 1883. More explicitly than the previous two inquiries, this third was intended primarily "to inquire into the Complaints which have been made by Line and Drift-net Fishermen of injuries sustained by them in their calling, owing to the use of the Trawl-net and Beam-trawl in the Territorial waters of the United Kingdom."[126] Should these complaints be found legitimate, the Commission was charged with identifying "whether any, and what, legislative remedy can be adopted without

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[124] Robinson, *Trawling*, 102.
[125] Robinson, *Trawling*, 103.
[126] "Report of the Commissioners Appointed to Inquire and Report Upon the Complaints that have been made by Line and Drift Net Fishermen of Injuries sustained by them in their Calling owing to the Use of the Trawl Net and Beam Trawl in the Territorial Waters of the United Kindom; with Minutes of Evidence and Appendix," *House of Commons Parliamentary Papers* XVI (1884-1885): iii.

interfering with the cheap and plentiful supply of fish."[127] Five Commissioners were appointed: the Earl of Dalhousie, Edward Marjoribanks, Huxley (who at the time was indifferently serving as Buckland's successor as Fisheries Inspector), William Sprotson Caine, and the Irish Fisheries Inspector Thomas Francis Brady. Despite the calls for science and statistics in previous reports, the Commissioners found that they still had no source of scientific data with which to assess the impact of trawling; but this Commission was entitled to "invite such Persons as you may judge most competent by reason of their situation, knowledge, or experience to afford you correct information on the subject of this Inquiry" in order to partly remedy this lack of data, and to avoid turning this new inquiry into a useless repeat of previous ones. The Commissioners chose William Carmichael M'Intosh, recently appointed Professor of Natural History at the University of St. Andrews, to be their expert. In October of 1883, M'Intosh received a letter from Huxley, asking if M'Intosh would be willing to work for the Commission as a "scientific naturalist" on some trawling experiments, for which "a proper vessel will be provided."[128] M'Intosh accepted, and in his capacity as Sub-Commissioner he made ninety-three hauls with the trawler provided by the Commission, a modern steam trawler belonging to a modern fishing business, the Granton Steam Fishing Company (the manager of which was among many in the industry interviewed by the Commission).[129] Some science was better than none, and full reports of M'Intosh's experiments were included in the text of the Commission's report, but the report still reflected how

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[127] "Report of the Commissioners Upon the Complaints," iii.
[128] T. H. Huxley to W. C. M'Intosh, October 19, 1883, reproduced in A. E. Gunther, *The Life of William Carmichael M'Intosh, M.D. F.R.S. of St. Andrews 1838-1931: A Pioneer in Marine Biology*, (Edinburgh: Scottish Academic Press, 1977), 82. Aside from this early involvement and "his assistance from time to time", Huxley's health prevented him from being involved with the work of the Commission, and his name was not signed to their report. ("Report of the Commissioners Upon the Complaints," xliv.)
[129] A. E. Gunther, 82; "Report of the Commissioners Upon the Complaints," xxxiv.

unsatisfactory the state of marine science in Britain was, and how urgently new knowledge and statistics was needed. Tentatively concluding that flatfish were possibly declining in territorial waters from Aberdeen to Grimsby and soles probably declining offshore in the North Sea, but that trawling was not to blame for this, except "on particular grounds, especially in narrow waters," they asserted that "[i]n the absence of a proper system of fishery statistics and scientific observations, it is impossible to discover the causes, or measure the fluctuations of the fisheries."[130] The Commission's first five recommendations related to the need for fisheries administration throughout the United Kingdom, and eventually a "central authority", provided with funding "for the purpose of conducting scientific experiments and for collecting fishery statistics."[131]

The Commission's report appeared in 1885. Its conclusion that trawling was not unduly damaging fish stocks was received with fury by the traditional fishermen – some of those in St. Andrews, Scotland burnt an effigy of one of the Commissioners, local luminary and university professor William Carmichael M'Intosh, in expression of their feelings[132] – and with dismay by the NSFPA, who were not convinced. Its call for more science, however, was received with great enthusiasm by certain members of the British scientific community, particularly members of the increasingly marine-oriented biological community.

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[130] "Report of the Commissioners Upon the Complaints," xliii.
[131] "Report of the Commissioners Upon the Complaints," xliii.
[132] And that wasn't all: M'Intosh recalled in his autobiography that "signs of discontent amongst the fishermen…were apparent, some muttering threats and others using opprobrious epithets as I passed. The fisher-boys shouted also as I went through their quarters in North Street. When talking to Captain Burn a week or two before he made the curious remark: 'The fishermen would kill you.'…I saw in the dusk a great crowd of people and an effigy in their centre surging from Market Street into Hope Street. Shouting and making other noises, they came straight to my house, but before they reached it a suspicion that this was the outcome of the threats I had heard, caused me to lock the front door and gate. They halted opposite the house, rushed to and shook the front gate, tried the area gate, and yelled and shouted like demons. They set the 'effigy'…in front of my house, and kindled it, adding tar and other combustibles to make a blaze…The

A Role for Biology

While fish were on the ascendancy where economic importance and public awareness was concerned, a similar rise to prominence (albeit on a far smaller scale) was taking place within Britain's biological community. Just as increasing numbers of trawlers plied the waters for fish in the growing fishing industry, increasing numbers of British scientific workers were directing their efforts into the study of marine organisms, and the sea became a site for scientific industry. The middle decades of the nineteenth century had seen an amateur craze for the collection and study of marine organisms in simple aquaria that could be kept at home, beginning after the publication of Philip Henry Gosse's *A Naturalist's Rambles on the Devonshire Coast* in 1853 and thriving until the late 1860s. Encouraged by the publication of several popular handbooks on aquarium-making, enthusiasts descended on the beaches of Britain, collecting animals and plants for display in small parlour aquaria.[133] As the parlour aquarium craze ebbed, it became fashionable during the 1870s to visit large public aquaria, home to more exotic specimens like dolphins, octopi, and baby crocodiles.[134] David Elliston Allen's landmark *The Naturalist in Britain: A Social History* records that

> according to the president of the Birmingham Natural History and Microscopical Society, the study of marine zoology had attained an interest 'second to that of no other branch of natural history'. Suddenly, the existence and

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few police were helpless in so great a crowd, and all they could do was by and by to stamp out the smouldering embers." William Carmichael M'Intosh, *Autobiography*, quoted in A. E. Gunther, 86-87.

[133] Lynn Barber, *The Heyday of Natural History 1820-1870* (London: Cape, 1980), 115-123; David Elliston Allen, *Naturalists and Society*, (Aldershot: Ashgate Variorum, 2001,) V, 404-407; Matthew Goodrum, *The British Sea-Side Studies, 1820-1860: Marine Invertebrates, the Practice of Natural History, and the Depiction of Life in the Sea*, Ph.D. thesis, Indiana University, 1997.

[134] Barber, 121-123. Among wealthier Britons possessed of yachts or friends with yachts, dredging expeditions became a popular entertainment during this time. (David Elliston Allen, *The Naturalist in Britain: A Social History*, (London: Allen Lane, 1976,) 207-208.

> habits of the denizens of the sea were being discussed 'as familiarly in the newspapers of the day as the events of social and political life'; while public enthusiasm for the marine aquaria established in various leading towns had grown so immense that 'the arrival of the octopus attracted almost as much attention as the visit of a foreign emperor, and the death of a porpoise was mourned as a national calamity'.[135]

This enthusiasm persisted into the later decades of the century through such entertainments as Frank Buckland's fish hatching display and lectures at the Science Museum at South Kensington,[136] and the public zoological enthusiasm combined with the rising star of food fish at wildly successful Fisheries Exhibitions held at Edinburgh in 1882 and London in 1883.[137]

Within Britain's growing community of professional biologists, too, marine organisms were becoming sought-after objects of study. The Royal Society began sponsoring dredging trips in the 1840s, albeit on a small scale. The Royal Society's involvement, and the government's limited involvement, in sponsored expeditions expanded in 1868 with the sea voyage of the *Lightning*, a paddle-steamer that the Royal Society had convinced the Admiralty to loan them and had paid to have outfitted for the trip. Charles Wyville Thompson and William B. Carpenter, both of Queen's College, Belfast, had managed to convince the Royal Society to take on this supporting role, and convinced them again the following years. In 1869 and again in 1870, another loaned naval ship, the *Porcupine*, left Belfast to collect more samples and dredge even deeper, and the year after that an even larger expedition took place with the H.M.S.

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[135] Allen, *The Naturalist in Britain*, 208.
[136] Jester, 97-100.
[137] Margaret Deacon, *Scientists and the Sea 1650-1900: A Study of Marine Science*, 2nd edition, (Brookfield, VT: Ashgate, 1997), 378; Eric Mills, *Biological Oceanography: An Early History, 1870-1960*, (Ithaca: Cornell University Press, 1989), 190.

Shearwater.[138] The *Challenger* expedition of 1872, a major, round-the-world undertaking on a naval steam corvette, was again led by Wyville Thompson and Carpenter. Unusual in the generosity of its government funding – perhaps because of Carpenter's speech at the Royal Institution in 1871, in which he "called on Her Majesty's Government not to allow Britain's present lead in marine science to go by default"[139] – the *Challenger* expedition produced a massive body of results that translated into years of work, and thousands of pages of publication, for many British biologists.[140]

As well, with the emergence of evolution as an area of active scientific inquiry, marine organisms presented intriguing examples of the transition from invertebrates to vertebrates. Ernst Haeckel's ontogenetic law, stating that an individual organism's development is a recapitulation of its species' evolutionary history, suggested that the study of higher invertebrates like echinoderms and lower vertebrates like fishes was the avenue most likely to lead to understanding of such major evolutionary transitions. The promise of marine organisms as the source of evolutionary secrets was a major motivation in Anton Dohrn's establishment of the Naples Zoological Station in 1872; Dohrn himself devoted much effort to establishing what kind of creature the immediate ancestor to the vertebrates might have been.[141] The importance of marine organisms to the most pressing questions facing biology only increased as time went on, and it became

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[138] Deacon, *Scientists and the Sea*, 306-332.
[139] Deacon, *Scientists and the Sea*, 333.
[140] Deacon, *Scientists and the Sea*, 333-365; Margaret B. Deacon, "Crisis and Compromise: The Foundation of Marine Stations in Britain During the Late 19th Century," *Earth Sciences History* 12 (1993): 23.
[141] Deacon, "Crisis and Compromise," 28. Dohrn's evolutionary preoccupation is apparent when reading his letters to contemporary zoologists like Francis Balfour.

a widely held conviction that, as the Duke of Argyll expressed it in 1884, "the sea is the area in which and out of which we can best get at some of the secrets of organic life."[142]

Margaret Deacon has noted the remarkable number of highly-placed British zoologists during the 1870s and the 1880s who were working primarily on marine organisms.[143] Francis Balfour, who along with Michael Foster had revolutionized the way biology was taught at Cambridge University by incorporating a major hands-on, laboratory-based component, was the author of a definitive work on the development of elasmobranches (cartilaginous fishes like sharks, rays, and skates). E. Ray Lankester at Oxford worked on a variety of marine invertebrates, including the tunicates, invertebrates with a vertebrate-like larva. W. C. M'Intosh at the University of St. Andrews and his hated rival D'Arcy Wentworth Thompson at Aberdeen worked on fish, as did James Cossar Ewart at Edinburgh and W. A. Herdman (a former student of Wyville Thompson's) at Liverpool.[144] Marine creatures of various kinds were coming to dominate the landscape of British zoology.

However prominently they might occupy positions in the nation's universities and such bodies as the Royal Society and the British Association for the Advancement of Science, however, marine-oriented science in Britain had an infrastructure problem – that is, they lacked it, and as such lacked reliable access to the coasts, and to specimens of the species that they studied. Dohrn found enthusiastic support among British biologists for his plan to establish a marine station on the Mediterranean, and in an address given to the

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142 "Report of the Foundation Meeting of the Marine Biological Association," in "The History of the Foundation of the Marine Biological Association of the United Kingdom," *Journal of the Marine Biological Association of the United Kingdom* 1 (1887): 27.
143 Deacon, "Crisis and Compromise," 29.
144 For the story of Herdman and the fisheries research that he spearheaded at Liverpool, see Trevor A. Norton, "Fisheries Research at Port Erin and Liverpool University," *Buckland Occasional Papers* 2 (1996).

British Association at Liverpool in 1870, he stressed that the same sentiments that gave rise to the Naples station would yield other such places; the British Association duly established a committee to promote marine stations.[145] Simple aquaria could only go so far, and biologists studying marine organisms needed to be able to go to the seashore and properly study their organisms, freshly collected from their natural habitats. When Naples was established, British biologists were frequent visitors; the British Association and Cambridge and Oxford Universities paid to book work tables at various times, and a trip to Naples was a rite of passage for many up-and-coming biologists. But Naples was a long way to go, especially for the biologists of an island nation like the United Kingdom, some of whom looked with envy at their neighbours across the English Channel. In France, the Roscoff laboratory at Concarneau had been established for fish hatching projects way back in 1859 and continued to grow as a place for experimental scientific work; in Kiel, the Prussian government had paid for a Commission for the Investigation of the German Seas in 1870, which had carried out numerous "voyages of investigation"[146] throughout the decade and established sixteen permanent observation stations along the coast.[147] The work done across the Atlantic at the United States Fish Commission, founded in 1871, whose coastal laboratories were funded to an unheard-of extent by the American government, had made it the world leader in fish hatching and rearing, surpassing even the advanced state of Scandinavian work on the subject. Marine organisms might have been prominent within British biology, but British biology had,

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[145] Anton Dohrn, "On the Foundation of Zoological Stations," *Report of the British Association for the Advancement of Science* (1870); Deacon, *Scientists and the Sea,* 378. The British Association eventually had separate committees for the promotion of the Naples, Granton, and Plymouth stations, each of which administered small annual grants to the stations and presented annual reports.
[146] Cunningham, *Marketable Marine Fishes*, 9.
[147] Mills, 14-15.

with very occasional exceptions like the *Challenger* expedition, nothing like the institutional structure and government involvement that supported work on marine organisms in other countries.

Government involvement was the chief difficulty. Generally speaking, the various British governments of the late nineteenth century preferred not to hand out money to support science, much less commit to years of support for scientific institutions. However, there were exceptions to this rule. The government was willing to part with some money, as it had in the case of the 1883 Royal Commission's project of scientific experiments on trawling, undertaken by M'Intosh, if it seemed certain that science could clearly answer particular questions, or clearly promote a particular area of industry.[148] And, as Peter Alther has argued, the government was willing to part with significantly more money for scientific projects if the projects were seen as a matter of international prestige, as had been the case with the *Challenger* expedition.[149]

The particular set of conditions that prevailed in Britain in the 1880s, which saw fish (and other marine organisms used for food or bait, such as oysters and mussels) become a suddenly valuable currency in both the broader national and narrower scientific economies, made possible the establishment of homegrown marine laboratories, administered by biologists and funded *in part* by the government. Three successive and nearly identical government Commissions had demanded more science; setting up repeated multi-year traveling Commissions who could do little but ask the opinions of the very fishermen whose concerns had prompted the inquiries was becoming embarrassing, and worries about the fish and the fishing industry were only becoming more acute. At

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[148] Deacon, "Crisis and Compromise," 24; Peter Alter, *The Reluctant Patron: Science and the State in Britain 1850-1920* (Oxford: Berg, 1987), 52.

the same time, Britain was demonstrably behind its peer nations in yoking marine science to the fisheries in an attempt at scientifically-based policymaking. And in the midst of all of these practical concerns about fish, there was in Britain a thriving community of biologists who were actively studying marine organisms, yet lacking somewhat in proper access to organisms from their own coasts and envious of marine stations elsewhere. There was, that is, a contemporary economy in which marine stations might possibly happen, if all of these goals and motivations could be aligned convincingly. This particular economy did not go unrecognized within Britain's professional biologists, and certain among them began the work of doing the convincing.

Lankester was, like Dohrn, a supporter of the marine station movement wherever in the world it might take hold. In the pages of *Nature* in 1880, he wrote a near-tirade in support of the establishment of a permanent laboratory at Johns Hopkins University, building on the temporary laboratory run by W. K. Brooks. Marine stations were essential points of scientific access to the sea and its creatures, and their establishment could not be gone about by half-measures, he argued:

> That any enthusiastic young person who may unfold his umbrella on the sea-shore and contemplate under its shadow the starfish washed to his feet – should say that he has 'opened a zoological station' may be strictly true so far as the etymology of the words 'zoological' and 'station' respectively is concerned; but it is at the same time a misleading announcement, and likely to do more harm than good to the cause of zoological stations. There is no need to call a little sea-side laboratory by the pompous title which gains its connotation from Dr. Dohrn's magnificent institution on the Mediterranean shore, and it is a very satisfactory thing that such laboratories, open under certain conditions to naturalists who wish to make use of them, are coming into existence.[150]

[149] Alter, 66.
[150] E. Ray Lankester, "An American Sea-Side Laboratory," *Nature* 21 (1879-1880): 498.

Marine stations, that is, were something different from what had gone before: a place for serious naturalists to undertake serious work in well-equipped facilities. They were to be a departure from the amateur's pleasurable collecting trip to the seashore that is evoked by Lankester's young person unfolding his umbrella on the beach. Real marine laboratories were not for "any enthusiastic young person" collecting specimens for a home aquarium, but rather for professional scientists working on biological questions. Not that casual marine naturalists were to be entirely shut out: the public aquaria that were all the rage in 1870s Britain[151] would form an income-generating element of the private, professional marine stations.[152]

Two years later, the Edinburgh and London Fisheries Exhibitions brought public interest to its highest pitch yet.[153] Such international exhibitions had been held on the Continent at several points during the preceding decades, and one had recently been held in Berlin. John Ramster has noted that "[Fisheries Inspector Frank] Buckland had provided exhibits on behalf of the United Kingdom at all of them but his efforts had been outclassed by the national displays that had been put on."[154] The London Fisheries Exhibition attracted widespread public interest, attracting 2.7 million visitors and several prominent speakers, Lankester and Huxley among them. Huxley gave a notorious speech in which he declared several of the nation's important fisheries – cod, mackerel, herring,

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[151] Barber, 122-123.
[152] David Elliston Allen, "Tastes and crazes," 406-407.
[153] See Chapter Two for more on the Edinburgh Fisheries Exhibition.
[154] John Ramster, "Fisheries Research in England and Wales, 1850-1980," in *England's Sea Fisheries: The Commercial Sea Fisheries of England and Wales since 1300*, edited by David J. Starkey, Chris Reid, and Neil Ashcroft, 179-187. (London: Chatham, 2000.) See also Arthur J. Lee, "British Marine Science and its Development in Relation to Fisheries Problems 1860-1939: The Organisational Background in England and Wales," *Buckland Occasional Papers* 2 (1996), 36-37.

and pilchard – "inexhaustible" given the current level of fishing activity.[155] Lankester responded by objecting that overfishing was indeed possible, and could "upset" the "natural balance and chiefly in so far as the production of young fish is concerned."[156] What was needed in order to make an organized study of the sea and its fish was a research station, argued Lankester. The Exhibition provided both an audience and a motivation for the project of organized, institutionalized marine research in Britain.

Hot on the heels of this success, a group of prominent British biologists, among them Huxley, Lankester, Sir Lyon Playfair, and H. N. Moseley, took steps toward establishing a scientific society, the Marine Biological Association, dedicated to the cause of setting up a major marine station in Britain. A. J. Southward argues that

> In effect, the Association was founded at the 1883 Exhibition, and the group of scientists who signed an agreement then to establish marine laboratories on the British coast became the nucleus of the Council of the Association at its inaugural meeting. Huxley did not sign this agreement, which gave great prominence to plans for fishery investigations, but the dispute was patched up at the April 1884 meeting chaired by him, when he agreed to become the President of the Association.[157]

The *Times* was on their side (and was later thanked for its support on the day of the Plymouth Laboratory's opening),[158] and on the same day as the biologists met at the rooms of the Royal Society, there appeared a long endorsement of their cause, carefully

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[155] Ramster, 181.

[156] E. Ray Lankester, "Scientific Results of the Exhibition," in *The International Fisheries Exhibition, London, 1883* (Fisheries Literature 4), part 1, 405-428, cited in Ramster, 181.

[157] A. J. Southward, "The Marine Biological Association and Fishery Research, 1884-1924: Scientific and Political Conflicts that Changed the Course of Marine Research in the United Kingdom," *Buckland Occasional Papers* 2 (1996): 64-65. Accounts of the founding of the Plymouth laboratory are also given in Mills, 190-195, and in Joe Lester, *E. Ray Lankester and the Making of Modern British Biology*, edited by Peter Bowler, (Oxford: Alden Press, for the British Society for the History of Science, 1995), 105-113.

[158] "Opening of the Marine Biological Laboratory," *Journal of the Marine Biological Association of the United Kingdom* 2 (1888): 137.

touching on two arguments most likely to work in their favour, in the newspaper. "British naturalists have been long convinced," pronounced the *Times*,

> that both from a scientific and economical point of view it is high time that a permanent station on the model of that of Naples were established at some suitable point on the coast of England. The success of the recent Fisheries Exhibition has encouraged this prevalent feeling, and has led our leading scientific men to take definite steps to place England in this respect on a level with other countries.[159]

Huxley, the new President, was also President of the Royal Society, and Lankester became the Secretary. However, the Association, and its plan for a marine station, was really Lankester's baby, as Huxley made clear in addressing the gathered naturalists:

> In supporting what I understand to be the object of the proposal before us, which I may say is not in my hands, but chiefly in those of Professor Lankester, I simply express the interest in it which biologists feel, and the desire of the Royal Society to foster the new undertaking, which appears to promise well for the good of science. The establishment of laboratories for observation of the Fauna and Flora of the sea has now taken place in most civilized countries, and is, in fact, a necessary consequence of the great change which has taken place in the whole of the aims of biological science....But though from a purely scientific point of view this is one great reason for establishing laboratories of the kind now proposed, a more directly practical reason exists. We possess great fisheries, which are more or less regulated by legislation, and which are of great importance to very large masses of the population.[160]

As the *Times* had done, as Lankester would do in the near future, and as the Council and various Directors of the Plymouth Laboratory would continue to do in the years that followed, Huxley stayed on message: other nations had already established marine laboratories, and these marine laboratories would have practical benefits for the nation's

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[159] *The Times*, March 31, 1884, reprinted in "The History of the Foundation," 19-20.
[160] "Report of the Foundation Meeting of the Marine Biological Association," in "The History of the Foundation," 22-23.

fisheries. Other speakers at the meeting focused variously on the scientific and practical benefits of having such a laboratory in England, and W. B. Carpenter and Lyon Playfair especially expounded upon the advanced state of such facilities and work in America. Carpenter presented to the group part of the First Annual Report of the U. S. Fish Commission, by now eleven years old, which stated the several aims of that organization, which ranged from detecting and preventing losses to fish stocks to collecting physical and chemical measurements of seawater to studying the natural history of marine organisms.

With the founding of the MBA, Lankester began in earnest the project of convincing government, industry, and a public keenly interested in the ongoing good fortunes of the sea fishing industry of the ability of marine biologists to work with the combined goals of scientific knowledge and practical improvements to the fisheries. The MBA already had support among Britain's scientific community; for the MBA's ambitions to be realized, Lankester had to win support from a much wider circle. This was the aim of a rousing speech that Lankester delivered to the Royal Society of Arts at the Society's twenty-first meeting on May 13, 1885, where he found just the right kind of audience. The Society of Arts dealt in a wide range of practical topics, from agriculture to canals to sewers to language instruction to inventions, and this particular meeting was chaired by E. L. Beckwith, Prime Warden of the Worshipful Company of Fishmongers, who was as well-acquainted as any in Britain at the time with the practical aspects of fish. In his remarks prior to introducing Lankester, Beckwith made clear the economic importance of the fisheries: five hundred tons of fish per day to Billingsgate Market alone and twice that to the rest of the United Kingdom, "an army of 200,000 men"[161] employed in the industry, and five million pounds of capital investment all rested on the health of the nation's fish population. "There was an old proverb to the effect that you should first catch your hare before cooking it;" Beckwith had remarked by way of bringing his introduction from fisheries back to the fish themselves, "but before catching it, it was first necessary that there should be a hare to catch; and it was especially with reference to this question in regard to fish that Professor Lankester was about to speak."[162]

Lankester's paper managed to draw on two major passions – and the two strategies that had any hope of winning government support – in recruiting the Society of Arts audience into the MBA's plan to build a Marine Laboratory on Plymouth Sound. He emphasized the immediate concerns that without better understanding of the biology of fish, Britain would wind up damaging not only an economically important industry but also the security of the national food supply. He also, and perhaps just as effectively, emphasized that Britain was being badly outdone by other nations when it came to taking a rational, scientific approach to its fisheries. Britain's approach to fishing was "still barbaric; we recklessly seize the produce of the sea, regardless of the consequences of the method, the time, or the extent of our depredations."[163] The result of this approach, when scaled up to its present level through "the increase of population, and the introduction of steam fishing boats and more effective instruments of capture," was that "some at least of

[161] "Twenty-first Ordinary Meeting," *Journal of the Society of Arts* 33 (1884-1885): 749.
[162] "Twenty-first Ordinary Meeting," 749.
[163] E. Ray Lankester, "The Value of a Marine Laboratory to the Development and Regulation of our Sea Fisheries," *Journal of the Society of Arts* 33 (1884-1885): 749.

our coast fisheries are being destroyed, and that others may follow in the same direction."[164]

This was a problem, as many were aware, and Lankester argued that a more serious problem lay beneath it: any attempt to act on concerns about the fisheries was stymied by the lack of information about the fish themselves. He began five paragraphs with "we do not know", each one peppered with versions of the phrase – "we cannot tell", "we have not sufficient knowledge", "we know nothing" – each one illustrating how a particular and troubling fisheries problem could not be addressed without the help of science:

> We do not know why soles are getting scarcer every year: we know nothing about soles, and so we can do nothing to remedy their constantly increasing diminution. We do not know why oysters are scarce, or how to make them more abundant…because we do not know all about oysters in the same precise and detailed way in which we know all about wheat, or all about pigs or chickens.[165]

Lankester managed to communicate a general sense of ignorance and panic when it came to Britain's sea fisheries, an awareness of problems paired with a complete inability to act on them: "we do not know why", as he said of the soles, "so we can do nothing".[166]

Meanwhile, on foreign shores, panic of just this kind had been averted when "[o]ther civilised nations" – France, Norway, "and above others, the Americans" – had "perceived the necessity of attempting to regulate the various kinds of sea-fisheries on rational principles – that is to say, on principles based on an exact knowledge of the life and habits of the fishes which it is desired to capture."[167] The Americans, under Spencer

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164 Lankester, "The Value of a Marine Laboratory," 749.
165 Lankester, "The Value of a Marine Laboratory," 750.
166 Lankester, "The Value of a Marine Laboratory," 750.
167 Lankester, "The Value of a Marine Laboratory," 750.

Baird of the Fish Commission, had actually managed to repopulate the waters of Gloucester Harbour in Massachusetts with cod, and several of the east coast rivers with shad, and were in the process of working similar miracles with oysters. The Fish Commission's work was fully supported by government money to the tune of (the equivalent in U.S. dollars of) seventy thousand pounds in 1884 alone. All that was needed to set Britain down the path to success was a smaller contribution of ten thousand pounds, half the cost of the Naples Zoological Station and about the same as the Americans were about to spend on a marine laboratory at Woods Hole, and of which some five thousand had already been raised. A small price to pay to cure ignorance and panic, this money would build the place where scientific answers were to be found:

> A laboratory on the sea-shore, provided with boats and fishermen, and having within its walls tanks for hatching eggs and watching sea fish, and conveniences for the work of naturalists trained in making such observations...The Association does not propose merely to build this place, but to arrange for the carrying out there of most important investigations on such questions as those I have a few minutes ago named."[168]

If others could provide the money, the MBA would set up the facilities and set the research agenda, an agenda that Lankester was doubly sure would be pursued, "because the arrangements and the studies which are necessary to answer the questions of the practical fishermen are also just those which are necessary to advance the knowledge of the order of nature which forms the single object of truly scientific investigation."[169]

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[168] Lankester, "The Value of a Marine Laboratory," 750. The eventual cost of building and equipping the Laboratory turned out to be £12,500. The MBA was able to raise "between £16,000 and £17,000...They have also an annual grant from the Government of £500." ("Marine Biological Laboratory," *Journal of the Society of Arts* 36 (1887-1888): 916.)
[169] Lankester, "The Value of a Marine Laboratory," 750.

Donors need not worry, that is, that their money would be used to support work with no bearing on practical questions.

Lankester had made a series of public promises on behalf of the MBA about what the Plymouth Laboratory would be, what kinds of work would go on there, and what practical questions relating to the fisheries would be addressed. Britain would catch up with "other civilised nations" when it came to fisheries management, and stop wasting efforts on gaining knowledge about the sea through channels other than scientific ones. This last point, in reference to the frequent Royal Commissions, was especially emphasized in the discussion that followed Lankester's talk, in which Sir Lyon Playfair said several times and in several ways that "the last person in the world to know anything about fish was a fisherman" and that the only sensible thing to do in future was "go straight to nature, who always told the truth, if she were approached in a reverential spirit."[170] Going straight to nature meant going through scientists in order to get answers about the sea. Lankester made ambitious claims about what science could get from nature if properly funded and equipped. His appeal moved the meeting's chair, Beckwith of the Fishmongers' Company, to ask for and receive from the fishmongers' Court of Assistants, who met the following evening, a promise of a grant of two thousand pounds, one thousand to be paid in 1885 and the rest to be paid out at two hundred pounds per year for the next five years.[171] Reporting three years later on the opening of the Plymouth Laboratory, the Society of Arts registered its satisfaction with having played a part, with Lankester's paper having been given when Beckwith was chairing: "The attention of the

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[170] Lankester, "The Value of a Marine Laboratory," 757.
[171] Lankester, "The Value of a Marine Laboratory," 758.

Court of the Company [of Fishmongers] was thus drawn to the movement, and the result was that the Company contributed largely to the necessary funds...".[172]

By 1885, the Association had raised some £8,000; by the next year, with £2,000 from the Fishmongers, £5,000 from the Government (with the promise of £500 per year for the next five years),[173] and numerous smaller donations from a total of 384[174] companies, individuals, and societies like the British Association, the MBA had raised almost £15,000, and enough was raised to begin construction the next year, in 1887, on the Marine Biological Association Laboratory at Plymouth.[175] The Plymouth Laboratory was a coalition venture, organized and run by the MBA but funded by a plethora of donors representing a variety of interests. Surveying the by then numerous marine stations of Europe, an American observer commented in 1910 that [t]he striking feature of the subscription lists is the large number of individuals contributing...nearly two thirds of the total, was accumulated by small subscriptions."[176] As Southward has observed, the "public appeal for funds to build a laboratory would have taken much longer if the fishery aspect had not been stressed; a considerable part of local assistance from Plymouth businessmen was due to this promise to study practical fishery problems," and

[172] "Marine Biological Laboratory," 916. Even when the Laboratory was being opened, the campaigning continued, with the Earl of Morley speaking on how much further advanced other nations, among them France, Austria, and Germany, were not only in having stations but in having significant government funding (and he provided some choice figures, such as the £1500 per year that the German government gave to the Naples station) supporting those stations. ("Opening of the Marine Biological Laboratory," 132-133.)
[173] David Elliston Allen, *The Naturalist in Britain: A Social History*, (London: Allen Lane, 1976), 212.
[174] Charles Atwood Kofoid, "The Biological Stations of Europe," *United States Bureau of Education Bulletin* 4 (1910), 146.
[175] "The History of the Foundation," 39.
[176] Kofoid, 146.

made possible the initial and future financial support of the usually reluctant government.[177]

In Scotland, W. C. M'Intosh established a marine research station with the help of the Fisheries Board for Scotland based on the same kinds of promises that Lankester and the MBA had made in England: that practical improvements come from scientific research, that the distinction between the two is unclear and perhaps even unimportant. Setting up a marine laboratory at St. Andrews was a long-cherished plan of M'Intosh's – his biographer describes it as "the ambition of his life"[178] – but it was not realized until M'Intosh could convince the University, and the University could convince the government, that such a station had both scientific and practical value. M'Intosh had begun scheming on such a laboratory while still a student in the 1850s, and as time went on, his biographer relates that "he read impatiently of what the French were doing [at Concarneau] and of the work of the U.S. Fish Commission. Dr. Anton Dohrn's visit to Britain in 1870, and the founding of the Station at Naples in 1872 increased his sense of frustration that nothing was being done in Britain."[179] When M'Intosh applied for the Chair of Natural History at St. Andrews in 1875, he made mention of his goal of establishing a marine station there, and once hired kept up his agitating; when the 1882 Edinburgh Fisheries Exhibition money went to John Murray's new marine station at Granton, which opened in 1884, M'Intosh's ambitions were deferred again.[180]

His big break came in the next year: a new Royal Commission had been appointed, and T. H. Huxley was again a member, this time with two hundred pounds to

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[177] Southward, 65.
[178] A. E. Gunther, 75.
[179] A. E. Gunther, 75.
[180] A. E. Gunther, 75. see also Deacon, "Crisis and Compromise," 36.

allocate for scientific investigations into the effects of trawling, and M'Intosh was called to join on as the supplier of scientific evidence to the Commission. He travelled to London to give evidence for the Commission and his experiments were presented in the resulting Report, and in the course of the whole process established himself as a widely-known authority on scientific approaches to fisheries questions.

Another important set of circumstances moved M'Intosh closer to his dream of a marine station at St. Andrews. The Fishery Board for Scotland had begun publishing annual reports, and in the second of these, issued in 1883, the Board proclaimed that when it came to finding a basis for advising government on the promotion and protection of the fisheries, "[k]nowledge was lamentably deficient".[181] The Board knew of M'Intosh's work for the Royal Commission, and in the spring of 1884 gave him some money in order that he could continue his studies of marine food fish by equipping an old building, the St. Andrews Fever Hospital, by the water to function as a laboratory.[182]

M'Intosh's personal project of establishing a marine laboratory, combined with his growing reputation as a naturalist with answers to fisheries questions, along with the involvement of the Fishery Board in his studies of food fish was enough to convince the University that a proper marine laboratory should be built. In May of 1884 the University issued a statement of their "resolve[] to approach Government"[183] seeking support for the project of a marine laboratory. St. Andrews was an ideal site for such a station, and had been praised as such by no less a figure than Dr. Dohrn himself; it would be useful to the University both in research and teaching; and it would attract prominent

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[181] Fishery Board for Scotland, *Second Annual Report*, 1883, p. xxi, quoted in A. E. Gunther, 91.
[182] A. E. Gunther, *William Carmichael M'Intosh*, 92; Deacon, "Crisis and Compromise," 37.
[183] "University of St. Andrews Biological Laboratories and Extension of the Museum," 21 May 1884, reprinted from M'Intosh's *Autobiography* in A. E. Gunther, 79-80; p. 79.

naturalists from Britain and abroad. The statement stressed as well the "industrial as well as scientific advantages"[184] of such a station to the nation's fisheries. The Fishery Board for Scotland had already involved itself with the "temporary Marine Station" at St. Andrews, the University argued, and a proper laboratory there would be "in the best possible position for obtaining the requisite material for solving the many interesting and important problems connected with the distribution, habits and development of our food-fishes".[185] The first set of visitors to the St. Andrews facilities turned up that summer, and included Lankester among their number.

The Plymouth and St. Andrews Marine Laboratories had certain differences in the circumstances under which they were founded, but had in common that they had been established on the strength of promises, explicit and implicit, that scientific research into the biology of food fish would yield practical guidelines for regulating and maintaining the nation's fishing industry. This dissertation examines what keeping these promises looked like. The bulk of the fisheries-directed marine biological work done at the MBA's marine station was undertaken by their staff naturalist, J. T. Cunningham. Cunningham owed his job to these promises, and embodied the effort to make good on them in every aspect of his work. In Chapter Two, I demonstrate how Cunningham's status as a scientific salaryman influenced not only what work he did, but how he did it and how he went about recording and reporting it. In Chapter Three, I show how Cunningham made use of his status as a fisheries marine biologist to recruit the local fishing community into his network of marine specimen supply chains, and how he gradually worked to bring the fish into his laboratory for more and more of their lives in

[184] "University of St. Andrews Biological Laboratories," 79.
[185] "University of St. Andrews Biological Laboratories," 79.

order to exert even greater control over them, all part of the promise of practical improvements to the fish stocks. In Chapter Four, I trace two simultaneous projects undertaken by Cunningham using a single population of laboratory-dwelling flounders. One project was of primarily scientific interest to questions of evolution and development, the other of major practical importance to questions of setting minimum landable fish size, but both shared primary concerns with the role played by the conditions of life, both in the laboratory tanks and in the sea outside. In Chapter Five, I examine the notion of fish as commodities as well as organisms, a notion that had its origin in the scaling-up of the sea fisheries during the preceding decades, and how Cunningham and M'Intosh as well as to the Plymouth station and the MBA used the salability of fish as a commodity of dietary, public-interest, and scientific consumption to help legitimate and promote their scientific work. And in Chapter Six, I explore the remarkably sophisticated scientific arguments used by M'Intosh to construct an argument for the inexhaustibility of sea fish, an argument that many wanted to believe but few did, and which carried him further and further from the scientific mainstream. None of these activities would have mattered much, or perhaps would not have been undertaken at all, except within the contemporary economics that were in place in Britain during that period as a result of the astonishing changes to the fishing industry, and the corresponding rise to prominence of the fish themselves.

Chapter 2

J. T. Cunningham, Naturalist for Hire: Scientific Work and Labour at the Marine Biological Association Laboratory

Compared to what had gone before, the world of nineteenth-century British science was a diverse place, and J. T. Cunningham occupied an interesting niche within that varied world: he was a salaried naturalist, employed at an institution founded and run by other scientists, an institution that derived its funding from a plethora of sources including government, industry, the scientific community, and a wide range of individual donors with their own combinations of interests. From 1884 until 1896, Cunningham was a paid scientific worker at a relative newcomer to the scene of British science: the seaside marine laboratory, which had been an institution in continental Europe for decades, and which became a reality in Britain with the high public profile of the state of the fisheries in the early 1880s, the successful Fisheries Exhibitions in London and Edinburgh, and the call for answers from science that had come from the 1883 Royal Commission appointed to look into the fisheries. Working first as Superintendent and Chief Naturalist beginning in 1884 at Britain's first marine station in Granton, Scotland, and then as Chief Naturalist beginning in 1887 at the new Marine Biological Association marine station in Plymouth, England, Cunningham occupied this niche of salaried naturalist-to-the-station. A scientist himself, he worked in the employ of other scientist-administrators who were able to exert influence on the kinds of investigations he undertook, and even on where he published the results of his studies. Cunningham was a kind of indentured free agent, pursuing his own interests within the realm of the fisheries biology he had been hired on to study, but always conscious of his various accountabilities to those who paid his salary and provided the facilities in which he worked. One of the reasons that Cunningham's scientific notebooks are such a useful historical resource, if at times an odd read, is that he wrote them with an apparent consciousness that he was being watched – as indeed he was. The notebooks are a record not just of the expected details about specimens collected and drawings made, but also of a personal time-clock, expense accounts, and justifications for absences, a feature that is very unusual and does not appear at all, for instance, in the notebooks of W. C. M'Intosh, who was doing very similar work to Cunningham's, at the same time and in the same country. The difference between their notebooks is due less to individual personality differences, and due far more likely to differences in circumstance between M'Intosh, who in addition to heading up the St. Andrews Marine Laboratory was first and also Professor of Natural History at the University of St. Andrews, and Cunningham, who relied for his pay upon the continued satisfaction both of the Council of the Marine Biological Association and of the Laboratory's Director (who was the only other paid worker at the MBA's Plymouth Marine Laboratory) with Cunningham's performance and results. Ensuring this continued satisfaction meant the submission of regular research reports for publication in the *Journal of the Marine Biological Association*; Cunningham's reports, articles, and research notes make up a very large proportion of the pages of the journal from its second volume in 1888 until 1897. It also meant, on at least one occasion, putting aside his own research program in order that Lankester might be supplied with specimens for his own studies of marine organisms. At Granton and again at Plymouth, Cunningham worked as a scientific salaryman.

It is an unchallenged fact that one of the distinguishing features of British science in the nineteenth century is the emergence of the professional scientist. But any inquiry into this apparently straightforward development immediately reveals that making sense of professionalization in scientific Britain is no easy task. There were, it transpires, a lot of people working as what might be called professional scientists, but the nature of their work was hugely variable. Some worked for universities, enjoying high social standing and nearly complete freedom to choose the direction of their research, and earning money from fees paid by students taking their classes: such a one was W. C. M'Intosh, Professor of Natural History beginning in 1882 at Scotland's oldest university, St. Andrews. Some, such as Robert Angus Smith, Alkali Inspector for the British government, paid £700 per year beginning in 1868 to work as what Roy MacLeod has called a "civil scientist", were salaried men of science, supervised by senior civil servants and expected to narrowly restrict their scientific activities to those advancing a particular set of goals on behalf of the state – in Smith's case, aiding in and enforcing the reduction of muriatic acid emissions from the sodium carbonate factories of the industrial Midlands in conformity with the Alkali Acts legislation.[1] Even such an apparently open-and-shut case as Joseph Dalton Hooker, head of the Royal Botanic Gardens at Kew and member of the X-Club, a group whose name is synonymous with the emergence of the professional biologist,[2] turns out to "conform[] indifferently"[3] to the catalogue of characteristics we

[1] Roy MacLeod, "The Alkali Acts Administration, 1863-84: The Emergence of the Civil Scientist," *Victorian Studies* 9 (1965): 85-112, reprinted in Roy MacLeod, *Public Science and Public Policy in Victorian England* (Aldershot: Ashgate, 1996).
[2] Roy MacLeod, "The X-Club: A Social Network in Late-Victorian England," *Notes and Records of the Royal Society of London* (1970): 305-322, reprinted in *The 'Creed of Science' in Victorian England* (Aldershot: Ashgate, 2000); Adrian Desmond, "Redefining the X Axis: 'Professionals,' 'Amateurs' and the Making of Mid-Victorian Biology – A Progress Report," *Journal of the History of Biology* 34 (2001).
[3] Richard Bellon, "Joseph Dalton Hooker"s Ideals for a Professional Man of Science," *Journal of the History of Biology* 34 (2001): 52.

might associate with the professional scientist: Richard Bellon has argued that for Hooker, professionalism was defined chiefly by "disinterested service to the nation. Pecuniary reward was a necessary adjunct to this service, but never the object of a professional."[4] Given such variety within just these few cases, it is no wonder that Adrian Desmond has claimed that "[e]ven getting an embedded, localized definition of 'professional' in Victorian times is itself becoming *the* problematic in the history of biology."[5]

Another of Hooker's criteria offers some guidance as to how Cunningham might be positioned in this zoo of professional scientists. Hooker believed, says Bellon, that the professionalization of science entailed the "subordination of helpmates and rank-and-file practitioners within a hierarchically organized community."[6] A professional scientist was, by definition, a paid worker, but perhaps more helpfully a professional scientist was one whose work took place at a particular level within a community organized by status and authority. Like Smith, the Alkali Inspector, Cunningham worked for a salary and under the supervision of senior workers, undertaking under this supervision a somewhat prescribed range of scientific work. Unlike Smith, Cunningham's supervisors were themselves scientists, running a private institution that was nonetheless beholden to government and industry for much of its funding. Cunningham's job existed exactly because the institution for which he worked was so beholden; some limits were placed on the scope of his research, and how he chose to publicize his results, because he worked for the institution and had to satisfy its expectations. The institution, in turn, had to satisfy

[4] Richard Bellon, "Joseph Dalton Hooker's Ideals for a Professional Man of Science," *Journal of the History of Biology* 34 (2001): 58.
[5] Desmond, 4.
[6] Bellon, 52.

certain expectations on the part of its donors, which shaped what was expected of Cunningham. He was a scientific worker, working for money, and operating within a hierarchy of other scientific workers: this was as true of Cunningham as it was of Hooker, Smith, or M'Intosh, uselessly broad though such a definition might be. Defining what The Professional Scientist looked like in a given place and time is bound to continue to be a difficult and slippery task. We might get a little further by instead working on describing what a Professionalized Science looks like at different times and in different places, how it is structured, and by what different kinds of workers is it populated. We would not thus be saved from Desmond's problematic of "getting an embedded, localized definition of 'professional'," with all of the dangers of anachronism that attend such a task, but the dizziness that sets in upon finding such a range of different scientific workers might subside a little.

Examining the nature of Cunningham's work as a hired naturalist provides even more evidence of what a heterogeneous group Britain's professional scientists were in the late nineteenth century, while at the same time showing the organizational and authority structures in which he operated. I hope, too, that the particularly interesting case of Cunningham will help develop a view of scientists and scientific work that is in some ways akin to the view that John LeCarré has developed about spies and spying. The Scientist, like The Spy, is too easily seen as a somewhat romantic figure, a self-determining seeker of knowledge nobly engaged in work that most of the rest of us cannot understand, but admire greatly for just that reason. Against this traditional view, or perhaps alongside it, LeCarré has shown us instead the figure of George Smiley: the spy as worker, as professional, as civil servant, as salaryman who must often contend with practical matters and regularly meet the demands of those positioned above him.[7] In this chapter, I give you J. T. Cunningham, the George Smiley of nineteenth-century British biology.

Cunningham at Granton

One of the reasons that Cunningham is such an interesting figure to follow is that his career tracked so closely with the arrival of the marine research station movement in Britain. He was there from nearly the beginning – the beginning of the movement, and from the beginning of two different British marine research stations, and only one degree of separation from another, even earlier one. This beginning is considered by most to be marked by the establishment of the Scottish Marine Station for Scientific Research at Granton in 1884; but Margaret Deacon announced in 1993 that there was a forgotten British marine station, the Scottish Zoological Station, that predated the Granton station and was the true beginning of marine research stations in Britain.[8] The short-lived Scottish Zoological Station was founded in 1879 by James Cossar Ewart, formerly assistant to E. Ray Lankester at University College London. In 1878 Ewart became the Professor of Natural History at Aberdeen University, and after spending time at Naples Zoological Station and at the French station at Roscoff to see how these marine stations worked, established the Scottish Zoological Station, a moveable laboratory that could be set up in a different place each year, beginning with a small fishing village near

[7] A major feature of the modern type of spy writing like Le Carre's, too, is their emphasis on spying as a profession, where earlier works had focused more on the self-funded, self-motivated amateur sleuth. (My thanks to Kendall Shields for this suggestion.)
[8] Deacon, "Crisis and Compromise," 30, 33-35.

Aberdeen.[9] The purpose of the Scottish Zoological Station was to provide advanced teaching facilities for Aberdeen students, and research facilities for Ewart, his colleagues, and visitors. The Scottish Zoological Station ground to a halt when Ewart moved to Edinburgh in 1882 and became a member of the Fisheries Board for Scotland, and began focusing his attention on the prospect of a marine research station somewhere on the Firth of Forth, near Edinburgh.[10] Cunningham, having graduated from Oxford in 1881, received "a summons from Professor Ray Lankester to demonstrate zoology at University College, London"[11] for a year, at the end of which Cunningham took up a fellowship from University College Oxford and entered the marine station life in "the zoologist's paradise at Naples",[12] Dohrn's Statione Zoologica, for a spell, before signing on to work as Ewart's assistant in Edinburgh.

A Firth of Forth marine station opened at Granton in 1884, but Ewart was not involved, and indeed according to Deacon became an enemy of the Granton station, perhaps because of "Ewart's hostility at seeing his own scheme forestalled and his assistant, J. T. Cunningham, leaving to act as superintendent of the Granton Station."[13] The Granton station came to be from a windfall donation of the £1400 profit resulting from the International Fisheries Exhibition, held at Edinburgh in 1882.[14] This exhibition was organised by the Lord Provost, Magistrates, and Town Council of Edinburgh, as well as organisations such as the Highland and Agricultural Society of Scotland, the Merchant

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[9] Deacon, "Crisis and Compromise," 34; J. A. Adams, "The Scottish Contribution to Marine and Fisheries Research with Particular Reference to Fisheries Research During the Period 1882-1939," *Buckland Occasional Papers* 2 (1996), 104-106.
[10] Deacon, "Crisis and Compromise," 35.
[11] "Joseph Thomas Cunningham (1859-1935)," *Journal du Conseil Permanent International Pour L'Exploration de la Mer* 10 (1935): 245.
[12] "Joseph Thomas Cunningham (1859-1935)," 246.
[13] Deacon, "Crisis and Compromise," 36.

Company of Edinburgh, and the Scottish Fisheries Improvement Association, and was a wildly popular event.[15] The money was in fact given to the Scottish Meteorological Society, because they had been undertaking research into the herring,[16] a fish that had long been of major importance to the Scottish, and especially the depressed Highland, economy. The Scottish Meteorological Society tried and failed to get extra money from the government towards the project of establishing a marine station, but the project went forward when John Murray, a prominent scientist who had taken charge of the publication of the reports from the *Challenger* expedition after the death of Wyville Thompson,[17] stepped forward and agreed to found such a station. With donations from subscribers and friends of Murray's, a guarantee of an annual grant of £250 from the Scottish Meteorological Society, and funds from the British Association and the Government Grant Committee, the project went ahead and the station opened in April of 1884.[18] The station was established in the flooded Granton quarry, on land leased to Murray for a mere fifteen shillings. The research laboratory itself was a floating vessel purchased in 1884 for four hundred and thirty-seven pounds,[19] and christened the *Ark* because the raised cabin that was built on after it was purchased "impart[ed] to the whole a striking resemblance to the craft of the toy-shops after which she is named."[20] In addition to the floating laboratory, the Granton station also purchased for £1123[21] a steam-driven, fifty-one foot, thirty-one ton vessel, the *Medusa*, for sounding and

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[14] William E. Hoyle, "The Scottish Marine Station and its Work," *Journal of the Marine Biological Association of the United Kingdom* 2 (1888): 218.
[15] G. N. Swinney, "From herrings to the atom bomb: The legacy of the Edinburgh International Fisheries Exhibition, 1882," *History Scotland* 4 (2004): 35.
[16] Hoyle, "Scottish Marine Station," 218.
[17] Deacon, "Crisis and Compromise," 35.
[18] Hoyle, "Scottish Marine Station," 218.
[19] E3, financial statement, 1 December 1884.
[20] Hoyle, "Scottish Marine Station," 222.

dredging.[22] Owning such a vessel made the Granton station the envy of the St. Andrews and Plymouth marine stations, but in fact the coast around Granton was too rough for such a craft, and the *Medusa* had by 1888 "for some time back been employed almost exclusively on the west coast; her build is rather light for the heavy swells often experienced in the Firth of Forth, and her place on the east coast has been supplied by the hiring of tugs, and by expeditions in steam-trawlers and herring-boats as occasion requires",[23] just as was done at St. Andrews and Plymouth.

Cunningham was at Granton from the beginning, too, arriving there before the station's official opening on April 14, 1884.[24] He had been hired by Murray as "naturalist in charge of the Marine Station", retitled as "Superintendent" in 1885, and back to "naturalist in charge of the Marine Station" in 1887, for which employment he received one hundred pounds a year.[25] Cunningham got down to work quickly, and within a year had placed an advertisement in the *Scotsman* for a laboratory servant,[26] and amassed supplies, glassware, aquaria, laboratory furnishings, and specimens, recording each acquisition on sheets of petty expenses submitted to Murray. An expense report covering October 18, 1884 to January 20, 1885 and totaling around three pounds includes such items as train fare for boxes of specimens to St. Andrews, a lamp for the laboratory, cloth for a tow-net, paraffin and turpentine, two sharks, money paid to a trawler, pens and nibs, and a lamp for his microscope.[27] Cunningham's task as hired naturalist at Granton, as it would be when he was hired naturalist at Plymouth, was to do the scientific work

[21] E3, financial statement, 1 December 1884.
[22] Hoyle, "Scottish Marine Station," 222.
[23] Hoyle, "Scottish Marine Station," 224.
[24] Or at least, his luggage did, arriving at the railway station on April 9, 1884.GSR- AB, "Expenditure at Marine Station," 9 April 1884.
[25] E1, quarterly salary receipts for 2 October 1884, 17 January 1885, and 5 October 1885.
[26] E2, "Account of money spent on behalf of Scottish Marine Station by J. T. Cunningham," April [1884].

that would fulfil the station's mandate, making good on the promises that had been made to the major donors of money that allowed the station to be established in the first place. In the case of Granton station, these promises concerned the research on herring, which had been started by the Scottish Meteorological Society and had been the explicit reason why they had received the profits from the Edinburgh Fisheries Exhibition. Cunningham took over the herring research as soon as possible, and Hoyle reported in the *Journal of the Marine Biological Association* in 1888 that

> During the year 1885, much of [Cunningham's] attention was devoted to the study of the development of the herring, for which purpose he not only worked in the Firth of Forth itself, but spent several weeks at the village of North Sunderland on the Northumberland coast. The eggs were collected during nocturnal trips in the herring boats, and kept whilst developing on glass plates in wooden boxes sunk near the shore, so that they could be examined when required.[28]

Cunningham had determined the time and temperature of hatching for the herring eggs, and had also made a number of observations about the Kupffer's vesicle structure in the developing embryo. He had also done work on the hagfish, a species of fish that had the twin attractions of being relevant to fishermen because of the great nuisance it caused to them as a predator of hooked fish, especially cod, and of great interest to naturalists as well because their unusual eggs had been so seldom seen. "Mr. Cunningham," commented Hoyle, "in spite of numerous efforts and much expenditure of time and money, was unable to obtain more [eggs], even by keeping adult animals for months in an aquarium, so he took advantage of the opportunity offered by his having numerous specimens at his disposal to make a careful investigation of the development of the

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[27] E2, "Petty Expenses," 18 October 1884 to 20 January 1885.

reproductive products, which has led to some interesting results."[29] While at Granton, Cunningham had also distinguished himself and the station by publishing in the *Proceedings of the Royal Society of Edinburgh* a study of "the eggs and young stages of food-fishes", "[a] department of knowledge in which science is at present very backward",[30] and a department of knowledge that was very much in demand among those interested in regulating the fisheries. At Granton, just as at Plymouth, Cunningham occupied a very particular niche: he was a salaried naturalist, accountable for his expenses and expected to work on projects that fulfilled promises made long before he arrived to work at the station.[31] By virtue of the resources available to him as an employee of the station, he was also a marine embryologist of sufficient reputation to be able to publish in some very prestigious places. And in both places, his work had to exist in the region between pure scientific study and obtaining practical results that could benefit the fisheries.

But not long after Hoyle was writing his report, the Granton station, and especially the marine and fisheries biology activities of the Granton station, were somewhat in decline. Both the *Ark* and the *Medusa* had been transferred to the west coast to work in the Firth of Clyde. Although some buildings on the quarry site had been

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28 Hoyle, "Scottish Marine Station," 234.
29 Hoyle, "Scottish Marine Station," 235.
30 Hoyle, "Scottish Marine Station," 235.
31 Cunningham was responsible for submitting an account of the work done at Granton to the British Association meetings each year, as part of the annual report of the Committee responsible for the B.A.'s annual grant of seventy-five pounds (increased to one hundred pounds for 1886) to the Granton Station. The Committee included, among others, W. C. M'Intosh, James Cossar Ewart, and John Murray. "Report of the Committee, consisting of Professor McKendrick, Professor Struthers, Professor Young, Professor McIntosh, Professor Alleyne Nicholson, Professor Cossar Ewart, and Mr. John Murray (Secretary), appointed for the purpose of promoting the establishment of a Marine Biological Station at Granton, Scotland," *Report of the British Association for the Advancement of Science* (1885); "Report of the Committee, consisting of Professor McKendrick, Professor Struthers, Professor Young, Professor McIntosh, Professor Alleyne Nicholson, Professor Cossar Ewart, and Mr. John Murray (Secretary),

adapted for scientific work and aquaria built in the basement, losing the *Ark* meant more than losing laboratory space. The Granton station had not previously had to develop a system of circulating sea-water tanks in the way that Plymouth and St. Andrews needed to: the *Ark*'s sheltered position in the salt-water flooded quarry had allowed for what Hoyle called "a kind of natural aquarium by enclosing specimens of various kinds in submerged cages, which were attached either to the 'Ark' itself or to suitable floats in various places."[32] Yet, the Granton station had never been completely oriented to biological studies, and the work done there encompassed a great deal of physical work, including measurements of water temperature and density, as well as Murray's own work on the effects of wind upon water temperature distribution. As well, there was a fair amount of chemical work, ranging from chemical analyses of seawater samples, to a project to create artificial seawater, to straightforward industrial chemistry testing the composition of local soil, coal, and food products. This chemical work became more and more dominant, and the Granton station eventually closed in 1893, its biological work having been entirely relocated to Millport on the west coast of Scotland. The Granton station had never been able to get funding from the Fishery Board for Scotland (Deacon suggests that Ewart's influence might have played a role[33]), and could not continue to operate on Murray's money and the other bits and pieces of funding that Murray could secure. By 1893, however, there was a new major player on the scene, with the establishment of the Marine Biological Association Laboratory at Plymouth in 1887. The Plymouth laboratory, which like the Granton station depended for much of its initial

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appointed for the purpose of promoting the establishment of a Marine Biological Station at Granton, Scotland," *Report of the British Association for the Advancement of Science* (1886).

[32] Hoyle, "Scottish Marine Station," 222.

[33] Deacon, "Crisis and Compromise," 36.

funding on promises made about the relevance of marine biology to Britain's fishing industry, required a staff naturalist to make good on these promises, and to keep a steady stream of original marine biological research flowing from the Laboratory. Cunningham was just such a naturalist, and in 1887 he was hired to work at Plymouth for £250 per year, and departed Scotland for the southwest of England. Yet again, a British marine research station was starting up, and Cunningham was there.

Ownership of Work: Cunningham's Publications for the MBA

From his new address at 6 Hoe Park Terrace in Plymouth, Cunningham wrote a letter to his previous boss at Granton, John Murray. He wished his replacement, J. Arthur Thompson, the best of luck at the Granton laboratory, writing that "I remember the place with some affection now I have left it, and especially do I regret that I never got embryos of Myxine."[34] Cunningham might well have felt affection for the well-appointed Granton station with its floating laboratory, submersed fish tanks, and steam-driven collecting vessel: upon his arrival in Plymouth, the Laboratory had not been finished enough for him to work in, and he had taken rooms in town and dissected bought fish there.[35] As well, he was operating under a new job description at Plymouth: no longer Superintendent or Naturalist in Charge of the Marine Station, Cunningham was instead the Naturalist, full stop, albeit at much better pay. The superintending and in-charge portion of his former job at Granton was at Plymouth a separate position, that of Director. As well, both he and the Director were accountable to the Council of the Marine Biological Association, headed in name by T. H. Huxley but in effect by E. Ray Lankester, who had fought for funding to establish the Plymouth station even harder than

[34] GSR-B, letter, J. T. Cunningham to John Murray, 17 September 1887.

he had tried to dissuade his former assistant, Ewart, from establishing a station of his own several years previously.[36] Lankester was already making his influence felt in Cunningham's letter to Murray, in which Cunningham wrote, "Lankester tells me he was at Rothesay a few days back, and found the Aquarium there was for sale at the price of £3500 which he though 'dirt cheap.' He says that you really ought to buy it. But I do not suppose you are inclined to do so in the present condition of affairs. Perhaps the Fishery Board will try to get the money to purchase it."[37] This comment reveals not just that Lankester was something of a busybody, but also that there were other reasons besides the improved pay at Plymouth that might have motivated Cunningham's decision to leave Granton. The financial realities that would cause Granton station to close in six years' time must have already been apparent, to Murray and Cunningham at least, when the job of Naturalist for the MBA came along.

As Naturalist in the pay of the Marine Biological Association, working under the roof of their new Laboratory stocked with equipment and materials that they had paid for, and studying specimens that, one way or another (the next chapter will detail these several ways), Cunningham had acquired using their money and reputation, Cunningham was expected to generate output that would make the Association look good. Looking good meant a couple of things for the Association. It meant that their naturalist in their laboratory should be seen to be generating new scientific knowledge about the local fish species. It also meant that their naturalist should be seen to be making good on the MBA's commitment to investigating problems of consequence to the ongoing uproar about the state of the fisheries. As we saw in the previous chapter, when trying to drum

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[35] D1, [1].
[36] Deacon, "Crisis and Compromise," 34.

up support for a marine research laboratory, Lankester had offered arguments in favour of, and received public affirmation of, the need for a facility that could put science in proximity to answers about fisheries questions. In the discussion that followed, the prominent scientist and Scottish Member of Parliament Sir Lyon Playfair had given a strong endorsement of the project, saying that he had had enough of Royal Commissions being called, only to have no scientific data to refer to. Instead, the Commissions had had to resort to asking the fishermen – the very fishermen who were, having been responsible for the concerns in the first place, in the least likely position to be objective about things – whether *they* thought the fish stocks were in trouble. Meanwhile, in America a more scientific approach had been taken, and as a result they "had already done one thing which, at first sight, seemed impossible": restocked parts of the coast and rivers with cod and shad. A marine station was required "merely to keep up to the level of other nations":

> Was England to remain the only country which did not carry out these inquiries scientifically? A small station had been established at Granton, near Edinburgh, but it was not sufficient for the purpose; they required several in different parts, and especially one central station, such as was proposed to be established at Plymouth. It was really disgraceful that a country washed all round by the sea – a maritime nation *par excellence* – should be the only one not conducting these scientific inquiries.[38]

It is remarkable to see how heavily Playfair's expressed support for Lankester's efforts was based on the potential benefits, already being realized in other countries, to the nation's sea fisheries, and at least one donor (and probably many more) was clearly

[37] GSR-B, letter, Cunningham to Murray, 17 September 1887.
[38] Lankester, "The Value of a Marine Laboratory," 757. Interestingly, the paper and meeting were reported in at least one account, clipped out and kept by W. C. M'Intosh, as "Sir Lyon Playfair on Sea Fisheries,"

supporting the Laboratory as a place of fisheries research: the Worshipful Company of Fishmongers, who with a donation of £2000 had been the largest single private donor to the Laboratory, and had even hosted a banquet on the occasion of the Laboratory's official opening in the spring of 1888. Cunningham had explicitly been hired in 1887 to work on practical fisheries questions in the Plymouth laboratory, and the nature of his employment was repeated on the inside back cover of each issue of the MBA's *Journal* as part of the "Objects of the Marine Biological Association": "A naturalist has also been appointed at a salary of £250 a year, whose duties are almost entirely confined to the study of food fishes."[39] It was important, given the promises that had been made when the Plymouth station was still a twinkle in Lankester's eye, and the promises that continued to be made in the "Objects" and in the Director's Reports, that relevant fisheries biological work be seen to be emerging from the laboratory as soon as possible, and in quantities as great as possible.

What was more, the MBA had decided to start a journal, the eponymously titled *Journal of the Marine Biological Association of the United Kingdom*, with the first issue to appear in 1887. The journal was to function as the MBA's newsletter, keeping subscribers to the Laboratory and members of the Association informed about the meetings and policies of the Association, the growth of the library through donations from prominent marine zoologists from Britain and elsewhere, and the important business of letting each member of the Association know who *else* was a member of the Association, and that the Association had acquired a patron in the person of H.R.H. the

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with Lankester's remarks hardly mentioned at all. A1, newspaper clipping, "Sir Lyon Playfair on Sea Fisheries," [mid-May 1885], [74].

Prince of Wales. One of the *Journal*'s main tasks, however, was in demonstrating in detail how the promises made prior to the Station's founding were being upheld. The many speeches given at the opening of the Laboratory in 1888, and reprinted in the *Journal*,[40] made frequent reference to the coexistence of scientific and practical questions, and the tremendous benefits that Britain's fisheries were sure to receive once the Laboratory began operating in earnest. After two small volumes in 1887 and 1888, the Journal began a New Series in 1889, increasing in size to royal octavo and being introduced by the outgoing Director, G. C. Bourne, as "a new series in which is intended to publish such scientific memoirs as have a direct or indirect bearing upon economic questions."[41]

Bourne dwelled extensively on the MBA's duty toward these economic questions, not least because the "Association is definitely pledged, in consideration of a grant from H. M. Government, to concern itself with economic questions relating to our fisheries. Mr. Cunningham's paper is a sufficient evidence that the pledge is being fulfilled."[42] Bourne presented an interesting take, in part by way of telling the *Journal*'s readers that they would have to wait patiently for Britain's fisheries questions to be answered by the MBA, on the difference between so-called practical and "theoretical" questions. Both dealt in causes and effects and used similar scientific methods, but the solution to practical questions was at once more complex to find and held to higher standards:

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[39] "Objects of the Marine Biological Association of the United Kingdom", *Journal of the Marine Biological Association of the United Kingdom* 2 (1888) (and repeated similarly, with wording changes to reflect changes in the Laboratory's needs, on the inside back cover of each issue).
[40] "Opening of the Marine Biological Laboratory," *Journal of the Marine Biological Association of the United Kingdom* 2 (1888).
[41] G. C. Bourne, "The Director's Report. – No. 1," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 1.
[42] Bourne, "The Director's Report. – No. 1," 3.

> A "practical" investigation, then, differs from a "theoretical" only in its greater complexity and in the larger amount of accurate knowledge required for its objects, -- practical investigations are nothing but scientific investigations of the highest possible order. A scientific opinion upon fishery questions must be founded upon such a number and variety of observations as to be equivalent to a statement of fact, and those who act upon the opinion, whether legislators, pisciculturists, or fishermen, have a right to expect an immediate advantage as the result of their following it.[43]

Not only the fate of Britain's fisheries but the fate of science's involvement in Britain's fisheries was at stake: if scientific inquiry yielded bad advice and failed policies, "the popular voice is apt to pass from a particular to a general statement, and to say that all scientific opinion on such matters is equally without value."[44] Not only "Mr. Cunningham's paper" in the first of the new series of the *Journal*, of course, but Mr. Cunningham *himself* in his capacity as Naturalist was evidence of the MBA's pledge, and his contributions to the *Journal* were a measure of how well that pledge was being fulfilled.

The *Journal* was also to include reports on scientific work done by visitors to the Plymouth laboratory as well as reports from other marine stations in Britain and Europe,[45] but much of the scientific content had to be supplied by Cunningham, who

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[43] Bourne, 3-4. Bourne's next Director's Report, in the second number of the *Journal*, adopted an even more cautionary (or slightly defensive) tone, beginning by saying "That most of the memoirs published in the present number of the Journal are purely zoological or botanical may seem to belief the promise made in my first report," and then explaining the various difficulties and failures of the practical work undertaken in the previous months, as well as emphasizing that even the most seemingly "purely zoological" work may still have "an indirect bearing on fishery questions, for it is only by a study of marine life as a whole and by the knowledge of the habits of a large number of marine animals that we can hope to deal in a satisfactory manner with problems relating to the fisheries." (G. C. Bourne, "The Director's Report. – No. 2," *Journal of the Marine Biological Association of the United Kingdom* 1(1889-1890): 111.)

[44] Bourne, 4.

[45] M'Intosh, in addition to donating many of his publications to the library at Plymouth, submitted reports on the activities at the St. Andrews Marine Laboratory; T. Wemyss Fulton reported on the scientific work of the Fishery Board for Scotland; W. A. Herdman donated his publications to the library and reported in the *Journal* on the work being done by the Liverpool Marine Biology Committee; and William Hoyle of

along with the Director was the only worker who spent the whole year at Plymouth, and could undertake the kinds of long-term studies that made for many-page papers. In Cunningham's ten years of publications in the *Journal*, there were lots of these many-page papers, from his early forty-five page "Studies on the Reproduction and Development of Teleostean Fishes occurring in the Neighborhood of Plymouth,"[46] to his lengthy account of the mysteries of the reproduction and development of the conger eel,[47] to his detailed studies of "The Immature Fish Question" and his "North Sea Investigations."[48] He also contributed numerous shorter papers on work he was doing in the Laboratory as well as in the Plymouth fishing community, and many, many short "Notes and Memoranda" reporting work in progress or curious specimens.[49] A full list of his contributions to the MBA's *Journal* is available in Appendix 1.

During his time as Naturalist at Plymouth, Cunningham also published two books, *A Treatise on the Common Sole (*Solea Vulgaris*), Considered Both as an Organism and as a Commodity* (1890) and *The Natural History of the Marketable Marine Fishes of the British Islands* (1896). The aims and content of these books will be considered in

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the Scottish Marine Station at Granton submitted a report to the 1888 number of the *Journal*. A report from Professor A. A. W. Hubrecht of the Netherlands Zoological Station, a moveable wooden building that was stationed at different points off the coast of the Netherlands each year (in much the same manner as the first Canadian marine station, described in Jennifer Hubbard's recent book *A Science on the Scales: the rise of Canadian Atlantic fisheries biology, 1898-1939* . Toronto : University of Toronto Press, 2006) beginning in 1876.

46 J. T. Cunningham, "Studies on the Reproduction and Development of Teleostean Fishes occurring in the Neighborhood of Plymouth," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890).

47 J. T. Cunningham, "On the Reproduction and Development of the Conger," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892).

48 J. T. Cunningham, "The Immature Fish Question," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895); J. T. Cunningham, "North Sea Investigations," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1897).

49 See, e.g., J. T. Cunningham, "Anchovies in the English Channel," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890); J. T. Cunningham, "On Some Larval Stages of Fishes," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892); and the "Notes and Memoranda" in every volume from 1888 until 1897.

Chapter Five, but the title pages of each (see Figure 6) reveal the extent to which the MBA attached its stamp to Cunningham's publications, making clear that he was their worker and that he published under their direction. His *Treatise on the Common Sole* was published in Plymouth by the Association,[50] and according to the "Report of Council" of June 1889 Cunningham was "acting under instructions from the Council" in preparing the book,[51] so perhaps it is not too surprising that directly under the title, the words "Prepared for the Marine Biological Association of the United Kingdom by" appear, before the author's name, followed by "Late Fellow of University College, Oxford; Naturalist to the Association" (Figure 6a). However, the Association did not publish Cunningham's second book, *The Natural History of the Marketable Marine Fishes of the British Islands*; it was published by the prominent firm Macmillan and Co., of London.[52] Despite, or perhaps because, Cunningham's second book was not obviously an MBA product by virtue of its place of publication, the Association's stamp is even

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[50] The *Journal of the Marine Biological Association* was also published by the Association in Plymouth.
[51] "Report of Council," *Journal of the Marine Biological Association of the United Kingdom* 1 (new series, 1889-1890): 106. The Marine Biological Association relied heavily on private donations and was always desperate for money in the years following the establishment of the Laboratory; perhaps their efforts to portray Cunningham as acting entirely upon their instructions and under their supervision was because they wanted their present and future donors to know that they weren't paying Cunningham to work on practical questions only to see him pursue his own projects and make his own name, which was already somewhat established before he came to Plymouth. The Granton report (Hoyle, "The Scottish Marine Station and its Work"), lists nine papers by Cunningham in its references list.
[52] Perhaps Cunningham's second book was not published by the MBA because they were experiencing financial constraints in 1896-1897 as they attempted to buy a proper steam-driven boat and support work both at Plymouth and on the North Sea coast as well, work begun by E. W. L. Holt and taken over by Cunningham. In their report for 1895-1896, the MBA Council reported that "no special donation has been forthcoming to provide for the continuation of this work." "Report of the Council, 1895-96," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1896): 303. By the next issue, Cunningham's name was no longer on the masthead, (nor was anyone else's there as a replacement Naturalist, until the next issue, when Holt and Walter Garstang, former assistant to the Director, were both listed as Naturalists) and the Director, E. J. Allen, reported that the North Sea work had "now ceased owing to lack of the necessary funds for its maintenance." ("Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1896): 416). In the next issue, the Council reported that Cunningham had "left the service of the Association during the past year, and has been appointed Lecturer to the Technical Education Committee of the Cornwall County Council." ("Report of the Council, 1896-97," *Journal of the Marine Biological Association of the United Kingdom* 5 (1897-1899): 101.) It is also

more strongly on the title page of *Marketable Marine Fishes* than it was on *Treatise on the Common Sole*. Directly beneath the title (see Figure 6b), it is made clear on whose orders Cunningham produced this work, and for whose benefit: "Prepared by order of the Council of the Marine Biological Association Especially for the Use of Those Interested in the Sea-Fishing Industries". Beneath Cunningham's name and the usual mention of his previous affiliation with University College, Oxford, the reader learns why Cunningham is the appropriate author to have been ordered to produce such a study: not only is he "Naturalist on the Staff of the Marine Biological Association," but he is also a "Corresponding Member of the Deutscher Seefischereiverein," the German Sea Fisheries Society (recall the admiration and envy among the British for the advanced state of fisheries research in other coastal nations). The MBA further makes its mark on Cunningham's title page through the mention of a preface written by Lankester, Oxford professor and "President of the Marine Biological Association." The rhetorical effect of this title page is to minimize the identity of the book's author, making clear in several ways that this book is a project not just authorized by but *ordered by* the administration of the Marine Biological Association.

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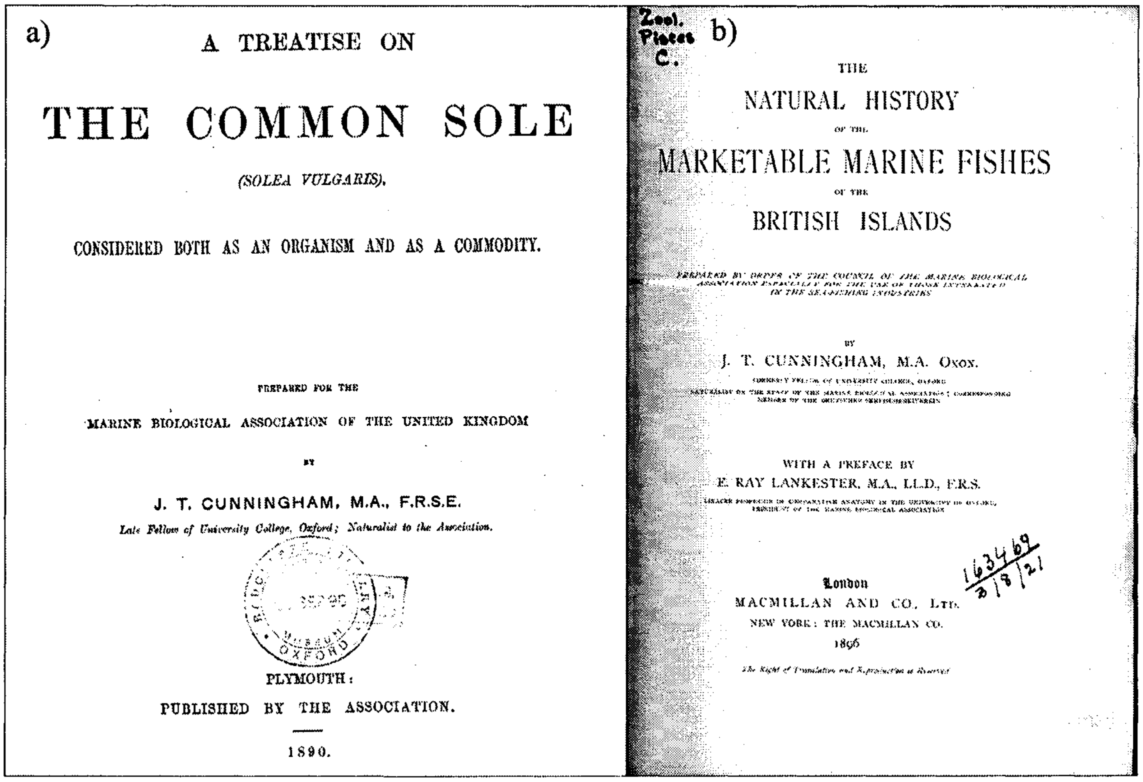
possible that the MBA envisioned a much larger audience for this book than for *Treatise on the Common Sole*, and for that reason sought a large publisher instead of trying to handle it themselves.


a)

A TREATISE ON

THE COMMON SOLE

(SOLEA VULGARIS).

CONSIDERED BOTH AS AN ORGANISM AND AS A COMMODITY.

PREPARED FOR THE

MARINE BIOLOGICAL ASSOCIATION OF THE UNITED KINGDOM

BY

J. T. CUNNINGHAM, M.A., F.R.S.E.

Late Fellow of University College, Oxford; Naturalist to the Association.

PLYMOUTH:

PUBLISHED BY THE ASSOCIATION.

1890.

b)

THE

NATURAL HISTORY

OF THE

MARKETABLE MARINE FISHES

OF THE

BRITISH ISLANDS

BY

J. T. CUNNINGHAM, M.A. OXON.

WITH A PREFACE BY

E. RAY LANKESTER, M.A., LL.D., F.R.S.

London

MACMILLAN AND CO., LTD.

NEW YORK: THE MACMILLAN CO.

1896

Figure 6. Title pages of Cunningham's two books published while he was Naturalist for the MBA, *A Treatise on the Common Sole* (a, published in 1890) and *The Natural History of the Marketable Marine Fishes of the British Islands* (b, published in 1896). Both books were clearly marked as products of the Marine Biological Association.

Macmillan and the Association promoted Cunningham's book to the readers of the MBA *Journal* with a two-page advertisement, including a copy of the title page and a table of contents, followed directly by an advertisement for *Treatise on the Common Sole* (and back issues of the *Journal*).[53] Regardless of the publisher or the author, both books were MBA exports.

Cunningham's laboratory notebooks reveal another incident in which the Association's Council exerted their ownership over his work. In 1891, Cunningham had published an account of some of his marine zoological work in a prominent German

[53] "Just Published," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1896): 418-419; "Publications of the Association," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1896): 420.

journal, the *Zoologischer Anzeiger*.[54] The paper gave the first published results from Cunningham's most exciting (and least fisheries-related) work at the Plymouth laboratory, inducing colouration on the lower, usually unpigmented side of developing flounders (see Chapter Four). He had done this using glass and mirrored tanks to expose the undersides to light, and his results showed an important environmental input into development. The publication of these results, along with two other recent papers in the *Annals and Magazine of Natural History*[55] and the *Proceedings of the Zoological Society*,[56] had of course not escaped the attention of the Council of the MBA, who kept track of "the results of work done at the Plymouth Laboratory and published elsewhere than in the Journal of the Association"[57] and published lists of these papers periodically in the *Journal of the Marine Biological Association.* Visiting naturalists who had paid to use the Laboratory tables, or MBA members whose membership dues entitled them to the use of the Laboratory, seemingly faced no restrictions on where they chose to publish the results of their studies (though they were required to submit a short report upon leaving), and indeed having work done at Plymouth appear in a range of publications at home and abroad was a good way to spread the reputation of the Laboratory and the Association, which was always seeking new members and naturalists from Britain and abroad to rent the tables. Cunningham, however, was neither a visiting naturalist nor a member of the

[54] J. T. Cunningham, "An Experiment concerning the Absence of Colour from the Lower Sides of Flat Fishes," *Zoologischer Anzeiger* 14 (1891).
[55] J. T. Cunningham, "On some Disputed Points in Teleostean Embryology," *Annals and Magazine of Natural History* 7 (1891).
[56] J. T. Cunningham, "On Secondary Sexual Characters in the genus *Arnoglossus*," *Proceedings of the Zoological Society of London* (1890).
[57] "Notes and Memoranda," *Journal of the Marine Biological Association of the United Kingdom* 3 (1891-1892): list of papers from other journals reporting work done at Plymouth; this list includes Cunningham's *Zoologischer Anzeiger* and *Annals and Magazine of Natural History* papers. A similar list appears in the 1889 *Journal*, and includes Cunningham's *Proceedings of the Zoological Society* paper: G. Herbert Fowler,

MBA. He was an employee of the MBA, and the Council was not pleased that their naturalist was swelling the pages of a German journal with work done at their station while in their pay, when the pages of their own journal wanted swelling as well. It is not clear what exactly raised the ire of the Council: Cunningham's publication in a prominent German journal, his publication in the *Zoologischer Anzeiger* specifically of a major discovery about flounder development obtained while working for the MBA, or just his recent tendency, since beginning to get good results in 1890, of publishing these results in journals that were not the MBA's, just as though he had been a rent- or dues-paying visitor. (It is interesting, too, that after starting as Naturalist at Plymouth almost no more of his results appeared in Lankester's journal, the *Quarterly Journal of Microscopical Science*, which had published much of the very similar work Cunningham had done while working as hired Naturalist and Superintendent at the Granton marine station. The *Quarterly Journal* had published two of his papers in 1885, one in 1886, two in 1887, and one (with Cunningham as second author) in 1888.[58] No more appeared until a lone paper in 1891, in which Cunningham asserted his priority over the Norwegian naturalist Fridtjof Nansen for some observations of spermatogenesis in *Myxine*;[59] a very similar

"The Director's Report. – No. 4," *Journal of the Marine Biological Association of the United Kingdom* 1 (new series, 1889-1890).

[58] J. T. Cunningham, "The Significance of Kupffer's Vesicle, with Remarks on other Questions of Vertebrate Morphology," *Quarterly Journal of Microscopical Science* 25 (1885); J. T. Cunningham, "E. Von Beneden's Researches on the Maturation and Fecundation of the Ovum," *Quarterly Journal of Microscopical Science* 25 (1885); J. T. Cunningham, "On the Relations of the Yolk to the Gastrula in Teleosteans, and in other Vertebrate Types," *Quarterly Journal of Microscopical Science* 26 (1886); J. T. Cunningham, "On the Structure and Development of the Reproductive Elements in Myxine glutinosa, L.," *Quarterly Journal of Microscopical Science* 27 (1887); J. T. Cunningham, "Dr. Dohrn's Inquiries into the Evolution of Organs in the Chordata," *Quarterly Journal of Microscopical Science* 27 (1887); Rupert Vallentin and J. T. Cunningham, "The Photospheria of Nyctiphanes Norvegica, G. O. Sars," *Quarterly Journal of Microscopical Science* 28 (1888).

[59] J. T. Cunningham, "Spermatogenesis in Myxine glutinosa," *Quarterly Journal of Microscopical Science* 33 (1891). A similar pattern of declining publication activity can be seen in Cunningham's contributions to the Royal Society of Edinburgh, although this pattern is perhaps better explained by Cunningham's absence from Scotland once he began working in the south of England.

assertion appeared next to his colouration paper in the 1891 *Zoologisher Anzeiger*.[60] Another six years passed until Cunningham appeared again in the *Quarterly Journal*: in 1897, after Cunningham had left the MBA's employ, Lankester published a long memoir by Cunningham on fish ovaries and eggs with a note stating that the "researches described in this memoir were carried out in the service of the Marine Biological Association in 1895 and 1896."[61]) At any rate, the *Zoologischer Anzeiger* paper provoked the Council into action. They flexed their muscles, passing two resolutions limiting Cunningham's freedom not only to publish where he pleased but also, in the short term, to work on research projects of his own choosing, which tended to involve the zoological, rather than especially fisheries-oriented, study of development in flatfish like flounders and soles. W. L. Calderwood, who as part of his job as Director of the Laboratory also served as the Assistant Secretary of the Association and the editor of the *Journal of the Marine Biological Association*, had the job of passing the news on to Cunningham, who recorded the encounter in his notebook. After a routine report of a lecture he had given at the Mechanics Institute and of his ongoing attempts to establish an artificial fertilization system in the laboratory, Cunningham brought up Calderwood's visit:

> Calderwood communicated to me two resolutions of the Council. One à propos of my paper in the Zoologischer Anzeiger, that no one on the staff should publish any work done in the Laboratory in any publication except those of the Association, without permission from the Council. That the Director be instructed that it is important to advance as rapidly as possible the solution of the Anchovy question: that he be allowed a sum not exceeding £100 for the expenses: and that the Naturalist be instructed to place

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[60] J. T. Cunningham, "Spermatogenesis in Myxine," *Zoologischer Anzeiger* 14 (1891).
[61] J. T. Cunningham, "On the Histology of the Ovary and of the Ovarian Ova in certain Marine Fishes," *Quarterly Journal of Microscopical Science* 40 (1897): 101.

> his services at the disposal of the Director for this purpose.[62]

Recall that the scientific staff of the Laboratory consisted of two paid employees, one of which was Calderwood, and the other Cunningham himself,[63] and that the publications of the Association were nearly all in the form of the *Marine Biological Association Journal*, and it becomes clear that Cunningham was having his knuckles rapped for having acted as a free agent in publishing his results elsewhere. The second resolution cannot help but look like punishment for this offence, either: "the Anchovy question," which involved research into the abundance of anchovies along the English coast and into the possibility of establishing a commercial anchovy fishery in England, was one of somewhat less interest to Cunningham compared to his laboratory-based work on the sole or his studies of the inducement of colouration in flounders, the subject of the offending *Zoologischer Anzeiger* paper. Being informed that the Anchovy question was to be heretofore a priority, and what's more reminded of his obligations to his superiors by having one level of bosses, the Council, "instruct" another level of boss, Calderwood the Director, to "instruct" Cunningham "to place his services at the disposal of the Director" – to put his current work aside to labour on anchovies on Calderwood's behalf because the Council

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[62] D2, 26 March 1891, 154.

[63] Though not part of the scientific staff as such, Cunningham's assistant, "the Laboratory fisherman" (G. C. Bourne, "The Director's Report – No. 2", *Journal of the Marine Biological Association* 1 (new series): (1889-1890): 115) William Roach, of whom much more in the next chapter, published an article in the *Journal*'s first number of the New Series, "Notes on the Herring, Long-line, and Pilchard Fisheries of Plymouth during the Winter 1889-90" (382-390). A local fisherman with an eye for interesting specimes, Roach is identified in his byline for this article as an "Associate Member, Marine Biological Association, Plymouth," a title that was created in 1889 ,"Report of Council, June 26th, 1889," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 107)to encourage contributions from fishermen. An article by Calderwood in the next issue of the *Journal* was entitiled "The Plymouth Mackerel Fishery of 1889-90. From Data collected by Mr. Wm. Roach, Associate Member M.B.A," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892), and that same issue contained the second installment of Roach's article "Notes on the Herring, Long-line, and Pilchard Fisheries of Plymouth (continued)" (180-188). Cunningham's other important (and unpaid) helper and supplier of flatfish specimens, Matthias Dunn, was also listed as an Associate Member beginning in 1889.

wanted to see some anchovy results – amounted to a powerful reminder to Cunningham that he was not only not free to publish where he liked, but he was not completely free to determine the course of his research, even within the range of inquiries which he had been hired to study. He was, at the risk of sounding overly sinister, under surveillance, and subject to control from above.

After this incident, the content of the MBA's *Journal* due to Cunningham increased greatly (see Figure 7, and Appendix 1 for details), though the incident was likely only partially responsible for this increase. The four articles and two notes that he contributed to the 1889-90 *Journal* pale in comparison to the eleven articles and six notes of his in the 1891-1892 volume, and the ten articles (one a lengthy, multi-part article that is really five individual articles) and four notes of his in the 1893-1895 volume. It appears that the Council did not very often use its right of first refusal to do anything but accept Cunningham's work for publication in the *Journal*.[64] However, the issue of Cunningham's freedom to publish elsewhere did not go away. His discoveries about the role of the environment in the development of flounders were sufficiently major, and by 1893 the effect of light in the induction of pigmentation had been so successfully demonstrated, that they merited publication in the *Proceedings of the Royal Society*. Moreover, Cunningham now had a co-author, Charles MacMunn, a naturalist with an interest in pigmentation and chromatophores who had been a paying visitor to the Laboratory in 1889, and was a frequent contributor to Lankester's *Quarterly Journal*. Cunningham and the Council seem to have reached a compromise: a seven-page

[64] When it came to publishing primary results, at least. Cunningham published several essays on evolutionary topics in *Natural Science* from 1892 onward, on flatfish, social insects, and variation. (J. T. Cunningham, "The Evolution of Flat-Fishes," *Natural Science* 1 (1892); J. T. Cunningham, "The Problem

summary of his results appeared in the *Journal of the Marine Biological Association*, ending with a note to readers that "The full description of the investigations, with illustrations, is contained only in the memoir communicated to the Royal Society."[65] His "Additional Evidence on the Influence of Light in producing Pigments on the Lower Side of Flat-Fishes" appeared in the next (and Cunningham's last) volume of the *Journal* (see Chapter 4 for discussion of this work, and of the differences in tone of its several public appearances.)

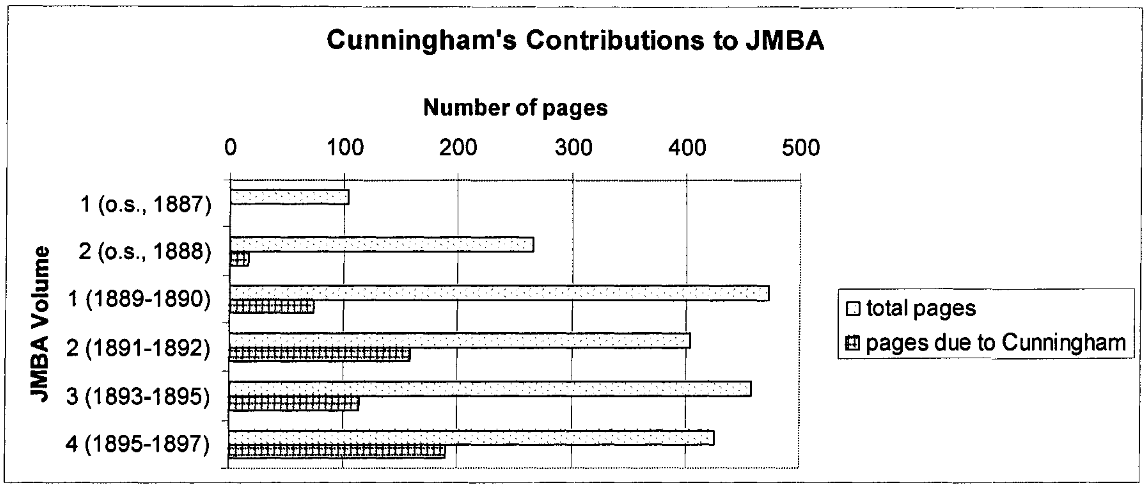

Figure 7. Cunningham's contributions, in number of pages, to the total length of the *Journal of the Marine Biological Association of the United Kingdom*. His contributions, in a variety of formats and on a wide range of topics, made up a large proportion of the total.

An interesting feature of Cunningham's notebook entry reporting Calderwood's communication of two essentially punitive measures enacted by the Council is that he records both resolutions, but makes no comment at all upon them, but instead just ends that day's entry after copying them down. This avoidance of any comment is, I think, due to the fact that Cunningham's notebooks were not for his eyes only: his work and his records belonged to the Association, and could be examined by Calderwood, and

of Variation," *Natural Science* 3 (1893); J. T. Cunningham, "Neuter Insects and Darwinism," *Natural Science* 4 (1894).)

evaluated in various ways. The notebooks are distinctive for their justificatory tone, which is entirely a product of Cunningham's position as a naturalist for hire.

In the Report of the MBA Council of May 1890, the MBA membership and readers learned that, in addition to having prepared a treatise on the common sole, the Naturalist "also ha[d] gathered much valuable information about the occurrence of the anchovy in English waters, and the possibility of an English anchovy fishery."[66] A newspaper report on "An English Anchovy Fishery" from the *Morning Post* from around that time presents a similar picture of progress toward such a fishery:

> Marine zoologists in this country have latterly been devoting a good deal of attention to the anchovy and it has been stated that the investigations carried out by the naturalists of the Marine Biological Institution at Plymouth may lead to the establishment, or rather re-establishment, of a regular anchovy fishery on our southern coasts...Mr. J. T. Cunningham, of the Plymouth Laboratory, made some inquiries among the pilchard and herring fishermen of Plymouth, and ascertained that every time they shoot their nets they find anchovies entangled in the meshes.[67]

This newspaper clipping was found inside a notebook of Calderwood's, entitled *Anchovies*. This was a project that had obvious appeal to the MBA: if the work of their staff could come to be credited with establishing an entire commercial fishery along the Channel, it would be a remarkably impressive accomplishment, validating the promises made by Lankester and Playfair as well as the generosity of the Fishmongers and many other donors. From the beginning, the MBA had associated itself with the improvement

[65] J.T. Cunningham, "Researches on the Coloration of the Skins of Flat-fishes," *Journal of the Marine Biological Association* 3 (1893-1895): 118.
[66] "Report of the Council, May 1890," *Journal of the Marine Biological Association* 1 (1889-1890): 358.
[67] WLC, " An English Anchovy Fishery," loose *Morning Post* newspaper clipping, dated July 21[1892], .

of the sea fisheries: their statement of "Objects of the Marine Biological Association" included in each issue of the *Journal* stressed this point:

> The Association owes its existence and its present satisfactory condition to a combination of scientific naturalists, and of gentlemen who, from philanthropic or practical reasons, are specially interested in the great sea fisheries of the United Kingdom. It is universally admitted that our knowledge of the habits and conditions of life of sea fishes is very small and insufficient to enable either the practical fisherman or the Legislature to take measures calculated to ensure to the country the greatest return from the "harvest of the sea"....The purpose of the Association is to aid at the same time both science and industry.[68]

The project of establishing a commercial anchovy fishery where none had existed when the MBA first arrived at Plymouth was clearly an important one. Its progress relied heavily on the time that Cunningham put into it, and as the *Morning Post* article reflects, he was the public face of the MBA's involvement with the local fisheries (more on this in the next chapter). So perhaps it is not surprising that he was carefully watched to make sure that he was devoting sufficient time to this fisheries project and not, say, to raising flounders in glass-bottomed tanks to see what would happen. To this end, Calderwood had a notebook called *Anchovies*, which included a list of literature related to anchovies, and some correspondence with Matthias Dunn about the possibility of establishing an anchovy curing business at Megavissey. The bulk of the notebook, though, is a record of what Cunningham had been doing on the anchovy question: what laboratory and field work, as well as what letters received and sent, in relation to this question. This record includes a section entitled "J. T. C. Black Book notes": Calderwood had gone through

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[68] "Objects of the Marine Biological Association," *Journal of the Marine Biological Association of the United Kingdom* 2 (1888).

Cunningham's notebooks from late 1889, looking for anchovy work, and he wasn't too impressed. Under his summary of the January 13, 1890 entry in Cunningham's notebooks, Calderwood had written in large letters, and *circled*, "Nothing more till Oct. 23?" The implication is clear: Cunningham had not been doing enough work on the anchovy question, and Calderwood's surveillance of Cunningham's notebooks had detected this. Calderwood was himself being monitored by the MBA Council, who expected progress on the anchovy question. The Council's demand one year later that Calderwood make the anchovy question a priority was a rebuke to both the Director and the Naturalist: for the latter, to stop carrying on like a visiting naturalist, studying whatever and publishing wherever he pleased; for the former, to keep a tighter rein on the Naturalist and be more, well, Directive of the work undertaken in the Laboratory.

Just as the results he obtained while working as Naturalist for the MBA were not his to do with what he liked, Cunningham's notebooks were not his private property.[69] This fact was reflected in their tone, and in one very unusual feature: from January of 1888 until the spring of 1889, Cunningham used his notebooks as a personal time-clock, clocking himself in and out of the laboratory every day, and providing an explanation if he arrived late or left early. That he did not begin his notebooks with this practice, or

[69] A story from my research trip to the MBA Archives at Plymouth helps to underscore this fact. Prior to leaving, my supervisor, Polly Winsor, had advised me that the informational content of archival materials isn't restricted to the words on the page, but that there is information to be had from the location of the documents – what was next to what. This advice was given, I think, by way of reminding me to put the documents in a folder or box back in the order in which I'd received them. However, it assumed a new dimension at Plymouth. I was looking for a particular letter to Cunningham, and the finding aid indicated only that it was in a certain big box. The head librarian, Ms. Linda Noble, very generously allowed me to go with her into the archival storage room to look through the box for the letter. I found it eventually, inside Calderwood's *Anchovies* notebook in that box. The letter wasn't particularly an exciting find in and of itself – as I recall, it was along the lines of "Dear Mr. Cunningham, I am afraid that we have no anchovies to send you this week, etc." – but it was strange to find it with Calderwood's things, and next to Calderwood's notebook I found six notebooks of Cunningham's (not labelled as such, but unmistakably in his handwriting). Finding these items together is, it seems to me, a piece of positional information about (or at least a fortunate illustration of) the access Calderwood had to Cunningham's notebooks.

continue it past the middle of 1889, suggests that it was done at the request of, or in some way because of, a particular laboratory Director or resolution of Council. The Laboratory changed Directors several times during Cunningham's time in Plymouth; during this time there were two: first Walter Heape, who was then succeeded by G. C. Bourne in June of 1888. Cunningham routinely arrived in the Laboratory at nine-thirty or ten in the morning, and left at five in the afternoon. Any variance from this schedule was provided with a justification, most often explaining that other work-related matters had caused the late arrival or early departure. Thus, Cunningham's 4 p.m. departure on January 26, 1888, was in order "to hang diagrams at Athenaeum for a lecture I was to give in the evening on Fish eggs"; his arrival at noon the next day was due to "having been from 10 to that time reading up some things at the Athenaeum Library."[70] (Reading at the library was a frequent reason for late arrivals.) On days when there was no arrival at all, Cunningham made note of that as well when he was back in the Laboratory: "Saturday Feb 18. Did not come to the Laboratory."[71] Even time taken out of work to eat was occasionally explained: he "Took 1 hour for lunch"[72] on February 14, 1888, according to his notebook. Cunningham made occasional multi-day trips out on trawlers, keeping very late hours: after such a trip, he would go "home to sleep and refresh; came to the Laboratory at 12.0 noon."[73] Only very occasionally did Cunningham invoke personal reasons for lateness, as he did when arriving at 11:30 on a Saturday in 1888, "having been detained by business connected with my home & landlord,"[74] and one week later "having been detained by private business connected with moving from one house to

[70] D1, 27 January 1888, [74].
[71] D1, 18 February 1888, [94].
[72] D1, 14 February 1888, [92].
[73] D1, 8 March 1888, [108].

another."[75] On some days, the only entry Cunningham would make in his notebook would be the times at which he'd arrived and departed from the Laboratory.[76] Cunningham's notebooks were, like the notebooks of his contemporary naturalists like W. C. M'Intosh of St. Andrews, records of his scientific work: his observations, the sources of his specimens, his struggles to make experiments and apparatus work. But they were a record not just of his work, but of his *labour*, and they were written as they were not just for his own reference, but for the reference of those who employed him. There is no better explanation for an entry which reads, in its entirety, "March 28th and 29th. Omitted to enter up diary but worked in the Laboratory as usual. March 30th Good Friday."[77] Such an entry has zero scientific content in the sense given above; its value to Cunningham and to the MBA is as a record of time clocked, of hours put in. Its value to the historian lies in elaborating a view of scientific work as labour, seeing the work done by Cunningham as distant in time but not in spirit from Le Carré's Cold War era civil-servant spies or from my father's increasingly frequent references to "pensionable hours". There may seem to be something disappointing about seeing science as this kind of activity, watching Cunningham mark time and report that, as on March 5, 1889, he "[i]n afternoon, entered up diary, and wrote report for quarter ending Feb. 28th",[78] with no indication of what he and the fish eggs might have been up to in the morning. I prefer to see it as a kind of useful disenchantment of how we might view scientific work, placing quarterly reports and – as we will see in the next section – expense reports within the

[74] D1, 10 March 1888, [111].
[75] D1, 19 March 1888, [116].
[76] E.g., D1, 17 March 1888, [116]: "At the Laboratory from 10.30 to 3.30."
[77] D1, 28 March 28 1888, [119].
[78] D2, 6 March 1889, 19.

field of documents that we consider as part of science, and adding to the way in which we view, and use, scientific notebooks as historical records.

In addition to submitting quarterly reports to the MBA Council, in which he accounted for his time, Cunningham had also to submit accounts of the expenses he had incurred. He sometimes prepared these lists in his notebook, and they were remarkably detailed: his list of "Expenditures on behalf of the Marine Biological Association" covering only four days in August 1888 is a page and a half long, and includes entries for every conceivable type of expense associated with taking collecting trips on Plymouth fishing boats, from six shillings and sixpence on "Note-books" and another shilling and three pence on "Pen, ink & note paper" the following day, to expenditures for "Specimens of fishes", "Hire of boat", "Man's services", "Chart of Plymouth Sound", "Jars for specimens", "Methylated spirit" for preserving collected specimens, jars for storing the methylated spirit, "1 bread-pan" presumably for collecting eggs and milt, rope, "Muslin for tow net", "1 Gross glass slides" and "½ oz. thin covers" for examining sectioned specimens, one shilling and sixpence for a "Cab from [Laboratory Director Walter] Heapes to New St", two shillings on a boat to transport him to the trawler, and a two shilling "Tip to crew of Cambria".[79] The whole list amounts to about three pounds' worth of expenditures incurred, and also gives a revealing picture of Cunningham's specimen-collecting work and the tools of his trade.

Even the smallest trip could be the subject of an expense report. In September of 1890, Cunningham received word from Dunn that pilchards were spawning off Falmouth, and got the next train to Falmouth to try and collect some for artificial fertilization. He went out in a pilchard boat with "an old man a man and a boy" looking for spawning pilchards. "As the nets were hauled the men picked out for me all the large pilchards they could find",[80] Cunningham reported in his notebook upon returning. Though only one ripe fish, which was about as useful to Cunningham as zero ripe fish, had turned up among the large pilchards, the help he had received from the three-generation pilchard boat crew had still to be paid for, and several other tiny expenses had also to be reported: return train fare to Falmouth, three cab trips, six pence on the cloak room and four pence each way to the porter, and a five shilling tip to the pilchard fishermen for taking him out with them, and letting him squeeze all of the large fish.[81] As we will see in the next chapter, Cunningham relied heavily on the fishermen of Plymouth and nearby areas in order to get the specimens he needed for his work. He relied on their labour – they shot and hauled the trawl or pilchard and mackerel nets, and eventually even would collect and fertilise eggs and milt and bring the fertilised eggs back to the laboratory for Cunningham to study. He relied even more, though, on their capital: they had boats. Even if he had done all of the netting and collecting and fertilising himself, which he or Roach did (with the tow-net, though not the heavy trawl) if they went to sea with the boat, it was the fishermen's boats that allowed him to do collecting work offshore. The MBA and the various Directors of the Laboratory badly wanted some collecting boats of its own, but couldn't afford them. Eventually they acquired a few boats, but none that could replace the Laboratory's reliance on the local fishing fleet for offshore work. Through donations, the Laboratory by 1890 had acquired three collecting boats: a small steam launch, the "Firefly"; a small three ton hook and line craft, the "Mabel"; and an

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[79] D1, 9-15 August [1888], [224-225].
[80] D2, 17 September 1890, 145.
[81] D2, 17 September 1890, 146.

eighteen foot dinghy, the "Anton Dohrn".[82] However, as the Director, G. C. Bourne (who had donated the "Mabel"), remarked in his report on the largest of these, this "boat is by no means an ideal steamer for dredging purposes, being suitable only for local expeditions in calm weather;"[83] the Laboratory's fleet was useful for inshore collecting work only and did not replace the essential role played by local trawlers. Plymouth trawlers like the *Cambria* and the *Lola*, who were willing to take Cunningham to sea with them, allowed him access to essential specimens; their cooperation also allowed the MBA to continue to appear to be working on problems related to Britain's fisheries. The entire fishing controversy had much to do with the role played by trawlers able to make multi-day offshore fishing voyages, and if the Laboratory's access to food fish specimens was limited to what the naturalists could collect on boats suited only for inshore work, it would be hard to argue that much insight was being gained into the problems of the modern fishing industry. The Laboratory did not have and could not afford to own a trawler, so Cunningham needed to make use of his connections with the fishing fleet, and this meant spending money on hiring boats and tipping the crew for their help and patience. Given the financial realities of the Laboratory in these early years, studying the relevant fish species (of which sole and plaice, never found inshore except as very young individuals, are key examples) meant, in a sense, outsourcing (offshoring, if you will) much of the work of collecting specimens to those who owned or worked on the expensive offshore trawlers, with Cunningham as the go-between and trainer. His

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[82] "Report of the Council, May, 1890," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 357.
[83] G. C. Bourne, "The Director's Report, No. 2," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 117.

expense reports and notebooks reveal the costs, in his (and ultimately the MBA's) money as well as his time, associated with this outsourcing.

Chitons for Lankester: Cunningham as Supplier

It is clear, and will be even clearer in the next chapter, that Cunningham enlisted many different people, and local fishermen especially, into the work of supplying specimens for his studies of food fish. But sometimes Cunningham himself was enlisted as a supplier by those above him. We have seen that part of his job was to put himself at the disposal of the Director in aiding the Director with whatever projects he might be working on, and Cunningham quite often went out dredging the bottom for invertebrates with or for the current Director, even though the bulk of Cunningham's own work focused on free-swimming vertebrates. In the winter of 1888, for instance, at the peak of Cunningham's enthusiasm for collecting fish eggs floating on the surface of the water, when he was often making inshore tow-netting trips with Roach and had just written a memorandum to himself to "Get tow nets worked as often as possible out at sea from trawlers",[84] Cunningham and Roach seemed to be doing quite a lot of dredging as well, on behalf of then-Director Walter Heape. The contents of the dredges were examined by Heape, or by Heape and Cunningham together, in the weeks that followed.[85]

There is nothing particularly surprising or unusual about Cunningham acting as a supplier for Heape or any other Director of the Laboratory. After Cunningham, the Director tended to be the person who contributed the most in number of articles and notes to the *Journal of the Marine Biological Association*, and was expected to keep up a

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[84] D1, 7 January 1888, [60].
[85] D1, 28 January 1888 ([74]), 8 February 1888 ([88]), 9 February 1888 ([89]), and 15 February 1888 ([93]).

course of scientific study, at times following priorities set by the Council, in addition to his various administrative and editorial duties. However, it wasn't only the Director who enlisted Cunningham to collect specimens on his behalf. During that same period in early 1888, when Cunningham was directing most of his energies toward getting and hatching pelagic fish eggs, and just after the January and February of dredging for invertebrates for and with Heape, Cunningham was enlisted as a specimen supplier by a man higher up than Heape – indeed, higher up than anyone else in the MBA. The MBA's Secretary and main founder, E. Ray Lankester, wanted specimens of chitons, a marine mollusk, and expected Cunningham to get them for him. The day of March 1, 1888, was spent by Cunningham on a collecting trip with Roach "for specimens of Chiton for Prof Lankester."[86] None could be found. The next day, Cunningham sent Roach to go dredging, in hopes that some chitons might be found attached to the stones brought up with the dredged material, but no such luck. Later that afternoon, in a seemingly desperate move to find chitons for Lankester, Cunningham sent Roach down to the fish quay in order to get some conger eggs, but also to ask the trawlermen if he could get their "scruff" – the mixture of rocks and debris that becomes caught in the trawl-net as it drags along the sea bottom – to bring back to the Laboratory. Cunningham planned to go through the scruff and see if any chitons were attached to the rocks. Apparently none were, for despite these extreme measures, the next day saw Cunningham going out on yet another collecting trip to look for chitons for Lankester, and yet again to find none.[87]

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[86] D1, 1 March 1888, [99].

[87] D1, 1 March 1888 ([99]), 2 March 1888 ([100]), and 3 March 1888 ([102]).

Two days later, Cunningham and Roach went on a three-day collecting trip to sea in the trawler *Lola*, and Cunningham was busy to his heart's content with collecting and examining fish eggs for the following fortnight; but the saga was not yet over. On the twenty-fourth of March, Cunningham put in a very short day of work, reporting in his notebook that Roach had been "to S[outh] side of Drake's Isl[and] at low water this morning to search for Chitons for Prof. Lankester but found none." Cunningham then sent Roach to dredge another possible site, east of the Promenade Pier; "he took three hauls, and br[ough]t. back the stones, but there were no chitons."[88] Three days later, Cunningham was still pursuing Lankester's specimens, going "with [W. F. R.] Weldon and Roach to collect on shore of Drake's Island. Looked carefully for Chiton but found none."[89] (There is, I think, something defensive in that "looked carefully", as though to emphasize to anyone who read his notebooks (a group that included the Director and could, presumably, include Lankester himself) that he had tried as hard as possible to find chitons for Lankester, and had failed despite these efforts.) Easter weekend and another trip out on the *Lola* put an end to the quest for chitons, and for the rest of the year, he seems to have collected and hatched fish eggs without much interruption.

The chitons-for-Lankester saga, although it was not repeated,[90] offers a revealing look at one of the most interesting features of Cunningham's position as a scientific salaryman. Cunningham was a naturalist, with his own interests and projects generally in

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[88] D1, 24 March 1888, [117-118].
[89] D1, 27 March 1888, [119].
[90] Or at least, if such a situation was repeated, this was not reflected in Cunningham's notebooks. This is of course possible, but unlikely given the special nature of his notebooks as records of labour. If he were spending his time searching for specimens for Lankester or any other MBA member or visiting naturalist, he would have been likely to make note of this fact, for the information of the Director and of Council.

keeping with the kind of work he had been hired by the MBA to undertake; he was also an employee operating within a particular corporate structure, under a particular set of supervisors and higher-ups who had to remain satisfied with his performance. For Cunningham, doing a satisfactory job could mean different things on different days, but it always meant combining his own goals with the goals of the Laboratory and the MBA. Collecting, hatching, and observing fish specimens meant making progress on his own research into the development of food fish, thereby satisfying the Laboratory's mandate of fisheries-oriented research, and the MBA's stated goal of combining science and industry. Cunningham's laborious efforts to get adequate hatching jar and fish aquarium systems up and running was for the good of his research into embryonic, young, and adult fish, as well as for the Laboratory's aim of charging public admission to the tank room and the MBA's goal of advancing marine biology by providing specimens and salt-water tank facilities to British and foreign naturalists. Cultivating and making use of a network of people and boats who could keep him supplied with specimens from both inshore and offshore made his studies of the sole, the species about which he became known as the expert, possible, and also made possible the MBA's claim to their present and potential donors that their aim was in large part to improve Britain's trawler-based sea fishing industry, despite the fact that they could not afford to own a trawler. Cunningham's doomed quest to get chitons for a member of the MBA Council was just a slightly more amusing version of the kind of multiple service that characterized large portions of Cunningham's working time at the MBA Laboratory. While tow-netting for fish eggs, why not also dredge for Heape's invertebrates? While sending Roach down to the fish quay to get conger eggs (saving the Laboratory's money by asking Roach to first buy a conger, then remove its eggs, then sell it back), why not also get him to bring back the trawler scruff to search the rocks for chitons for Lankester? While keeping track of which fish eggs were collected, where, whether and when they'd hatched, and how their chromatophores were developing, why not also keep a record of hours worked and expenses incurred? Just as the MBA was accountable to show that it was, or at least could be, useful to the fishing industry that had provided, directly or indirectly, much of the motivation and money to get the Plymouth Laboratory established, Cunningham was accountable to the MBA to show that his work was on their behalf and was in line with their goals. In the next chapter, we'll see how Cunningham established the same kind of symmetry of goals with the fishermen who were his most important source of specimens.

Chapter 3

Managing Marine Life:
Control, Access and the Specimen Supply Chain

Introduction: Getting and Keeping

The embryological work undertaken during the late 1880s and early 1890s in England by J.T. Cunningham, first Chief Naturalist of the Marine Biological Association's Plymouth Marine Laboratory, and in Scotland by W.C. M'Intosh, Professor of Natural History at the University of St. Andrews and head of the St. Andrews Marine Laboratory, had many similarities in its published form. Both workers laboriously studied the development, from the egg through the larval to the adult stage, of marine food fishes that were important to the British fishing industry and the British food supply alike: flat fish like sole, flounder, and plaice, round fish like cod and whiting, and other groups such as herring and eels. The embryological studies they produced were characterized by a large number of carefully wrought drawings of each developmental stage of a given species, with careful attention to the placement of such features as oil globules and pigment granules. Though M'Intosh did not always approve of Cunningham's methods of answering questions of importance to fisheries regulation (as we will see in Chapter Six), in their embryological work they followed remarkably similar procedures, and encountered many of the same difficulties. Chief among the difficulties facing the zoologist who undertook to study fish was the problem of supply: how to get hold of fish and keep them alive long enough to study. In the case of fish embryology, the task was even more taxing. Tiny, transparent, and very, very perishable, fish embryos were hard to come by and hard to hold. Indeed, the difficulties of finding and keeping alive the embryos of marine organisms was one of the chief motivators for a major movement in biology during the latter decades of the nineteenth century: the establishment of marine research laboratories on the coasts of many European nations, Britain eventually among them. In their new full-time facilities, specially equipped for studying marine organisms and their embryos, Cunningham and M'Intosh enjoyed year-round access to the realm of marine life, and to the means for sustaining it while under study, that was a novelty and a luxury among British naturalist-biologists. Of anyone working in Britain at that time, they should have had the easiest time obtaining specimens for study, and the best odds at getting the specimens to live long enough to learn something from them.

This probably was the case; the funds and initiative furnished by bodies like the Marine Biological Association (via the Fishmongers and the Prince of Wales, among many others) and the Fishery Board for Scotland combined to place Cunningham and M'Intosh in the most advantageous places possible to do their work, and their research lives *were* made far easier by their easy access to salt water holding tanks in the laboratory and the presence of that great salt water holding tank, the sea, just beyond the laboratory doors. Yet the course of their embryological work was anything but smooth. The laboratory notebooks kept by both men record an endless stream of troubles when it came both to obtaining fish eggs and embryos from the sea and to maintaining them in the laboratory. What's more, these two kinds of difficulties were a closely linked pair. If a particular specimen was hard to obtain, then there was greater pressure not to let any go to waste by letting them die in the tanks. If the laboratory tanks proved to be poor at supporting a particular kind or stage of embryo, then pressure shifted to the supply side, and a steady stream of specimens had to keep coming into the laboratory in order to keep the work moving forward. What was more, the supply of eggs and embryos from the sea was seasonally determined, adding extra pressure to the struggle to get and keep, never mind study, food fish embryos.

In order to keep up this supply, neither Cunningham nor M'Intosh could go it alone. They used their advantageous position at the seaside to go on frequent collecting trips, true, but they took advantage of their location to do another kind of collecting. Plymouth and St. Andrews were both coastal towns home to thriving fishing communities, and both men enlisted allies from within these communities in order to keep themselves supplied with fish. Establishing relationships with local fishermen allowed Cunningham and M'Intosh not just to go out on extended, offshore collecting trips with the fishermen, but eventually to enroll the fishermen into the business of collecting and even manipulating specimens when there was no naturalist present. Both men also engaged an assistant from the local community who could undertake many collecting duties, as well as serving as a connector between the community and the marine laboratory, spreading the word that Mr. Cunningham or Professor M'Intosh was seeking a particular kind of specimen and was willing to pay for it. In this chapter, I will explore the links that these two marine station scientists formed between themselves and the specimens they needed to bring from the sea to the laboratory. I will show, too, the ways in which they attempted to shorten these chains or break free of them entirely, aiming to reduce their reliance on the supplier role played by the sea and by the local fishermen, while maximizing the amount of activity taking place within the laboratory tanks themselves. Access to the required eggs, embryos, and adults, and control over them once obtained, became easier and more powerful the more the steps could take place away from the sea; in certain cases, such as Cunningham's study of light-induced abnormal colouration in flounders, his study of artificially fertilized hybrid plaice/flounder embryos, and in M'Intosh's study of hybrid turbot/brill embryos, such control was absolutely essential.

This emphasis on enlistment of allies and the struggle to successfully establish something like control over their specimens, which will be such a useful way to understand the relationship between Cunningham's and M'Intosh's work and their access to their experimental subjects, is a major distinguishing feature of the work of the French sociologist of science Bruno Latour. In *Science in Action*, Latour stresses the importance of understanding scientific work as a process not just of consensus-building within the scientific community, but far more broadly as a process of aligning a variety of interests, bringing others into one's work by convincing them that by helping you, they are helping themselves as well.[1] Like most of Latour's work, this theory is presented in the context of a warlike world in which the scientist must act tactically in order to manipulate the resources at his disposal. I do not share this view of science,[2] but Latour's work has been incalculably valuable in the way in which he directs our attention to otherwise ignored aspects of scientific practice, be they the enlistment of allies, the role of non-human

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[1] Bruno Latour, *Science in Action: How to follow scientists and engineers through society* (Cambridge: Harvard University Press, 1987), 109-128. Another famous study of this type is more marine-oriented: Michel Callon's study of scallop-farming in France, which argued that both the scallops themselves and the fishermen had to be brought into line in the course of marine biologists' efforts to increase the scallop populations. Michel Callon, "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieuc Bay," in *Power, Action and Belief: A New Sociology of Knowledge*, edited by John Law, 196-233, (London: Routledge & Kegan Paul, 1986.)

[2] Donna Haraway, whose work is definitely influenced by Latour's sociology of science, has voiced similar objections, saying that while Latour's work has been "energizing", its vision of the battle of constructing scientific facts places the scientist firmly in the old-fashioned role of the heroic general: "The action in science-in-the-making is all trials and feats of strength, amassing of allies, forging of worlds in the strength and numbers of forced allies…'nature' is multiply the feat of the hero, more than it ever was for Boyle." Donna J. Haraway, *Modest_Witness@Second_Millenium.Femaleman©_Meets_OncoMouse™: Feminism and Technoscience*, (New York: Routledge, 1997), 34-35.

actors, or the small-scale details of scientific work as it happens. This chapter will follow Latour's advice, if not exactly his methods or worldview, in tracing the chains of allies on which the marine naturalists relied in order to do their work; in particular, I will focus on the "specimen supply chains" built and used by Cunningham and M'Intosh, and the efforts of Cunningham in particular to cultivate and then become as independent as possible from these supply chains. It will, as well, listen to Bruno Latour's warning that a particular piece of scientific work, once completed and closed, has a very different look from that same science in action. This warning followed me to Plymouth, where I worked by the window of the National Marine Biology Library, looking out at Plymouth Sound where Cunningham got his specimens and his seawater: his notebooks, especially early on, told the tale not of exciting new insights into the development of the sole (which would become the subject of his 1890 *Treatise on the Common Sole*) or revealing ruminations about the relationship of pure and applied science (which I had been expecting, or at least hoping for), but instead the story of Cunningham's everyday struggle to find and identify eggs and embryos, to get them from sea to boat to land intact, and to keep them from being killed in (and indeed *by*) the laboratory tanks.

To find these eggs, eventually to fertilize them if possible, and to bring them back to shore healthy and developing, Cunningham came to lean on the fishermen of Plymouth, as well as on his assistant William Roach and Roach's own helpers. He got this help in exchange for money, certainly, money provided by the Marine Biological Association and for which Cunningham carefully accounted in the periodic expense statements that he drew up in his notebook and submitted to the Association. However, there were other reasons why the fishermen were enrollable allies. Nobody, not even

Cunningham, who owed his living to the ongoing obsession in Britain with understanding and protecting the fishing industry and food supply, had more at stake than the fishing communities of Britain in the findings of fisheries scientists. It had been the fishermen's alarm about the state of the fish stocks and the dangers of trawling that had prompted the Royal Commission of 1883. They had been the major source of information about fish and fishing to that Royal Commission, and to the earlier 1863 Royal Commission as well. The report of the 1883 Royal Commission, and the reaction to it by such influential figures as Sir Lyon Playfair, had been emphatic in transferring to science, and away from the fishermen, the responsibility for providing information about the biology and abundance of fish and the influence of trawling. But it was still the fishermen whose lives would be the most powerfully influenced by legislation, or worse yet by the disappearance of the fish stocks that were their livelihood and staple food. Now that Britain had entered the age of marine research stations and professional fisheries scientists, men like Cunningham and M'Intosh were the government's main source of access to information about fish and fisheries. But fishermen could retain their place in the loop, so to speak, when Cunningham and M'Intosh recruited them into their research supply chain. They were, I suspect, not difficult to recruit, not just because of the money or because of the naturalists' great personal charm (indeed, accounts of both M'Intosh and Cunningham alike make them sound quite the opposite), but because their interests were already pretty well aligned. For the fishermen, joining the fish(eries) biology supply chain – and they supplied not only specimens and labour, but also knowledge and advice – meant regaining some of their earlier influence.

Cunningham witnessed early on in his time at Plymouth what an active, organized, and influential group the fishermen could be. On October 10, 1887, he arrived at the quay at the Barbican, Plymouth's fishing village in Sutton Harbour,[3] to find "there was a good deal of excitement among the men and all the trawlers in port."[4] The trawlermen were incensed that fish, particularly hake, was being landed at Plymouth from boats based elsewhere, and this fish was being preferentially sold to the local fishmongers, so they had seized the boxes of hake and dumped them in the harbour. Their actions resulted in a meeting early that afternoon, which Cunningham attended, and found that "[a] considerable amount of bad feeling was manifested between the men and the salesmen, while between the owners of boats and the men the best agreement understanding [sic] existed, except that the former earnestly deprecated violent and illegal action which the men in their excited state were inclined to support."[5] This meeting ended with the appointment of a committee of seven trawler owners, chaired by a trawler skipper, to "have a conference with the salesmen in the afternoon, and hoped to report a satisfactory arrangement to another meeting of the men at 7.0 p.m."[6] The fishermen were able, and entirely willing, to exert their collective influence. Cunningham needed to understand the dynamics of Plymouth's fishing community in order to make connections that would help him get specimens, and made frequent trips to the Barbican during his time at the Plymouth marine laboratory.

[3] When Plymouth was almost completely destroyed during the Second World War, the Barbican was one of only a few places not levelled during the bombing or during the rebuilding following the war.
[4] D1, 11 October 1887, [28-29].
[5] D1, 11 October 1887, [30].
[6] D1, 11 October 1887, [30].

II. Getting Specimens, Getting Help

As we saw in the last chapter, when Cunningham first arrived at Plymouth in early August of 1887, his laboratory was not yet ready for him, so he took rooms down at the harbour and began work anyway, purchasing some fish at the quay and dissecting them in his quarters.[7] Buying dead adult fish at market was an approach that he occasionally took later on, as well, especially if he had just become interested in a particular species and wanted to become acquainted with its internal anatomy, as happened with conger eels and with soles in November of 1887. But dead adult fish were no way to study embryology, nor any way to fill his laboratory tanks once they were ready, and Cunningham very quickly began to go fishing himself, collecting specimens using a tow net or a dredge and listing his catches in his notebook.[8] By the time he left his previous job at Granton, Cunningham had gone far beyond working in rented rooms cutting up dead fish bought at market: he had developed connections both within and without the station to help him obtain specimens and artificially fertilize eggs. In a lengthy 1888 paper given to the Royal Society of Edinburgh and based upon work done at the Scottish Marine Station in Granton, Cunningham gave some idea of the sources of supply he had access to:

> The necessary operations [of artificial fertilization] were carried out, in some cases by myself, on board fishing boats – usually steam trawlers from Granton. In many instances I did not myself go out in the boats, but the ova were obtained and brought to me at the laboratory by ALEXANDER TURBINE, keeper of the station. But in every case there is no uncertainty as to the species of the fish

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[7] D1, 9 August 1887, [1].
[8] D1, 9-12 August 1887, [2-5].

> from which the ova were taken; if there was any doubt, specimens of the parent fish were brought with the ova.[9]

At Granton, then, Cunningham had established an enviable set of circumstances with regard to the supply of specimens for his developmental work: he had access to commercial fishing vessels, on which he could either go out himself or send the reliable station keeper, Turbyne, to collect and fertilize the eggs in his stead, and to remove any uncertainty about the species of the eggs and resulting larvae by bringing back the adult fish from which the eggs and milt had come. Moving to Plymouth meant a pay increase, but it also meant the loss of this system of supply. Cunningham would have to construct a new one if he was to have access to egg, larval, and adult fish specimens, as well as sufficient time to study them in the laboratory.

In November 1887, Cunningham added an important link to his new specimen supply chain. He received word from Walter Heape,[10] the first Director of the Plymouth Marine Laboratory, that an assistant, William Roach, had been hired on as "servant ~~to~~ for the Laboratory" to aid Cunningham in his work. Cunningham, eager to get started, "sent him a note asking him to come & see me at 12.0 noon today at my room in the Laboratory. He came at the appointed time, bringing with him a small flat-fish alive in a bottle of sea water. He said he had taken it out of a lobster pot of his own this morning."[11] Cunningham's short account of meeting Roach for the first time captures exactly why Roach would be such a valuable intermediate between the laboratory and the sea, and between Cunningham and the local fishermen: a fisherman himself, Roach also

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[9] J. T. Cunningham, "The Eggs and Larvae of Teleosteans," *Transactions of the Royal Society of Edinburgh* 33 (1888): 97.

[10] Heape, who had studied at Cambridge under the famed and beloved embryologist Francis Maitland Balfour, was later to become famed for his founding role in reproductive biology/endocrinology, for his work establishing the role played by hormones during development.

had a naturalist's eye for the kinds of specimens that might be of interest to Cunningham. Roach also had a boat, the *Quickstep*, which immediately and greatly increased the supply of specimens available for study. Sometimes Cunningham and Roach would go out trawling together; sometimes they would bring along W. F. R. Weldon, who was working at the Plymouth laboratory mainly on invertebrates; and, increasingly, Roach would go out trawling alone in Plymouth Sound and bring his catch back to Cunningham, who would dutifully list its contents in his notebook.[12] Cunningham also began to benefit immediately from Roach's local knowledge of the times of the year when particular fish species were likely to be found: when his new interest in conger eels[13] led Cunningham to purchase some at market and dissect them and then to ask Roach to fish him some more, Roach told him that November was late for congers, but that he would try. Roach managed to bring in two congers that afternoon, and then caught the end of the season by purchasing seven more small ones at market.[14] Roach himself had an assistant on occasion, sometimes one referred to only as the "boy", and for a time a fellow named Sam, who would accompany him on some of his tow-netting excursions.[15] More and more, Cunningham stayed in the laboratory, busily sketching the interesting features of the specimens Roach had procured, and assigning station numbers to the various locations most frequently trawled.[16]

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[11] D1, 7 November 1887, [39-40].
[12] D1, 6-20 December 1887, [50-59].
[13] Cunningham's struggles to understand the reproduction of the conger eel spawned, so to speak, its own dedicated notebook, titled *Conger*; these animals strongly resisted his best attempts to understand them or even, sometimes, to determine their sex.
[14] D1, 7 November 1887, [40].
[15] D1, 16 January 1888, [65].
[16] Being able to stay indoors while Roach voyaged after specimens alone had many advantages, and the Laboratory came to rely heavily on his services. The perils of going to sea after specimens, and the extent to which Roach was an essential member of the Laboratory staff, are revealed in part of an 1889 Report of then-Director G. C. Bourne: "At the end of February the Laboratory fisherman, W. Roach, was sent to

By the beginning of 1888, Cunningham's enthusiasm had shifted almost entirely to getting specimens of the many floating fish eggs that Roach could capture in the surface tow-net. Roach would go trawling, then distribute the contents of the net into seawater-filled bottles; Cunningham would then comb through these bottles looking for fish eggs. From "5 or 6 bottles of tow-net captures" brought in by Roach in late January of 1888, Cunningham was able to find "3 different kinds of pelagic ova, but only in small numbers,"[17] which he sketched that afternoon. Two days later, these eggs had hatched into larvae before Cunningham's eyes, and he spent the morning of that day making still more sketches.

In the exciting couple of weeks that followed, many fish eggs came into the laboratory and some of these hatched. Cunningham discovered a new challenge in studying the development of specimens brought in from the sea: it was very difficult to identify which species a given egg belonged to, and the hatchlings didn't tend to live long enough to help much with this identification. The simplest-seeming conditions were not always easy to meet. Keeping the fish eggs floating was necessary if they were to be kept alive, but the eggs turned out to be remarkably sensitive to the density of the seawater that filled the hatching jars. During one hectic week in April 1888, Cunningham sent Roach and Sam all over Plymouth Sound, for several days in a row, to gather seawater at different depths and locations in a desperate effort to re-float some gurnard ova. The following Saturday found "most of the ova alive but not looking healthy" and

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Mount's Bay in a trawler to procure the fertilized ova of the whiting (*Gadus aeglefinus*) and pout (*Gadus luscus*). The weather turned out very cold and boisterous, and during his week's work Roach was not only unable to keep alive the ova that he collected but caught a chill and was laid up for some days afterwards, so that dredging operations were nearly at a standstill." (G. C. Bourne, "The Director's Report. – No. 2." *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 115.)

[17] D1, 21 January 1888, [69].

Cunningham anxious; on Monday "Roach informed [Cunningham] that Sam had left on Saturday without notice," and Cunningham, pausing to note the passing of the gurnard eggs, promptly sent Roach out on his boat to "make another strenuous effort to get sole's ova. Some of the ova were still alive but did not look healthy."[18] Cunningham's hatching jars seemed at any moment to contain laddered series of live eggs just collected from the sea, eggs somewhat further along in development but rapidly approaching death, and eggs recently pronounced dead; as the gurnard ova passed away, the sole ova clung to life, and Roach raced out to bring fresh ones in from the sea.

Under these conditions, Cunningham settled for distinguishing the different kinds of eggs from one another (not a simple task either, since they were all basically small transparent spheres), assigning each kind a number, and describing the findings from a new tow-netting as, for instance, containing a few of "Sp. 7", some of "Sp. 8", and a few that might be "Sp. 3".[19] Cunningham began a separate notebook titled *Ova of Teleosteans*[20] in an effort to identify the fourteen different types of pelagic fish eggs that he could distinguish. The entries for species one through fourteen included information about where the specimen was collected, measurements of the sizes of eggs, yolks, oil globules, and the perivitelline space. If an egg happened to hatch, the larva was painstakingly described as well, especially with regard to pigmentation, which was a distinguishing feature as well as a developing trait that was of special interest to Cunningham. Each entry was a work in progress. Eggs of Species 5, for instance, eventually hatched into larvae described as having "had black & yellow dendritic

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[18] D1, 29 April- 5 May 1888, [141-145]; quoted passage, [145].
[19] D1, 2 February 1888, [79].

chromatophores: eyes deeply pigmented." To this entry was later added above a tentative species identification, "Pleuronectes platessa"; a later note added below states that "Plaice egg is 1.95 mm in diameter, acc. to my Edinb. Trans. paper. This ovum is doubtless the Plaice."[21]

But Cunningham's attempts at identification based on found eggs and short-lived larvae could sometimes be very inaccurate: the entry for Species 4, at some point Cunningham assigned the identification "Probably Pl. flesus", the common flounder, only to later cross it out and identify Species 4 not as a flat-fish at all, but as "Gadus luscus", the bib, a member of the cod (gadoid) family of round fish. The flatfish and the gadoids were the two most economically important groups of groundfish in Britain, and looked remarkably different as adults, but at least early in development were so difficult to tell apart that even Cunningham could be fooled. Through the recruitment of Roach and the extra access to the sea, through the time, skill, local knowledge, boat, and net that Roach represented, Cunningham was able to obtain a wide range of fish eggs. But even so primary a task as identifying these eggs was hampered by Cunningham's relative lack of control over his specimens. There were three possible ways to bring certainty to the task of identifying fish eggs. The first was to collect free-floating, naturally-fertilised eggs from the sea as usual, but somehow keep these eggs alive in hatching jars and laboratory tanks until they hatched into larva, metamorphosed and grew into easily recognisable adults. The second was to extract unfertilized eggs and milt from known adult fish and combine them, causing the eggs to become artificially fertilized in jars. For the eggs and

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[20] This notebook, along with five others in the same hand, was found in a large box in the MBA archives and was not known to be Cunningham's, as it was not labelled with his name. However, the handwriting is unquestionably Cunningham's.

[21] OT, [1888], [5].

milt to have any chance to be intact and likely to combine, this artificial fertilization would have to be done at sea; in order for Cunningham not to be at sea all the time, he would have to enroll some trustworthy fishermen to identify males and females of particular fish species, to collect and combine the eggs and milt in jars, and to transport the specimens to the Plymouth laboratory. Cunningham's further extension of his specimen supply chain through the enrollment of local trawlermen in this task will be the focus of the next section. The third way to bring certainty to the identification of fish eggs was to bring the entire process of fertilization and growth indoors, transplanting hardy adults into the laboratory tanks, isolating species from one another as they approached the reproductive period, and then collecting the fertilized eggs from the surface. These eggs could then be isolated and their development studied as far as possible. From 1889, many entries in Cunningham's *Ova of Teleosteans* notebook dealt with ova of known origin, some collected from tanks; the tank system also allowed Cunningham to comment on the behaviour of the adults towards the fertilized eggs. In early 1890, Cunningham began a new notebook of *Aquarium Notes*; his pursuit of indoor control over his specimens will be the focus of a later section.

Nor were these provisional lettering and numbering systems limited to Cunningham's laboratory notebooks: even in published material, he would report on unidentified fish eggs, and the processes by which they might be identified. In an 1888 article on "The Eggs and Larvae of Teleosteans," Cunningham included a section on "Species not identified" that included descriptions of unidentified fish eggs of species labelled numbers twelve to fifteen, and noted that "[t]here are two possible methods of identifying an unknown species of pelagic ovum. One is to compare it, or the larva hatched from it, with figures and descriptions of ova or larvae already known; the other, to keep a number of specimens of the ovum in question alive until they hatch, and then to keep the larvae till they attain the specific characters of the adult fish. Both of these methods are liable to error."[22]

William Carmichael M'Intosh, up at St. Andrews, benefited as well from an assistant with one foot in the fishing community and the other in the laboratory. In the summary of his career's-worth of fishery notes published in 1927 in *The Fishing News*, M'Intosh paid tribute to his assistant of many years:

> From 1883 onwards for nigh 40 years the Marine Laboratory had the help of a...fisherman and naturalist in Alexander Wallace Brown, of St. Andrews. His evidence before Lord Dalhousie's Commission so interested the members that several wrote me in his favour when the post was instituted. In seamanship, the making and the use of nets, in laboratory work, care of engines, skill in photography and general handiness he excelled. He was of signal service in supplementing the details of the captures.[23]

Like Roach, Brown had the local knowledge of a fishermen but the eye for the unusual that made him a valuable assistant to M'Intosh. M'Intosh often made mention in his notebooks of Brown's observations or recollections, as for instance in an entry of 18 May, 1892: "Brown states that he knows that once a Turbot of 30 lbs. was got in Bay (not ripe), [and] one of 24 lbs in salmon-nets at pier. The latter was useless as found."[24]

M'Intosh, who together with Brown gathered fish ova using a surface net, made use in his notes of a system of letters, rather than numbers as Cunningham had done, to identify floating fish eggs that he had collected. His notebook for March of 1891

[22] Cunningham, "The Eggs and Larvae of Teleosteans," 105.
[23] FF, 5.
[24] NB, 18 May 1892, 68.

described collecting specimens of "larval E", and "Larval fish G", along with "2 peculiar gelatinous ova",[25] which in the following days he described in extensive detail, with attention to any possible distinguishing features such as pigmentation, yolk, and oil globules. As some were identified, others remained a partial or total mystery: a collecting trip on April 24, 1891 yielded "Ova of gurnard, sprat, Pleuronectid B (one)."[26] Like Cunningham, M'Intosh's system of provisional lettering made it into published reports of his research. In their comprehensive, nearly three hundred page-long contribution to the *Transactions of the Royal Society of Edinburgh* "On the Development and Life-Histories of the Teleostean Food- and other Fishes," M'Intosh and E. E. Prince[27] provided descriptions of "Unknown Larval Pleuronectid? (A)," "Ovum of Pleuronectid (B)," which the authors noted had previously been described by Cunningham as well, and "Unknown Ovum (C)."[28] These ova were captured by tow-net, sometimes at the surface, often "a fathom or two" lower down, and occasionally on the bottom (perhaps "due to cold at the surface or to the effect of currents") and fish ova of some kind were found to "occur almost in every haul on suitable ground."[29]

In addition to the paid assistance from which Cunningham and M'Intosh benefitted, both men occasionally received useful specimens through another route, when local naturalists would send along what they had found. M'Intosh would occasionally receive material from the Fishery Board for Scotland; on one occasion in 1891 he

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[25] NB, 6 March 1891, 15.
[26] NB, 24 April 1891, 30.
[27] E. E. Prince was M'Intosh's junior colleague at the St. Andrews Marine Laboratory, and later went on to become the Commissoner of Fisheries for the Dominion of Canada.
[28] W. C. M'Intosh and E. E. Prince, "On the Development and Life-Histories of the Teleostean Food- and other Fishes," *Transactions of the Royal Society of Edinburgh* 35 (1887-1888): 852-854.
[29] M'Intosh and Prince, 670.

reported being sent a single halibut egg.[30] He and Prince received specimens of fertilized plaice eggs from one "Captain Burn, late of the 11th Hussars"[31] that were described in detail in their immense 1888 paper to the Royal Society of Edinburgh. M'Intosh was a well-known figure, and when he and his junior colleague A.T. Masterman published in 1897 *The Life-Histories of British Marine Food-Fishes*, a book of fish biology aimed at a broader audience than any of his earlier publications, M'Intosh took the opportunity to recruit his readership into another specimen supply chain. A three-page appendix to the book explains to readers all about the process of artificial fertilization: how to collect eggs and milt, how to identify fertilized eggs, how to keep them alive, and how to pack them for transport "by the fastest train to the St Andrews (Gatty) Marine Laboratory with the name and date of fertilization, locality and name of sender. A caution to keep the jar cool is also important. No hesitation need be felt about distance."[32] Interested readers were also instructed on which species were of particular interest, and, should they be successful in artificial fertilization, urged to keep some of the fertilized eggs until the eyes appeared, and then to pack them up and send them to M'Intosh as described. Natural history, and specimen collecting in particular, had been a popular hobby in Britain for some time, as had the building and stocking of parlour marine aquaria (see Chapter Five); add to that the current national preoccupation with the status of food fish stocks, and M'Intosh's appeal seemed unlikely to fall on deaf ears.[33]

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[30] NB, 20 May 1891, 34.
[31] M'Intosh and Prince, 840.
[32] William Carmichael M'Intosh and Arthur Thomas Masterman, *The Life-Histories of the British Marine Food-Fishes*, (London: C. J. Clay and Sons, 1897), 465-467.
[33] Unfortunately, I don't know how successful it was, although M'Intosh was always receiving new jars of ova. In comparison with Cunningham, M'Intosh tended far less to comment on the provenance of his specimens, though he was just as active (and in the case of the *British Marine Food-Fishes* appeal, even more inventive) in cultivating sources who could supply him with specimens.

Cunningham had a very useful and generous supplier in the person of Matthias Dunn, of nearby Megavissey. Cunningham and Dunn established a written correspondence, and early in his time at Plymouth, Cunningham sent Dunn the first number of the new *Marine Biological Association Journal*.[34] Cunningham would occasionally make a trip to go to visit with Dunn, who knew all of the best collecting spots in the area, particularly when it came to larval and young flatfish. It was Dunn who provided the young flatfish used by Cunningham in his experiments on the influence of light on the development of pigmentation,[35] to be discussed in the next chapter.

III. Enrolling the Fishermen

Hiring paid assistants like Roach and Brown from within the fishing community to go on collecting trips was an important way in which Cunningham and M'Intosh accessed the specimens they needed. However, the most powerful source of specimens was a chain that made use of the naturalist himself, his assistant, and a third group of essential allies, the local fishermen. The fishermen possessed another wealth of local knowledge about where and when particular fish species could be found, and by making connections with the owners and captains of local trawlers, it became possible to access far more specimens from much farther afield than previously. Trawler-owners and captains who would, for a fee, allow the naturalist and his assistant to come along on a several days' fishing voyage were allowing themselves, their boats, and their crews to become enrolled as specimen suppliers, and they might not have known just how

[34] D1, 10 March 1888, [111]. Cunningham occasionally received interesting specimens from others in the area, as on September 21, 1891, when the Honorable Evelyn Ellis, of nearly Looe, sent first a telegram and then visited in person to give to Cunningham "a curious double-sided plaice," which turned out to be a flounder coloured on both sides (D2, 21 September 1891, [221]).

extensive this involvement could become, and that it would come to include voyages in which there was no naturalist aboard. Cunningham's notebooks in particular reveal how going to sea on a trawler was not just a way to collect specimens from further offshore than Roach's small boat could take them, but also a chance to engage the officers and crew in the work going on at The Laboratory (as it was known in Plymouth), to identify interested and reliable men, and to instruct them on methods of collecting particular specimens, as well as how to collect eggs and milt and perform artificial fertilization and transport the fertilized eggs back to shore. Cunningham periodically went to sea on trawlers throughout his time at Plymouth, but the connections he established meant that during the long periods of time when he stayed ashore and worked in the laboratory, his access to the ocean still extended out to Plymouth Sound and to the Channel beyond, and the specimens he needed continued to cross the laboratory threshold.

Cunningham had scarcely been working in Plymouth for a week when he managed to join a trawler embarking from Plymouth Harbour; while aboard the *Cambria*, Cunningham took the opportunity to ask the skipper what he knew about the conger eel.[36] The first few months of his time in Plymouth, while the laboratory was still being finished, Cunningham spent a great deal of time making inquiries of fishermen, both trawlermen and bulterers,[37] visiting the quay often and even taking a trip to Dartmoor to talk with oyster dredgers. In early November of 1887 Cunningham and Roach made a trip on the sailing trawler *Lola*, bringing with them a box of bottles, two large jars, a tow

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[35] D2, 3 April 1889 ([58]) and 16 April 1889 ([69]).
[36] D1, 15 August 1887, [5].
[37] That is, line-fishermen, who worked inshore out of small boats with long lines of many baited hooks. My thanks to Ms. Linda Noble, librarian at the National Marine Biological Library at the Plymouth Laboratory, for help with this bit of local terminology.

net, a sounding line and another small line, and a thermometer.[38] This trip marked the beginning of an extraordinarily productive relationship between Cunningham and Prouse, the skipper of the *Lola*. By January and February 1888, Prouse was bringing to Cunningham the stomachs of soles, turbot, brill, dorey, and haddock that Cunningham had requested for his study of the fishes' diet, preserved in jars of spirit that Cunningham had prepared and sent to the *Lola*.[39] This essential link to the sea established, Cunningham wrote himself a memo: "Mem. Get tow nets worked as often as possible out at sea from trawlers."[40] Cunningham and Roach would visit the Plymouth quay, meeting with trawler skippers to arrange for trips or specimen orders, as well as cultivating new relationships, as on the February afternoon when, going down to the quay and finding Prouse of the *Lola* away on a voyage, then met "Ridge, whose brother is skipper of a trawler & he proposed that I should use her to go out in…Then I saw the skipper of the Prosperous a small trawler wh[ich] carries 37 ft beam. Went on board & arranged to go out on Monday morning at 7.0 a.m. if weather is fine."[41]

It was during these trawling trips that Cunningham took steps to obtain developing eggs of known origin through artificial fertilization, carried out at sea. In a long trip down to Land's End in the *Lola*, in preparation for which Cunningham had "hired the cabin furniture and utensils from the owner, Mr Watson,"[42] Cunningham scoured the trawls for ripe soles. Having managed to extract "a few ripe eggs which I squeezed into bottles," he then could not find any ripe males, a situation which became a

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[38] D1, 9 November 1887, [42-43].
[39] D1, 17 December 1887 ([58]), 5 January 1888 ([59]), and 10 February 1888 ([90]).
[40] D1, 7 January 1888, [60].
[41] D1, 4 February 1888, [83].
[42] D1, 5 March 1888, [103].

major impediment to performing artificial fertilization at sea.[43] He settled for extracting and dicing the testes and dropping them into the collecting jar with the eggs, "in hopes that fertilisation might be effected so."[44] He made another such trip the next month and ran into the same problem, finding "a good number of ripe sole's eggs, but could not find a male from which milt could be squeezed: so I had to resort to the same expedient as at the previous haul [and previous voyage], namely cutting out testes cutting them up and placing them in the bottle with the ova."[45] On this voyage, Cunningham was reminded that although specimen collection might be his main objective and one that Prouse was willing to help with, Cunningham's need for fish eggs would not be allowed to hinder the main task of trawling for saleable fish. Cunningham's notes from the second day of the voyage describe what happened when these two objectives fell out of alignment, showing the limits of the alliance between the *Lola* and the Laboratory:

> I sent down a small tow-net fastened on to the trawl beam. Prouse was unwilling that I should do this saying that the breeze was light enough for towing without the additional strain of the tow-net! But he grudgingly consented. I could not suggest putting over another at the surface, he would have been sure to refuse.[46]

Fishing had to come before collecting, and Cunningham was not willing to risk losing his most productive ally for the sake of a few extra fish eggs on this one voyage, recognizing that by collecting at all during times of light winds he was already straining the relationship. On yet another trip out on the *Lola*, Cunningham recruited another reliable

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[43] Even when the female fish were found to be distended with ripe eggs, Cunningham and his allies often could not find any males with milt, a situation which he found both baffling and frustrating, but eventually became accustomed to: on an April 1889 voyage with Prouse, Cunningham described fertilising several hundred eggs by cutting up testes and adding them to the egg jar, "as usual" (D2, 9 April 1889, [63]).
[44] D1, 3 March 1888, [106].
[45] D1, 4 April 1888, [123].
[46] D1, 4 April 1888, [121-122].

ally from among the crew, helping to strengthen the ties between his aims and the *Lola*'s. The winds in this case were not at all too light for collecting, but instead so high and the sea so rough that Cunningham became ill, and "had to give up squeezing the fish" for eggs and milt and go below. Frank, the *Lola*'s second hand, picked up where Cunningham had left off "and took some red gurnard, some merry soles, some hake, and also said he got one sole with ripe ova, which he tried to fertilize as usual by cutting out the testes & putting them in the water with the ova."[47] The weather was sufficiently rough that Prouse put the *Lola* ashore at Falmouth for the night (Cunningham leaving the boat at that point, probably very gladly, and travelling back to Plymouth by land).[48] It is remarkable that any member of the crew was willing to collect and squeeze fish under those conditions, much less dissect out the testes of a sole and mix them with eggs in a jar: Frank of the *Lola* thereby joined the ranks of Cunningham's trusted contacts down at the fish quay.

The supply-chain combination of Roach and the trawlermen allowed Cunningham to extend his reach very far indeed. Not only could he send Roach and his boy out on the *Lola* in his stead to tow-net for floating fish eggs, near shore or on a Plymouth trawler,[49] but sometimes Roach's work took him even farther afield: at the end of April 1888, Roach rode on a trawler in the Irish Sea, making trawls and collecting specimens, returning to Cunningham with written records of the contents of each trawl (with fish species divided into "Best Fish" and "Common" categories), which Cunningham duly copied out into his notebook. Roach also brought back with him several bottles of ripe eggs fertilised with cut-up testes (since, yet again, no milt could be found) along with

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[47] D1, 14 May 1888, [151].
[48] D1, 14 May 1888, [151].

careful records of the locations, times, depths, and surface and bottom temperatures measured where the eggs were collected.[50]

In order to further extend the range of species as well as the range of distance of his access to specimens, Cunningham had to cultivate different relationships with different fishermen depending on the kind of specimens he was seeking: with beam trawlers for flatfish, roundfish, and their eggs; with shallow-water shrimp trawlers for young flatfish; and with mackerel- and pilchard-netters for these fish and their fertilized eggs. A pilchard-fisherman named Johns had an arrangement with Cunningham, via Roach, that he would receive three shillings per bottle of fertilized pilchard spawn;[51] when the first two bottles came back filled with unripe eggs, "the men not understanding the process," Cunningham sent Roach out on Johns' boat, the *Prima Donna*, the next day "to show them how and to try to get pilchard spawn."[52] The time spent either by Cunningham or Roach in training the crew was well worthwhile. If a simple request for fish of a particular species was all that he needed to make, a simple written notice posted in the Barbican would suffice, since Cunningham of The Laboratory had become a known figure among the fishermen: in November of 1890, for instance, he reported in his *Fishery Work* notebook that "In consequence of my notice in the Fish-quay a number of Anchovies were brought up this morning from the pilchard boats."[53] Local boys also knew that The Laboratory was always seeking fish, and that Cunningham was willing to pay for them, and would send them on repeat errands for more of whatever interesting thing they had brought in. The boys proved to be especially useful as suppliers both of

[49] D1, 16 January 1888 ([65]) and 23 January 1888 ([70])
[50] D1, 28 April 1888, [139-141].
[51] D1, 1 June 1888, [164].
[52] D1, 5 June 1888, [168].

specimens and of information when it came to young flatfish: the shallow waters where young flatfish lived were frequently scoured by the boys, who would bring a valuable specimen such as a young turbot up to Cunningham, promise to bring several more tomorrow, and tell Cunningham exactly were the fish had been found so that he and Roach could go tow-netting for more.[54] Within a year or two of arriving at Plymouth, Cunningham managed to build a remarkably effective specimen supply network, without which his studies of fish development, and his stocking of the laboratory's tanks and aquaria, would have been impossible.

There is evidence that M'Intosh at St. Andrews relied on connections within the fishing community as well, though perhaps not to the same hands-on extent that Cunningham seems to have done. In an early research report given to the British Association in 1886, M'Intosh along with his colleagues reported that in the past year,

> for the first time, the ova of the ling (*Molva vulgaris*) were examined, and the development followed to a fairly advanced stage. These were procured by a long-line fisherman of Cellardyke (who with others was supplied with suitable earthenware jars[1] and encouraged by a visit to the Laboratory), fertilized about 100 miles off the Island of May, and safely brought, after a considerable land-journey, to St. Andrews. The fertilised ova of the plaice and lemon-dab were similarly brought by Captain Burn, late of the Hussars, from the Moray Firth; for the Laboratory had then no boat suited for procuring a supply nearer home.[55]

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[53] FW, "Notes on Fisheries" section, 25 November 1890, [7].
[54] FW, "Aquarium Notes" section, 27 October 1890, [8]. In addition to the young, Cunningham cultivated a relationship with an elderly bulterer named George Philips, first paying him one shilling and sixpence per hour to go fishing with Cunningham and his "Norwegian box for carrying live fish…he has the box still to bring in some more fish." (D2, 9 May 1891, [179].)
[55] RC, 2.

Enlisting the local fishermen into the specimen supply chain had the effect not only of greatly increasing the supply of specimens, but also of freeing up a great deal of time that would otherwise have been used in collecting specimens and in performing other tasks, such as recording places and circumstances of collection, removing and preserving or cutting up organs, or in effecting artificial fertilization, that had also been passed on to interested and reliable fishermen. The more a naturalist could pass on to others the work that had to take place on the water, the more time and attention he could devote to the work that could be done inside the laboratory. Under these circumstances, the prospect of having to leave the laboratory as seldom as possible became more and more attractive, and the naturalists devoted more and more effort into hatching, maintaining, and even collecting specimens within the confines of the laboratory tanks. As their work shifted from the description of naturally-occurring specimens collected from the sea to the experimental manipulation of these specimens under laboratory conditions, and to the rearing and description of abnormal hybrids, the need to maximize the control that could be exerted over these specimens became more pressing.

IV: Exercising Control, Maximizing Access

By the summer of 1888, Cunningham's specimen supply chains were becoming established: he knew who to ask, or who to send Roach to ask, if he needed a particular kind of fish or fish egg. The crew of the *Lola*, (particularly the skipper, Prouse, and the second hand, Frank) and the *Wildfire*, skippered by William Potter, could bring back adults or artificially fertilised eggs of many species of flatfish, as well as red mullet, hake, gurnard, dorey, and other trawled fish; Johns of the *Prima Donna* could be counted on for pilchard and mackerel eggs and adults, and another mackerel boat, the *Grace*, had been enlisted in the search for pilchard ova.[56] Although he continued to take trips out on trawlers, Cunningham's attention began to be increasingly occupied by how to keep his specimens alive. He could only learn so much from preserved eggs and fish stomachs, and from fish eggs unidentifiable because they only lived for a short time after being brought into the lab and kept in jars. In addition to the challenges of keeping the eggs floating, it was necessary to set up some kind of water circulation system within the jars if the eggs were to be kept alive for any more than a few days. Cunningham began working on a circulation apparatus at the end of April, when Roach brought back fertilised gurnard eggs from his trip on the Irish Sea trawler *Merganser*, coming in on a rare Sunday to try and get one to function, so desperate was he to keep the eggs alive.[57] It was not to be – they were dead by the following Saturday – but Cunningham remained determined to find a way to support fish eggs and fry under laboratory conditions.

In addition to simply wanting not to lose his specimens before he could positively identify them or learn anything new from them, Cunningham had other reasons for wanting very much to keep eggs, young, and adult fish alive under laboratory conditions. One important reason was that, for any given species, there was only a short period of time during the year that eggs could be collected. By May, Roach was reporting that in nearby Mount Bay, all of the soles, the fish about which Cunningham would write an entire book, were "spent" – that is, they had released all of their eggs or milt for the year. Although a trip out on the *Lola* yielded a few more sole eggs, Cunningham could not manage to keep them alive, and had to switch to studying mackerel for the next several

[56] See D1, 14 May – 3 July 1888, [145-180]. Roach and the boy also trawled down the shore on a steamer, the *Lucy*. (D2, 18 June 1889, [98].)

months.[58] The seasonally limited availability of the specimens that Cunningham was most interested in made the need to keep hold of any specimens he did get during that time even more pressing; it also made the prospect of someday collecting vast numbers of eggs from an in-house population of soles who would live, mate, and spawn within the Laboratory's tanks even more attractive.

A second reason was that the Laboratory was preparing for its official opening at the end of June. This was to be a gala event, with prominent scientists from all over Britain being invited, with a large banquet being hosted by the Fishmongers who had footed much of the bill for the Laboratory. Part of the plan for the laboratory was to have a public aquarium, so that, as was the case at Naples, the paying public could view the many species of marine life that were living indoors in tanks.[59] Cunningham was involved in setting up this aquarium, even to the extent, as he recorded in his notebook, of making arrangements on "matters connected with blinds, chairs, plugs, new slate shelves and injector for the aquarium."[60] Beginning in late 1890, Cunningham kept a book of "Aquarium Notes" detailing which species of young and adult fish were added to which tank, and who, from among his supplier network of Cunningham himself, Roach, fishermen, or local boys in search of some small reward, had brought each specimen in. By May 1891, the "tank room" was accessible to the interested public six days a week, and attracting both admission money and public interest to the Laboratory.[61] By the end of the next year, Director W. L. Calderwood reported happily that "Constant additions

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[57] D1, 29 April 1888, [141].
[58] D1, 14 May 1888 ([149-152]) and 21 May 1888 ([152-154].
[59] A list of yearly attendance figures for the old Aquarium still hangs in the front lobby of the Laboratory.
[60] D1, 21 May 1888, [154].
[61] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 2.

are also being made to the aquarium. ... The attendance of visitors keeps up, and when some special animals are on view, and an advertisement has been inserted in the local papers, the money taken at the door often becomes considerable. The aquarium is becoming recognised as one of the sights of Plymouth."[62]

For all of these reasons, the Laboratory tanks needed to be kept stocked with fish – not feeble ones at death's door or frail, shrivelling eggs, but healthy live fish of a variety of species and ages. Cunningham set out to solve the problem most affecting his own embryological work by establishing a system for hatching eggs and raising larvae. Every possible variable had to be considered, even the seawater itself. Expecting a new delivery of fertilised merry sole eggs from the *Lola* and the *Wildfire* and fertilised mackerel eggs from the *Prima Donna*, Cunningham became suspicious of the galvanized metal buckets that Roach had been using to bring in seawater for the hatching jars, and had him collect the water in wooden buckets instead.[63] When the fish eggs arrived developing and in good condition, Cunningham snapped into action. There would be no more humble hatching jars for him, but tried, tested and true system from those world leaders in fish hatching, the Americans: "In the afternoon I went out and ordered the glass cylinders & a washing tray in order to set up the U.S.A. apparatus for keeping pelagic ova, as the...one I had does not answer well."[64] The materials for the Ryder apparatus, as

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[62] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 294.

[63] D1, 21 May 1888, [154-155].

[64] D1, 26 May 1888, [158]. It is difficult to say which apparatus Cunningham knew as the "Ryder apparatus". John A. Ryder of the United States Fish Commission had reported in *Science* in January of 1886 his successful hatching of the cod in an apparatus that he credits to "H. C. Chester, superintendent of the Wood's Holl station, of the commission." This apparatus is illustrated and appears very similar to Cunningham's illustration of the "Chester apparatus" in *Treatise on the Common Sole*, with one key difference: the hatching jars were protected from the incoming and outgoing water flows by two pieces of wood, such that the incoming water travelled down a narrow pathway, swept beneath the hatching jars, and travelled up a narrow passage and out of the siphon. It is possible that this variant of Chester's apparatus

the American system was known, arrived, and Cunningham set up a system of two jars in a wooden washing tray, stocking one with hatched mackerel larvae and the other with merry sole eggs just received from Roach after a trip on the *Wildfire*. The next day, most of the merry soles and all of the mackerel were dead; by the following day none at all remained alive, and Roach brought in another round of fertilised mackerel eggs from Johns to try again, which died in the Ryder apparatus overnight.[65] Discouraged, Cunningham noted that "[t]he merry soles fertilised Tuesday 8.0 p.m. were dying so fast in the Ryder apparatus that I took them out and put those that were living in a jar of water without circulation. The Ryder apparatus is apparently fatal on account of some poisonous stuff taken up either from the white lead used for the joints or from the new wood."[66] Some fruitless weeks followed, in which Cunningham's work space was finally moved from an upstairs room downstairs into the main laboratory space and the deadly Ryder apparatus, admirable though it may have been in America, claimed many more rounds of victims in its Plymouth incarnation.[67] By month's end, with the Laboratory officially open and the "mackerel dying too fast even to sketch,"[68] Cunningham decided to abandon the Ryder apparatus and order some new jars in order to try the Chester hatching method, another American innovation in fish hatching, in hopes that things would improve (Figure 8). In the meantime, he focused over the summer on stocking the various tanks of the aquarium with flatfish, conger eels, and other local fish species.[69]

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could have been considered the "Ryder apparatus" by Cunningham, but not certain. John A. Ryder, "Success in Hatching the Eggs of the Cod," *Science* 153 (1886), 26.

[65] D1, 1 June 1888, [163-164].

[66] D1, 1 June 1888, [163-164].

[67] D1, 20 June 1888, [164-165]. The "Ryder" of apparatus fame was the embryologist John A. Ryder of the United States Fish Commission, an organization that was, as we have seen, widely admired in Britain for its practical successes in hatching and rearing young food fishes.

[68] D1, 4 July 1888, [181].

[69] D1, July 1888, [180-196].

His *Treatise on the Common Sole* was in the works, and he was focused on getting "living soles brought in for breeding next spring, and dead ones for dissection now", sending a large tub down to the trawlers for the purpose of bringing back live fish one at a time, and keeping a table tank in the laboratory reserved for soles.[70]

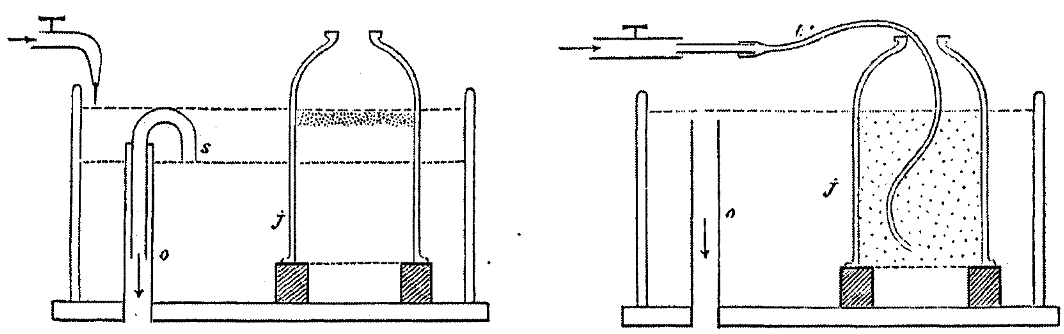

Fig. *E.* Fig. *F.*

Fig. *E.* Diagram of a transverse section of an apparatus for hatching buoyant fish eggs, arranged according to the method devised by Captain Chester, of the United States Fish Commission.

Fig. *F* exhibits the modification of the apparatus adopted in the Laboratory of the Marine Biological Association.

j, The glass hatching jar, resting on two bricks in a small tank containing sea-water; *o*, the overflow tube; *s*, siphon in the overflow tube; *t*, an indiarubber tube conducting the inflowing water into the interior of the hatching jar. The dots represent the eggs.

Figure 8. Cunningham's apparatus for successfully hatching marine fish eggs, derived from the Chester apparatus used by the United States Fish Commission, whose work was so admired in Britain. This was the second Fish Commission apparatus that Cunningham tried, having first lost many embryos to the Ryder apparatus. The difficulties in rearing fish from eggs were many: the apparatus had to be made of inert materials that did not release toxins into the water, and the water had to circulate vigorously enough to keep the contents fresh and oxygenated, but not so much as to carry away or buffet unduly the eggs and young. Cunningham's version of the Chester apparatus was arranged with the water inflowing directly into the hatching chamber, rather than into the tank surrounding the hatching chamber.

By the early spring of 1889, fertilised flatfish eggs were once again flooding into the laboratory at a great rate from his suppliers at sea. Cunningham continued to experience the loss of many of these eggs, whether due to the water circulating too rapidly or not enough or a variety of other factors, but he had successfully established a large hatching system of many jars connected by rubber tubing. Perhaps because of the scaling-up that had taken place, he treated this mortality as somewhat more routine. This

[70] D1, 26 October 1888 ([210]) and 3 November 1888 ([212]).

might, as well, have had something to do with an important change that was taking place in his system of specimen supply. In late February of 1889, he reported "[f]ertiliz[ing] some flounder spawn in the aquarium, from a female brought up from the Primrose & a ripe male wh. I had in stock."[71] The winter spent stocking the aquarium tanks with young and adult fish meant that Cunningham now had an in-house supply of spawning fish. The notebook reference to having a ripe male, that most elusive of specimens when it came to performing artificial fertilization at sea, "in stock" is a very revealing expression. In assembling the Laboratory's aquarium, Cunningham was also assembling the means to have an indoor supply of fish embryo specimens. The specimen supply chain culminating in the fishermen had been, and definitely continued to be, an essential and productive source of fish and egg specimens, but what it had offered in access it had lacked in control. Collecting hundreds of fish eggs without happening upon a single ripe male was an excellent source of fish eggs, but an inefficient way to get fish embryos. It was becoming possible for Cunningham to maintain an inventory of reproductively active males and females of species of interest; with this population at his fingertips, he could collect a huge proportion of their eggs and milt and combine them himself, or he could allow them to combine naturally and then skim the floating embryos from the tops of the tanks when it was time to isolate, hatch, and study them. The supply of live adult fish, which though still tricky were far easier to maintain than developing embryos and fry, came to rival the supply of eggs at times when Cunningham had to assign priorities to his suppliers in the fishing community. "Roach says the Merganser is in Cattewater:," observed Cunningham in the spring of 1889, "but I think I shall not go out next week, as I

[71] D2, 26 February 1889, [15].

have sole's eggs here, but get them to bring in live spawning soles."[72] Live spawning soles brought into the laboratory were even more valuable than eggs: they could give all their eggs this year and perhaps for some years afterward to Cunningham's cause, especially if the eggs could be combined with the milt from an "in stock" breeding male, and the newly fertilised eggs transferred immediately into Cunningham's hatching system, rather than enduring two or three days in a closed jar aboard a trawler in uncertain weather.

The in-house supply system started to pay off impressively in the spring of 1890, when Cunningham received a report that there were eggs floating on top of Tank Number Three. "I went to the Laboratory & found as I expected that they were plaice eggs," he wrote in his notebook on March 15, 1890. He skimmed the plaice eggs off of the surface of the water and "distributed [them] in different apparatuses in the aquarium" – table tanks, jars, inverted jars, Weldon's personal tank, a shallow tub, a wooden tank, and let some remain in Tank Number Three.[73] Inconveniently (not to mention ironically), a planned trip to sea on the *Merganser* the next day to get sole eggs would hinder his embryo-collecting work in the laboratory just as the hatching was beginning, so he "arranged with [then-Director] Bourne to turn the hatched larvae into tanks in order to rear them to the perfect condition."[74] A laboratory in which naturally or artificially fertilised eggs could be collected in massive amounts from tanks containing breeding adults, then transferred to hatching facilities, and from there to larvae tanks where they would be, of course with some losses along the way, reared "to the perfect condition" for study – and even if this situation only happened occasionally, it was still enough to make

[72] D2, 30 March 1889, [41].
[73] D2, 15 March 1890, [109].

Cunningham speak much more confidently in his notes – was a very different place than one in which round after round of eggs were brought in from the sea to languish and die in hatching jars. The establishment of a system of tanks for adults, eggs, and larvae gave Cunningham vastly more access to, and control over, his specimens when it came to collection, maintenance, and study. More than that, establishing successful hatching and maintenance systems for fish within the laboratory was a first step towards raising fish in large numbers to be released into the sea for trawlers to catch, which if it could be achieved would constitute a major contribution of marine biology to the nation's fisheries. Cunningham's account of hatching the eggs of the sole, a fish of very high market value, are presented in the "Economical" section of his *Treatise on the Common Sole* and described as "my own experiments on the artificial propagation of the species." Cunningham was motivated by the practical need to keep fertilized fish eggs, embryos and larvae alive long enough to positively identify them and trace their development, but there was another more generally practical motivation as well. As he explained in the *Treatise*, "[t]he commonest and most obvious method of attempting to increase the supply of a valuable fish is to hatch its eggs. The young when hatched may be…kept in captivity and regularly supplied with food until large enough to be valuable, or they may be set free in the natural haunts of the species so as to replenish its numbers."[75] Hatching and raising fish indoors on a small scale sustained Cunningham's work on food fish development; hatching and raising fish indoors on a much larger scale held the promise of sustaining Britain's active commercial fisheries.

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[74] D2, 23 March 1890, [113].
[75] Cunningham, *Treatise on the Common Sole*, 129.

V. Accidental Hybrids and Experimental Flounders

By 1891, though he continued to go on trawler trips and receive numerous specimens from his network of fishermen, Cunningham was also harvesting eggs, particularly plaice eggs, from the laboratory tanks for study. It was during one of these harvests that Cunningham made an error that led him down an unexpected pathway of investigation into fish embryology. On the afternoon of March 30, 1891, Cunningham "took out some plaice for artificial fertilization."[76] He had one ripe male and one ripe female plaice in stock, and just squeezed each so their eggs and milt escaped into a jar, then went away for an hour, returned and transferred the jar contents into a hatching box, *tout simplement*. There was some plaice milt left over, however, and seeing "another female fish which was enormously distended with ova," Cunningham "squeezed her into a bottle of water containing plaice milt. A very large quantity of ova flowed from her, but after I had returned her to the tank I found she was a flounder."[77] As well, many of her eggs appeared dead, but since the mistake had been made anyway, Cunningham "left them in the bottle to see if any were alive and would fertilize with the plaice milt."[78] Two days later, Cunningham recorded that "[t]his morning I found some of the hybrid flounder ova were alive & segmenting".[79] Moved into a clean jar, the hybrids survived and were found to be "developing apparently normally"[80] for several days, and when one hatched Cunningham attempted to sketch it, making note of its yellow pigmentation. In the published report of these hybrids, Cunningham gave a slightly different account of his actions:

[76] D2, 30 March 1891, [157].
[77] D2, 30 March 1891, [157-158].
[78] D2, 30 March 1891, [158].
[79] D2, 1 April 1891, [158].

> In the same tank were two ripe female flounders, but no males of the same species. I squeezed a large number of eggs from these, and made the experiment of mixing them with milt from a male plaice. Fertilization occurred in a certain number of the ova, about half, and a few of these lived till they were hatched, and the larvae lived several days. They died, however, like all my larvae, soon after the absorption of the yolk-sac.[81]

Interestingly, Cunningham painted the creation of hybrid plaice/flounders as an intentional experiment rather than the accident it was – yet offered really nothing by way of interpreting the results of this "experiment". They were no different from the rest of his laboratory-dwelling larvae, at least, in that they died young. In the very same spring in St. Andrews, M'Intosh examined embryos and larvae of hybrids of two other easily confused flat fish species, the turbot and brill. In a notebook entry titled "Ova of Brill 6th May 91," M'Intosh recorded that on the second of May he had received "ovoid spat from of [sic] B. fertilized by milt of Turbot."[82] These embryos hatched, and M'Intosh reported with some excitement that "The larval Brill…fertil. with the milt of turbot differs from anything seen in St A. Bay in regard to the remarkable develop^t of pig^t if Prof. Prince's fig. is correct."[83]

These two instances of hybrid larvae – one a mistake arising from a routine artificial fertilisation taking place within the laboratory, one seemingly having been done during an at-sea artificial fertilisation, but brought along to M'Intosh anyway in case they might be of interest – have some interesting features. For one thing, although both Cunningham and M'Intosh were very interested in the development and appearance of

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80 D2, 2 April 1891, [159].
81 J. T. Cunningham, "Breeding of Fish in the Aquarium," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 195.
82 NB, 6 May 1891, 30.
83 D2, 6 May 1891, [31].

the hybrid embryos and the larvae that hatched, neither man's notebooks gives any evidence that any further hybrids were created for study, despite the fact that in Cunningham's laboratory at least it would have been as simple as selecting an in-stock ripe male plaice and an in-stock ripe female flounder. I can only venture that neither man was interested in creating, or wanted to be seen as interested in creating, hybrid flatfish embryos to study when so much basic biology of naturally-occurring flatfish species still remained to be done. We saw in Chapter Two how very closely Cunningham's activities were monitored, and M'Intosh's prominent position in Scottish fisheries science, as well as his prominence as the Professor of Natural History in a small-town, high-status university would have guaranteed that none of his research went unnoticed, either. However, that neither man pursued this line of inquiry any further is still very strange, since the only thing that would have distinguished later attempts from the original cases would be that the later hybrids would have been created intentionally – but not through any procedure more manipulative or sinister than the mixing water containing the eggs of one species with water containing the milt of another, a process that must go on every year in the sea anyway. Indeed, the very possibility of naturally-occurring hybrids, in embryo and larval form at least if not as adults, raises a degree of uncertainty concerning the species identities of the eggs, larvae, and young that the naturalists and their allies collected in such numbers for study in the laboratory. Perhaps the prospect of undetected hybrids in the sea, when distinguishing one egg or embryo from another was already such a challenge to naturalists, was too unsettling. The control over the parentage of a particular group of eggs-embryos-larvae afforded by an in-house system like

Cunningham's could only become more attractive with this knowledge of new uncertainties outside of the laboratory door.

The plaice and flounders whose eggs Cunningham was able to collect from the tanks were hardy estuarine species, able to tolerate a range of water conditions and live in laboratory tanks without too much trouble. This feature made flounders in particular a likely species when Cunningham sought a subject for experimentation. His studies of fish growth, size, and age (see Chapters Four and Six) were only possible if he could be absolutely certain of the age of the fish, which in turn was only possible if he had the means to keep the fish under his control, living in the laboratory tanks, all the way from at least larvahood to adulthood; by 1891, Cunningham could manage this feat with flounders.

In addition to having the means to keep flounders alive in tanks for a considerable length of time, Cunningham had a ready source of young flounders in his ally Matthias Dunn, who sent him batches of young flounders periodically from the spring of 1889 onward. These young flounders had metamorphosed – one eye had migrated around the head to join the other, making them recognisably flatfish – but were only beginning to develop the characteristic adult pigmentation, where colour is found only on the same side as the eyes, leaving the bottom ("blind") side unpigmented. Cunningham began a study, on the influence of available light on pigmentation in flounders (on which much more in Chapter Four), that lasted for some years. In a preliminary attempt, Cunningham took the several flatfish (which he initially mistook for plaice) that Dunn had sent up from Megavissey, "drew one and placed the rest in a glass pan so that light was equally strong above and below them."[84] Only a few days later, he was wondering if the light might be having an effect: one of the fish had some pigment on the underside, near the tail. "This may have developed in consequence of the light admitted from below the last two days: but as the larva when hatched is equally pigmented on both sides, it is probably the remains of the original pigment. If so it shows that the depigmentation of the left side has already begun,"[85] noted Cunningham. Eleven days later, even his cautious excitement had faded: "I spent some time in arranging the dish containing the young flat fish sent up by Mr. Dunn: they live well and feed well but show no great pigmentation on the blind side, no more than they had when first brought up."[86] All but one had died of starvation less than a month later, but Cunningham was ready to try a new tack: "I have put a wooden cover charred inside over the vessel containing them so that the only light they get is from the bottom. I put in some pounded shrimps to feed the one. The dead ones had a few pigment cells both black & coloured scattered on the underside, but very few."[87] Cunningham's generosity with the shrimps did in the remaining fish, but the next week he travelled down to Megavissey to collect young flatfish with Dunn in an especially low tide.[88] In Megavissey Harbour he found a treasure trove:

> The old harbour...is quite empty of water at low tides in spring, and was empty when I saw it. A little fresh water runs down it, and numbers of pools and channels are left when the tide has receded[.] These pools and channels were all swarming with the said flat-fish. There were millions of them.[...] A number of the young Pleuronectes, some of them still not completely metamorphosed, swimming on edge with one eye on each side, but the left

[84] D2, 3 April 1889, [58].
[85] D2, 5 April 1889, [59].
[86] D2, 16 April 1889, [69].
[87] D2, 7 May 1889, [88].
[88] D2, 15 May 1889, [89-90].

> eye had begun to move towards the dorsal edge. The fish in this stage were very slightly pigmented, and were chiefly distinguished by the bright pigmented eyes which were conspicuous in the sunlight.[89]

Cunningham's amateur naturalist Dunn had led him to an enormous stockroom of young flounders, not to mention a number of young soles which Cunningham snapped up. When Cunningham returned to Plymouth the next day, Dunn had sent along even more young flounders and soles by rail. Confident in his supply of specimens, and placing the local boys on notice that he was looking for transparent young flatfishes, Cunningham took up his pigmentation experiments in earnest, placing "the unmetamorphosed flatfish which I brought yesterday [and] which Dunn sent to day in the apparatus downstairs illuminated only from below to see if I could develop pigment on the under surface. I placed others in a bottle under ordinary conditions for comparison."[90]

While the flounders were spending their time in the half-light, Cunningham was also at work doing experiments with laboratory tank-dwelling fish of the species with which his name was becoming most associated, the sole. In May of 1890 he initiated a series of "Experiments to find if Sole can live in Fresh Water" by simply transferring a sole from a laboratory tank into a tub of fresh water. The sole was dead by noon the next day, and Cunningham decided to take a more gradual approach. Using a half-grown sole (thanks to the Megavissey Harbour stockroom, young soles were not the precious commodity they had once been) in a laboratory tank, Cunningham measured the density, and then began reducing the density by several units each day by adding fresh water. By the second of June, the sole was living in a tank of entirely fresh water, and did so until

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[89] D2, 15 May 1889, [89-90].
[90] D2, 16 May 1889, [91].

its death eleven days later, at which point it had been refusing food. Another run of this experiment in November of 1890 had a sole living in fresh water for fifteen days.

Two factors allowed Cunningham's experiments with flounder colouration and sole salinity tolerance to happen at all: access and control. Experiments involve manipulation, and in order to manipulate and observe the results of this manipulation in individual fish, it has to be possible to maintain them in isolation, out of their natural habitat and within the view of the experimenter. And experiments involve casualties: specimens can be lost in failed experiments, and even successful experiments require repeated runs, so the experimenter needs to have access to a plentiful supply of specimen-subjects. Because of his position as Chief Naturalist at the Plymouth laboratory, Cunningham was as well placed as anyone in Britain to obtain the specimen-subjects he needed, and to maintain them in appropriately controlled conditions so that experimentation could take place at all. Because of his network of specimen suppliers, and particularly in this case his access, through Matthias Dunn, to a massive inventory of young flounders and soles, as well as because of his role at the Laboratory in stocking and maintaining the public aquarium as well as his own private fish stocks in a variety of tanks, jars, and boxes, Cunningham was able to build up the structures and supplies necessary for him to work sometimes as an experimental fisheries biologist. The cast of characters involved in making this possible is a vast and diverse one, ranging from Cunningham himself, to his assistant Roach, to Matthias Dunn and to all of Cunningham's allies among the Plymouth fishing community, who we met in this chapter; to the Marine Biological Association and its administrators, who gave Cunningham his status and facilities and in turn exercised some control over what he could do, as we saw in Chapter Two; and to the organisms themselves, the eggs and larvae and adults of a vast range of species who not only needed to be present and living in order for Cunningham's work to take place, but whose importance to the national economy and food supply had given the impetus for a paid position as Chief Naturalist in the first place, and who will be the focus of the next chapter.

Chapter 4

"No artificial conditions except captivity": Cunningham's Aquarium Flounders and the Conditions of Life

Introduction

J. T. Cunningham's most significant contribution to what was known about the biology of flat fishes was his demonstration, in several publications during the 1890s, of what he privately called "the effect". If flounders, normally pigmented on their up-facing sides and unpigmented on their lower sides, were kept in a tank set up such that light was able to shine up through the bottom of the tank, the flounders would develop pigmented areas on their lower sides. Cunningham reported this discovery in several different places – indeed, it was the appearance of preliminary results in the German journal *Zoologischer Anzeiger* that prompted the reprimand by the Marine Biological Association Council discussed in a preceding chapter. Largely experimental in its methods, largely environmental in its assumptions, and largely evolutionary in its implications, the seemingly minor discovery of "the effect" had major implications for the several large biological questions in which flat fish had for some time been playing a role. The study was undertaken to establish the influence of what was known generally as "the conditions of life" in the evolution of species and the development of individuals; its presentation and reception in published form also raised such issues as the importance of individual variation, the usefulness of abnormal or monstrous specimens as evidence, and the significance of symmetry and asymmetry in animal development and evolution. Cunningham undertook this work while he was working as Naturalist at the Plymouth Laboratory of the Marine Biological Association, and it was through one of the supply chains discussed in the previous chapter that he obtained the flounder populations that served as his experimental and control groups. As we have seen, Cunningham's mandate as Naturalist was to study questions of practical importance to the fisheries. His experimental demonstration of "the effect" hardly fell into that category, but the work was in fact another example of the double-tasking that so characterized Cunningham's time at the MBA Laboratory: the brood of young flounders used in the colouration experiments were simultaneously taking part in Cunningham's study of age, size, and sexual maturity in flatfish. This fish growth study, which had clear practical implications in light of the fishing industry's demands for minimum size limits on trawled fish, was published in many parts and at great length in the *Journal of the Marine Biological Association*. In these twin studies of colouration and of growth in flounders collected as a population from tide pools in the wild and raised as a population in the laboratory tanks, Cunningham grappled with the notion of "the conditions of life" in its several forms: as an evolutionary and developmental influence; as a determinant of the size at which fish should be thrown back in order to protect fish stocks and the industry they sustained; and, at least as importantly, as a source of epistemological anxiety about what was natural about the necessarily contrived conditions in which his studies took place, and what could be confidently known about fish in the sea based on what was discovered about fish in the tanks.

Flatfish Evolution and the Conditions of Life

In an article on "The Evolution of Flat-Fishes" in the May 1892 issue of the popular journal *Natural Science*, Cunningham began by setting out what he saw as the chief "structural peculiarities of the Flat-fishes."[1] "All we have to do is to compare them carefully with various symmetrical fishes," he suggested, to notice that flat fishes had both eyes on their upper side; no pigment on their lower side; a dorsal fin extended forward and marking the separation of the eyed and blind sides of the head; and in some species, a "greater development of the jaws and teeth on the lower side."[2] All of this was the sober morphological way of saying that flat fish are very strange-looking creatures: strikingly asymmetrical, with an abnormal twisted head and two eyes pointed upwards, looking like one of nature's monsters, a fish gone horribly awry. That such a creature should have attracted the attention of zoologists long before Cunningham arrived at Plymouth is not at all surprising. Indeed, the curious appearance of flat fish had been seen for some time as a phenomenon in need of explanation, and in particular in need of evolutionary explanation.

William Yarrell, the British naturalist responsible for the classic 1836 manual *A History of British Fishes*, was seemingly not especially disturbed by the oddness of the major features of the flatfish. Taking a sanguine view of the decidedly strange Pleuronectidae, which included plaice, flounders, dabs, pole, "holibut", turbot, brill, topknots, whiff, scaldfish, and soles, among others, Yarrell reflected that "various and seemingly obvious anomalies are perfectly in harmony with that station in nature which an animal bearing these attributes is appointed to fill."[3] He even went so far as to use the word "beautiful" – not about the flatfish themselves, exactly, but to say that their very strangeness was "a beautiful instance of the design employed for the preservation of

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[1] J. T. Cunningham, "The Evolution of Flat-fishes," *Natural Science* (1892) 1: 191. He re-used this description to begin the section on "The Flat-Fish Family" in his later popular book *The Natural History of the Marketable Marine Fishes of the British Islands* (London: Macmillan and Co., 1896), 208.
[2] Cunningham, "Evolution of Flat-fishes," 191.

species."[4] They did not look like typical fish, certainly, but their differences made them better suited to their particular conditions of life: hiding on the bottom of the sea, their flatness made them hard to spot, and their two eyes faced away from the bottom and up towards the sea above them.

Yarrell also noticed another unusual feature of the Pleuronectidae that continued to be important during and after Cunningham's time: besides being abnormal with respect to other species of fish, individual flatfish were often found that were abnormal with respect to their conspecifics. What he called "varieties" of flatfish were a relatively common occurrence; the flounder in particular was known for its inconstancy. "Varieties of the Flounder," noticed Yarrell, "occur much more commonly than those of any other species of flatfish."[5] *A History of British Fishes* includes descriptions of several abnormal flounders, as well as other flatfish varieties: reversed[6] turbots and soles, and an illustration of a brill with a malformed head.[7] Flatfish, and flounders especially, were thus doubly abnormal. The two salient facts about the group were that with respect to the rest of the fishes, they were squashed and contorted and asymmetrical, and that with

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[3] William Yarrell, *A History of British Fishes*, (London: John van Voorst, 1836), vol II, 217.
[4] Yarrell, 216.
[5] Yarrell, 217.
[6] That is, wrong side up with respect to the majority of individuals of the species. Turbots are "sinistral" fishes: their upper side, with eyes and pigment, is their left side, so a reversed turbot would have eyes and pigment on the upward-facing right side. Reversed flounders (with the left rather than the right side up, as flounders are considered a dextral fish) were very common, and Cunningham tells a story in *Marketable Marine Fishes* to illustrate this fact: "Once, on board a trawler, between the [Plymouth] Eddystone and the land, I noticed that there were almost as many flounders left-sided as right-sided among a large number caught. The crew of the boat asserted that the left-sided were males and the right-sided females; but I found that each kind included both sexes." (Cunningham, *Marketable Marine Fishes*, 231.) Cunningham had a few years previously solved another false distinction among the flatfish, establishing that what were thought to be two different species, named *Arnoglossus lophotes* and *Arnoglossus laterna* by Albert Günther of the British Museum (Günther, "A Contribution to our Knowledge of British Pleuronectidae," *Proceedings of the Zoological Society of London* (1890) and references therein), were in fact the males and females respectively of "a single sexually dimorphic species." (Cunningham, "On Secondary Sexual Characters in the Genus *Arnoglossus*," *Proceedings of the Zoological Society of London* (1890): 541.
[7] Yarrell, 217 (flounders), 238 (turbot), 258 (soles), and 242 (brill).

respect to the standard description of a given flatfish species, individuals frequently occurred that failed to match the description. They were an intriguing bunch.

Yarrell's description of the flatfish's strange set of features as perfectly fitting them for their particular conditions of life is interesting, given that it was both preceded by a couple of decades *and* followed by a couple of decades by the inverse view: to wit, that the flatfish's strange set of features had been shaped *by* their particular conditions of life. The evolutionary view that preceded Yarrell's book was presented in 1809 as one of many examples in Jean Baptiste de Lamarck's treatise *Philosophie Zoologique*. In offering an explanation of the "influence des circonstances" in shaping the bodies of organisms, Lamarck devoted over a page to the flatfish, whose shallow-water habits forced them to swim on their sides.

> In this position, they receive more light from above and below and stand in special need of paying constant attention to what is passing above them; this requirement has forced one of their eyes to undergo a sort of displacement, and to assume the very remarkable position found in the soles, turbots, dabs, etc. (*Pleuronectes* and *Achirus*).[8]

This transformation reached its symmetrical perfection in the skates and rays, Lamarck argued, and the flatfish's odd appearance was that way because the transformation from symmetrical round fish to symmetrical flat skate was at an intermediate stage. Through the process of morphological change through use and disuse, dictated by the organism's

[8] J. B. Lamarck, *Zoological Philosophy*, trans. Hugh Eliot. (London: Macmillan and Co., 1914), 120. The original passage reads, "Dans cette situation, recevant plus de lumière en dessus qu'en dessous et ayant un besoin particulier d'être toujours attentifs à ce qui se trouve au-dessus d'eux, ce besoin a forcé un de leurs yeux de subir une espèce de déplacement et de prendre la situation très-singulière que l'on connaît aux yeux des *soles*, des *turbots*, des *limandes*, etc. (des pleuronectes et des achires)." J. B. Lamarck, *Philosophie Zoologique*, (Paris: Librarie F. Savy, 1873. Vol. 1), 251.

habits and conditions of life on the seafloor, the flatfish had been molded into its present curious and unattractive state that nonetheless so well suited the way it lived.

Decades after Lamarck and Yarrell, Herbert Spencer advanced a somewhat more detailed account of how the odd features of flatfishes might have been shaped by the setting in which they found themselves. In his *Principles of Biology*, Spencer devoted much discussion to the topic of the serial repetition of parts and the relations of symmetry in organisms.[9] Discussing symmetry, Spencer came to the issue of asymmetrical animals, and provided an evolutionary explanation of his own. Where Lamarck had argued that the conditions of life shaped the features of the flatfish as a whole, Spencer broke things down somewhat further, suggesting that the shape and planes of symmetry of animals is related to the way in which they position their bodies while moving through their environment. Conceiving of this environment as in fact a set of differing local environments, he predicted that when "the habits are such that the intercourse between the organism and its environment, does not involve an average equality of actions and reactions on any two or more sides, there may be expected either total irregularity or some divergence from regularity."[10] The flatfish were a "significant piece of evidence"[11] that this prediction held for lower animals, like the gastropods, and higher animals alike. Instead of considering the flatfish as a single unit experiencing a single set of external

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[9] Herbert Spencer, *The Principles of Biology*, (London: Williams and Norgate, 1867, Vol. 2). For more discussion of Spencer's views on parts and wholes, see James Elwick, *Compound Individuality in Victorian Biology, 1830-1872*. (Ph.D. thesis, University of Toronto, 2004). Spencer developed a very interesting account of variation that was also marked by his interest in what might be called microclimates. Innate variation existed among individuals, but this variation was greatly amplified by minute differences in their conditions of life: "The members of a species inhabiting any area, cannot be subject to like aggregates of forces over the whole of that area. And if, in different parts of the area, different kinds or amounts or combinations of forces act on them, they cannot but become different in themselves and in their progeny." (Spencer, *The Principles of Biology*, (London: Williams and Norgate, 1864, Vol. 1), 271.)

[10] Spencer, *Principles of Biology*, Vol. 2, 168.

[11] Spencer, *Principles of Biology*, Vol. 2, 188.

conditions, Spencer posited that the significant differences between the upper and lower sides of the Pleuronectidae were due to the influence of a different set of environmental conditions acting on each side and influencing the evolutionary outcome. The flatfishes must, "on the hypothesis of evolution…have arisen from an ordinary bilaterally-symmetrical type of fish, which, feeding at the bottom of the sea, gained some advantage by placing itself with one of its sides downwards, instead of maintaining the vertical attitude."[12] The two well-known features of flatfish development – that the larvae began as symmetrical fishes, and that abnormal specimens of flounders in particular were commonly observed – Spencer considered to be evidence of this evolutionary history. Certainly it was not difficult to see how different the two sets of conditions experienced by the flounder's two sides were: on the lower side, the flatfish experienced a lack of light, and friction resulting from its contact with the sea bottom, while on the upper side, the flatfish was in direct contact with water and light. Spencer's view of the effects of the conditions of life on the twisted morphology of the flounder shared with Lamarck's use-and-disuse view an emphasis on how the conditions of life shaped organisms. Spencer differed greatly, however, in his assessment of how very local these relevant conditions of life might be, as well as in taking a very different view of organisms, less as striving beings and more as aggregates of matter molded by physical forces. If symmetry and serial repetition were due to the regular experience of a consistent set of external forces by the several sides of an organism, then asymmetry, especially asymmetry as pronounced as that found in the flat fishes, was due to the experience of two distinct sets of conditions of life on two distinct sides of the organism.

This suggestion of Spencer's came in for criticism in 1871 with the publication of

[12] Spencer, *Principles of Biology*, Vol. 2, 188.

St. George Mivart's *On the Genesis of Species*, a work better known for its sustained and effective criticism of Charles Darwin's theory of evolution by natural selection, particularly in its suggestion that natural selection was an ineffective mechanism to explain the evolution of incipient structures. The Pleuronectidae make a couple of appearances in Mivart's book, and their strange, twisted heads and upward-facing eyes are the subject of two very similar illustrations, one deployed against Darwin and another later on in the book used in criticising Spencer. Spencer "explains the very general absence of symmetry between the dorsal and ventral surfaces of animals by the different conditions to which these two surfaces are respectively exposed, and in the same way he explains the asymmetry of the flat fishes (*Pleuronectidae*), of snails, etc.,"[13] but Mivart was not impressed:

> Abundant instances are brought forward by him of admirable adaptation of structure to circumstances, but as to the immense majority of these it is very difficult, if not impossible, to see *how* external conditions can have produced, or even tended to have produced them. For example, we may take the migration of one eye of the sole to the other side of its head. What is there here either in the darkness, or the friction, or in any other conceivable external cause, to have produced the first beginning of such an unprecedented displacement of the eye? Mr. Spencer has beautifully illustrated that correlation which all must admit to exist between the forms of organisms and their surrounding external conditions, but by no means proved that the latter are *the cause* of the former.[14]

What is to be explained in the case of the flatfish is not simply the disappearance of the eye from the lower side – Spencer's theory could perhaps account for that much – but the "unprecedented displacement" that saw the lower eye relocate to the upper surface next

[13] St. George Mivart, *On the Genesis of Species*, (New York: D. Appleton, 1871), 179.
[14] Mivart, 179-180.

to the upper eye. The external conditions particular to the lower surface of the flatfish, Mivart suggests, perhaps correlate with the absence of the eye, but that is a long way from having caused the absence, much less having caused the displacement.

The lack of a plausible evolutionary explanation of the cause of the lower eye's migration to the upper surface is a major argument in Mivart's criticism of Darwin's natural selection, and one to which Darwin responded at some length in the 1872 sixth edition of the *Origin of Species*. At the end of Mivart's chapter on "Incipient Structures" – the chapter whose criticisms Darwin took quite seriously – Mivart gives a general statement of his problems with natural selection as a mechanism of change:

> The author of this book can say that, though by no means disposed originally to dissent from the theory of "Natural Selection," if only its difficulties could be solved, he has found each successive year that deeper consideration and more careful examination have more and more brought home to him the inadequacy of Mr. Darwin's theory to account for the preservation and intensification of incipient, specific, and generic characters. That minute, fortuitous, and indefinite variations could have brought about such special forms and modifications as have been enumerated in this chapter, seems to contradict not imagination, but reason.[15]

The twisted, two-eyes-up heads of the flat fishes were a striking example of the "special forms and modifications" that Mivart had in mind, made the more striking by the observation that the head does not have this strange appearance throughout the fishes' lives, but instead becomes that way during a metamorphosis undergone by the larval fish: "In all these fishes the two eyes, which in the young are situated as usual one on each side, come to be placed, in the adult, both on the same side of the head."[16] Mivart grants

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[15] Mivart, 74.
[16] Mivart, 49.

that in the unlikely event that this odd modification had "appeared at once, if in the hypothetically fortunate common ancestor of these fishes an eye had suddenly become thus transferred",[17] then perhaps it could be passed on through natural selection; for it to have evolved gradually, as Darwin would have it, is impossible to fathom: "if the transit was gradual, then how such transit of one eye a minute fraction of the journey toward the other side of the head could benefit the individual is indeed far from clear. It seems, even, that such an incipient transformation must rather have been injurious."[18] If the variations leading to the displacement of the lower eye to the upper surface were supposed to be "minute", then it was hardly possible that they were also "fortuitous" or beneficial until the displacement was entirely or nearly complete, and if the variations weren't beneficial, then it was hard to see why the gradual movement would have kept going.

Darwin's response to Mivart's flatfish example, in a new seventh chapter called "Miscellaneous Objections" added to the sixth edition (first published in 1872) of the *Origin of Species*,[19] was several times longer than Mivart's objection of the previous year. Darwin, engaging in his usual avalanche-of-evidence approach, made note of the two significant features of the flatfishes: they "are remarkable for their asymmetrical bodies" which develop during larval metamorphosis, and among typically dextral or sinistral species, "occasionally reversed adult specimens occur."[20] During this development, the eye migrates, and it is

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[17] Mivart, 50.
[18] Mivart, 50.
[19] Charles Darwin, *The origin of species by means of natural selection, or, The preservation of favoured races in the struggle for life; with additions and corrections from sixth and last English edition,* (New York: D. Appleton, 1927.)
[20] Darwin, *Origin*, 290.

> obvious that unless the lower eye did thus travel round, it could not be used by the fish whilst lying in its habitual position on one side. The lower eye would, also, have been liable to be abraded by the sandy bottom. That the Pleuronectidæ are admirably adapted by their flattened and asymmetrical structure for their habits of life, is manifest from several species, such as soles, flounders, &c., being extremely common. The chief advantages thus gained seem to be protection from their enemies, and facility for feeding on the ground.[21]

However, Darwin argues at some length, there is no reason to believe that the transformation from ancestral to present-day flatfish morphology could not have been gradual. Even the existing range of flatfish species show different degrees of eye migration. Moreover, Darwin presents Malm's 1867 studies of flatfish development to show evidence of the evolutionary history of the group:

> The Pleuronectidæ, whilst very young and still symmetrical, with their eyes standing on opposite sides of the head, cannot long retain a vertical position, owing to the excessive depth of their bodies, the small size of their lateral fins, and to their being destitute of a swimbladder. Hence soon growing tired, they fall to the bottom on one side. Whilst thus at rest they often twist, as Malm observed, the lower eye upwards, to see above them; and they do this so vigorously that the eye is pressed hard against the upper part of the orbit. The forehead between the eyes consequently becomes, as could be plainly seen, temporarily contracted in breadth. On one occasion Malm saw a young fish raise and depress the lower eye through an angular distance of about seventy degrees.[22]

Even in fishes destined to be symmetrical as adults, individuals observed to spend time lying on their sides as larvae produce distortions in the bones of their flexible skulls as they strain their eyes to look away from the bottom. These distortions go away as the fish mature and pick up the habits of adult symmetrical fishes; "[w]ith the Pleuronectidæ, on

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[21] Darwin, *Origin*, 290.

the other hand, the older they grow the more habitually they rest on one side, owing to the increasing flatness of their bodies, and a permanent effect is thus produced on the form of the head, and on the position of the eyes. Judging from analogy, the tendency to distortion would no doubt be increased through the principle of inheritance."[23] The use of the eye on the upper surface and the fishes' efforts to look upward with both eyes can account for the strange-looking upper side of the fish, and "[d]isuse, on the other hand, will account for the less developed condition of the whole inferior half of the body, including the lateral fins."[24] Much of what Mivart claimed that natural selection could not explain, Darwin put down to use and disuse. The immediate conditions of life specific to the lower side of the flatfish are proposed to account for the lack of colouration on the lower surface: "From the colourless state of the ventral surface of most fishes and of many other animals, we may reasonably suppose that the absence of colour in flat-fish on the side, whether it be the right or left, which is undermost, is due to the exclusion of light." The flatfishes' colouration is in fact the only aspect for which Darwin maintains a role for natural selection:

> it cannot be supposed that the peculiar speckled appearance of the upper side of the sole, so like the sandy bed of the sea, or the power in some species, as recently shown by Pouchet, of changing their colour in accordance with the surrounding surface, or the presence of bony tubercles on the upper side of the turbot, are due to the action of the light. Here natural selection has probably come into play, as well as in adapting the general shape of the body of these fishes, and many other peculiarities, to their habits of life.[25]

The case of flatfish as a whole becomes for Darwin an example of the way in which two

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[22] Darwin, *Origin*, 291.
[23] Darwin, *Origin*, 292.
[24] Darwin, *Origin*, 293.

key mechanisms of change, use/disuse and natural selection, work together to generate evolutionary change, such that "[f]or all spontaneous variations in the right direction will thus be preserved; as will those individuals which inherit in the highest degree the effects of the increased and beneficial use of any part. How much to attribute in each particular case to the effects of use, and how much to natural selection, it seems impossible to decide."[26] Earlier editions of the *Origin of Species* had contained no mention of the Pleuronectidae; through the ideas of Lamarck and Spencer, and the criticisms of Mivart, the flatfish emerged from the sixth edition as a prime example of the power of the immediate conditions of life to mould the behaviour, and in turn the evolutionary pathway, of organisms and the species to which they belonged.

The second aspect of flatfishes' abnormality attracted Darwin's attention as well. In *The Variation of Animals and Plants Under Domestication*, Darwin made note of the marked variability of flatfish, and the frequency with which abnormal individuals, and in particular "reversed" specimens, were observed, especially in the flounder. Darwin saw in flatfish the possibility of "latent characters", those which "apparently exist in a latent state, ready to be evolved under certain conditions"[27] – that is, characters that are dependent upon particular conditions of life being present in order to become apparent.

> Many animals have the right and left sides of their body unequally developed: this is well known to be the case with flat-fish...In most flat-fishes the left is the blind side, but in some it is the right; though in both cases "wrong

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[25] Darwin, *Origin*, 293-294.
[26] Darwin, *Origin*, 294.
[27] Charles Darwin, *The Variation of Animals and Plants Under Domestication*, (London: John Murray, 1868. Vol. 2), 51. Just how sensitive to particular changes in conditions of life variability might be was uncertain. Darwin believed that "slight changes of treatment" like the simple act of confinement – a first step toward domestication – might induce variability; he cited an Indian study showing that "several species of fresh-water fish are only so far treated artificially, that they are reared in great tanks; but this small change is sufficient to induce much variability." (Darwin, *Variation*, vol. 2, 259) As we will see shortly, how far rearing fish in tanks could be considered as artificial was far from a settled question.

> fishes," which are developed in a reversed manner to what is usual, occasionally occur, and in *Platessa flesus* the right or left side is indifferently developed, the one as often as the other.[28]

Flatfish, and flounders especially, were a potentially hugely variable group; moreover, their variability was such that the characters they expressed had much to do with the immediate environs in which a particular individual found itself during development. The group thus embodied, and not only to Darwin, a nexus of evolution, development, and the environment in which these processes took place.

Flatfish Development: Accounting for Asymmetry

The possibility of a long-term transformation from a symmetrical ancestor to the strikingly asymmetrical present-day flatfish species was, we have seen, intriguing to naturalists of an evolutionary persuasion. The fact, then, of a short-term transformation from a symmetrical embryo to a strikingly asymmetrical adult flatfish could not fail to attract the attention of naturalists interested in development. The attention of those naturalists in the latter decades of the nineteenth century focused in particular on two aspects of development. One major focus of study and disagreement was the development of the torqued flatfish head and the corresponding transit of the eye. The distinctive head morphology was the defining character of the flatfish family. In William Carmichael M'Intosh and Arthur Thomas Masterman's 1897 work *The Life-Histories of the British Marine Food-Fishes*, the lengthy chapter covering the pleuronectids begins with a short statement diagnostic of the whole group before going on to discuss each

[28] Darwin, *Variation*, Vol. 2, 53.

flatfish species in detail: "ACANTHINI PLEURONECTOIDEI. The head is asymmetrical."[29] It is therefore not surprising that a fair amount of effort went in to determining what happened during the larval metamorphosis to change the head from an unremarkable larval fish head into something unusual enough to define the entire group.

As we saw in the previous chapter, as embryos and pre-metamorphic larvae, flatfish were nearly indistinguishable from the young stages of round fish like the cod, despite the extreme differences in their eventual adult appearance. Even naturalists like M'Intosh and Cunningham, expert in the study of the life-histories of sea fish and experienced in examining fish embryos collected from the surface of the sea, were forced to make use of diagnostic characters that had nothing to do with asymmetry in order to determine what kind of embryo they were studying. In particular, they relied heavily on colouration; the distinctive pattern of developing chromatophores (pigment-containing bodies) and spots of colour were often the only way to tell one species from another at the early stages of development. Perhaps this is why the development and physiology of colouration comprised the other major area of inquiry into flatfish biology during the nineteenth century.

As M'Intosh never tired of pointing out, prior to the establishment of marine research laboratories in Britain, little had been discovered by British naturalists about the development of marine fish. The literature of the period reflects this, making use of the developmental work done by naturalists in other nations with significant coastlines. In Darwin's response to Mivart discussed above, he mentions the work of the Scandinavian

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[29] William Carmichael M'Intosh and Arthur Thomas Masterman, *The Life-Histories of the British Marine Food-Fishes*, (London: C. J. Clay and Sons, 1897), 315.

naturalists A. W. Malm and J. C. Schiödte on flatfish development and asymmetry.[30] It was Schiödte's observation that the species in the flatfish family comprise "a long series of forms exhibiting a gradual transition from Hippoglossus pinguis, which does not in any considerable degree alter the shape in which it leaves the ovum, to the soles, which are entirely thrown to one side."[31] Schiödte offered a somewhat preformationist explanation of the asymmetry of adult flatfish. It had been there since the beginning, but not apparent to the naturalist's eye: Darwin explained that "Schiödte believes, in opposition to some other naturalists, that the Pleuronectidæ are not quite symmetrical even in the embryo; and if this be so, we could understand how it is that certain species, whilst young, habitually fall over and rest on the left side, and other species on the right side."[32]

Malm, a Swede, became well known for his role in a dispute during the 1860s with yet another Scandinavian interested in the development of the asymmetrical head in flatfish, the Danish naturalist Jap Steenstrup.[33] Without naming many names, Cunningham gave his popular readership an account of the disagreement in his *Marketable Marine Fishes* – an account that not only told the story, but educated his readers as to the merits of the laboratory-based approach to studying fish development. It all happened "[a] good many years ago," when "[t]he mode in which the extraordinary

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[30] Darwin, *Origin*, 186-187. For publications by these naturalists, see e.g. A. W. Malm, "Ichthyologiska Bidrag till Skandinaviens fauna," *Förh. Skand. Naturf.* 9 (1865): 405-414; J. C. Schiödte, (1868), "Om øiestillingens udvikling has flynderfiskene," *Naturhist. Tidsskr. Kjøbenhavn* (Ser. 3) 5: 269-275. These references were retrieved, but not read, from FishBase, www.fishbase.org/Eschemeyer/EschRefFamily.cfm?Family=Bothidae (accessed May 31, 2006).

[31] Schiödte, quoted in Darwin, *Origin*, 186.

[32] Darwin, *Origin*, 187.

[33] see, e.g. Jap. Steenstrup, "Skjaevheden hos Flyndrene," *K. Danske Vid. Selsk. Forh* (1865), cited in H. M. Kyle, "The Asymmetry, Metamorphosis, and Origin of Flat-Fishes," *Philosophical Transactions of the Royal Society of London B* 211 (1923); Wyville Thompson, "Notes on Prof. Steenstrup's Views on the Obliquity of Flounders," *Annals and Magazine of Natural History* 15 (1865).

position of the eyes was brought about gave rise, when the transformation of the larvae was first discovered, to a lively controversy."[34] Malm had observed a number of young one inch long flatfish whose eyes appeared to be in transit around their heads, occupying positions intermediate between the symmetrical larval and eyes-together adult stages. He concluded that the movement of the eye took place around the edge of the head. "About the same time," recounted Cunningham, "in the years between 1860 and 1870, a naturalist in Denmark obtained some small specimens of flat-fishes in which the eye of the lower side was in various stages of migration through the head, sinking in on one side and coming out again on the other."[35] Cunningham goes on to explain to the readers of *Marketable Marine Fishes* that the "two naturalists accordingly had a dispute as to the way in which the eye of the lower side in a flat-fish gets to the other side, one maintaining that the eye went through the head, the other that it went round."[36]

Cunningham's story had a happy ending: "both were perfectly right, each gave a correct account of what he saw, and formed a true judgement upon it, and neither had any reason to throw doubt upon the statement of the other."[37] Their observations were reconciled by "a naturalist in America" who was able to solve the problem by using an approach close to Cunningham's heart: the American naturalist "actually kept alive specimens of young flat-fishes in some of which the eye went round the head, and in

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[34] Cunningham, *Marketable Marine Fishes*, 211.
[35] Cunningham, *Marketable Marine Fishes*, 212-213. Oddly, though Cunningham reprints Steenstrup's figures in his book and gives him proper credit, he does not use Steenstrup's name in his story. In an 1892 paper in the *Journal of the Marine Biological Association*, however, Cunningham states clearly that "It is well known that Steenstrup in 1863 described transition stages of Pleuronectids, obtained from the North Atlantic, in which the eyes, after metamorphosis, were on the left side, and the right eye passed through the head to reach that side." (J. T. Cunningham, "Ichthyological Contributions," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 328.)
[36] Cunningham, *Marketable Marine Fishes*, 213.
[37] Cunningham, *Marketable Marine Fishes*, 213.

others went through. He watched the little fish while the change was taking place."[38] Unlike those who used the flatfish to speculate about evolutionary change across wide stretches of time, naturalists working on the development of flatfish had the opportunity to do just what the heroic naturalist in America did: they could, if possessed of the right skills and facilities, "watch the little fish while the change was taking place." Removing the developing organisms from the sea and placing them in laboratory tanks meant that naturalists could have access to the entire play of development, as it were, instead of merely still tableaux out of which they had to construct a story themselves. Being able to watch the changes as they happened made studying fish living in the laboratory in this sense truer to nature than studying specimens collected in the field. Graeme Gooday has written convincingly about how laboratory-based naturalists in Britain during the mid-to-late nineteenth century had to construct lines of reasoning that placed their indoor work on par with or better than traditional natural history fieldwork in terms of its proximity to "Nature", specifically with reference to the microscope.[39] In identifying the laboratory as a place where, unlike in the sea, developmental changes could be observed as they took place, it seems to me that Cunningham, like many before him, was participating in that kind of legitimation and privileging of indoor work.

The heroic American naturalist of Cunningham's tale was Alexander Agassiz, and indeed there is something quite appealing about the way Agassiz tells his own tale of reconciling the controversy, in an 1878 paper given to the American Academy of Arts

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[38] Cunningham, *Marketable Marine Fishes*, 213.

[39] Graeme Gooday, "'Nature' in the laboratory: domestication and discipline with the microscope in Victorian life science," *British Journal for the History of Science* 24 (1991); see also Graeme J. N. Gooday, "Instrumentation and Interpretation: Managing and Representing the Working Environments of Victorian Experimental Science," in *Victorian Science in Context*, ed. Bernard Lightman, 409 (Chicago: University of Chicago Press, 1997).

and Sciences, by raising flounders in the laboratory so that their development could be "traced."[40] The confidence he shows in statements like "I had myself traced quite a number of Flounders,""In the late summer of 1875, however, I traced to my satisfaction the development of a very transparent Flounder," and "I had thus succeeded in actually tracing, in one and the same individual, the passage of the right eye to the left side through the head"[41] is contagious, and must have been the more so for Cunningham, possessed as he was with the perfect location and facilities in the Plymouth Laboratory for taking up Agassiz' approach of "tracing" fish development. Agassiz traced the transit of the eye in a range of flatfish species, and found the compromise that ended the Scandinavian controversy: in the species examined by Steenstrup, the dorsal fin extended anteriorly beyond the eyes, and the eye passed through the developing fin tissue as it migrated along the edge of the head; in the more common species examined by Malm, the dorsal fin stopped before the eyes, and the eye passed around the edge of the head without encountering any fin tissue in the way.

While Agassiz across the Atlantic was tracing flatfish eye development, the other major line of flatfish research, into the nature and control of colouration, was being pursued across the English Channel, at the French marine laboratory at Concarneau. Established in 1859 by the French embryologist and ichthyologist Victor Coste, who was unusual among his peers for his interest in problems related to France's commercial fisheries, Concarneau was later run from 1885 until 1894 by the prominent French

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[40] Alexander Agassiz, "On the Young Stages of Bony Fishes," *Proceedings of the American Academy of Arts and Sciences* 14 (1878).
[41] Agassiz, "On the Young Stages," 7, 10.

embryologist and physiologist Georges Pouchet.[42] Pouchet won the Académie des sciences prix de Physiologie expérimentale in 1874 for his work on colouration in a range of marine species; his main experimental subjects were turbot. Pouchet began the massive article reporting his findings in the *Journal de l'Anatomie et de la Physiologie* with a tribute to Coste, who had made it possible for Pouchet to obtain his specimens: "Nous devons avant tout rappeler ici le nom de Coste. C'est dans le laboratoire installé par lui à Concarneau que nous avons trouvé les principaux matériaux de ces recherches..."[43] Pouchet's study had both an anatomical and a physiological section. The anatomical section was a lengthy examination of the various pigments and pigment-containing bodies (chromatoblasts, chromatophores, and iridocytes) found in marine animals. The physiological section was a detailed study of the control of colouration in the body. As was "vaguement connu des pêcheurs", many fish could change their colouration. Through a series of experiments made mostly upon turbots, Pouchet established that changes of colouration were controlled by the nerves; damaging certain nerves would result in the animal's loss of control over colouration in the area of the body associated with the cut nerve. When placed against a dark (or light) background, the rest of the turbot would change colour to match, but the enervated area would not change.

Pouchet's study was experimental in nature, focused on the fishes' internal

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[42] Mark Pavé, "'To Capture or Not to Capture?': French Scholars, Scientists, and alleged fish depletions during the Eighteenth and Nineteenth Centuries," (2005), Paper presented to the VII Congresso de la Asociación Española de Historia Económica, Santiago de Compostela, September 13-16.

[43] Georges Pouchet, "Des changements de coloration sous l'influence des nerfs," *Journal de l'Anatomie et de la Physiologie Normales et Pathologiques de l'Homme et des Animaux* 12 (1876): 1. This was Pouchet's most famous, but certainly not only, anatomical and physiological study of colouration control in marine animals carried out at Concarneau; see, for instance, Georges Pouchet, "Sur les rapides changements de coloration provoqués expérimentalement chez les crustacés et sur les colorations bleues

structures and systems of control over their colouration. It made use of an assumption shared by other French embryologists and physiologists such as Camille Dareste, Yves Delage, and Laurent Chabry (who had worked on tunicate abnormalities at Concarneau),[44] one that was fundamental to the French field of experimental teratology and became a basis of the broader movement of experimental embryology in the following decades. Stated plainly by Pouchet in Conclusion 16, it was this: "Les cas pathologiques confirment la donnée experimentale."[45] Experimental manipulation produced abnormal specimens; naturally-produced abnormal specimens, so common among the flatfish family, functioned as another, confirming category of evidence. Just as Cunningham at Plymouth had his sources of supply, Pouchet at Concarneau had his. In September of 1874, a M. Alfred Guillon brought to Pouchet an abnormal turbot, whose "côté gauche (dos) était nettement partagé en deux régions de teinte différente," normally coloured in the anterior region and entirely black posteriorly. This abnormal turbot turned out to have a lesion on the grand sympathetic nerve. Previously, Pouchet had experimentally produced bicoloured specimens identical to this one by damaging the same nerve, and this natural monster was an exciting confirmation of his theory of nerve control of colouration: "On avait la reproduction exacte du résultat auquel nous étions précédemment arrivé expérimentalement."[46] The plethora of abnormal flatfish specimens, often seen by fishermen and naturalists alike, had value beyond mere

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des poissons," *Journal de l'Anatomie et de la Physiologie Normales et Pathologiques de l'Homme et des Animaux* 8 (1872).

[44] See Jean-Louis Fischer and Julian Smith, "French Embryology and the 'Mechanics of Development' from 1887 to 1910: L. Chabry, Y. Delage, and E. Bataillon," *History and Philosophy of the Life Sciences* 6 (1984): 25-39; and Frederick Churchill, "Chabry, Roux, and the Experimental Method in Nineteenth-Century Embryology," in *Foundations of Scientific Method*, ed. Ronald Giere and Richard S Westfall, 161, (Bloomington: Indiana University Press, 1973).

[45] Pouchet, "Des changements de coloration," 161.

[46] Pouchet, "Des changements de coloration," 125.

curiosity: they could be enrolled as evidence, and naturalists interested in flatfish biology did well to cultivate, as Cunningham and M'Intosh did, a group of contacts who knew that abnormal specimens should find their way into a marine research laboratory.

Pouchet's study was experimental and physiological, but yet still environmental: it focused both on the organism itself and on the environment in which the organism found itself, its conditions of life and its habits. "La fonction chromatique, comme toute autre, est influencée par l'habitude,"[47] Pouchet both assumed and concluded, and many of his experiments explored how the animal's physiology of colouration was connected to its environment. This connection, he concluded, was mediated through the eyes; when he surgically removed both eyes or optic nerves in turbot and roundfish experimental subjects, he found that the animal lost its ability to regulate its colouration, and its chromatoblasts no longer dilated and contracted. If only one eye was removed, the animal did not lose its ability to change colour. Only when the visual connection to the external environment was completely destroyed did the animals lose their ability to respond to changing background colour conditions.

Pouchet's and Agassiz's flatfish studies were the gold standard: thorough, novel, and compelling in their methods and findings. Both Pouchet's use of abnormal flatfish specimens as evidence and Agassiz's method of "tracing" flatfish development made themselves felt in Cunningham's flounder colouration studies, which were also marked by a keen interest in the evolutionary implications of his findings. He combined these influences with his own characteristic ability to work simultaneously and symmetrically on more esoterically scientific and more obviously practical questions, in this case employing single flounder populations in both tasks.

The "Brood of 1890" I: *Inducing Colouration*

In the previous chapter, we saw the extent to which Cunningham relied on a network of suppliers to keep him in specimens. The fishermen were essential in supplying the eggs and adult specimens that were to be found well offshore, but when it was inshore-dwelling larval flatfish that Cunningham was after, there was no more useful ally than Matthias Dunn of Megavissey, down the shore in Cornwall. The tidepools of Megavissey Harbour were a veritable warehouse of young flounders in particular, and Cunningham's delight – glee, really – upon being shown this warehouse by Dunn in May of 1889 was, as we've seen, apparent even in his notebook description of the place. Dunn's generous shipments of larval flounders from Megavissey Harbour furnished the material basis for Cunningham's simultaneous studies of colouration and growth carried out at the Plymouth Laboratory during the first half of the 1890s.

Cunningham reported on his first set of colouration experiments in three different places. The first and somewhat preliminary report appeared in the 1891 volume of the German journal the *Zoologischer Anzeiger*;[48] it was this article (and almost certainly not another article by Cunningham in the same issue, a short assertion of the priority of his views on the spermatogenesis of *Myxine* against a Scandinavian challenger, Fridtjof Nansen)[49] that prompted the MBA to order him to bring his work first to the MBA's own journal. When the much longer memoir on the full set of experiments was completed, complemented by Charles MacMunn's detailed physico-chemical study of fishes'

[47] Pouchet, "Des changements de coloration," 161.
[48] J. T. Cunningham, "An Experiment concerning the Absence of Color from the lower Sides of Flat-fishes," *Zoologischer Anzeiger* 14 (1891).
[49] J. T. Cunningham, "Spermatogenesis in Myxine," *Zoologischer Anzeiger* 14 (1891).

chromatophores and iridocytes, and the pigments and other substances contained therein, a compromise was reached. Lankester communicated Cunningham and MacMunn's paper to the Royal Society,[50] and Cunningham prepared a shorter account of the experiments, a "mere brief summary of general results and conclusions",[51] along with a photograph of the experimental setup and a promise of much more information to be had in the paper's Royal Society incarnation, in the *Journal of the Marine Biological Association.*

In all three papers, Cunningham makes reference to the flounders used in the experiments, making clear that in addition to being experimental populations, they were also natural populations, with each experiment using a single brood from a single source – namely, the tidepools of Megavissey Harbour. Those specimens placed in the glass-bottomed, dark-covered tanks were members of the same natural population as those specimens used as a control group; and the control-group specimens lived a laboratory life that was as much like their Megavissey life as could be. "The young Flounders used in Experiment I.," wrote Cunningham in the Royal Society account,

> were some of a large number, several hundred, which were received from Megavissey between May 3 and May 8, 1890. The rest of these specimens were placed in table tanks at the bottom of which was a layer of sand, and in these they were fed regularly, and carefully reared. Excepting the confinement, these Flounders were living under normal, natural conditions, burying themselves in the sand when alarmed.[52]

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[50] J. T. Cunningham and Charles A. MacMunn, "On the Coloration of the Skins of Fishes, Especially of Pleuronectidae [Abstract]," *Proceedings of the Royal Society of London* 53 (1893); J. T. Cunningham and C. A. MacMunn, "On the Coloration of the Skins of Fishes, Especially of Pleuronectidae," *Philosophical Transactions of the Royal Society of London B* 184 (1893).
[51] J. T. Cunningham, "Researches on the Coloration of the Skins of Flat-fishes," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895): 118.
[52] Cunningham and MacMunn, *Philosophical Transactions*, 792.

The "brood of 1890"[53] were the basis of a great deal of Cunningham's published work over the next few years, and he took pains to stress what a natural group they were, whether fresh out of Megavissey Harbour or dwelling in the Laboratory's aquarium. In the MBA *Journal* account, Cunningham also assures his readers of the natural state of the population that served as the control group for the first experiment and the source of specimens for later experiments of a similar kind: "It is important to point out that these four specimens were taken from a number reared in the aquarium in tanks with sand at the bottom, and subjected to no artificial conditions except captivity."[54] Moreover, Cunningham made sure to reassure his readership that bringing young fish to live in the laboratory was a routine and straightforward matter – a far cry from his earlier desperate attempts to keep flatfish eggs alive for more than a few days, and indeed a far cry from the frequent accidental flounder deaths that are reported in the same MBA *Journal* paper that confidently states on the first page that "The rearing of flounders from this stage to maturity, although requiring minute and constant attention, presents no great difficulty."[55]

In the experiment presented in the *Zoologischer Anzeiger* paper, and presented as "Experiment I" in the Royal Society and MBA papers, Cunningham placed "fifteen or sixteen larval Flounders, in process of metamorphosis" from the brood of 1890 in a one foot wide cylindrical glass vessel, covered on the top and sides with black paper and illuminated from below by a "mirror, inclined at an angle of 45° to the plane of the south window, opposite which the apparatus was placed."[56] This unusually-lit tank was designed to test the "connection between the difference of the two sides in relation to

[53] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Blological Association*, 113.
[54] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 113.
[55] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 111.
[56] Cunningham and MacMunn, *Philosophical Transactions*, 791.

light and in coloration",[57] but did not otherwise throw the larval flounders' development off of its natural course, and in particular did not affect the process of metamorphosis, which was "far from complete" at the beginning of the experiment: "The new conditions to which they were subjected produced no apparent change in the course of the metamorphosis. Their eyes passed into the normal adult position, and the colour disappeared at first, as far as could be seen, from the under side as usual."[58] After four months, Death came for the experimental flounders in the form of "an accident to the circulation of the tank",[59] and upon examining the thirteen victims, Cunningham found "a striking development of pigment"[60] on the lower sides of three of them, some development of pigment on eight of them, and no pigment at all on the lower sides of two of the fish.

Cunningham's second experiment, begun in September of 1890, made use of four members of the brood of 1890, who had by then been living in the Laboratory aquarium for nearly five months. These four had thus already undergone metamorphosis before being placed in the experimental tank, which had had the black covering removed from the sides but not the top, in a largely fruitless effort to curb what Cunningham called the fishes' "usual objectionable habit of clinging to the sides of the tank, instead of lying on the glass bottoms with their lower sides exposed to the light"[61] like well-behaved experimental subjects might be expected to do. As the flounders became larger, they could no longer keep clinging to the sides; the larger they got and the longer they stayed

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[57] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 111.
[58] Cunningham and MacMunn, *Philosophical Transactions*, 792. Agassiz had attempted to retard metamorphosis by exposing young flounders to light from below in 1878, and had seen no effect. (Cunningham, "An Experiment concerning the Absence of Colour," 31.)
[59] Cunningham and MacMunn, *Philosophical Transactions*, 792.
[60] Cunningham and MacMunn, *Philosophical Transactions*, 792.
[61] Cunningham and MacMunn, *Philosophical Transactions*, 793.

in the experimental tank, the more pigment developed on their lower sides, with two developing significant pigmentation. The remainder of the brood of 1890 was still living in the aquarium, and Cunningham examined them again as his control group for the second experiment. "The only difference in conditions between the two cases was that in one the Fish were living on a layer of sand and only exposed to light coming from above, while in the other the Fish were living on a glass plate beneath which was a mirror to reflect the light upwards,"[62] and only one of the ninety members of the control group had any pigmentation at all on the lower side. Nor were the aquarium-dwellers the only control group. Flounders in particular were known for their variability and the frequency with which abnormal individuals could be found in the wild, but even taking the success rate of his experiment at a conservative fifty percent, "the effect" could be demonstrated by comparison with both laboratory-raised and wild-raised control groups: "although pigment occurs on the lower sides of flounders living under natural conditions as an occasional abnormality, the percentage of such specimens is nothing like 50 per cent."[63] Using in effect two control groups did more than establish that "the effect" was due to the experimental conditions and not chance alone: it also established the naturalness of the aquarium-raised control group, making it a legitimate basis for comparison. The rate of abnormal specimens in the laboratory control conditions was similar to the rate in the sea: occasional, but nothing like the frequency of pigmented specimens when subjected to artificial conditions.

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[62] Cunningham and MacMunn, *Philosophical Transactions*, 795.
[63] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 113.

Cunningham's third experiment involved one of the rare abnormal specimens of "this same brood"[64], a specimen with a spot of pigment on its lower side that was transferred into the experimental tank in order to see if it might be, by virtue of its existing spot, more prone to show "the effect". The abnormal specimen did not disappoint: "pigment developed on its lower side with extraordinary rapidity."[65] After its death by misadventure three months after entering the experimental tank, Cunningham observed that it had "after only 3 months' exposure exhibited more pigmentation"[66] than the most darkly pigmented of the experimental fish from the second experiment, which had been living under experimental conditions for thirteen months.

His fourth experiment was really a re-run of the first experiment, with two differences: a new glass-bottomed tank, with wooden sides rather than glass, and a new year's brood of "larval Flounders in process of metamorphosis, procured from Megavissey Harbour"[67] in May of 1891. These new fish had the same objectionable habits and apparent death wish as the previous year's class: they had no glass sides to cling to, but they "continually sought the dark corners of the tank, and remained with their lower sides in contact with the wooden sides, so that the lower sides of the Fish were by no means always exposed to the light coming through the glass bottom," and many died accidentally, "several having killed themselves by getting fixed in crevices."[68] Nevertheless, many of this brood developed pigmentation on their lower sides as well,

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[64] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 113.
[65] Cunningham and MacMunn, *Philosophical Transactions*, 795.
[66] Cunningham and MacMunn, *Philosophical Transactions*, 796.
[67] Cunningham and MacMunn, *Philosophical Transactions*, 796.
[68] Cunningham and MacMunn, *Philosophical Transactions*, 796.

and in the eight flounders Cunningham transferred to an inverted bell jar directly above the mirror, "pigment developed very considerably on the lower sides".[69]

Cunningham had clearly established that changing the conditions of life experimentally such that the lower sides of flounders were exposed to light could induce pigmentation on the normally white surface – but to what ends? It is hard to say exactly what ambitions Cunningham had for his experimental flounder population. Cunningham was one of many naturalists in 1890s Britain who were interested in the role of use and disuse and the inheritance of acquired characteristics in evolution, part of the neo-Lamarckian "eclipse of Darwinism" that Peter Bowler has demonstrated.[70] We have seen previously how the asymmetry of flatfish made them a favourite example of evolution by the inheritance of acquired characteristics, and Cunningham was continuing in this tradition – and was ideally placed to do so, given his job at the MBA's Laboratory in Plymouth and the newfound status of flatfish as an economically important, and not just scientifically interesting, group.

In Cunningham's first presentation of his induced-pigmentation results, in the *Zoologischer Anzeiger* of 1891, his agenda appears to be very ambitious indeed. The paper's first sentence – "One of the most interesting questions which biological research has still to decide is whether adaptations in organisms are due to the natural selection of indefinite variations or to the definite influence of the conditions of life"[71] – kicks off an opening paragraph in which the adaptationists, led by Weismann, are challenged to come up with an adaptive explanation for the lack of colouration on the lower side of flatfish,

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[69] Cunningham and MacMunn, *Philosophical Transactions*, 796.

[70] Peter Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around* 1900, (Baltimore: Johns Hopkins University Press, 1983); Peter Bowler, "Revisiting the Eclipse of Darwinism," *Journal of the History of* Biology 38 (2005).

while Cunningham asserts the "more probable" explanation "that it is due in some way to the fact that little or no light can fall on the lower sides of these fishes, because these sides are generally in contact with the ground."[72] However, later in the paper, hints of an even more grandiose plan for his experiments emerges, in which the inducement of colour is only the first step to a complete reengineering of metamorphosis by changing the conditions of life over a longer term. Discussing Agassiz's similar experiments, in which no changes in metamorphosis were observed, Cunningham reveals the scheme: "The idea on which I found my experiments is that the inherited tendency will cause the metamorphosis to take place even when the conditions are reversed, but that when the reversed conditions are kept up long enough a new metamorphosis will be induced in the opposite direction to the first."[73]

By the time that Cunningham's results were published in Britain, however, this plan appears to have been largely abandoned, in favour of more modest claims about what the ability to induce colouration in living flounders might say about the evolutionary history of colouration in ancestral flatfish. The reason for this change is not clear. In the MBA *Journal* presentation of the results, only on the last page does Cunningham assert that his experiments mean that "the disappearance of the pigment from the lower side in the normal flat-fish is an hereditary character, and not due to the withdrawal of the action of light in the individual",[74] and somewhat more boldly that

> the fact that in these experiments the pigment, after prolonged action of the light, actually reappears is strong evidence (to my own mind a proof) that originally in the

[71] Cunningham, "An Experiment concerning the Absence of Colour," 27.
[72] Cunningham, "An Experiment concerning the Absence of Colour," 27.
[73] Cunningham, "An Experiment concerning the Absence of Colour," 31.
[74] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biolological Association*, 118.

> beginning of the evolution, the pigment disappeared in consequence of the withdrawal of the lower sides from the action of light. If this be granted, it follows, of course, that a character originally acquired has become hereditary.[75]

In addition to having simply dropped the statement of a research program aiming to reverse flounder morphology by inverting the conditions of life experienced by each side, the MBA paper differs from the *Anzeiger* paper in placing these interpretive statements at the paper's end, and not presenting them at the beginning as the motivation for undertaking the study at all.

The paper in its Royal Society form was similarly inductive in presentation, claiming in its first sentence that the study was "originally commenced with the object of obtaining some reliable definite evidence on the inevitable question, why is the lower side of a Flat Fish white and the upper side coloured?"[76] Considering the mildness of this paper in light of the fact that Cunningham was a known and vocal Lamarckian, Peter Bowler suggests in a footnote that this article "was communicated to the Royal Society by [a Darwinian and Cunningham's boss] Lankester, which perhaps explains Cunningham's reluctance to engage in Lamarckian theorizing."[77] In contrast with the guns-blazing opening paragraph of the *Anzeiger* paper two years earlier, the Royal Society paper's opening sentence can only be described as artificially naïve, and it is strange that Cunningham does not speculate about the results of his experiments much beyond safe statements like "the experiments prove clearly that the amount of pigment produced increases steadily with the duration of the exposure."[78] Lankester's influence – not just on the Royal Society paper, but on the MBA *Journal* paper as well – very likely

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[75] Cunningham, "Skins of Flat-fishes," *Journal of the Marine Biological Association*, 118.
[76] Cunningham and MacMunn, *Philosophical Transactions*, 765.
[77] Bowler, *Eclipse of Darwinism*, 240.

might explain some of the difference in tone and ambition between the 1893 accounts and the 1891. However, it is also possible that as his experiments carried on over two more years, Cunningham might have lost confidence in the ability of the conditions of life to effect such a wholesale change in morphology over a relatively short amount of time, and modified his expectations accordingly.

Like many believers in the inheritance of acquired characteristics, Cunningham also favoured the theory of recapitulation, and saw in the development of flatfish an excellent example of recapitulation in action, as he made clear in two papers published in *Natural Science* during the same period as the flounder colouration experiments. The first, which appeared in 1892, told the reader matter-of-factly that "the statement that flat-fishes were evolved from symmetrical ancestors, and that their individual metamorphosis is a recapitulation of their secular evolution, is now a mere truism. We have to consider the causes of the evolution and the causes of the metamorphosis,"[79] before proceeding to critique Weismann's adaptationism in much the same manner as he had done one year previously. In another paper published in 1893 on the subject of variation, Cunningham again sees recapitulation in action during flatfish development:

> Nothing is more certain than that no theory of variation is worthy of attention unless it takes into account the phenomenon of recapitulation. In a very large number of cases an organ, or a character, or an individual, passes through in development stages which reproduce, more or less exactly, an ancestral condition. In other words, new characters are added usually at the end of ontogeny. We have convincing evidence, then, that the modified individual first resembled its parent, and afterwards became different. The ancestor of the flat-fish was symmetrical; but, at some time or other, individuals of this pedigree,

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[78] Cunningham and MacMunn, *Philosophical Transactions*, 798.
[79] Cunningham, "The Evolution of Flat-Fishes," 193.

> after developing into symmetrical fish, became more or less asymmetrical, and every flat-fish *at the present day* develops first into a symmetrical fish, and then turns into a Pleuronectid.[80]

The theory of the inheritance of acquired characters, argued Cunningham, was the only one that took recapitulation properly into account, not least because "the actual ontogeny of these changes of structure in the individual has been shown in some instances to be largely dependent on conditions."[81] The flounder colouration studies, then, have to be understood in the context of Cunningham's views on evolution, development, and the primary role played by the conditions of life in each.

Abnormal Specimens as Evidence

The question of variation was one of which Cunningham was particularly aware. To say that flatfish were known not just for their abnormal appearance but also for the frequency with which specimens were found that were abnormal compared with others of their species is as much as to say that flatfish are a group which show a great deal of variation, an observation which had not escaped Darwin and which Cunningham both made use of and had to contend with in his work.

The frequency of abnormal flatfish specimens living under presumably normal conditions of life provoked criticism of Cunningham's 1891 paper from the French fish biologist A. Giard, who had described to the Société de Biologie an abnormal turbot specimen, with its right eye on one edge of the head and colouration on both sides. Giard suggested that such a "double" turbot must swim vertically, and rest rarely on its right side. Cunningham's ire was raised by Giard's suggestion that Cunningham's apparent

[80] J. T. Cunningham, "The Problem of Variation," *Natural Science* 3 (1893): 283.

inducement of colouration in flounders was not due to changed conditions of life at all, but instead due to variation within the population making itself apparent: "he points out," fumed Cunningham, "that, of all the flat-fishes, it is in the flounder that reversed specimens are most common," and "that the specimens upon which I experimented may have numbered reversed individuals among their ancestors, and the pigmentation adduced in evidence by me may have been due to inheritance from one of these ancestors which was pigmented on the left side."[82] Cunningham both dealt with this criticism and attacked Giard's claims about the abnormal turbot using the same approach: he asserted his greater proximity to the populations in question, the population of double flatfishes. Giard's slight about the ancestry of the experimental flounder population was parried by Cunningham asserting his familiarity with the flounders of Plymouth, where "reversed flounders are exceedingly common, almost as abundant as normal specimens…it is obvious that if inheritance in the flounder acted as Giard supposes, there would be none but double flounders, since right-sided and left-sided individuals are always breeding with one another."[83] And Giard's speculations about the habits of his double turbot, which one suspects Cunningham might have let pass had he not been otherwise provoked, were destroyed, along with all of the literature Giard had cited in support of his speculations, by Cunningham's evocation of the power of laboratory-based "tracing". Aside from his many observations of adult double flatfish, Cunningham had "observed living young flat-fishes many times during the period of their metamorphosis" and had "now under observation a living double specimen of the plaice" which "instead of showing a tendency to continue swimming in the water, cannot even be induced to leave

[81] Cunningham, "The Problem of Variation," 284-285.
[82] Cunningham, "The Evolution of Flat-Fishes," 198.

the bottom long enough to enable me to see whether it holds itself perfectly horizontal or not...I have had this specimen for more than six months."[84] Giard's speculations about the habits of a single, dead double turbot could hardly hold up, Cunningham made it appear, in the face of actual, long-term close observations made on a variety of double flatfish by an observer well-acquainted with abnormal specimens both indoors and out.

Giard's reply, which appeared very shortly thereafter in the same journal, shows clearly how important it was for each man to make it appear that he was the one with privileged access to abnormal flatfish specimens. Cunningham might think that double and reversed specimens were the abnormal individuals in question, but compared to what Giard had, they were commonplace and barely interesting. Giard's turbot wasn't merely abnormal; it was monstrous: "I have carefully distinguished those *monstrous* Pleuronectids which show arrested development (without stoppage in growth) from the double flat-fishes whose existence Cunningham says is recorded in almost every ichthyological work of any importance."[85] Those flatfish that Cunningham placed himself in such close proximity to "are very numerous; all the fishermen know them," and Cunningham's observations "of a double plaice (not monstrous)" have little to say about Giard's original claim "that not the *double* but the *monstrous* specimen (which is very different) swims vertically".[86] In scientific discourses about a group of animals known for their odd appearance, especially odd individuals were another level of currency, showing even more inside access to the biology of the group.

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[83] Cunningham, "The Evolution of Flat-Fishes," 198.
[84] Cunningham, "The Evolution of Flat-Fishes", 197.
[85] A. Giard, "The Evolution of Flat-Fish," *Natural Science* 1 (1892): 356.
[86] Giard, 358.

That possessing abnormal specimens was a marker of extra scientific authority is particularly apparent in examining the large final section of Cunningham and MacMunn's Royal Society paper that catalogues a huge range of abnormally pigmented flatfish. Perhaps to forestall any further objections like Giard's that what looked like an effect induced by changing the conditions of life was in fact random latent variation, the paper's fourth section offers a long list of what they called "ambicolorate" specimens of flounder, plaice, sole, turbot, brill, and several other species, preceded by the rationale for including such a list: "The experiments described in the previous section seem to indicate that the action of daylight determines the appearance of black and yellow chromatophores in the skin of the lower sides of Flounders...but it must be remembered that specimens abnormally coloured are of not infrequent occurrence in various species of Flat Fishes."[87]

The list that follows is a remarkable testament, both to Cunningham's very close acquaintance with the population of ambicolorate flatfish and to another set of specimen supply chains in action, this one specialising in abnormal flatfish specimens. The paper's fourth section is not arranged by species or by pattern of colouration, as one might expect, but instead by supplier. The first category includes specimens "collected at Plymouth", through the channels that have been explored in another chapter. Specimens in the second category were sent to Cunningham from the English trawling port of Grimsby by E. W. L. Holt, another MBA employee, whose work on fish and fisheries problems in the North Sea was taken over by Cunningham in 1895 when Holt became too ill to work. Holt also sent, in place of the specimen itself, an unpublished description of a

[87] Cunningham and MacMunn, *Philosophical Transactions*, 801.

live abnormally-coloured brill "in his possession."[88] The observations were Cunningham's to publish, but the specimen itself remained "Mr. Holt's Brill".[89] The third category represents a longer supply chain: these abnormal specimens were collected and observed by the German marine biologist Brandt at Kiel marine laboratory, and Brandt's notes were in turn communicated to Cunningham by G. H. Fowler "with [Brandt's] consent."[90] The fourth category made use of a supply chain available to every naturalist, salaried, seaside, or otherwise: previously published descriptions of ambicolorate specimens from a range of people and places. The four groups together make up a kind of literary cabinet of one particular type of curiosity, and a lovely piece of descriptive natural history in a paper that otherwise, between Cunningham's experiments and MacMunn's spectroscopic examination of pigments, is rather a modern piece of biological work.

Having assembled such a catalogue, Cunningham seemed not entirely sure what to make of it, and in the course of gesturing at the possibility of evolutionary reversions or atavisms, he made a suggestion that intrigued a former MBA colleague of his who had developed a healthy interest in the problem of variation. Cunningham made reference to "some occult tendency, in certain individuals, of the lower side to imitate the upper"[91] whether for reasons related to the organism's development, or evolutionary past, or something else entirely.

[88] Cunningham and MacMunn, *Philosophical Transactions*, 807.

[89] Cunningham and MacMunn, *Philosophical Transactions*, 807. Holt made extensive studies of plaice in the North Sea, and voiced his objections to Cunningham's (and William Bateson's) symmetry-based account of colouration control in plaice: see E. W. L. Holt, "North Sea Investigations (Continued)," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895).

[90] Cunningham and MacMunn, *Philosophical Transactions*, 804.

[91] Cunningham and MacMunn, *Philosophical Transactions*, 809.

The next year, William Bateson gave a paper to the Zoological Society on "the phenomenon of Symmetry" and the ways in which it "may contribute to the production of a definite result in Variation that is presumably sporadic."[92] The animal subjects of the paper were a brill and a plaice sent to Bateson by one of Cunningham's best suppliers, Matthias Dunn. Both specimens were abnormally coloured on their lower sides, and the abnormal spots of colour were mirrored by other spots: in the brill, the lower side had two lines of spots extending down the fish's lower side, while in the plaice, the spots on the lower side corresponded exactly with spots on the upper side. This was, for Bateson, a case of variation expressed "*similarly and simultaneously*,"[93] showing two versions of bilateral symmetry even in animals chiefly noted for their apparently complete lack of symmetry of any kind.

In the fourth experiment reported in the Royal Society paper, Cunningham had recruited an abnormally coloured flounder from his control population to serve as an experimental subject, to see if its natural variation toward lower side colouration made it more susceptible to the effects of light. In 1894, Cunningham reported another experiment of this type, this time on a flounder at the other end of the spectrum of colour variation, a "piebald plaice" with no colour at all on the lower side and a large patch of colour missing on its upper side. Based on his previous musings about the "occult tendency" for the two sides to resemble one another, Cunningham wanted to test whether exposing the lower side to light might induce pigmentation on *both* sides; when the test succeeded, he declared the result "the most remarkable of all that I have obtained in the

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[92] W. Bateson, "On two Cases of Colour-variation in Flat-fishes illustrating principles of Symmetry," *Proceedings of the Zoological Society of London* (1894): 246.
[93] Bateson, 247.

course of these researches"[94] and a vindication of what he had earlier described, in a previous paper on the same piebald plaice specimen, as "the explanation adopted by Mr. Bateson and myself…that in certain cases one side, instead of developing normally, partially or completely imitates the other."[95]

Cunningham's studies of colouration in flounders are an interesting example of what Peter Bowler has characterized as the somewhat desperate struggle of Lamarckian scientists at the end of the nineteenth century to find some hard experimental evidence in favour of their views. Examining these studies also shows us something else: we see Cunningham deploying every resource he had access to in order to promote and defend these views. His main advantage was access: access to the sea around Plymouth and knowledge of the flatfish populations three; access to a network of suppliers of specimens and observations of flatfish, abnormal or otherwise; and access to nature by way of the laboratory, which allowed him to be present to watch changes as they happened. These same kinds of access, and the same kinds of concerns about the roles of variation and the conditions of life, also characterized the work Cunningham was doing at the same time on the growth of flatfish in the laboratory.

The "Brood of 1890" II: *Tracing Growth in the Laboratory*

In the previous sections, we've seen how Cunningham tried hard to reassure his readers of the non-artificiality of the lives lived by the flatfish that he was studying. He had good reason to take such pains to argue for the near identity of the conditions of life

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[94] J. T. Cunningham, "Additional Evidence on the Influence of Light in producing Pigments on the Lower Sides of Flat Fishes," *Journal of the Marine Biolgical Association of the United Kingdom* 4 (1895-1897).
[95] J. T. Cunningham, "Experiments and Observations made at the Plymouth Laboratory," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895): 271.

in the Plymouth Laboratory tanks and in the fishes' natural habitat in the sea outside the Laboratory. The brood of 1890, in addition to furnishing experimental subjects and a control group for the flounder colouration experiments, was the basis for Cunningham's ongoing laboratory study of a tricky question with huge practical implications: how big, and how old, did fish get before they reached reproductive age? This issue will be more fully discussed in Chapter Six, but Cunningham's attempt to answer this question using the laboratory-raised flounders of the brood of 1890 was closely linked to his attempts to induce coloration in some of these same fish, and cannot be considered separately. The population of young flounders imported to the Plymouth Laboratory from the Megavissey Harbour tidepools on two days in early May of 1890 were monitored closely by Cunningham over a period of four years as he "traced" their growth and reported his findings in a series of articles in the MBA's *Journal*. In the first, published in 1890, the brood had been living in the laboratory for three months, during which time they had "grown very considerably, although the size of different individuals varied very much."[96] Just how much they had grown, and just how much they varied, Cunningham could not say for certain "without killing the whole number"[97] in order to measure them, which would of course preclude any future measurements. Even when living inside the laboratory, the fish were not wholly accessible for study – tracing had certain limits where questions of populations and variation were concerned. Some young brill that arrived a few days afterward, brought in from Sutton Pool in Plymouth by "boys and fishermen,"[98] proved to be less than model subjects for a laboratory-based study of

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[96] J. T. Cunningham, "Notes on Recent Experiments Relating to the growth and rearing of food-fish at the Laboratory," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 371.
[97] Cunningham, "Notes on Recent Experiments," 371.
[98] Cunningham, "Notes on Recent Experiments," 371.

growth. The young brill preferred to eat young flounders over any other type of food, and even with his access to Matthias Dunn and the Megavissey flounder warehouse, that was going to be a problem one way or another. If the brill were allowed to eat flounders, the study of flounder growth, and the colouration study as well, were bound to suffer. But if the brill were fed on something else, the resulting growth figures risked being dangerously artificial and so inapplicable: "It is possible that in the natural condition young brill prey upon smaller fishes, and that then they grow faster than my specimens have on the diet supplied to them. But I could not afford to feed them on living young flounders, and I shall have to find some future opportunity of comparing the growth of the captive specimens with that of free individuals."[99] "Captive" and "free" are opposites, not analogues; Cunningham's choice of language reveals the epistemological anxiety bound up in drawing conclusions about sea-fish based on tank-fish results.

In each of the next two years, Cunningham's monitoring of the brood of 1890 made up part of a lengthy and detailed report on fish growth and sexual maturity in the MBA *Journal*. In the 1891 report, Cunningham begins by reporting on the recent fieldwork to determine the minimum and maximum sizes of sexually mature food fishes, carried out by Wemyss Fulton of the Scottish Fishery Board. Cunningham stresses the extent of his own fieldwork on the topic – "I have searched for young specimens in all possible ways, and have measured and preserved all I could meet with" – before turning to the laboratory component of his studies. The trouble with fieldwork to study fish growth is similar to the trouble with fieldwork to study fish development: it's not possible to watch the changes yourself. As Cunningham observed, "it is difficult in this way to ascertain the maximum growth for one year, since a number of specimens of one species

[99] Cunningham, "Notes on Recent Experiments," 372.

often form a continuous series in size, and it becomes difficult or impossible to say where those of one year old end and those of two years begin."[100] And so to the laboratory, "to get more certainty on the question" by "rearing specimens of known age of as many species as possible in the tanks of the aquarium."[101] This increased certainty is bought at the price of another kind of uncertainty, however, and Cunningham anticipated this criticism within the same paragraph:

> It is, of course, a question in the case of each species how far the growth of specimens in captivity is normal, whether it exceeds or falls short of the growth of those living in the free state. This question can only be answered by comparing the size of captive specimens with that of young specimens collected from the sea at various times of the year.[102]

This solution is, of course, subject to the same uncertainty that led Cunningham to study growth in the laboratory in the first place. This paradox led to a kind of fishy Uncertainty Principle, to be discussed in more detail in Chapter Six; and it was not limited merely to individual fish size. Population variability was another source of anxiety for Cunningham: on the subject of his laboratory flounders, he mused that the variation in size was very great. Variation in nature was considerable as well, he mused – hence the very difficulty of distinguishing one year's brood from another in samples taken at sea – but "[w]hether there is as much variation under natural conditions is a question that immediately suggests itself."[103]

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[100] J. T. Cunningham, "The Rate of Growth of some Sea Fishes and their Distribution at Different Ages," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 96
[101] Cunningham, "The Rate of Growth," 96.
[102] Cunningham, "The Rate of Growth," 96.
[103] Cunningham, "The Rate of Growth," 97.

Cunningham's best growth data was that gleaned from the laboratory-dwelling flounder brood of 1890. The rest of the article is a series of charts for growth of a variety of species, including wild-caught flounders. In only one chart, that representing the brood of 1890 dwelling in the laboratory, is there a column headed "Age". In every other chart the column is headed "Calculated age": Cunningham took the patterns of size and age from the laboratory flounders and used it to calculate ages for sea-caught flatfish of several species, and even to revise his earlier assumptions. On the sole, the species on which Cunningham had just published an entire book, he writes that "[t]he specimens taken in the Sound up to 19.7 cm. in length I formerly considered to be two years old, but my observations on the flounder in captivity show that some soles probably reach this length at the beginning of their second year."[104] For the brill, whose conditions of life in the laboratory were sufficiently altered to cast doubt on whether they were genuinely representative of sea-dwelling brill, Cunningham uses the flounder data as well, concluding that it "is probably that the growth of my captive [brill] specimens is abnormally checked."[105] When the living conditions of captive specimens were judged to be too different – that is, when the laboratory tank was judged not to be analogous to the sea – it was preferable to extrapolate from the laboratory data for another *species* to make claims about sea fish.

In Cunningham's view, the internal, species-level differences between flounders and brill (and, given that young brill preferred to eat flounders, while young flounders were happiest with chopped-up worms, these species differences might be thought to be fairly substantial) was less of a problem than the external differences in the conditions of

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[104] Cunningham, "The Rate of Growth," 103.
[105] Cunningham, "The Rate of Growth," 106.

life experienced by laboratory-dwelling brill and their sea-dwelling counterparts when attempting to make claims about patterns of growth. This is perhaps less surprising than it might otherwise be when we take into account how extremely important it was that any answer to the question of fish age, size, and sexual maturity be applicable to the fish in the sea. "The fishermen of the east coast, who have been so strongly moved on the subject of immature fish"[106] were waiting for a straightforward guideline, arrived at by Britain's fish biologists, as to how small a fish was too small to land. This decision needed to be reached soon, but just as importantly, it needed to be true of the fish in the sea: "the question of the harm done to our sea fisheries by the destruction of under-sized or immature fish is constantly being agitated, and cannot be too carefully considered."[107]

Answering this question correctly and advising on how to limit this harm was a test of the practical utility of Britain's newly-prominent marine biology, but it was also a test of the utility of the laboratory as a place for studying live fish at all. If the laboratory – and especially the likes of the Plymouth Laboratory, situated on the coast, able to use real seawater, and possessed of the newest equipment and full-time salaried staff – could not adequately recreate the conditions of life in the sea, any observations made there were of very limited applicability and interest. The flounder brood of 1890 had been collected from the tidepools as a single, natural population, they dwelled in the Plymouth Laboratory tanks as a population under "no artificial conditions except captivity", and they were expected to stand in for the much larger population of flounders off Britain's coasts and beyond. What's more, they were expected to stand in for the range of flatfish

[106] Cunningham, "The Rate of Growth," 115.
[107] Cunningham, "The Rate of Growth," 115.

species that were not so easy to obtain and raise in the laboratory. The brood of 1890 bore a heavy load.

Cunningham was ready with a recommendation for the fishing industry in his 1891 growth study that simplified the tangled web of fish age, size, and maturity as much as possible: "If it were possible to limit the capture of each sea fish to those above a certain size, I think the limit should be determined by the size of the full-grown fish, and not by the size at which it begins to breed. At the same time the limit should not be fixed below the minimum size of sexually mature individuals."[108] Unfortunately, he was not ready to say what "a certain size" might be – though he did suggest doubling the size of the mesh of trawl nets in order to allow more young fish to pass through.

The next year, Cunningham presented more measurements of the brood of 1890, many of which had grown to eight inches in length, and a only a few of which had become sexually mature, to Cunningham's surprise. He revised his earlier estimation, and concluded that " the greater number of young flounders breed for the first time when three years old, while a small proportion begin to breed at two years. If this were the case normally in flounders and other flat-fishes, not only under artificial but under natural conditions, then in this respect the life-history of the flat-fish would resemble that of the salmon, which has been so long and so attentively studied."[109] And Cunningham's confidence in his laboratory flounder population to represent wild flatfish was growing: "there is here some evidence that the development of the reproductive organs is not greatly modified by confinement,"[110] it appeared, and the "results thus obtained for the

[108] Cunningham, "The Rate of Growth," 116.
[109] J. T. Cunningham, "On the Rate of Growth of some Sea Fishes, and the Age and Size at which they begin to Breed," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 226.
[110] Cunningham, "On the Rate of Growth," 227.

flounder probably apply more or less exactly to other kinds of flat-fishes."[111] Thus emboldened, Cunningham ventured an even more specific recommendation for the regulation of landable fish size (though he still did not give a particular number of inches that would have been the most useful, if blunt, tool for regulating the fisheries): "when the smallest size of the mature female in a given species has been ascertained, many females do not reach maturity until they are somewhat larger than this. Therefore, in order to exclude all immature individuals, a limit of size must be taken which is above the minimum size of mature females."[112]

In 1894, those following the ongoing adventures of the brood of 1890 got the final report:[113] of the several hundred that had originally arrived from Megavissey Harbour, sixty-five had made it to 1892, and of these fifty were still surviving, and many appeared to have reached reproductive age. The spawn of some of these ripe flounders was used to begin a new generation of flounders, one that had never known the sea. This brood of the brood of 1890 was short-lived, however, surviving for only a month. Their parents' generation had enjoyed (or at least participated in) an extraordinarily successful partnership with Cunningham, generating hundreds of pages of articles, in several journals, in two major areas of inquiry.

Conclusion: Double Flounders

One year after this flurry of publication, Cunningham revisited his colouration studies, reporting in the MBA *Journal* that he had made an observation "which confirms

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[111] Cunningham, "On the Rate of Growth," 228.
[112] Cunningham, "On the Rate of Growth," 228.
[113] J. T. Cunningham, "Notes and Memoranda," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895).

my previous results in a most striking manner." This striking confirmation occurred "accidentally, or at least incidentally, and was due to conditions which had been quite unintentionally produced"[114] – produced, in fact, in the course of another phase of Cunningham's laboratory study of fish growth. Having taken such pains to establish the naturalness of the laboratory setting, and as such the applicability of his growth measurements to the sea fish needing protection, he had all the same made a change to their conditions of life that made itself apparent in the appearances of the plaice, soles, and flounders living in one of the laboratory tanks. The bottom of this tank was slate, not sand, and pigmentation appeared on the undersides of these fish in the precise areas where they had not been in close contact with the slate bottom. Cunningham demonstrated that this was due to "a common cause acting in all of them, and not indefinite 'variation'" and that the "common cause was access of light to the pigmented areas"[115] through such ingenious tests as dropping the smallest flounder onto a piece of dry slate, which left a wet mark of exactly the same shape as the unpigmented area of the flounder; and placing that same flounder on a piece of photographic paper that was then exposed to light, and when developed showed that light had reached the paper in exactly the areas where the flounder showed pigmentation on its lower side.

This piece of "additional evidence" in support of Cunningham's discovery that light could induce pigmentation in flounders is as well a strong piece of historical evidence that Cunningham's work while at the MBA was all of a piece. Though the distinct questions of fish growth and induced colouration might be of interest to quite

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[114] J. T. Cunningham, "Additional Evidence on the Influence of Light in producing Pigments on the Lower Sides of Flat Fishes," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1897): 53.

[115] Cunningham, "Additonal Evidence," 54.

different groups, and might easily be said to represent the applied and pure parts, respectively, of Cunningham's research, this easy separation in principle becomes very difficult to maintain in practice. In this case, "[t]he fish were simply reared in order to see their growth, and it was not supposed that any conditions affecting pigmentation were present," and yet as Cunningham reflected, "it certainly seems that the pigmentation was produced more constantly and more rapidly in the unintentional experiment here described, than in those recorded in the previous memoir, which I took so much trouble to arrange."[116] The laboratory setting, the various tanks, the fish populations, the methodology of "tracing", the emphasis on variation, and the epistemological concerns generated by the laboratory setting were all largely shared between these two major projects of Cunningham's. Both kinds of work required very similar conditions in order to thrive. We might well ask whether any distinction between them is worth the effort of upholding.

[116] Cunningham, "Additional Evidence," 57.

Chapter 5

Organism and Commodity: Marketing Marine Organisms and Marine Biology

Introduction

In 1880s and 1890s Britain, food fish enjoyed a kind of fame. Transformations in the nineteenth-century British diet and in the mode by which fish were caught combined to make fishing not just a living but a huge industry, and to make the fish themselves not just a group of animals but an economically important commodity. As food fish became more and more indispensable, and the use of steam-driven trawlers became more widespread, people became increasingly worried about the future of this new commodity, and hungry for any information about fish. The source of this new information about fish and fisheries was the newly-prominent science of marine and fisheries biology. In previous chapters, we have seen how the economic importance of the fisheries, and the ensuing anxieties over the future supply of fish, were a powerfully motivating force, playing a huge role in the establishment of marine research laboratories in Britain, shaping the kinds of research that was expected to emerge from these laboratories, and aligning the interests of naturalists and fishermen in a way that allowed workers in these laboratories access to a range and quantity of specimens far in excess of what their mere proximity to the seashore alone could warrant. Had food fish remained merely fish, just another group of organisms interesting to science, the structure of marine biology in Britain would have looked quite different. It was the transformation of fish into food, into a commodity vital both for its support of the national diet and its support of an increasingly large-scale fishing industry – the commodification of these organisms – that largely shaped much of what we've seen in previous chapters, and in particular made itself felt in the career of J. T. Cunningham. What makes this period distinct from the century and more that followed it is the newness of the realization that food fish were a commodity, a product that the country couldn't do without, and the fears that under current practices, they might someday have to. In addition to these considerations, there was simply the issue of fashion: the huge successes of the Edinburgh and London Fisheries Exhibitions gave food fish a kind of celebrity, a currency, that made other information about them more likely to be enthusiastically consumed by the public.

In this chapter, I want to examine this notion of the commodification of fish, taking my cue from the title of Cunningham's first book, *Treatise on the Common Sole, Considered Both as an Organism and as a Commodity.*[1] What might it mean to consider an organism as a commodity? Cunningham's title was motivated by his own work situation, in which he had always to consider fish as both an object of scientific investigation and an object of public consumption. In what different ways can organisms be commodified; in what different ways can they be seen as an object of consumption? My own approach in this chapter will also be informed by notions of fish as an object of consumption. I once saw a cooking show in which the featured dish was called "Tuna, Three Ways". I want this chapter to be a dish called "Food Fish, Three Ways": three linked but separate examinations of the notion of commodification of organisms, two historical and one historiographical.

In the first, I explore how the fact that food fish were an object of public consumption made it possible for fish *biology* to itself become publicly consumable. Both Cunningham and M'Intosh, in addition to feeding the public hunger for information

[1] J. T. Cunningham, *Treatise on the Common Sole, Considered Both as an Organism and as a Commodity*, (Plymouth: Marine Biological Association, 1890).

about food fish by giving public lectures, produced two books explicitly intended for a broader audience than the kind of scientific readership that would have had access to their work through the *Journal of the Marine Biological Association* or the *Proceedings of the Royal Society of Edinburgh*. Cunningham's books, the aforementioned *Treatise on the Common Sole* and his 1894 book *The Natural History of the Marketable Marine Fishes of the British Islands*, were produced by the Marine Biological Association in order to communicate to the public the content of marine biology, and its relevance to the fisheries. M'Intosh's books, *The Life-History of British Marine Food-Fishes* (1897, written with his assistant A. T. Masterman) and *The Resources of the Sea* (1899), were created in order to reach the popular audience, and to take M'Intosh's message of the indestructibility of the fish stocks directly to those who were most ready to believe it.

In the second, I return to my discussion in the previous chapter of Cunningham's attempts to gain control over his fish by setting up aquarium systems, and this time examine the practical aspects of this control, and the relationship between commodification and control. The Marine Biological Association aimed to become, in addition to its other projects, a mail-order supplier of marine organism specimens for research and teaching purposes, and to do so it needed to have a wide range of organisms available upon command – "in stock", as Cunningham put it with reference to his own laboratory fish stocks. As well, the prospect of transforming the fishing industry from something resembling hunter-gathering to something resembling agriculture had not escaped the MBA's, or anyone else's attention, especially given the recent successes in America with fish hatcheries. Turning fish stocks into something like livestock was another possible act of commodification, one requiring what might be called *in* *vitro*fication, taking fish out of the ocean for at least part of their lives in order to control their conditions of life.

In the third, I examine a historiographical trend that has been prominent in the history of biology for the last decade, an approach known as "animal studies" or "model organism" history. This approach, which has focused primarily on the role of experimental organisms and the transformation of a select few species – the fruit fly *Drosophila*, the mouse, and the roundworm *C. elegans* – into model systems or model organisms, has told the story of twentieth-century laboratory biology by focusing attention on the organism itself, as both historical actor and object of historical and scientific study. I'll consider several aspects of this school of thought. Is it a good fit with my story? Is there any sense in which food fish (flounders and sole, for instance) in the hands of someone like Cunningham can be seen as model organisms? Or did the practical commitments to advancing the fisheries made by the MBA or by M'Intosh and the Fisheries Board for Scotland make the possibility of modeling, of using one species to stand in for another, unacceptable? What does it mean to call something a model organism? I will explore a second and I think less often considered aspect of this idea: a model organism as a well-behaved, if not exactly domesticated, organism. In this sense, my study has something strongly in common with this approach. I have already examined the ways in which the goals of an organization like the MBA, its sponsors, its naturalist, and his specimen suppliers needed to remain aligned in order for scientific work to take place. In this chapter, I examine another link in these chains of allegiances, the fish themselves, and explore commodification as a process of establishing reliable access to, and control over, the organism in question.

Marketing Food Fish Biology

William Carmichael M'Intosh was a man of considerable reputation, not only in St. Andrews, the Scottish town in which he was born and where he worked as Professor of Natural History at the University, but within the communities of British fisheries science and marine biology as well. His involvement in the 1882 Royal Commission to investigate the effects of trawling had raised his profile among the general public, as well – but that was not the only way his name and reputation spread among the public. One of the things that M'Intosh was well known for within St. Andrews was his incredibly large collection of drawings of marine species, and even more so his enormous collection of preserved specimens. The fame of this collection of spirit preparations, which made an annual public appearance at the Professor's yearly introductory natural history lecture, attended by all those registered in the class as well as everyone who was anyone in St. Andrews, was not limited to St. Andrews, and indeed helped to spread M'Intosh's reputation to the public across Scotland. An article on "Our Scottish Food-Fishes" in the *St. Andrews Citizen* dated November 17, 1888 reports that M'Intosh had sent two cases of specimens to the International Exhibition being held in Glasgow. To be found "in the Chemical Section, just opposite the exhibit of Messrs Coats of Paisley" were over three hundred bottled specimens of marine fish, including "the floating eggs of the flounder tribe too numerous to mention; the young stages of the same tribe, and especially of the fluke, dab, turbot, 'witch', and plaice."[2] Some six hundred of M'Intosh's jars of specimens illustrating the developmental stages of food-fish made the trip to the Sea Fisheries Exhibition of the Imperial Institute, an exhibition to which the MBA and

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[2] A1, "Our Scottish Food-Fishes", *St. Andrews Citizen* 17 November 1888, [18].

Lancashire Fisheries Committee also contributed "interesting exhibits of a similar kind".[3] The British public was keenly interested in its food fish, perhaps more than ever before or since, and M'Intosh's specimen collection allowed them easy access to this object of interest. Not only could the attendees of the Exhibitions view a wide range of food fish species, and especially many species of the monstrous-looking but delicious flat-fish family, but M'Intosh's collection allowed them to view the development of each species, each stage frozen in time and the whole developmental series laid out in a series of jars of preserved specimens, lined up as though along a pantry shelf for the consumption of the assembled public.

As well as sending the preserved specimens to meet the British public themselves, M'Intosh also brought the food-fish to the public in the form of lavishly illustrated lectures. On the same scrapbook page as the report of the Exhibitions is pasted an undated newspaper account of M'Intosh's address to the Natural History Society of Glasgow, with E.E. Prince in attendance, on "The Development and Life-Histories of Some of the Food-Fishes," his discussion of types of fish eggs, developmental changes, nutrition, and colouration illustrated by a "very extensive series of lantern views" of stages of fish development.[4] M'Intosh delivered what was likely a similar lecture on "The Life-History of a Marine Food-Fish" on February 1, 1889 as a Royal Institution Friday evening lecture for members and their families,[5] and spoke again on January 28, 1891 for the Gilchrist Educational Trust lecture at the Greenock Philosophical Society (promoted as "Science Lectures for the People") on "The Development of Fishes (Illustrated by the

[3] A1, undated newspaper clipping, [61].
[4] A1, undated newspaper clipping, [16].
[5] A1, "Royal Institution list of Friday evening lectures," 17 December 1888, insert between [13-14].

Oxy-Hydrogen Lantern)."[6] Cunningham, too, was accompanied by numerous drawings of developing fish when he delivered public lectures in Plymouth and the south-west of England at such places as the Athaeneum and the Mechanics Institution.

Both men eventually expanded their commitments to popularizing their brand of fisheries-directed marine biology: in the 1890s each published two books directed at the same interested publics that would have attended their lectures. M'Intosh and Cunningham's successes as popularizers is suggested by a comment made by the author of another, somewhat later, popular work on the fisheries with some attention to fisheries biology, F. G. Aflalo's 1904 book *The Sea-Fishing Industry of England and Wales: A Popular Account of the Sea Fisheries and Fishing Ports of Those Countries.*[7] Aflalo's book, which includes chapters on "Life in the Sea" and "Scientific Investigations", mentions in the introduction that

> Important semi-popular works on the natural history of our sea fish, like those of Professor McIntosh and Mr. J.T. Cunningham, have doubtless reached a large number, but so much can scarcely be said for the very admirable publications of the Scotch Fishery Board, the Marine Biological Association, or the Permanent Int'l Council for the Exploration of the Sea.[8]

It was true that a member of the interested but non-scientific public might not have made much progress trying to learn about fish biology from these latter three works. The *Journal of the Marine Biological Association* was distributed to MBA members and subscribing libraries, and only appeared once every couple of years; the Fishery Board for Scotland's annual "blue books" were densely scientific and full of large tables; and

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[6] A1, lecture announcement pamphlet and newspaper clipping,. [70]. A1, newspaper clipping, [53].
[7] F. G. Aflalo, *The Sea-Fishing Industry of England and Wales: A Popular Account of the Sea Fisheries and Fishing Ports of Those Countries*, (London: Edward Stanford, 1904).

the publications of ICES tended also towards density, and towards physical measurement of factors like salinity and currents more than towards the biology of the fish themselves. Cunningham's and M'Intosh's popular works were something else entirely: accessibly written, beautifully illustrated with colour plates and, in M'Intosh's case, with some photographs as well, presenting the reader with information not just about the food fish in question but also about the places of scientific work where this information could be found out. Elsewhere, I have argued that acts of popularization have multiple purposes.[9] Popularization functions to disseminate information from the scientist who has it to the public that wants it, certainly. But, especially in fields of study that are relatively new, as was fisheries marine biology in Britain in the 1890s, popularizations do another job: they popularize the particular kind of scientific activity associated with the topic being popularized (as distinct from the knowledge that activity produces). Acts of popularization can take a wide range of forms: public lectures, articles, books, courses for fishermen such as those that Cunningham sometimes ran, tours of laboratories such as those that M'Intosh gave to fishermen in order to interest them in supplying him with specimens, charging admission for public access to view the laboratory aquaria as was the case at the MBA Laboratory at Plymouth. All of these acts reward the effort on the

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[8] Aflalo, *The Sea-Fishing Industry of England and Wales*, x.

[9] Gillian Gass, "Harold Clayton Urey, The Miller Experiment, and the Dissemination of Knowledge and Disciplines," Paper presented to the Joint Atlantic Seminar for the History of Biology, Rockefeller University, New York, NY, April 2003. Traditional models of the popularization of science, which see popularization as a simplification and a corruption of scientific information as it is passed from scientists to the public, have undergone some scathing and very useful criticism of late. Both Richard Whitley and Stephen Hilgartner have argued convincingly that popularization – which is I think more usefully called "dissemination" – functions to allow groups of scientists to communicate with one another, and that communication is not merely one-way: popularizations "feed back" to their place of origin in interesting ways, and scientists engage with popularized accounts of many kinds, and "these shape their beliefs about both the content and the conduct of science." (Stephen Hilgartner, "The Dominant View of Popularization: Conceptual Problems, Political Uses", *Social Studies of Science* 20 (1990): 522; see also Richard Whitley, "Knowledge Producers and Knowledge Acquirers: Popularization as a Relation Between Scientific Fields

part of the popularizer by raising the public profile of the particular type of scientific activity in which the popularizer is engaged, as well as raising the public profile of the popularizer him- or herself. Popularization is a way to build up new scientific disciplines or fields of study, and implicit in or mixed-in with the presentation of information about the natural world is an argument for the merits of the discipline. Popularizations are also opportunities for recruitment of new practitioners and supporters of the science being popularized.

We have seen the efforts of the MBA to legitimate their kind of marine biology in Britain by linking it to practical outcomes; Cunningham's popular works were another forum in which to make these arguments. To this view, I also want to add another function of popularizations: they participate in a kind of positive feedback loop that functions to raise further the public profile of the objects of scientific study themselves. The public appetite for information about, in this case, food fish generated the opportunity for Cunningham's and M'Intosh's books to be published and read; but once published, the existence of these books further raised the profile of food fish as an object of public interest and as a likely topic for further popular works, such as Aflalo's book on sea fisheries.

Although Cunningham was a younger man than M'Intosh (who had had a previous career as a psychiatric physician before becoming Professor of Natural History at St. Andrews in 1882), he was the first of the two to produce popular books on the topic of the natural history of food fish. This probably had less to do with individual differences between the two men, and more to do with the different structures in which

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and Their Publics", in *Expository Science: Forms and Functions of Popularization. Sociology of the Sciences* 9 (1985), edited by Terry Shinn and Richard Whitely, 3-28.)

they worked. M'Intosh, as Professor of Natural History and head of the St. Andrews Marine Laboratory, had far more autonomy than Cunningham, the naturalist for hire. M'Intosh was partially accountable to the Fishery Board for Scotland, who had paid for the establishment of the marine laboratory at St. Andrews, and to the British Association, who provided a small yearly grant to support the laboratory's operation, but his accountabilities were largely discharged by providing scientific material for the FBS annual reports, and a yearly report on the laboratory's activities to the British Association. On the other hand, we have already seen that Cunningham was accountable to and directed by the Plymouth Laboratory's Director and the MBA Council, and that his two books, *Treatise on the Common Sole* (1890) and *The Natural History of the Marketable Marine Fishes of the British Islands* (1894), were produced and presented to the public very much on the behalf of the MBA.

Treatise on the Common Sole was a gorgeous quarto volume, sold for twenty shillings to MBA members and twenty-five shillings to non-members, and published by the Association at a cost of 282 pounds, ten shillings, and eight pence, more than Cunningham's annual salary of 250 pounds.[10] with eighteen full-page colour plates, nine of them striking watercolour drawings painted for Cunningham by Annie Willis (see Figure 9), about which Cunningham said in his Preface that the "beauty, minuteness of detail, and artistic finish of her work are evident enough in the lithographic copies, and I need only add that the drawings were executed from the actual objects under my direct supervision, and that I can answer for their perfect faithfulness."[11] The inclusion of these watercolours functions to enhance the aesthetic appeal of the book's object of study: they

[10] "Report of the Council, 1890-1891," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 85.

are beautiful portraits of the sole in its various parts and poses, and mark Cunningham's book as not just a popular work – since his own line drawings of the sole would certainly have sufficed were this same information published in the *Journal of the Marine Biological Association* – but as one for which no expense had been spared. It was, in fact, a fitting presentation of the sole, which long before the commodification of sea fish in Britain had been a luxury item brought specially to London's Billingsgate fish market for the consumption of the very wealthy, continued at the time of the publication of Cunningham's book to be an expensive and sought-after fish, and still has that status in Britain today.[12] The "common sole" was not especially common.

[11] J. T. Cunningham, *Treatise on the Common Sole*, viii.
[12] It still has this status in Plymouth, certainly, where Dover sole is sold at variable (and invariably very high) market prices at restaurants, and visiting historians have to settle for the lesser lemon sole when they want to go eat a meal of flat fish down by the fish quay. The lemon sole is not actually a sole, but is so called in order to summon the positive associations of actual sole: Cunningham makes mention of this practice in his *Treatise*: "a fish which resembles the plaice and flounder far more closely than it does the sole is sold under the name of merry-sole in Devonshire and lemon-sole in London. This may be partly due to an inclination to enhance its value in the eyes of the consumer." (4)

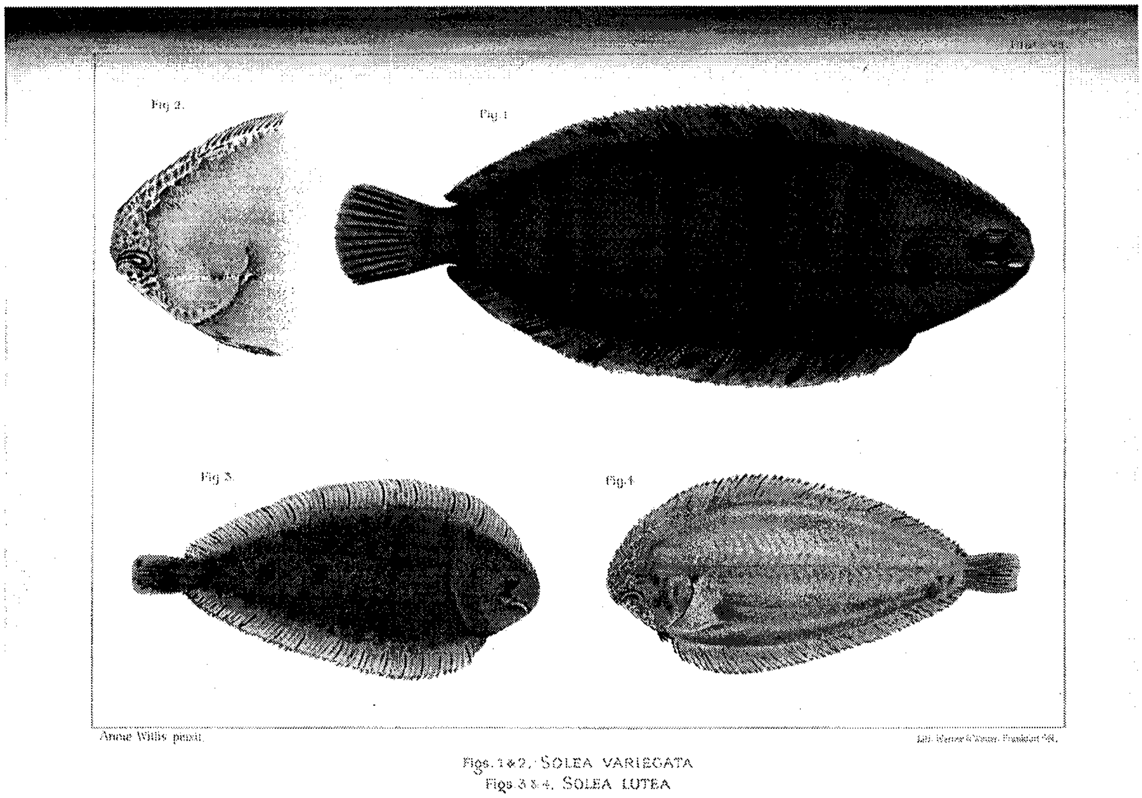

Figure 9. One of the several gorgeous watercolour plates from Cunningham's *Treatise on the Common Sole*, painted by Annie Willis.

Cunningham's book was in many ways a public presentation of the MBA itself. This was the case not just in the presence of "Marine Biological Association" embossed on the book's spine as the publisher or in "Prepared for the Marine Biological Association of the United Kingdom" and "Plymouth: Published by the Association" on the book's title page, but in the content of the book as well. *Treatise on the Common Sole* is a sustained argument for the close linkage between, and indeed perhaps for the inseparability or indistinguishability of, the kinds of work normally identified as "scientific" and "practical". This argument is evident immediately from the title, with its intriguing and revealing "Both as an Organism and as a Commodity"; it is also right away evident from the book's table of contents, which is divided into four sections: "Part I. Taxonomical" (the different kinds of soles), "Part II. Morphological" (the anatomy and embryology of the sole), "Part III. Bionomical" (the distribution, habits, breeding, and development of the sole), and "Part IV. Economical" (covering "Artificial Propagation", "The Sole Fishery", and "Practical Measures"). Cunningham's book, a product of the Marine Biological Association, communicates clearly before the text even begins the message that the scientific work directed by the Association is concerned in some large part with issues of direct practical relevance.

Cunningham's preface elaborates on this message, revealing that "from Professor Lankester I have received much guidance as to the scope of the investigations and the plan of the book,"[13] and making clear that the relationship between the scientific and practical work presented in the book and done at the MBA Laboratory is not even so simple as the division between sections in the table of contents. Beginning with the idea that "[t]he distinction between theory and practice is one that is generally recognised in all departments of human affairs",[14] he then sets out to complicate the distinction, which "has existed since the very commencement of human civilisation."[15] Scientific and practical work might be distinguishable, but they are not and have never been independent:

> To some extent the development of pure science and that of practical science have proceeded independently, but to a large extent they have influenced one another. Practical science has often received a great impetus from the discoveries of abstract inquiry, and pure science has often made enormous strides by the study of the results exhibited by industrial processes. On the whole the tendency of the development of the two is towards a perfect harmony in which the knowledge of the universal interaction of natural forces would completely explain all that takes place in

[13] Cunningham, *Treatise on the Common Sole*, vii.
[14] Cunningham, *Treatise on the Common Sole*, v.
[15] Cunningham, *Treatise on the Common Sole*, v.

> industrial processes, and on the other hand industrial processes would be perfected by the application of a complete knowledge of the mechanism of nature.[16]

We might be able to distinguish between practical and abstract science, but our goal should be to bring them closer and closer together. This is, Cunningham argued, especially desirable when it comes to fish and fishing, since "it must be confessed that the fishing industry has hitherto, in this country at least, not been greatly benefited by the scientific knowledge of fishes hitherto available."[17] The "in this country at least" refers, as Cunningham elaborates, to the successes in Dutch and French oyster culture and American fish hatching, and is an echo of the arguments used by Playfair, Lankester, and the rest of the MBA of Britain's need, especially as a country surrounded by water, to catch up with the rest of the world when it came to practically-directed marine biology. "It remains true," wrote Cunningham next, "that there exists a great deal of scientific knowledge of marine fishes which has hitherto not at all affected the sea-fishing industry. But it is also true that no great endeavours have yet been made to bring science and practice in this direction in relation to one another, and also that our knowledge of the life of marine fishes is in many respects still extremely limited."[18] He did not insist that all scientific knowledge was directly relevant to practical questions of the fisheries, but instead that it cannot be known how much one has to give the other until the two aims, abstract and practical, are brought and kept together.

The tactical advantages of this argument came in for some scrutiny by a later observer. F. G. Aflalo commented in his 1904 popular book on *The Sea-Fisheries of England and Wales* that "It is because the solution of the economic problems is so closely

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[16] Cunningham, *Treatise on the Common Sole*, v-vi.
[17] Cunningham, *Treatise on the Common Sole*, vi.

bound up with a knowledge of the biological conditions that some of those in charge of marine laboratories show reluctance to distinguish between the two branches of investigation. For this uncompromising view of the breadth of marine biological research there is much to be said, but it also has its dangers."[19] These "dangers" related to the possible use of this argument to convince governments to fund work that had no real practical basis or outcome because of scientists "strain[ing] their prerogative and to include in their programme of 'practical' research such academic matters as the structure and functions of the head kidney in teleosteans, the renal organs in crustacea, the accessory visual organ in flat fish, and similar microscopic exercises."[20]

The importance of the sole as a commodity, and the ability of fisheries marine biology as practised at the Marine Biological Association Laboratory to better understand and maintain as a commodity such a significant food fish, were the subjects of Cunningham's book. But the book also achieves another aim: it trades on the fact of the commodification of the sole to make an essential commodity or product out of the science that is associated with the sole. The public was already sold on sole: Cunningham's book uses the image of the sole, and the reading public's curiosity to know more about the sole, to sell the public on the MBA's brand of marine biology.

Cunningham's and the MBA's second, and much larger-scale, effort at book-length popularization of food fish science appeared in 1896 as *The Natural History of the Marketable Marine Fishes of the British Islands*. *Marketable Marine Fishes* was to the sole monograph as the plaice was to the sole itself: cheaper, more common, and far less luxurious, printed in a smaller medium octavo size with lots of simple black-and-white

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[18] Cunningham, *Treatise on the Common Sole*, vi.
[19] Aflalo, 2.

line drawings of the fish being discussed, and sold for less than a third of the price. This book, with its much broader focus on a range of food fish species, was expected to appeal to an even broader audience than Cunningham's first book. Initially, the MBA planned to publish the monograph itself, reporting in 1895 that "Mr. Cunningham has prepared for press a natural history of marketable sea fish, to appeal to a wider public than the Journal can attract, which will also be issued as a special number of the Journal, and reprinted for distribution and sale."[21] The MBA had not come close to breaking even on their previous, deluxe publication of a Cunningham book, and when it developed that his second monograph had "assumed a larger and more complete form than was at first intended, it [was] decided to issue it as a book. The work will be very fully illustrated, and arrangements have been made by which it will be published for the Association by Messrs. Macmillan and Co."[22] By this time, the MBA had become quite strongly established, and was even making use of another research station at Cleethorpes, near Lowestoft on the North Sea, to which Cunningham would be transferred in 1895. The preface to *The Marketable Marine Fishes* was written by Lankester, and was intended to further promote the MBA in particular, and to recruit more practitioners, members, and donors to the cause: Lankester stated confidently that "[i]ts aims are national in their importance and not merely local; it requires and invites the co-operation of all those who are interested in the study of marine life, or in the perilous labours of the sea

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[20] Aflalo, 3.
[21] "Report of the Council, 1894-95," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1897): 83.
[22] "Report of the Council, 1895-96," *Journal of the Marine Biological Association of the United Kingdom* 4 (1895-1897): 304.

fisherman."[23] The book's readers might buy the book for the food fish it contained, but the preface, at least, was selling the MBA:

> The national importance of the sea-fisheries industry is recognized by the legislature. But the right way of developing and directing that industry, and indeed whether it is possible to do anything to improve that industry, are questions which seem still to be matters of doubt to all but the professed students of marine life and its conditions. This book, together with the several volumes of the journal of the Marine Biological Association and the finely illustrated monograph by Mr. Cunningham on the common sole, may be taken as setting forth the results of the work done by the Marine Biological Association in the direction of contributing to a better knowledge of sea-fishes and sea-fisheries.[24]

This passage demonstrates beautifully the value of popularizations not just in disseminating knowledge to the readership but also in recruiting this readership to be supporters of the scientific movement that the popularization represents. This recruitment value was especially vital to the MBA, who had staked so much on the actual and perceived practical value of the research that went on under their auspices. If, until now, only the inner core of marine biologists truly understood how best to develop the fisheries with the guidance of science, now the reader could join in that understanding. Having planted that seed, Lankester next seeks the reader's support in expanding the scale of fisheries science in Britain. Calling for "a larger and much more costly series of investigations...nothing short of a physical and biological survey of the North Sea and of the area within the hundred-fathom line on our southern and western coasts" to be paid for by the government, Lankester says that such a study, along with

[23] Cunningham, *The Natural History of the Marketable Marine Fishes of the British Islands*, (London: Macmillan and Co., 1896), viii.
[24] Cunningham, *Marketable Marine Fishes*, vi.

> the collection (even though costly) of statistical information as to the capture of fish on specified fishing-grounds, which at the present moment is entirely neglected though practicable, are the two requirements of those who desire to improve, and preserve by intelligent action, our fishing industry. I believe that Mr. Cunningham's book will place the reader in a position to appreciate the importance of these requirements.[25]

By reading this book, Lankester both promised and hoped, members of the public would join the ranks of "those who desire to improve, and preserve by intelligent action, our fishing industry." The great power of a popularization is its ability to act as an invitation, from the author (or, the author and the author's even more esteemed colleague) to the reader, to join a like-minded community of experts. To join the experts, it is only necessary to become like-minded, and to become like-minded, it is only necessary to read Mr. Cunningham's book. The main text of the book is a kind of natural historical field guide to the economically important fish species, as well as a source of information on "Practical Methods of Increasing the Supply of Fish", supplemented by one hundred and fifty-nine illustrations and two fisheries maps. *The Natural History of the Marketable Marine Food-Fishes of the British Islands* was a handbook to the hot commodity of the day, combining the interests of the amateur naturalist with those of the concerned citizen into a volume that functioned as both a demonstration of the value of the MBA's mandate and an appeal for continued and further support of their kind of marine biology.

In *The Life-Histories of the British Marine Food-Fishes*, published one year after Cunningham's second book, senior author William Carmichael M'Intosh was also peddling his own brand of fisheries marine biology, the kind practised at the St. Andrews Marine Laboratory, of which he was the head. The St. Andrews Marine Laboratory, established by the Fishery Board for Scotland and part of the University of St. Andrews, had been founded in 1884, three years before the MBA's Plymouth Laboratory and in the same year as the Granton station. Throughout his career as head of the St. Andrews Laboratory (which became the Gatty Marine Laboratory in 1896 after a large private donation), M'Intosh maintained that it had been the first marine laboratory in Britain, or at least the first one of any use and with any staying power; he was of the opinion that the organizers of the Edinburgh International Fisheries Exhibition would have done better to direct their profits towards St. Andrews, rather than to the spendthrifts at Granton. M'Intosh's participation in the Royal Commission to investigate the effects of trawling had made him a recognisable public figure, and his profile had been kept up in the years following by his schedule of public lecturing and the presence of his famous specimen collection and illustrations at various exhibitions and public events.

His 1897 book, written with his junior St. Andrews colleague A. T. Masterman, was aimed at those who might previously have found that information about the popular commodity of food fish was from its "strictly technical nature and method of publication inaccessible to many to whom the facts themselves would be fraught with interest."[26] It was, like Cunningham's book of the previous year, essentially a field guide to food fish, a handbook describing the development and features of a long list of economically important fish species; the more practical, fisheries-oriented content appeared in M'Intosh's second popular book, *The Resources of the Sea*, published two years later. Both books were illustrated with beautiful painted frontispieces; *British Marine Food-Fishes* was dressed up as well with twenty densely illustrated plates depicting a wide

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[25] Cunningham, *Marketable Marine Fishes*, vi-vii.
[26] M'Intosh and Masterman, *British Marine Food-Fishes*, vii.

range of fish at various stages of development, and *The Resources of the Sea* featured sketches as well as a series of photographs placed throughout the text, taken by M'Intosh's assistant Alexander Wallace Brown.

In the preface to *The Life-History of British Marine Food-Fishes*, the authors present the book in an interesting way. It was intended, as promised by the title, as a general reference work on British Marine Food-Fishes, certainly, but also as something much more specific – as a product of St. Andrews marine biology: "In presenting this little volume to our readers we do not primarily lay claim to any special elucidation of new facts, but it has rather been our intention that it should constitute a popularised epitome of the results achieved by British and foreign scientific workers at the St Andrews Marine Laboratory and elsewhere."[27] The "and elsewhere" might seem to make the book a simple work of reference that only happens to have been produced by two workers at St. Andrews, but the rest of the preface establishes a greater and greater role for the contributions of St. Andrews marine food fish biology to the sum of knowledge on the topic. Commenting on the book's value to the scientific community, not just to the general public for whom it was intended, M'Intosh and Masterman point out that "in dealing with each species of fish we have in many cases been enabled to add, by a careful examination of the type-collection and the fresh forms at St Andrews, important links in their life-histories which till now have escaped observation."[28] What's more, "[f]rom no other work have we quoted more largely than from McIntosh and Prince's *Researches*, which may be said to have attempted for Teleosteans what the lamented Frank Maitland

[27] M'Intosh and Masterman, *British Marine Food-Fishes*, vii.
[28] M'Intosh and Masterman, *British Marine Food-Fishes*, vii.

Balfour did for Elasmobranchs"[29] – that is, to have been the complete, broadly accepted, and lasting account of the biology of the group. The leading role of St. Andrews marine biology in the general scheme of things is made clear when "one of us" (M'Intosh) reflects on how far marine biology in Britain has come since the days of his early trawling experiments for the Royal Commission "towards the end of 1883. Then the life-history of not a single British marine food-fish was known, at least from observations in our country. In the present work between 80 and 90 species are dealt with, the majority of the important forms more or less exhaustively. The larger share of this work has fallen to the St Andrews (now the Gatty) Marine Laboratory," and the authors give a list of seventeen St. Andrews workers besides themselves who have advanced marine biology in Britain to its current elevated state.

The case for St. Andrews as the capital of British fisheries marine biology is made in 1899's *The Resources of the Sea* as well, not so much in words as through the series of photographs distributed throughout the book. These photographs demonstrate the long standing of marine biology in the community, one picturing "The old (wooden) St Andrews Marine Laboratory, the pleasant home of marine zoologists for twelve years, 1884-1896"[30] (see Figure 10), two picturing the solid-looking stone Gatty Marine Laboratory, [31] and another of "The sailing-boat 'Dalhousie' of the Gatty Marine Laboratory with her 'crew' on board".[32]

[29] M'Intosh and Masterman, *British Marine Food-Fishes*, vii.
[30] M'Intosh, *The Resources of the Sea*, facing page 232.
[31] M'Intosh, *The Resources of the Sea*, facing pages xiv and 242.
[32] M'Intosh, *The Resources of the Sea*, facing page 144.

Figure 10. Photograph of the St. Andrews Marine Laboratory, from W. C. M'Intosh's *The Resources of the Sea*. This is one of several photographs in the book showing the relationship between fishing and scientific work in St. Andrews.

Drawings in the text depict interesting fish species, but the photographs, in keeping with the strongly fisheries-oriented tone and subject matter of *The Resources of the Sea*, instead show that the scientific work in St. Andrews goes on within an active fishing community: fishwives shelling mussels, baiting lines, and carrying fish in a basket;[33] men baiting crab pots and repairing fishing lines.[34] St. Andrews was not, at the time of the book's writing, a trawling community – the book argued strongly against the current policy of the Fishery Board for Scotland which had closed many bays, including St. Andrews, to trawling – but it was down the shore from Aberdeen, a major trawling port that was equal in importance to the great English trawling ports of Hull and Grimsby, and photographs in *The Resources of the Sea* of Aberdeen steam-driven and sailing trawlers, and the great Aberdeen fish market,[35] (see Figure 11) again visually make the connection between the fishing industry and the marine biology practised by the St. Andrews workers.

[33] M'Intosh, *The Resources of the Sea*, facing pages 112, 75, and 90, respectively.
[34] M'Intosh, *The Resources of the Sea*, facing pages 43 and 82, respectively.
[35] M'Intosh, *The Resources of the Sea*, facing pages 210, 78, and 102, respectively.

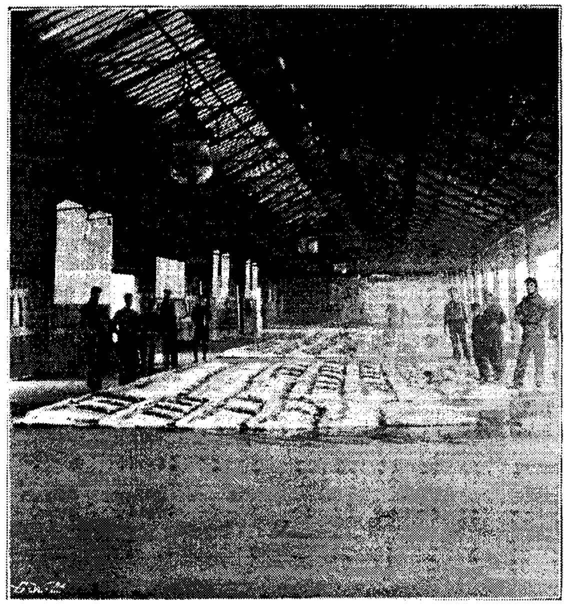

Figure 11. Photograph of the Aberdeen fish market, from W. C. M'Intosh's *The Resources of the Sea*. Aberdeen was Scotland's main trawling port, and the inclusion of this and other images from Aberdeen helped make the case for the relevance to practical fisheries questions of the biological work done at St. Andrews.

The preface to *The Resources of the Sea* is a strange document; it is essentially a list of the names of every important fisheries biological worker of the time, not just in Britain, but in the United States, Canada, France, Denmark, Germany, Sweden, Belgium, Italy, Russia, Spain, Russia, Australia and even Japan, while still maintaining that "In our own country the earliest work was done at St Andrews, which has never lost touch with the subject, and the workers from which, *e.g.* [a list of several names], and others have extended our knowledge of the subject in a noteworthy manner."[36] There are kind words for the workers at Plymouth and those under the Fishery Board for Scotland. M'Intosh was invoking all of these allies, I think, because he was about to make a very controversial argument, and make it directly to the public. In *The Resources of the Sea*, M'Intosh was trading on the popularity of food fish to sell the public not just on St. Andrews-style marine biology, but also on a set of arguments about the nature of the sea itself, the inability of man to influence what went on there, and the senselessness of the restrictive policies implemented by the Fisheries Board for Scotland. The details of his pitch, the style of his arguments, and his opposition to the mainstream view represented by the likes of the Marine Biological Association will be the topic of the next chapter.

I will now turn back to the Marine Biological Association for the second of my inquiries into the commodification of marine organisms: how the goal of a ready, controlled supply could be shared by science and industry alike.

The MBA Enters the "Specimen Trade"

In Chapter 3 I developed the notion of a specimen supply chain, the set of connections that, once established, both ensured a steady flow of needed specimens into the laboratory and greatly expanded the geographical range, variety, and volume of specimens to which the naturalist had access. I also explored Cunningham's gradual movement towards an in-house supply of specimens, such that specimens likely to be needed could be kept "in stock", greatly increasing the naturalist's control over the specimens and making his access to them easier, while still not doing away with the need to maintain specimen supply chains stretching far beyond the laboratory.

But the demand for specimens of scientifically interesting marine organisms was not limited to the MBA Laboratory. Stationed at the seaside at Plymouth, workers at the MBA enjoyed privileged access to marine organisms, and to the people and boats who could extend their access to marine organisms even further. But not everyone could, or wanted to, travel to the seaside whenever they required specimens of marine organisms for their research or teaching. And many did want marine organisms: the new

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[36] M'Intosh, *The Resources of the Sea*, xi.

prominence of marine biology in Britain, spurred on not just by fisheries questions but probably even more by the perceived importance of marine organisms, both invertebrates and lower vertebrates like fish, in revealing the evolutionary history of vertebrates, created an inland market for marine organism specimens at English university campuses.

During the heyday of the aquarium craze of the 1850s and 1860s, inland demand for marine plants and animals had been high due to the many hobbyists looking to begin or develop home aquaria. Some journeyed to the coasts themselves to collect specimens, but not all specimens were equally easy to collect. Lynn Barber points out that one of the most popular aquarium manuals, G. H. Lewes' *Sea-Side Studies*, advised collectors to arrive equipped with "a largish geological hammer, a cold chisel, an oyster-knife, a paper-knife, a landing-net, a small crowbar, a large, flat-bottomed basket, various phials from the chemist's...and a selection of jamjars."[37] Collecting organisms attached firmly to rock surfaces, and especially the marine plants and seaweeds that were essential to the survival of the aquarium's other residents, required a physical effort far greater than that which had gone into Britain's two previous natural history crazes, for shells and ferns. The work of collecting for aquaria must have taken some of the fun out of a day at the seashore, and it is therefore not surprising that enterprising individuals were able to make a business out of supplying specimens for those building home aquaria. Just as supplying food fish to inland markets required the fish to be caught at the coast, transported inland, and purchased from city-based vendors, aquarium specimens were obtained by aquarium stockists such as William Thompson of Weymouth, friend of the greatest popularizer of home aquaria, Philip Henry Gosse, and sent inland by rail,[38] either directly to the

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[37] Barber, 120.
[38] Barber, 118.

customer or to one of the many aquarium supply shops that sprung up in the cities.[39] With the lapsing of the home aquarium craze, however, the shops closed down and the movement of marine specimens inland to individual hobbyists largely ceased.[40]

As demand among one group of consumers ebbed, however, the demand among Britain's burgeoning population of university-based naturalist-biologists studying marine organisms was only increasing. This demand was partly met in the 1870s with the founding of British biology's home-away-from-home, the Naples Zoological Station. The Station received visiting naturalists from abroad, including many British ones; it also sent abroad multitudes of marine specimens collected from the sea around Naples. The British Association committee responsible for reporting on the Naples Station faithfully presented to the Association meetings each year a long list of which specimens were provided to which biologists in which countries. These lists show us the true extent of the reach of Dohrn's station. Not only was the Naples Station a powerful attractor, bringing in naturalists from all over the world to work there; it was as well a powerful distributor, sending specimens procured by the Station to laboratories and naturalists all over Europe and beyond. Many of these specimens made their way to Britain: the "List of Naturalists to whom Specimens have been sent" presented to the 1885 Meeting of the British Association at Aberdeen records shipments made to H. N. Moseley at Oxford and E. Ray Lankester at London (both foundational members of the MBA), a shipment of several species to the influential Morphological Laboratory at Cambridge, and shipments

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[39] Allen, *Naturalists and Society*, 405. Other suppliers (including the railway companies) provided city-dwellers with seawater, or with the salts necessary to approximate seawater at home. (Barber, 118.)
[40] Barber, 122.

of specimens to such later Plymouth fixtures as Rupert Vallentin and W. F. R. Weldon.[41] British naturalists at inland universities, then, were already able to obtain particular teaching and research specimens from a distant supplier; they were a ready base of customers for a coastal specimen supplier based nearer home, and specializing in species native not to the Mediterranean, but to British waters.

With such customers in mind, the MBA itself set out to become part of another kind of specimen supply chain: it would not just be the end user of specimens obtained by fishermen and Laboratory employees, but instead take and fill orders, functioning as a link between the sea and the inland demand. Like any mail-order business, they published a catalogue: in the 1889-1890 volume of the *Journal of the Marine Biological Association*, there appears a four page "Price List of Zoological Specimens". These "specimens suitable for class purposes and dissection," according to the catalogue, "are kept in stock at the Plymouth Laboratory", and could be "obtained at short notice on application to the Director" for delivery as needed.[42] At the end of the lengthy list of standard specimens, the reader learns that the MBA will also do special orders: "It must be understood that the above does not pretend to be a complete list, even of the commoner species procurable at Plymouth. A more extensive list is given in No. II (old series) of this Journal)."[43] This "more extensive list" is an interesting citation, in that it refers to an article that is a catalogue of specimens only in the general or the poetic sense of the word, and not at all in the commercial sense. The article was a lengthy but

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[41] "Report of the Committee, consisting of Professor Ray Lankester, Mr. P. L. Sclater, Professor M. Foster, Mr. A. Sedgwick, Professor A. M. Marshall, Professor A. C. Haddon, Professor Moseley, and Mr. Percy Sladen (Secretary), appointed for the purpose of arranging for the occupation of a Table at the Zoological Station at Naples," *Report of the British Association for the Advancement of Science* (1885): 471-473.
[42] "Price List of Zoological Specimens," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890), 342.
[43] "Price List of Zoological Specimens," 346.

"Preliminary Report upon the Fauna and Flora of Plymouth Sound", prepared by the Laboratory's first Director, Walter Heape, in an effort to get his and the MBA's bearings as to what kinds of organisms could be found in the environs of the Laboratory.[44] Heape's Preliminary Report was put together by compiling existing organism lists (for instance T. Boswarva's list of local algae), reports by local fishermen who had been willing to share with Heape what kinds of organisms they were accustomed to seeing, and the results of Laboratory collecting trips in the area. But when, a year or two later, the MBA entered the specimen supply trade, Heape's report was repurposed into another, more detailed catalogue of specimens that the interested naturalist could get through the MBA.

Heape's report was not the only one that was reframed into a specimen catalogue. The Price List encouraged prospective customers to consider the *Journal*'s entire contents as an indication of available specimens, offering that "[r]eference may also be made to special reports on different groups published in this Journal."[45] The specimens contained in the Price List, in Heape's list, in the *Journal*'s articles, and indeed the British Coast more broadly could be had through the MBA's specimen supply service: "application for any particular British species should be made to the Director."[46] One way or another, the MBA would obtain the desired specimens and get them to their customers. Most specimens were shipped in alcohol-filled jars; some, such as plaice, could be purchased either preserved or fresh, "in which case the prices will be correspondingly

[44] Walter Heape, "Preliminary Report upon the Fauna and Flora of Plymouth Sound," *Journal of the Marine Biological Association of the United Kingdom* 2 (1888).
[45] "Price List of Zoological Specimens," 346.
[46] "Price List of Zoological Specimens," 346.

reduced."[47] The costs associated with shipment, including packing cases and train fare for the specimens, were extra.

Interestingly, no mention was made in either Directors' or Council reports of the plan to begin selling and shipping specimens prior to the appearance of the first price list. These sales had been going on informally, however: the balance sheet for the 1889 year includes a figure of forty-five pounds thirteen shillings of revenue derived from "Rent of Tables, Specimens and Journals sold,"[48] and that figure had risen to 185 pounds 5 shillings the following year.[49]

In the issue of the *Journal* that followed the publication of the price list, the Laboratory's interim Director, G. Herbert Fowler, reported (in the process of complaining about the Laboratory's lack of a boat of its own) as "worthy of notice that live and dead specimens have been supplied to nearly every University and College which offers a zoological course to its students, and to many private individuals. With this constantly enlarging sphere of usefulness, however, **the want of a suitable vessel is every day more strongly felt.**"[50] From that point onward, the yearly balance sheets issued by the MBA council listed the revenues from table rents and specimen sales separately, along with another new revenue stream, the admissions paid by the public to view the Laboratory's tank room, which was open six days a week beginning in 1891.[51] Table

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[47] "Price List of Zoological Specimens," 342.
[48] "Report of Council, June 26th, 1889," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 110.
[49] "Report of the Council, May, 1890," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 363.
[50] G. Herbert Fowler, "Director's Report – No. 4," *Journal of the Marine Biological Association of the United Kingdom* 1 (1889-1890): 365. Bold in original.
[51] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 2. Calderwood reported that "The tank room of the Laboratory, from being open to the public only one day in the week, is now open every day (Sunday excepted), a small charge being made for admission. The system is in every way proving a success; the attendance, especially on holidays,

rents and tank room admissions remained relatively constant from year to year, but specimen sales grew rapidly and soon constituted the major source of revenue generated at the Laboratory itself (see Figure 12). (Grants and private donations were by far the greatest source of operating funds for the Laboratory.)

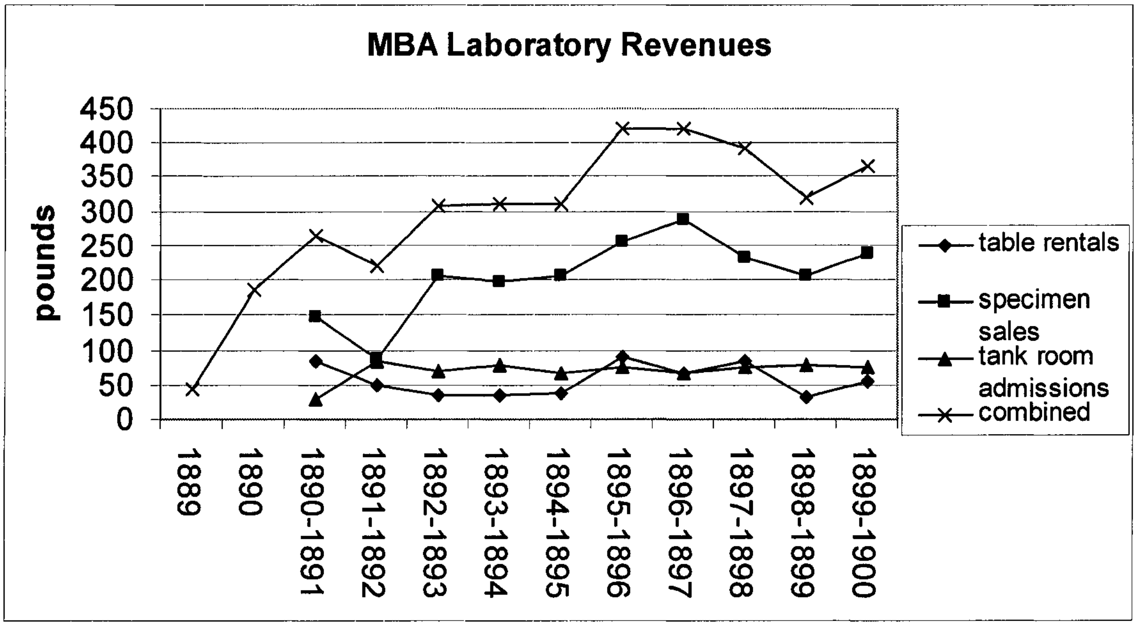

Figure 12. Sources of revenue generated at the Plymouth Laboratory. Specimen sales brought in significantly more money than table rentals and tank room admissions combined. Data obtained from yearly financial reports of the MBA Council, printed in the *Journal of the Marine Biological Association of the United Kingdom.*

As sales increased and both teaching and research on marine organisms in Britain made use of specimens sent from Plymouth, ensuring quality of specimens became a concern. At the end of 1891, Calderwood reported that the

> preservation of specimens has been much more carefully attended to than formerly. One man now devotes almost his entire time to this work. This is necessary, since it is only by constant practice in the treating and handling of delicate specimens that really satisfactory results can be obtained. Our trade in specimens is now becoming extensive, and it is therefore highly desirable, both for the

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remaining all but up to the former standard. Members are of course still admitted at any time free of charge." (2)

> credit of the establishment and for its purse, that the material supplied should be prepared with the utmost care.[52]

The "one man" was Joseph Walker, who kept an eye on the preservation techniques being used by his counterpart at Naples. His was an important job; Calderwood observed in his next Report that the "demand for specimens for use in laboratories and museums throughout the country increases, and requires constant attention. We can supply specimens which, in very many cases, could not otherwise be obtained." The rapid expansion of the Laboratory's specimen supply trade led them to pay more attention to quality of specimens, and the resulting higher quality of specimens led them to increase prices:

> Quite recently a new price list of zoological specimens has been issued. This is the second list, and from the experience gained by the first, it has been found necessary to raise very many of the prices. If, however, the quality of the material produced is better, we do not think that any of the prices will be found to be too high, and it has been our aim to keep the prices of animals most commonly used for teaching purposes, and therefore ordered in large quantities, as moderate as possible.[53]

The specimen supply service initially worked fairly simply: specimen orders would come in, a Laboratory fisherman would go out and catch the desired specimens, which might then be kept in the Laboratory's tanks for a short time, Walker would preserve and package the specimens, and they would be sent out by rail. Relying on this kind of just-in-time system, however, had its risks; a certain number of specimens could, as claimed,

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[52] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 87.
[53] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 210.

be "kept in stock at the Plymouth Laboratory," but as the supply service grew and large numbers of particular organisms had to be sent inland, the reliability of the service became more exposed to the disrupting whims of nature. Calderwood began his next Report in 1892 with what at first appears to have been an innocuous naturalist's observation of the local marine life: the weather had been "magnificent" of late, and lots of "unusually interesting surface forms, and at the same time almost a total absence of some of the creatures usually found in abundance. For instance, Aurelia has for some unaccountable reason not been found in the Sound this summer." Unusually interesting surface forms were well and good, of course, especially from the perspective of naturalists occupying tables at Plymouth; but the absence of the common forms posed a real problem for the specimen supply chain from the Plymouth coast to the inland universities and colleges. "I particularly note the absence of Aurelia," admitted Calderwood in the next paragraph, "since it is a form used as a type in teaching, and therefore often ordered from us. It will be impossible to supply Aurelia through the coming winter."[54]

Being caught without adequate supplies of one of the country's most important teaching specimens (Aurelia, the usually-common "moon" jellyfish, would have been used as the representative of the entire jellyfish group) on hand, combined with ongoing improvements to the Laboratory's ability to keep organisms alive in tanks, probably contributed to a change in the way the specimen supply service managed its stock in trade. The MBA Council report for the 1892-1893 year revealed that a second naturalist, Walter Garstang (formerly Assistant to the Director), was being kept on for another year

[54] W. L. Calderwood, "Director's Report," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892): 292.

specifically "to superintend the collection, preservation, and supply of material. The character of the specimens supplied by the Laboratory has improved very greatly under his care."[55] The work of collecting, preserving, and packaging the specimens was now managed as "the specimen department," based in the "laboratory known as the physiological room, occupied at present by Mr. Garstang."[56] From these quarters, the specimen supply service became even more ambitious. The Laboratory's new Director, Edward J. Bles, reported in 1894 that the specimen trade was expanding:

> After numerous trials it has been found quite feasible to send most of the animals caught in dredge and tow-net alive to any part of the United Kingdom and Wales. Accordingly a new price list has been prepared, giving a selection of the commonest and most interesting animals of each group, with two prices for each; one for the living animal, and the other, slightly higher, for the preserved specimen. By giving ample notice teachers and others can always rely on obtaining certain living animals on certain days, as we can secure them in advance when necessary and keep them alive until wanted.[57]

This expansion marked a significant shift: not only could animals be shipped alive, rather than the previous two choices of preserved or fresh dead specimens, but the Laboratory was willing to use its tank facilities as miniature warehouses, to keep organisms on hand and in stock, to be shipped out just in time to arrive at the teaching or research laboratory on "certain days." Nor was this to be merely a specialty service; both "the commonest and most interesting animals of each group" were made available according to this arrangement, and its users could "always rely" on timely delivery. The specimen trade had moved, with the addition of the live organism option, from a system

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[55] "Report of the Council, 1892-93," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895): xii-xiii.

[56] Edward J. Bles, "Director's Report – No. II," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895): xviii.

of just-in-time inventory to one of just-in-time delivery, with specimens being held in inventory to make reliable delivery possible.

The MBA's venture had become a success. In his confident preface, discussed above, to Cunningham's *The Natural History of the Marketable Marine Fishes of the British Islands*, Lankester could claim in 1896 that the MBA was an organisation that was national in scope and importance, and that its Laboratory at Plymouth "is also the means of providing University and College teachers with large supplies of marine organisms for teaching purposes and special investigations."[58] Reports on the specimen supply service were more often the province of the Director than of the MBA Council, but in 1900 the Council reported that

> [t]he Laboratory has continued to supply preserved specimens of marine animals and plants to the Universities and other teaching institutions, as well as to a number of museums in different parts of the world. Many private workers have also been provided with specially-prepared material for their own researches. This specimen trade pays its own expenses, and is of considerable advantage to biological science in general, as well as to the Association.[59]

And the trade kept growing. In his 1910 survey of the European biological stations for the United States Board of Education, Charles Atwood Kofoid reported that in 1908 the Plymouth Laboratory had earned four hundred pounds selling specimens, almost four times as much as had been taken in by admission fees to the Laboratory's public aquarium and fully forty times as much as sales of its publications.[60] Nor were these specimens sold only to distant buyers otherwise lacking access to the sea. Visitors to the

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[57] Bles, "Director's Report – No. II," xix.
[58] Cunningham, *Marketable Marine Fishes*, viii.
[59] "Report of the Council, 1899-1900," *Journal of the Marine Biological Association of the United Kingdom* 6 (1900-1903): 285.

station were customers as well: "The association does not permit naturalists to make zoological collections at the station, but provides these for sale at fixed prices. Price lists are sent on application."[61] Soon after initiating their role as marine organism specimen supplier to the nation's naturalists, the MBA succeeded in making a commodity out of these organisms. Their achievement is made more interesting because the very existence of the Plymouth Laboratory had been so heavily contingent on the previous commodification, as food, of at least some of these same marine organisms, which had also become important commodities with the rise of an inland market hungry for fish. This dissertation has concentrated greatly on the ways in which the MBA endeavoured to prove itself useful to the nation's fisheries; at the same time, the Laboratory needed to appear relevant and useful to the nation's biologists. What Fowler called the Laboratory's "sphere of usefulness" to biology had much to do with its role as specimen supplier to the nation. The revenue generated for the Laboratory by the specimen supply service, though much greater than that earned by table rents and aquarium admissions, was still far less than that brought in by grants and donations. But the service was greatly profitable in other ways. In particular, the service made the Plymouth Laboratory indispensable to British biology in particular, as a reliable source of specimens for education and for research.

In running such a specimen supply service, though, the MBA and its Laboratory were taking a risk. They were promising their customers, among them many teachers of courses who were on a schedule of instruction, that the specimens the customers wanted were "kept in stock at the Plymouth Laboratory", with the implication being that the

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[60] Kofoid, 150. The Laboratory had spent only fifty-six pounds purchasing specimens in the same year.
[61] Kofoid, 152.

Laboratory was a kind of specimen warehouse, and shipping out the desired specimens was as simple as pulling the appropriate number of them off of the shelf – or, in this case, out of the tank. Such a promise meant that the Plymouth Laboratory had to be able to keep a range of marine organisms alive in their indoor tanks, as well as be able to know where to go or who to ask if a particular specimen had to be collected from the wild.

As we have seen so often already, Cunningham's work as Naturalist in the MBA Laboratory served these purposes as well as those of his own research and the Laboratory's mandate for research of practical value to the fisheries. His struggles to get the Laboratory tanks up and running were in support of his own research projects, of his role in establishing the tank room as a public aquarium, and of the MBA's need to have a reliable supply of specimens to ship inland; his own efforts to have necessary research specimens "in stock" paralleled the need for the Laboratory to act as a specimen warehouse for its supply service; his own need to cultivate supply chains among the fishermen and amateur naturalists in and around Plymouth was aligned with the MBA's need to keep its promise that all of the species listed in Heape's report were available for special order by the clients of their supply service.

Moreover, the very notion of keeping food fish species in particular "in stock" had a great deal of appeal for those interested in the upkeep of the fishing industry. In *Treatise on the Common Sole*, having concluded from the scanty statistics available that the sole population appeared to be decreasing, Cunningham turned to a chapter on "Practical Measures" by which this population might be protected and enhanced. "The question we have here to consider is: Can soles be made more plentiful by measures which are not only possible but practicable, that is, which are sufficiently easy to be carried out, when they are understood and have become familiar, without a degree of exertion and expense too great in comparison with the results gained?"[62] he asked, and answered that these measures would either involve leaving the soles alone or helping the soles along. Cunningham's views on the former will be treated briefly in the next chapter. The latter of these "two ways in which human action can be applied to a valuable wild animal"[63] tied in nicely to the goal of keeping marine organisms *in vitro* that Cunningham and the MBA were already working on for other purposes. Helping such organisms along would involve, Cunningham wrote in the *Treatise*, "keep[ing] the animal under entire control by taking it into captivity: this may be called domesticating the animal, the word domestication being used with a wide signification, and not necessarily implying the taming of the animal."[64] Domestication meant control, "entire control" of the organism's food and environment for at least part of its life; this kind of control meant removing the fish from the sea to live in tanks or protected pools. It was the vulnerable young stages of food fish that would benefit most from living under such "entire control"; in the case of soles, Cunningham reported that "[t]he [Marine Biological] Association is now making arrangements to keep young soles in a large piece of enclosed sea-water at Sheerness."[65] However, maintaining the population of soles in the sea – and thus allowing the existing fishing industry to continue working as it currently did – was the real aim.

The advanced state of fisheries biology research in America was often mentioned by British scientists interested in the topic. One of the most envied results in America

[62] Cunningham, *Treatise on the Common Sole*, 142.
[63] Cunningham, *Treatise on the Common Sole*, 142.
[64] Cunningham, *Treatise on the Common Sole*, 142.
[65] Cunningham, *Treatise on the Common Sole*, 147.

was their success with artificial propagation of economically important fish through the use of hatcheries, which protected fish during their vulnerable larval stage, and then released them into the general population where they could be fished as usual. Using hatcheries, American fisheries scientists had successfully propagated the salmon. In his *Treatise on the Common Sole*, Cunningham discussed the American success with hatcheries, asking rhetorically,

> If during the close season pisciculturists are permitted to catch a number of the spawning salmon in the river, strip them of their eggs and milt and hatch them in the most perfectly arranged hatchery, and then turn the fry at a certain stage again into the river, what will be the result? If a greater percentage of eggs can be hatched under artificial conditions than under the natural, then of course the result will be to increase the number of fry produced in the river.[66]

Hatcheries for river-spawning fish were a fairly simple matter, however, and had been operating in Europe since the mid-nineteenth century, the depletion of fish populations in bodies of fresh water having become apparent much earlier. The concern in Britain, and for Cunningham's discussion of soles especially, was the depletion of sea fish. It was another American fisheries biology success, with the estuarine species the American shad, that was really impressive, and which it was hoped could be duplicated in Britain with important sea fish such as soles and plaice. The biology of the shad was such that restricting fishing seasons was not a workable option: "a close season would have meant prohibiting the fishing altogether," commented Cunningham.[67] Of the two classes of human intervention described by Cunningham – leaving the fish alone or helping the fish

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[66] Cunningham, *Treatise on the Common Sole*, 145.
[67] Cunningham, *Treatise on the Common Sole*, 145.

along – the first was not a possibility, and the American Fish Commission had instead pursued the second. Cunningham described their actions approvingly:

> The pisciculturists, under the direction of the Fish Commissioner, therefore first invented methods and apparatus by which the eggs of the shad could be artificially fertilised and hatched, and then they organised a system under which every year a large number of the ripe fish captured were stripped of their eggs and milt which were returned either to the same river or others in the form of healthy fry. The consequence is that now the estuaries on the Atlantic side of the United States yield annually a rich harvest of adult shad, and this valuable fishery is absolutely dependent on the piscicultural operations of the National Fish Commission.[68]

It is easy to see what might be appealing about this type of system if it could be applied to Britain's marine food fish species. As Cunningham commented, "It is almost impossible to interfere with trawling,"[69] and this system would not require that the fishing industry itself make any structural changes: trawlers could still put to sea and collect fish in their nets and bring them to market. Fishing would continue to resemble hunting, and would not have to be transformed into something like agriculture. Secondly, and very appealingly for Cunningham, the MBA, and fish biology generally, adopting a system for hatching British sea fish as the Americans had used for shad ensured a permanent role for science in the fishing industry, the kind of science that marine biologists were doing anyway: finding ways to fertilise, hatch, and keep alive various fish species. True, admitted Cunningham,

> the expense of providing properly equipped hatcheries and maintaining a staff of men who would collect the eggs and take care of them during development would be very large.

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[68] Cunningham, *Treatise on the Common Sole*, 146.
[69] Cunningham, *Treatise on the Common Sole*, 146.

> But if this expense were provided from the public funds, some return for it would be received by the public in the form of more abundant and cheaper soles. Whether the national account on this item would result in an annual profit or an annual loss I am not yet prepared to say.[70]

Cunningham also wisely avoided saying that it might have been worthwhile to take an annual loss on the national account in order to maintain the trawling industry at its current state, in which it provided what had become a dietary staple to all classes and employed a great number of people both directly and indirectly. The transition from lining to trawling, and from sail trawling to steam trawling, had made the fishing industry more and more capital intensive and had become the engine of rapid growth in port cities like Aberdeen, Hull, and Grimsby. A collapse or even a winding-down of the trawling industry would have far-reaching economic consequences. Such economic effects were even a consideration when it came to considering limiting the activity of trawlers through closed areas or seasons: "If trawling were prohibited who would compensate the fishermen and capitalists engaged in the business?"[71]

There was another option that might help maintain the sole population, which would cost less but would mean giving up some control over the young fish, as well as requiring more responsibility and work by the fishermen: "It is conceivable," Cunningham commented, "that the ripe females and males captured by the trawl during the spawning season should be stripped by a man on board a trawler, and the fertilised eggs so obtained should be simply thrown overboard."[72] But what kind of man? "It would scarcely be possible to send out a trained pisciculturist in every trawler."[73] But it

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[70] Cunningham, *Treatise on the Common Sole*, 146.
[71] Cunningham, *Treatise on the Common Sole*, 147.
[72] Cunningham, *Treatise on the Common Sole*, 146.
[73] Cunningham, *Treatise on the Common Sole*, 147.

might be possible to train a pisciculturist on every trawler: that, "in imagination at least, ... the skipper of every trawler should carry out the necessary operations, though it would probably be a long time before trawling skippers would possess the necessary knowledge or care sufficiently for the future."[74]

Under this system, too, there would be a permanent role for men of science like Cunningham, to do for every trawler skipper what Cunningham was already doing with Captain Prouse of the Plymouth trawler *Lola*: training the men how, and why, to perform artificial fertilisations at sea in the midst of all of the other work of fishing. "If such operations were expected of them," wrote Cunningham of the trawler skippers, "they would have to be trained in practical pisciculture, and pass an examination in that subject as they are now required to pass in seamanship."[75] Cunningham was already accustomed to giving such training, and to teaching to fishermen more generally on such topics as preserving nets and culturing oysters. In both their training and monitoring capacities, scientific workers were nearly as indispensable to this system as they were in a system of controlled fish hatcheries. Again, though, such a system would require a change to the fishing industry itself, requiring more training and more work, as well as more monitoring for compliance, of the trawlermen, and all of this would cost money as well. It is no wonder that sea fish hatcheries seemed an appealing option. This option was not lost, either, on Cunningham's previous boss, Professor Ewart now of the Fisheries Board for Scotland, which opened a plaice hatchery at Dunbar, Scotland, in 1892, which was by 1894 releasing twenty-six million plaice larvae into the sea each year.[76]

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[74] Cunningham, *Treatise on the Common Sole*, 147.
[75] Cunningham, *Treatise on the Common Sole*, 147.
[76] Cunningham, *Marketable Marine Fishes*, 30. The Dunbar hatchery will be the subject of my postdoctoral research next year.

In her recent history of the standard laboratory mouse, Karen Rader uncovered another such specimen trade, this one trading entirely in live animals. Established somewhat unwillingly as a source of emergency funds in 1930, C. C. Little of the Jackson Laboratory in Bar Harbour, Maine began informally selling mice to the Public Health Service. As the trade expanded to six thousand mice per year and beyond, Little in 1933 issued a price list cataloguing not, as the MBA had done, the many species for sale – there was only the one species, though in ten varieties – but such features as the stock's homogeneity and tendency to grow tumours.[77] But what began as "a small 'cottage industry' designed to keep the institution going"[78] caused a major expansion in the amount of space and work that went into maintaining the inventory of saleable mice. We saw a similar expansion in the MBA's case, but on nothing like the transformative scale of the Jackson Laboratory, which became less and less a place of active research, and assumed an ever more important role as the mouse warehouse for America's biomedical researchers.[79]

The MBA's specimen supply service succeeded and grew, but did not scale up to the point of transforming the host institution from a site of research to a site of production, as Rader argues happened at the Jackson. Much of the work associated with the Jackson mice – feeding, housing, breeding, and raising thousands of specimens – was literally offshored in the case of the MBA specimens, which in most cases could be collected in quantity from the wild and housed only temporarily in the Laboratory's tanks. Customers ordering marine specimens for a class dissection or a histological study

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[77] Karen Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955*, (Princeton: Princeton University Press, 2004), 131.
[78] Rader, *Making Mice*, 100.
[79] Rader, *Making Mice*, 133-134.

did not require the kind of control over genetic background that was essential to the experimental disease research undertaken by the twentieth-century biomedical researchers who used Little's service. The work of collecting, maintaining, preserving, and shipping the specimens – and the work of supervising all of these tasks – did, as we have seen, become large enough to require its own naturalist. After Garstang left, the Director, Edward Bles, reported that "[t]he specimen trade has since his departure been in my hands. I am glad to report that it is increasing rapidly; to such an extent, in fact, that it is interfering with a higher phase of my duties, the investigations which have been slowly growing under my hands."[80] Bles' successor, E. J. Allen, who became the first Director of the Laboratory to stay on for any considerable length of time, never mentioned the specimen trade, the proceeds of which continued to appear in the annual balance sheets, in his regular reports in the *Journal*, which focused almost entirely on the Laboratory's research activities.

It is unfortunate that in Rader's otherwise excellent chapter on the Jackson mouse trade, she does not devote any time to examining the notion of a "commodity" and the associated process of "commodification", despite the word's appearance in the chapter's title. It is an interesting notion, especially in the scientific context, and could do with some examination. A commodity is a product, usually something that there is a lot of, and each individual instance of that commodity is very like each other individual instance of that commodity: when paying six hundred dollars for an ounce of gold, one expects that every ounce purchased is alike, and like the ounces of gold purchased by everyone else at that price. This is the sense of commodity that has the most value for Rader, I

[80] Edward Bles, "Director's Report – No. III," *Journal of the Marine Biological Association of the United Kingdom* 3 (1893-1895): xxvi.

think: mice were commodified in that they could be bought and sold in quantity, and at around the same time they were made into a commodity in that they became standardized such that customers could be sure that every mouse from a particular strain was alike. A commodity, too, is something that some people have and other people want; a commodity is something that can generate trade, the exchange of things between people and groups; and a "hot commodity" can generate not just trade but excitement, publicity, and concern. This is the sense of commodity that is most useful for my purposes: commodities connect groups within economies. When the same objects function as a commodity in more than one economy, they serve as a connection between these economies and some of the groups within them. Marine organisms were a valuable commodity in two late nineteenth-century British economies. In the emerging economy of biology, they were sought after and exchanged as objects used in research and teaching, forming the basis of many scientific careers; in the booming economy of the fisheries, they were sought after and exchanged as food and formed the basis of many livelihoods. These two economies shared a common currency in marine food fishes such as plaice and sole (as well as invertebrates used as food and bait, and recognized as the food of the food fish), and this common currency, under the right conditions, connected marine scientists to fishermen and to government. Food fish were a hot commodity, one that in the broader national economy had become important – indispensable, really – and increasingly a source of worry about the stability of the supply. Fisheries-oriented marine biology of the type practised and promoted by the managers and workers at the Plymouth and St. Andrews marine laboratories promised to maintain the stability of the food fish commodity, and in so doing established itself during the late nineteenth century as important, and perhaps even indispensable.

Rader's study of mice is in large part the story of how mice became indispensable to biomedical research, particularly cancer research. Another study a decade previously, undertaken by Robert Kohler, showed how the fruit fly *Drosophila* became indispensable to the study of genetics.[81] Both explore, among other things, the ways in which organisms can function as currencies in particular scientific economies; Kohler's and Rader's works are the two most successful examples of an organism-oriented approach to the history of biology.

Animal Studies: Organisms as Historiographical Commodity

During the 1990s, there appeared a rash of books and articles in the history of biology that all took a very similar approach to telling their historical stories. Rather than restricting their areas of inquiry to such traditional topics as the motivations and decisions of particular scientists, the development of disciplines, or the roles played by institutions or politics, the authors of these publications focused their attention on a previously overlooked group of actors: the organisms themselves. In particular, the authors paid attention to the kinds of organisms that had become known in contemporary biology as "model organisms", the relatively small number of organisms that form the basis for entire research programmes, the few about whom so much is now known, and which are accepted as stand-ins for the many organisms that have been comparatively neglected by biologists.

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[81] Robert Kohler, *Lords of the Fly:* Drosophila *Genetics and the Experimental Life*, (Chicago: University of Chicago Press, 1994).

Robert Kohler took the most famous model organism, *Drosophila*, and retold one of the history of biology's favourite stories, the Morgan fruit fly laboratory at Columbia University, this time putting the fly in the driver's seat. Kohler showed how the fruit fly's particular characteristics – its easy availability in the wild, its rapid and prodigious breeding, its willingness to live in jars as long as mashed bananas were provided – allowed it to become first a low-status bit player the laboratory, then a major player when it was found to be well-suited to a particular study of evolutionary mutation periods that Thomas Hunt Morgan wanted to do, and then to in a sense direct the work of the laboratory into genetics and mapping when the number of visible mutants became sufficiently large. The key to this transition, Kohler argues, was the process of scaling-up: simply increasing the numbers of fruit flies, combined with the flies' astonishing reproductive capacity, transformed the flies from mere organism of study into what Kohler calls a "breeder reactor", a kind of make-work project that changed the course of work in the laboratory. What had previously been the somewhat clerical work of "working up" new *Drosophila* mutants to determine the inheritance pattern of each type of mutation that occurred in the course of Morgan's experimental evolution study became the main work of the laboratory.

Kohler's *Drosophila*-centred history of Morgan's laboratory was among the first, and is still among the best of this type of history-writing, but there have been many others: Karen Rader on C. C. Little and the laboratory mouse (as discussed), Rachael Ankeny and Soraya de Chadarevian on the roundworm *Caenorhabditis elegans* and Sydney Brenner, Bonnie Clause on the Wistar rat and Henry Donaldson, Doris Zallen on the algae *Chlamydomonas* and *Chlorella* and Otto Warburg, and Larry Holmes on the frog and anyone who ever worked on frogs.[82] As well, philosophers of science interested in biology, such as Richard Burian, Kenneth Schaffner, and Rachael Ankeny, have taken up the notion of the "model organism", as well as the allied notion of "the right organism for the job" that is a feature of much of the historical writing in this style.[83]

There are risks to this approach that go hand in hand with its strengths. Focusing on the organism pays off when it leads historians to focus very closely on laboratory practise and to take a fine-grained, science-in-action perspective; but the notion of "rightness" of a particular organism for a particular type of biological research always risks devolving into Monday-morning quarterbacking, giving the organism and the scientist that chose it credit for every success.[84] If the experiment works or a productive research programme results, it is too easy to assign the causes for that success to the organism itself and lose sight of the structures in which the organism and scientist were operating. Karen Rader has pointed out, too, that animal studies history risks being

[82] Rader, *Making Mice*; Karen A. Rader, "'The Mouse People': Murine Genetics Work at the Bussey Institution, 1909-1936," *Journal of the History of Biology* 31 (1998); Karen A. Rader, "Of Mice, Medicine, and Genetics: C.C. Little's Creation of the Inbred Laboratory Mouse, 1909-1917," *Studies in the History and Philosophy of Biological and Biomedical Sciences* 30 (1999); Rachel A. Ankeny, *The Conqueror Worm: An Historical and Philosophical Examination of the Use of the Nematode* C. elegans *as a Model Organism*. (Ph. D. Dissertation. Pittsburgh, PA: University of Pittsburgh, 1997); Soraya de Chadarevian, "Of Worms and Programmes: *Caenorhabditis elegans* and the Study of Development," *Studies in the History and Philosophy of Science* 29 (1998); Bonnie Clause, "The Wistar Rat as a Right Choice: Establishing Mammalian Standards and the Idea of a Standard Mammal," *Journal of the History of Biology* 26 (1993); Doris Zallen, "The 'Light' Organism for the Job: Green Algae and Photosynthesis Research," *Journal of the History of Biology* 26 (1993). See also Adele E. Clarke and Joan Fujimura, eds., *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences* (Princeton: Princeton University Press 1993) and references therein.

[83] Richard M. Burian, "How the Choice of Experimental Organism Matters: Epistemological Reflections on an Aspect of Biological Practice," *Journal of the History of Biology* 26 (1993); Kenneth F. Schaffner, "Model Organisms and Behavioural Genetics: A Rejoinder," *Philosophy of Science* 65 (1998); Rachel A. Ankeny, "Model Organisms as Models: Understanding the 'Lingua Franca' of the Human Genome Project," *Philosophy of Science*, 68 (2001 Supplement: Proceedings of the 2000 Biennial Meeting of the Philosophy of Science Association).

[84] Or every failure: in his discussion of "rightness", Richard Burian emphasizes the local nature of rightness, which "depends not only on the job, but also on the techniques employed and the social or institutional support system for doing that job" (352), and proposes a kind of symmetry principle for

merely something old masquerading as something modern: focusing on organisms, yes, but just to tell stories about the same important men of science, "reinforc[ing] the conclusion that the scientific fates of individual organisms are intimately tied to the actions of individual scientists," rather than generating any actual insights into the "creatures' own agency".[85]

Focusing on the organism is a beneficial approach chiefly because it allows thoughtful historians to see the organism as several things at once: as an historical actor, capable at least of setting limits on what is and is not possible, and at most of exerting a transformative effect on the research programmes and institutions in which it becomes established; as a piece of technology that interacts with those who would use it and is in certain ways created and altered by so doing; as an object of scientific study; and as a kind of currency, a moveable object that travels between people, between laboratories, between institutions, between nations, and between fields of scientific study. This emphasis on the portability of objects is derived from the *interessement* approach of Bruno Latour, part of his actor-network theory which analyzes how scientists are able to persuade other groups of people that their goals are shared by scientists, thereby convincing these groups to lend their support to what is going on in the laboratory. Susan Leigh Star and James Griesemer further developed this view in 1989 to include in it an account of what they called "boundary objects", objects that have significance to more

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historians, such that they should also examine cases in which choosing the wrong organism led to a failed programme of research.

[85] Rader, "Of Mice, Medicine, and Genetics", 319. She then falls a little bit into this very trap, arguing three pages later that "the human-centred process of [the inbred mouse's] generation is ultimately what we need to look at more carefully when we label and study a research organism's 'creation'." (322)

than one group of people, and the meanings of which might well differ depending on the group.[86]

I have not been concerned, as such, with bringing to the story of early fisheries marine biology in Britain an account of the "creatures' own agency" – yet at the level of laboratory work, this agency was apparent in various ways. Whether a particular fish species would or would not consent to be hatched and/or raised in a laboratory tank greatly influenced how it could be studied; in the case of the brill, for instance, its insistence on eating young flounders meant that its growth could not reliably or economically be studied in the laboratory tanks at Plymouth, and the flounder had to stand in as a model of brill growth (see Chapter Four). Food fish had another kind of agency as well: they served as a kind of engine, motivating and lending their relevance and influence to those scientists who could claim to have special access to the fish, access that could help sustain the food fish and the industry that had been built on them. On this view of the animals, their own agency is not the issue, or not exactly – instead, the fish act by way of, or have the effect of, making influential agents out of those seen to be speaking on their behalf, the kind of agent that Latour called "the spokesperson": "someone who speaks for others who, or which, do not speak."[87] The value of animal studies historiography lies in its direction of our attention to the influence, if not exactly the agency, of organisms of scientific study in their multiple roles: in the case of fish, as food and as the basis of an industry; as specimens to be obtained and supplied and exchanged; as the end of supply chains and the motivators for building such chains in

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[86] Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translations,' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907 – 1939," *Social Studies of Science* 19 (1989); Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press, 1986); Latour, *Science in Action.*

order to access them. By paying attention to the dynamics of access and influence surrounding food fish, I've gained insight into the structures and supports of marine biology in Britain in a particular couple of decades when food fish were a focus of attention, anxiety, and action.

The aspects of animal studies historiography that deal with model organisms, a phenomenon mainly of the twentieth century, are in some ways less useful for my purposes. Mice, fruit flies, and roundworms all have in common that they became the foci of dominating research programmes, and indeed of nearly entire fields of biological inquiry of the large-scale, experimental, laboratory-based type that is a trademark of twentieth-century biology. The fruit fly at its zenith became a stand-in not just for fly genetics, or insect genetics, but for genetics far more generally. The research programmes of Cunningham or M'Intosh, on the other hand, tended to take a broad, multispecies approach (even Cunningham's *Treatise on the Common Sole*, which specified *Solea vulgaris* in the book's title, dealt with several species of sole in some detail), often focusing on careful description of individual specimens that, laboratory-based or not, followed in the tradition of natural history rather more than not. However, in the case of Cunningham's flounder work, and especially his study of growth, it is clear that a certain amount of standing-in was taking place. Not all economically important fish were equally willing to live in the laboratory or equally available to Cunningham through his suppliers, and flounders were the easiest to keep alive for long periods of time, and reliably plentiful in supply. These features were what made the flounder the "right organism for the job" of studying flatfish biology – these and not much else, as flounders had some potentially important differences from other flatfish species. For one thing,

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[87] Latour, *Science in Action*, 73.

unlike the rest of the Pleuronectidae they were an estuarine, not strictly marine, species; for another, they were famously variable, much more so than any other flatfish species, and as such they were distinctly un-standardized as research organisms. Despite these apparent disadvantages, flounders stood in implicitly for their flatfish relations when it came to Cunningham's discoveries of fish colour and his evolutionary and developmental interpretations of "the effect"; and they stood in explicitly for brill, sole, plaice, dab, and many more in Cunningham's attempt at solving the age/size conundrum. They were, in this sense, "model organisms", though using that particular term risks anachronism somewhat.

There is a second sense of "model organism" that is at least as useful: not "model organism" as stand-in or representative, in the sense that an orrery is a model of the universe, but instead "model organism" as useful, appropriate, good, or well-behaved organism, in the sense of "model student". It is when operating, explicitly or implicitly, under this second definition of model organism that historians consider the process of semi-domestication of an organism, when the organism comes inside the laboratory to live and, it is hoped, reproduce *in vitro*, whether in test tubes, glass jars, or fish tanks. Bringing organisms inside, a process that has been explored best by Kohler and Rader, is linked very closely with my key themes of access, control, and influence. Keeping organisms *in vitro* allows their keeper to control and change the conditions under which the organisms live – within a limited range dictated by the organisms themselves. Keeping organisms *in vitro* also allows their keeper ready access, at least in principle, to the specimens he or she requires, whenever they are required – that is, keeping organisms *in vitro* means, or can eventually mean, keeping them *in stock.* It's true both in industry and in science: domestication makes livestock, control means access.

And access makes influence: not only in the small-scale sense that a scientist can much more easily manipulate and experiment with organisms living in enclosed spaces in the laboratory, but also in the sense of influence in the scientific and funding communities, where authority and worth are derived from the perceived closeness of an investigator or organisation to the organisms of interest. In this and the previous chapter, we saw how Cunningham worked to bring food fish inside the laboratory to live, despite the presence of the sea at the laboratory door, and despite the existence of reliable supply chains that he had established. His efforts to partly domesticate the fish were linked up with the whole suite of goals that his work had to serve: his own studies and experiments, the Laboratory's aim of a publicly accessible tank room, the MBA's specimen supply service, and the eventual possibility of controlling part of the lives of fish in sea fish hatcheries in order to restock the sea for the trawlers. Access to specimens, and control over food fish as economic and scientific commodities: both could be enhanced if the fish could be brought indoors.

The indoors-dwelling fish were model organisms in one sense, however: they were stand-ins for the fishable wild population in the sea, and what was learned about them in the laboratory was expected to be applicable by analogy to the real commodity out there in the ocean. In the next chapter, however, we'll see that the issues of access to, and control and influence over, the ocean-dwelling food fish, were themselves subject to debate and doubt during this period, particularly from M'Intosh at St. Andrews. As fisheries marine biology itself underwent a significant scaling-up with the further successes of the Marine Biological Association and the founding of the International Council for the Exploration of the Sea, M'Intosh reacted by calling into question the very possibility of human action having any genuine effect on the fortunes of the wild food fish, denying at every turn the usefulness of the analogies that connect what we can know on land with what really goes on in the ocean's deeps.

Chapter 6

The Limits of Analogy, Access, and Influence: William Carmichael M'Intosh and "The Resources of the Sea"

Introduction: Asking Nature

Sir Lyon Playfair knew William Carmichael M'Intosh as more than a man of science when he publicly invoked the M'Intosh effigy incident as one more reason to stop asking fishermen what they thought about fish, and to start asking scientists. A remarkably accomplished member of an established St. Andrews family, Playfair was educated at St. Andrews University, and from 1868 until 1885 served as Member of Parliament for the St. Andrews-Edinburgh Scottish University seat.[1] M'Intosh was a local boy and later, as Professor of Natural History at St. Andrews University, became a figure of some prominence in the town. His yearly introductory lectures to the St. Andrews University Class of Natural History were a major social and intellectual event. Invitations to the event, bearing the lecture's title and a lovely line drawing of some creature – a jellyfish, for instance, for the November 13, 1889 lecture on "Medusae or Jelly Fishes"[2] or a newly-hatched fish larva for the November 9, 1892 lecture "On the Development of a Fish"[3] – that was to be the subject of the lecture, were printed up each year, and the enrolled natural history students might well have been in the minority on their first day of class. M'Intosh's sister Agnes – his lifelong companion and keeper of beautiful scrapbooks related to his illustrious career – noted proudly beside a selection of these invitations:

[1] Archives Hub, "Papers of Robert Lambert Playfair and other members of the Playfair family", University of St Andrews Archive. www.archiveshub.ac.uk/news/03031902.html.
[2] A1, Introductory Lecture Invitation, [87]. "1889" added in red ink on corner of invitation.
[3] A1, Introductory Lecture Invitation, [14]. "1892" added in red ink on corner of invitation.

> Prof. M'Intosh gave an Introductory Lecture every session, at the opening of the Class of Natural History. They were attended by the elite of St. Andrews. A large screen was covered with beautiful diagrams on the Subject, done in Chalk by himself, or Watercolours by his sister, these were much admired. On a large table, Specimens relating to the subject were placed, either dried, or in spirit. The Prof. could put his hand on a bottle in a moment containing what he wished to show. He was remarkable for his artistic drawings on the board, when he was demonstrating. It was a great pleasure for the intellectual community of St. A. to linger after the Lecture was over, & examine the splendid preparations in spirit & (Prof. M. had thousands of spirit preparations in his collection) the dried specimens.[4]

For those who did not rate an invitiation, full accounts of the content of the lectures were published in the local newspaper, the *St. Andrews Citizen.*[5]

M'Intosh, then, was a popular figure in the upper crust of St. Andrews, and certainly its most prominent man of science. The size of his specimen collection alone was evidence that he was a man with access to the natural world. Lyon Playfair's irate call, during the discussion that followed Lankester's appeal at the Society of Arts in 1885 for a British marine laboratory, for the British government to stop asking fishermen about fish and to "go at first hand to Nature and ask her how she conducts her processes"[6] was really the suggestion that men of science such as M'Intosh – indeed, M'Intosh in particular -- were already in this first-hand position, able to ask Nature, and needed only to be encouraged and listened to by those without first-hand access. In the course of denouncing the previous Royal Commissions' "stupid habit" of depending on fishermen for information about the sea, its fish, and the fisheries, Playfair noted that he knew of

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[4] A1, [88].
[5] For instance, an undated newspaper clipping from the *St. Andrews Citizen* pasted into A1, [35] summarizes M'Intosh's October 8, 1897 introductory lecture on "The Resources of the Sea".
[6] A1, "Sir Lyon Playfair on Sea Fisheries," [74].

only one attempt – that made by M'Intosh on the Commission's behalf – to proceed otherwise. A newspaper account of the meeting paraphrased:

> There had been another of those Commissions. They were incessant. This Commission had issued one admirable report – a report containing much scientific knowledge about the subject, of Professor M'Intosh, of St. Andrews. And what had been the consequence? The consequence had been that his friend Professor M'Intosh, for having written his admirable report, had been burned in effigy in the city from which he came, because he told so much of the truth.[7]

M'Intosh might very well have agreed that he suffered throughout his career from telling so much of the truth about the fisheries. Finding no sympathy with the local line fishermen, he later found himself at odds with the Fishery Board for Scotland as they continued with their program of inshore closures to trawling, and later again positioned against his scientific peers as they moved decisively and with the blessing of the government towards internationally coordinated and oceanographically oriented fisheries research, led in Britain by the MBA and by D'Arcy Wentworth Thompson, the young and talented Dundee biologist who became M'Intosh's *bête noire*. Despite being named by Playfair as one who had simply asked Nature when it came to the fisheries, however, M'Intosh was more aware than most of the complex practical and epistemological difficulties masked by a phrase like "just ask Nature." Indeed, his doubts along these lines, along with his rather stubborn certainties when it came to the question of the inexhaustibility of the seas and his insistence that science should cost as little money as possible but that no legislation should be brought in until conclusive scientific evidence was brought forward, had much to do with his alienation from the mainstream of fisheries research.

Limits to Knowledge I: *M'Intosh's Argument by Disanalogy*

In addition to a dizzying variety of pamphlets, essays, and public lectures addressing his views on government regulation of the fisheries, M'Intosh presented in two books the set of opinions, theories, and arguments that he came to call generally "the resources of the sea", beginning in 1897 with his Introductory Lecture to the Class of Natural History, in which he argued that "in some respects the fauna of the open sea, from its nature and environment, was to a large extent independent of man's influence."[8] These two books give the clearest demonstration of the reasoning behind M'Intosh's negative views of recent measures to protect fish stocks through legislation and his doubts regarding attempts to re-stock the sea through the use of sea-fish hatcheries. The first, *The Life-Histories of the British Marine Food-Fishes* (published in 1897 and co-authored with his junior colleague Arthur Thomas Masterman, who is also credited with preparing the index to M'Intosh's later work *The Resources of the Sea*),[9] aimed to be "a popularised epitome of the results achieved by British and foreign scientific workers at the St. Andrews Marine Laboratory and elsewhere,"[10] as a partial remedy to what M'Intosh saw as "the backward condition of British information on this head."[11] In a long series of chapters describing the development and features of the major families of economically important fish found in British waters, M'Intosh's views on the near impossibility of exhaustion of marine fish stocks occasionally surface. An early chapter

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[7] A1, "Sir Lyon Playfair on Sea Fisheries," [74].
[8] A1, announcement of Introductory Lecture on "The Resources of the Sea" on October 8, 1897, and accompanying newspaper article, undated, from *St. Andrews Citizen*, [35].
[9] McIntosh and Masterman, *British Marine Food-Fishes*. The preface to this work lays out the authorship of each chapter and the division of labour in regards to the descriptions of the major groups of fish; my attribution of particular views to M'Intosh in particular is based on this stated division. M'Intosh credits Masterman's contributions to *The Resources of the Sea* in the preface, x.
[10] McIntosh and Masterman, *British Marine Food-Fishes*, vii.

devoted to pelagic fauna such as appendicularians and crustaceans demonstrates to the reader

> the vast resources to be found in the sea for the nourishment of the food-fishes...an intimate acquaintance with this pelagic fauna alone, and leaving out of view for the moment all reference to the multitudes of animals in sand and mud, under stones and elsewhere, and which are all beyond interference by man, demonstrates the unsatisfactory position of those who labour, either through misapprehension or simply *ad captandum* – to prove that a beam-trawl deprives the food-fishes of nourishment by rendering the sea-bottom barren (*sic*) – just as a roving enemy might starve the flocks of a population by burning the grass.[12]

Not for the last time, M'Intosh uses the three-dimensional character of the sea in order to deny the usefulness of comparing it with the land: unlike sheep grazing on a square or patch of grass, fish have access to cube or column of water containing food of many different kinds, and the influence of trawlers on a single plane of that column has a minimal effect on the total food supply available to fish.

Two years later, M'Intosh's *The Resources of the Sea* presented a fully developed denial of the comparability of the sea with any other kind of habitat, be it land, fresh water, or even the air. The sea had special characteristics, notably its incomprehensibly great size, that led M'Intosh to introduce on the book's first page "the conclusion that, with some exceptions, the fauna of the open sea, from its nature and environment, would appear, to a large extent, to be independent of man's influence."[13] The book's frontispiece (Figure 13) bears this message, too: a beautiful sketch of an unfriendly-

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[11] McIntosh and Masterman, *British Marine Food-Fishes*, 5.
[12] McIntosh and Masterman, *British Marine Food-Fishes,* 40.
[13] McIntosh, *Resources of the Sea,* 1.

looking shark surfacing in a rough sea is captioned "Blue Shark, the type of a group which often ruins man's nets and hooks, and defies his influence."[14]

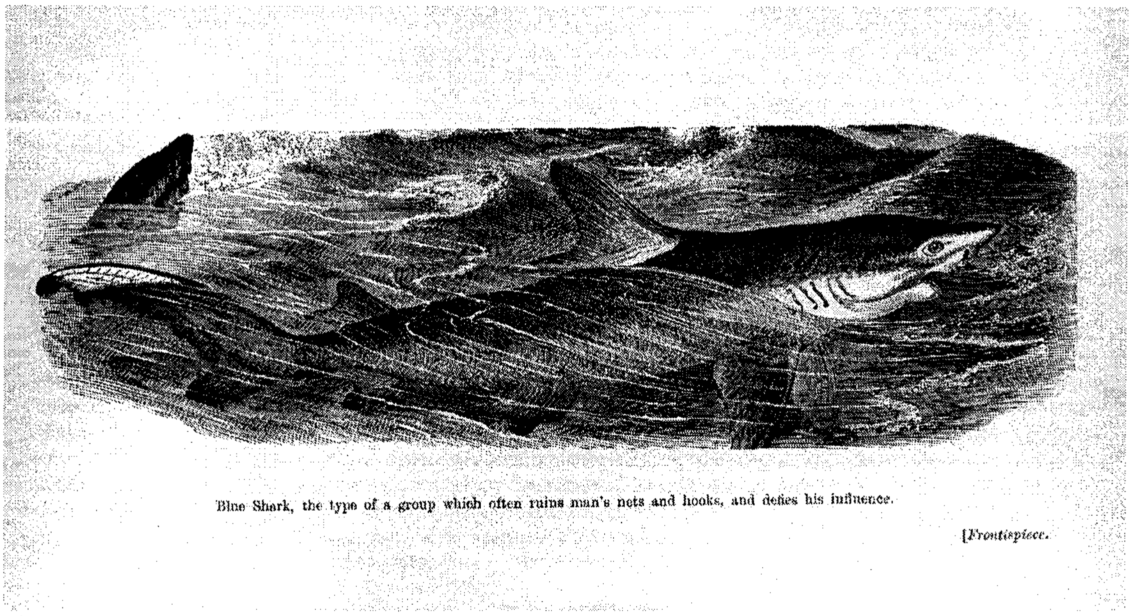

Figure 13. Frontispiece to *The Resources of the Sea*, 1899. The blue shark is a symbol of the wildness of the sea, and man's inability to make his influence felt there.

The open sea, for M'Intosh, was not just different but something more like alien, a foreign country with its own set of laws. In *British Marine Food-Fishes*, he had emphasized the newness of the marine realm, especially as concerned the fisheries, to British science: "We have only earnestly entered on the study of the subject in this country within the last few years, and much yet remains to be done, even in some of the most common marine fishes."[15] In *The Resources of the Sea*, M'Intosh addressed what he saw to be the scientific and practical dangers of this very newness, chiefly the temptation to make the different familiar by merely extending what was known about wild populations on land to apply as well to sea-fish.

> In examining the resources of the sea we are confronted
> with very different problems from those which meet us in

[14] McIntosh, *Resources of the Sea*, frontispiece.
[15] McIntosh and Masterman, *British Marine Food-Fishes*, 66.

> the consideration of the fresh waters and the land. Moreover, this peculiar divergence has a tendency to mislead those whose experiences have been accumulated in surroundings in which all the operations of nature and of man are readily observed and easily understood.[16]

Scientists, legislators, and laymen alike were subject to this danger; throughout the nineteenth century, argument by analogy had enjoyed a long spell of popularity in British science, encouraged by John Herschel's influential philosophy of science and by the dazzling example of Charles Darwin's *Origin of Species.*

C. Kenneth Waters has demonstrated convincingly that Darwin's arguments on behalf of natural selection were a carefully constructed Herschelian attempt to establish natural selection as a *vera causa* of change in the natural world. In order to demonstrate a *vera causa*, Herschel held, two separate tasks had to be accomplished: the cause had to be shown to exist and to be adequate as a cause of the effects in question, and the cause had to be shown to be actually responsible for the effects. That is, demonstrating a *vera causa* requires showing that the cause *can* do what it is said to do, and moreover it must be established independently that the cause *does* do what it is said to do. In the service of the first task, the demonstration of existence and adequacy, Waters claims that "Darwin argued for the adequacy of natural selection by appealing to the analogy between artificial and natural selection. His basic argument, presented in the fourth chapter, was that components akin to those for natural selection – variation and differential fitness – are adequate for transforming varieties in the domestic situation, so the similar (but much stronger) components in nature must be adequate for transforming species."[17] Darwin

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[16] McIntosh, *Resources of the Sea,* 1.

[17] C. Kenneth Waters, "The arguments in the *Origin of Species,*" in *The Cambridge Companion to Darwin,* edited by Johnathan Hodge and Gregory Radick, 116-139, (Cambridge: Cambridge University Press, 2003). See also David Hull, "Darwin's science and Victorian philosophy of science," in *The Cambridge*

proceeded, then, by characterizing the components and causal relationships in a smaller, more easily accessed domain – the artificial selection undertaken by breeders on domestic plants and animals, the consequences of this action – and then transferring these components and relationships to a much larger and far less easily accessed domain, wild nature and the natural selection that is a major cause of evolutionary change there.

While not all of Darwin's readers accepted natural selection as the chief method of evolutionary change – Cunningham didn't, and M'Intosh wasn't sure, and even Darwin drifted more and more towards emphasizing the role of use and disuse – there was nothing particularly controversial about the mode by which it was argued. Characterizing the conditions, components, and causal relationships of a smaller-scale domain that was more accessible to scientific investigation and then transferring that characterization to a much larger and less accessible domain was the basis of much of scientific practice, especially with the rise of the laboratory as a place of scientific work. In Chapter Four, we saw how Cunningham dealt with the anxieties inherent in proceeding by analogy from the laboratory tanks in which much of his work took place to the sea outside his windows, where the scientific and practical findings he arrived at had to hold true.

The most interesting feature of M'Intosh's "resources of the sea" argument is his nearly complete rejection of argument by analogy as a reliable method. Certainly, like Cunningham, M'Intosh held on to faith in the laboratory as a reliable site to draw conclusions about biological processes taking place in the sea outside *his* windows – though, as we shall see, he was far less confident in the laboratory's ability to stand in for

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Companion to Darwin, edited by Johnathan Hodge and Gregory Radick, 168-191, (Cambridge: Cambridge University Press, 2003.)

the sea when it came to matters of practical import like the relationship between fish growth and size. But when it came to another popular argument by analogy, this time between different (and differently well-known) sites in nature, M'Intosh rejected the approach, and indeed saw it as the way to wrong answers, poor legislation, and unfortunate consequences. When it came to the scientific study of marine fishes, M'Intosh urged strongly for a disanalogy between the land and the sea in particular. At the same time, he worked to establish the notion that there were some animals in every habitat, whether land, sea, or air, that by virtue of their habits, development, and reproduction, were effectively immune to man's influence.

M'Intosh was perfectly willing to grant that on land, the hunting and land-use activities of humans were responsible for the rapidly diminishing populations of such animals as hares, wolves, and badgers, as well as near-shore marine creatures such as dugongs and manatees. Nor did he hesitate to give credit to protective legislation in rescuing threatened populations such as red deer, roe, and foxes; "[a]s a rule, it is not difficult for man to influence land-animals whether for abundance or scarcity."[18] But there were certain land creatures in certain habitats that did not obey these rules: "certain forms, as the rabbit in Australia, the lemmings in Norway and Sweden, the rats and mice of our houses, prove more than a match for [man's] most potent checks."[19] In the plant world, too, there were weeds that seemingly failed to get any less numerous despite man's attempts to be free of them. M'Intosh attributed this resistance in many plants to be due to the massive excess of seed production, "far beyond what is required for the

[18] McIntosh, *Resources of the Sea*, 2.
[19] McIntosh, *Resources of the Sea*, 2.

preservation of the species."[20] Despite the two chief limitations on land, the relatively limited, two-dimensional space available and the proximity of human populations, that make most species living there susceptible to both predatory and conservatory human actions, some species can nevertheless overcome these limitations and persist despite man's attempts to be rid of them. Creatures of the oceans do not face either of these two limitations, and so the analogy between land and sea is a weak one when it comes to human influence.

M'Intosh next turns to the air, the domain of flying creatures such as birds, bats, and insects, which he sees as being more akin to the sea in terms of space and limited proximity to man. Indeed, the air "in its vastness excels that of the sea", covering the whole of the earth's surface and occupying even more three-dimensional space than the vast oceans.[21] But the creatures of the air as described by M'Intosh have one important difference from those of the sea: they rely at times on the reduced space available on the ground, and at these times have the same vulnerabilities as land animals:

> [the air's] denizens deposit their eggs or young on the surface of the land, and thus, though the adults may wing their way into the ether, and even feed therein, as in the case of the bats and swallows, the helpless young and eggs are within [man's] reach....The agency of man quickly diminishes many of the larger birds of flight, and this notwithstanding legislative protection.[22]

As he had done with land creatures, M'Intosh then mentions several air-dwelling creatures that man has seemingly not been able to reduce at all: "certain of its inhabitants, *e.g.* the insects, seem to defy man's power and ingenuity in limiting their numbers....Yet

[20] McIntosh, *Resources of the Sea,* 2.
[21] McIntosh, *Resources of the Sea,* 2.

in all instances the eggs are deposited on or near the surface of the earth and within man's reach."[23] It is the reliance on the land for eggs and young that is the common factor in the vulnerability of land and air creatures to the negative and positive influences of human activity, and yet despite centuries of such negative activity, there are some creatures in both domains that seem to show no effects. Having established this premise, M'Intosh turns to a discussion of the water-dwelling creatures.

Concluding his discussion of the insects, M'Intosh makes his move: "From age to age these denizens of the air have kept their ground against all the forces man could bring against them, and yet they would have required aerial eggs to have placed them on a similar footing to most of the food-fishes of our waters."[24] But even when it comes to water, there is more than one domain. Freshwater fishes are in much the same boat as land animals, confined to a small amount of space, three-dimensional though it may be, to live, feed and reproduce, and "[t]he elaborate laws framed for the protection of the more valuable fishes of the fresh waters of our country are sufficient proofs of the care which is necessary for their protection."[25] Like land animals, freshwater fish can be influenced both by over-harvesting and by restocking and protective measures: "just as – even in large areas of fresh water – the stock of fishes may be reduced to small dimensions by over-fishing, so restoration by artificial measures can be effectively carried out. In both respects, therefore, fresh waters offer a contrast to the sea."[26] In gradually shifting his discussion to the book's main topic of marine food-fish, M'Intosh

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[22] McIntosh, *Resources of the Sea*, 2-3. Interestingly, M'Intosh lists the passenger pigeon, now a famous example of extinction, as an apparent exception to this concern about man's influence.
[23] McIntosh, *Resources of the Sea*, 3.
[24] McIntosh, *Resources of the Sea*, 3.
[25] McIntosh, *Resources of the Sea*, 3.
[26] McIntosh, *Resources of the Sea*, 4.

divorces the marine domain even from its inland freshwater counterpart; though marine and freshwater fish may be morphologically and reproductively similar, their conditions of life make them different as far as their susceptibility to man's influence is concerned. Freshwater fish can be overfished, and they can be re-stocked through such measures as fish hatcheries; the bodies of water in which they live are small enough to show the effects of what humans take out and what they put back in.

"When we come to the ocean the problems connected with man's influence on certain of its denizens assume a much greater degree of complexity,"[27] M'Intosh asserts next, making explicit both the scale of the difference between the space available to land animals and the land-dwelling eggs and young of air-living creatures and the ocean's fundamental inaccessibility to man. Beyond the fact that the seas cover "about three-fourths of the earth's surface", instead of simply covering the earth's surface as land-dwellers to, the ocean's inhabitants "not only people the bottom, but glide over the latter, frequent mid-water and the surface, indeed, may be said to be scattered everywhere throughout its mass as well as fringe its margins."[28] In pursuit of sea-creatures for food or study, humans cannot even properly track and access them as they might a land, air, or freshwater creature:

> Moreover, while we can pursue the mammal on land, entrap the fish in the stream or lake, or follow the flight of the bird to a certain extent in the air, it is otherwise with the sea. Its ever-changing and often opaque and tempestuous waters offer a barrier to the pursuit of its larger forms, even were it possible to track them to its distant abysses, while the more minute for the most part escape observation. Thus with all the skill and perseverance of ages much yet

[27] McIntosh, *Resources of the Sea,* 4.
[28] McIntosh, *Resources of the Sea,* 4.

> remains to be accomplished in regard to our knowledge of
> the sea and its inhabitants, both plant and animal.[29]

The fundamental difference, mystery and inaccessibility of the sea, and its associated resistance to either positive or negative human influences, that is communicated by the book's frontispiece is brought home by this passage, and returns as a theme throughout the book.

The argument that M'Intosh presents in the opening pages of *The Resources of the Sea* and elaborates throughout the book is that the science of the sea is in many ways a new science, and risks error if it relies too much on what is known about the other domains of the earth occupied by species of interest to humans. He moves from land to air to fresh water to the ocean, attempting to illustrate how little the first and last share in common. Creatures of the land are consigned to dwell, feed and reproduce in a limited amount of space, often in proximity to human activity – and still some of them seem to show no ill effects. Creatures of the air enjoy a vastly increased amount of space, akin to and even greater than that enjoyed by sea-fish, for feeding and spending most of their adult lives, but must rely on the land, with all its limitations, as a place for eggs and young to live – and still some of them seem to show no ill effects. Freshwater fish benefit from the "aerial eggs" that air-dwellers lack, but the limited, albeit three-dimensional, space that they occupy renders them susceptible to human influences. Marine fish enjoy vast space, far more densely stocked with food than is the air, as well as the reproductive advantages of "aerial" eggs and young. Excepting such very large creatures as whales and partial land-dwellers like seals, sea-dwellers also have nearly complete immunity from human influence because of the inaccessible sea itself: wide, deep, stormy, and dark.

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[29] McIntosh, *Resources of the Sea*, 4.

However, M'Intosh develops this argument as a warning against proceeding by analogy from what was already known about land, air, and freshwater life to make general scientific and policy decisions about the sea, not (as the passage given above might seem initially to suggest) as a denial of the possibility of eventual scientific understanding. Indeed, in both *British Marine Food-Fishes* and *The Resources of the Sea*, M'Intosh presents the reader with a compelling account of what might today be called the marine ecosystem, showing the sea's many residents to be linked together in a network so complex, massive, and tightly integrated that while scientists, fishermen, and policy-makers might access and interfere with it for knowledge, food, and economic gain, they could never make their influence felt.

Limits to Influence I: M'Intosh and the "Wonderful Cycle"

A significant portion of M'Intosh's fisheries-related work once the St. Andrews Marine Laboratory was up and running involved characterizing the many types of marine plants and animals to be found in St. Andrews Bay. This work was carried out using sampling nets at three depths: a simple tow-net of the type commonly used by naturalists at the surface, a large twenty-four foot net at mid-water depth, held open by three ten-foot wooden beams and maintained at the desired depth with a lead weight and a galvanised iron float, and a trawl-net eighteen feet in length, held open by an eight-foot elm beam as it scraped along the sea bottom. These nets differed from those used in fishing by their extreme inclusivity: the netting was really more like cheesecloth or fine cotton, intended to let nothing pass through and so escape the naturalist's purview. Making frequent passes with these nets in a steam-powered boat yielded a wealth of data on an amazing variety of different creatures; these data were summarized monthly, and a year's worth of sampling in 1888 appeared in M'Intosh's contribution to the Seventh Annual Report of the Fishery Board for Scotland in 1889.[30] *British Marine Food-Fishes* presents a month-by-month description of "Pelagic life during the various months", with a page for each month describing in general terms which developmental stages of which animals were found in the tow-nets, and at what depths and distances from shore.

From such regular observation, M'Intosh was able to develop a sophisticated view of the marine realm and its yearly cycles of abundance of different types of animals and plants, incorporating the added complexity of the vertical migrations undergone by different developmental stages of marine animals. In February of 1889, he presented this system in a lecture to the Royal Institution of London, first emphasizing to the audience the remarkable fertility of seafish, which produced millions of eggs each year; what was more, "in British seas at any rate non-fertilisation is one of the rarest conditions in pelagic eggs."[31] These many young fish were sustained in "the economy of the sea" by the "enormous numbers, countless variety, and ever-changing nature of the small animals either directly or indirectly constituting the food of these little fishes,"[32] and were safe from harm done by fishing efforts: "the efforts of man can have little effect on the vast multitudes of the eggs and minute fishes. His trawl sweeps beneath them, or they are carried harmlessly through its meshes."[33] A decade later, McIntosh quoted a section of the text of this lecture in *The Resources of the Sea*. The presentation of what he called

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[30] McIntosh, *British Marine Food-Fishes*, 39.
[31] W. C. McIntosh, "The Life-history of a Marine Food-fish," *Royal Institution of Great Britain Proceedings* 12 (1889), 386.
[32] McIntosh, "The Life-history of a Marine Food-fish," 393.
[33] McIntosh, "The Life-history of a Marine Food-fish," 397.

the "wonderful cycle of life culminating in the food-fishes"[34] in *British Marine Food-Fishes* and *The Resources of the Sea*, emphasized the incredible fecundity and plenty of the sea: in the former, M'Intosh wrote, "It is a notion no longer tenable that during the winter and spring the sea, to a large extent, is devoid of the wealth of pelagic life...a vast abundance of minute life of all kinds is present throughout the entire year – and from the surface to the bottom."[35] The monthly descriptions of marine life made frequent use of such descriptors as "great profusion,"[36] "great numbers,"[37] "vast swarms,"[38] and "vast multitudes."[39] In April and early May,

> The huge mid-water net was filled like a balloon with [Appendicularians] and their gelatinous 'houses', so that the patience of the boatman was well-nigh exhausted by the constant and heavy strain, as well as the frequent ruptures of the net. It was a relief when they disappeared. There could be little doubt that like other ascidians they were eaten by fishes, and from their prodigious numbers they were thus important.[40]

In July, "[a]s in June, crustacean life swarmed from the surface to the bottom...On certain grounds some species (e.g. *Boreophausia*) rendered the nets semi-solid, and attracted many fishes."[41] These descriptions of the almost unimaginable abundance of living things to be found even within the limited area of St. Andrews Bay (and indeed in the more limited area of the sampling sites) encompass not just the many lesser forms eaten by the food-fish that are M'Intosh's main interest, but also the eggs of these fish:

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[34] McIntosh, *Resources of the Sea*, 25-26.
[35] McIntosh and Masterman, *British Marine Food-Fishes,* 36.
[36] McIntosh and Masterman, *British Marine Food-Fishes*, 42.
[37] McIntosh and Masterman, *British Marine Food-Fishes*, 42.
[38] McIntosh and Masterman, *British Marine Food-Fishes,* 44.
[39] McIntosh and Masterman, *British Marine Food-Fishes*, 44.
[40] McIntosh and Masterman, *British Marine Food-Fishes*, 45.
[41] McIntosh and Masterman, *British Marine Food-Fishes*, 50.

"No month presented a more conspicuous collection of pelagic eggs than April, the surface of the sea in many parts abounding with vast multitudes of them."[42]

The monthly profiles in *British Marine Food-Fishes*, as well as providing general information on the different animals to be found at different parts of the year and giving some indication of which animals served as food for one another and for the all-important food fishes, furnished a portrait of the sea as a massively fertile body that was always overrun, from surface to bottom, by several types of creatures. In *The Resources of the Sea*, M'Intosh instead organizes these many organisms into a long food chain with the food-fishes at the top, presenting the ocean's denizens as a "whole series – from minute floating plants and invertebrates to fishes, and find[s] that they constitute a complex cycle, linked together in intricate bonds, indissoluble by any agency man can devise." This "cycle"[43] begins with "the plentitude and variety of marine plants,"[44] including familiar red, green and olive seaweeds, dulse, and Irish moss as well as microscopic diatoms, present in a "vast abundance and variety...at all seasons, from January to December."[45] Diatoms especially are an important food for marine invertebrates, the animals termed by M'Intosh to be "fish-food". Quoting his 1889 report to the Fishery Board for Scotland in *The Resources of the Sea*, M'Intosh emphasizes the "remarkable fact that it is primarily to plants in inshore waters that the abundance and variety of animals are in many respects due...Thus nowhere are the swarms of Sagittae, Appendicularians, Crustaceans, and other groups of fish-food more conspicuous than in

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[42] McIntosh and Masterman, *British Marine Food-Fishes,* 44.
[43] Interestingly, there is nothing circular about the system M'Intosh presents, which begins with plants and ends with fish – but "cycle" was invariably the term he used.
[44] McIntosh, *Resources of the Sea*, 5.
[45] McIntosh, *Resources of the Sea*, 6.

the midst of a sea teeming with diatoms, Rhizosoleniae and other algoid structures."[46] Diatoms also serve as an important source of food offshore, not only for the "fish-food" but for another kind of fish-food-food, the "immense numbers of Foraminifera and Radiolarians upon which the lower invertebrates prey both at the surface and the bottom."[47]

M'Intosh details the abundance and seeming inexhaustibility of several groups of marine invertebrates, including annelids, jellyfish, and sponges. He details the completely fruitless attempts of fishermen to reduce the numbers of starfish (the perfect illustration of his three-dimensional view of the sea, being bottom-dwelling creatures with free-swimming larvae). Starfish were a frustrating nuisance to liners and mollusk fishermen alike for the damage they inflict to hooked fish and to mussel, clam, and oyster-beds, but "[n]otwithstanding the constant slaughter by liners, mussel-, clam- and oyster-fishermen this species does not seem to be less abundant than it was centuries ago, or within the memory of the oldest inhabitant."[48] Crustaceans present another interesting case for M'Intosh, because these invertebrates are sought-after food sources for a wide variety of creatures, humans included. At every stage, some group is eager to eat a crustacean, and yet they never appear to diminish in numbers: "In no class is the boundless wealth of the sea in all latitudes more conspicuous than in the crustaceans or crabs… [they] provide an inexhaustible supply of food over which human agency has no influence."[49] Like the starfish, crustaceans use the full range of space available in the oceans. Some, like copepods, are pelagic at all stages of life; others like prawns and crabs with bottom-

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[46] McIntosh, *Resources of the Sea*, 6-7, citing *7th Ann. Report, S.F.B.*, 1889, 259-310.
[47] McIntosh, *Resources of the Sea*, 7.
[48] McIntosh, *Resources of the Sea*, 13.
[49] McIntosh, *Resources of the Sea*, 14-15.

dwelling adult forms "continually send up a series of larvae to join the free-swimming multitudes, and at a later and larger stage they again pass to the bottom, thus giving the fishes a double opportunity, the smaller seizing them on their upward journey and in their pelagic stage, the older and larger as they descend."[50]

One of the most sophisticated aspects of M'Intosh's marine food chain is his incorporation of different developmental stages of both predator and prey, and their migrations through the water column as they develop. *The Resources of the Sea* is in many ways a recapitulation of views that M'Intosh had been expressing publicly for some time in his papers and lectures. His collection of pamphlets includes one particularly clear presentation of this view of development in an abstract of the public Introductory Lecture given to his Natural History class in November of 1886:

> In connexion with the consideration of the vast number of free oceanic animals near our own shores, a feature, perhaps not sufficiently appreciated, is the fact that a constant interchange takes place between the upper regions and the bottom, and this at very considerable depths...Thus many of the deep-sea starfishes have larvae which swim near the surface of the water, and they again descend when they have reached a stage in which the main features of the adults have been reproduced. Many of the Pleuronectidae, such as the plaice, turbot, and craig-fluke, which habitually live on the bottom, produce eggs which are truly pelagic and float near the surface of the water, until at a certain stage they take up their residence at the bottom, like their parents.[51]

This traffic from bottom to surface and back had important implications for the food supply to economically important fish species, but the young of these same species were part of this same food supply:

[50] McIntosh, *Resources of the Sea*, 16.

> The interchange again taking place between the bottom-fauna and the surface, viz. of egg and early larvae passing upward, and of older forms going downward, keeps up a constant stream of food for the young fishes, which have a similar migration. But they do not, unmolested, thus levy a tax on the lower types. The larger fishes prey perpetually on the smaller...Thus a check is kept on the enormous powers of reproduction so characteristic of the food-fishes – powers of reproduction which have hiterto been, and doubtless will continue to be, of great benefit to man.[52]

With the wealth of food available to them in their position atop a chain starting with "teeming" diatoms, which nourish the "immense numbers" of foraminiferans and radiolarians, which themselves along with the diatoms provide ample food for the sea's "boundless wealth" of crustaceans and other invertebrates, food-fish seem unlikely to be limited by the amount of food available. What's more, these food-fish are blessed with the equivalent of the "aerial eggs" that air-dwellers lack, and M'Intosh in *The Resources of the Sea* makes clear that nervous fishermen and those who would regulate their activities have nothing to fear:

> It is probable, however, that it is mainly to the vast number of eggs produced by each species, to their transparency and pelagic nature, and to the existence of boundless reserve-margins of the sea that the food-fishes have been enabled to resist extinction by natural agencies and by the various methods of fishing suggested by the ingenuity of man.[53]

These "boundless reserve-margins" were the unexploited, even unexplored depths and breadths of the sea, which could always restock the inshore regions that were more subject to man's access, and thus his influence:

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[51] PWCM, W.C. McIntosh, "Abstract of Introductory Lecture to the Class of Natural History, University of St. Andrews, November 13, 1886," 138.
[52] PWCM, McIntosh, "Introductory Lecture 1886", 145.

> Even if, in the waters within a reasonable distance of land, fishing were carried to such a degree that it would be no longer profitable to pursue it, it is possible that the adjoining areas and the wonderful powers of increase of the few fishes remaining would by-and-by people the waters as before, because everything in the sea around, including the plentitude of food, so nicely fitted for every stage of growth would conduce to this end…the not uncommon cry of 'ruin of the fisheries' seems to need qualification.[54]

The "not uncommon cry" being referred to here was a consequence of the rise of trawling, and increasingly of steam-driven, distant-water trawling. The longer voyages of these trawlers meant that hitherto unfished regions of the sea, which had probably been broadly accepted as "reserve-margins" of some kind, were now active fishing grounds, and no longer lying fallow and protecting the fish supply. M'Intosh paints a vivid picture of the three-dimensional nature of the sea, and re-asserts the disanalogy with land, in order to defend trawling against a common charge,

> the notion which places on the trawl the onus of removing the food of the fishes by rendering the bottom 'barren'…Where, when and how this barrenness has been found is not stated, but its results are said to be as disastrous to the fishes as the destruction of the grass in an enemy's country would be to the flocks and herds. Such writers appear to be unaware of the vast abundance and variety of pelagic beings – from fishes to plants – which own relationship neither to the bottom nor to the locality, but are swept hither and thither to nourish, with cosmopolitan liberality, the fishes of our own and the neighbouring shores…A trawl that would simultaneously remove the contents of the water from surface to bottom…has yet to be invented.

Trawlers might float on the surface of more of the sea's area than had previously been subject to fishing, and they might drag their nets over the sea's bottom, but to M'Intosh

[53] McIntosh, *Resources of the Sea*, 23.

this did not have to mean that the reserve-margins were vanishing. It was this "cosmopolitan" group of pelagic organisms that functioned as a sort of mobile reserve-margin, immune to any amount of trawling of the sea bottom. The sea was not the land: cutting the grass did not mean killing the flocks.

M'Intosh's marine food chain didn't always end with food fish. In addition to recognising the predation of larger adult food fishes upon the younger stages of larvae and adults as they migrated away from and toward the sea floor, he considered the predation of humans. Part of his strategy of minimizing[55] the conceivable impact of human influence upon the population of marine food fish was to cast humans not as an unnatural drain upon the food fish stocks, but instead as just another predator. Compared with their company atop the marine food chain, humans were in fact a predator which had vastly lesser access to, and therefore greatly reduced influence on, the supply of fish. Porpoises, dolphins, whales and sharks were diving sea-dwellers, able to catch their prey across the whole breadth of the ocean and at a range of depths; sea-birds such as gulls, guillemots, and red-throats could access much of the ocean's surface and were enthusiastic eaters. M'Intosh's later notebook entries frequently mention birds and porpoises catching fish in St. Andrews Bay, observed either by himself or an assistant. An entry on December 6th, 1903, reports that his assistant "Brown saw Gulls in great numbers feeding on sprats"[56] which were also being pursed by cod and porpoises. On the subject of gulls, M'Intosh had previously noted on January 17th, 1903, "How many fishes w^{d} the 630 birds have eaten in a day -- ? but w^{d} their dietary in no way affect the

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[54] McIntosh, *Resources of the Sea*, 23-24.
[55] Or, one might say, part of his pattern of thinking that had the result of minimizing the impact of human influence.
[56] NB, 6 December 1903, 150.

abundance of fishes in St.A. Bay. As they c^d not eat the larger fishes it is clear that the smaller must have suffered."[57] In the publication of his fishing notes in pamphlet form, the entry for that day was perhaps the most strongly worded of any of his notes in *The Fishing News*: "It is interesting to calculate how many small fishes the 630 birds would consume in a day, probably from 15,000 to 20,000, yet their depredations in St. Andrews Bay do not seriously affect the stock of fishes therein. Such considerations, and there are many others, demonstrate the wonderful resources of Nature in the sea whereby the food fishes are enabled century after century to hold their own against man's incessant attacks."[58] Not only did birds mass in the hundreds to prey on the same fish that were the target of human fishing efforts, but the birds targeted the smaller fish preferentially. This second observation was not an idle one, but a reference to a scientific question of great practical importance: the relationship between fish size and reproductive maturity. It speaks to another area of significant epistemological anxiety on M'Intosh's part about the limits to man's knowledge about marine fish, and to his exasperation that legislative decisions regarding the fisheries were being made without adequate scientific understanding of the subject – that people like him, generally acknowledged to have privileged access to nature, were nonetheless proving to have little influence when it came time to regulate Britain's fisheries.

Limits to Knowledge II: M'Intosh, Masterman and the Problem of Fish Size

The prolonged anxiety about overfishing that Britain experienced during the latter decades of the nineteenth century was focused primarily on modern steam-driven

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[57] NB, 17 January 1903, 133.

trawling. While in many ways steam trawling was a remarkable advance in efficiency when it came to getting the most fish to market in the shortest time for the lowest price, when it came to the fish themselves, steam trawling was considered by many to be an especially destructive and wasteful mode of catching fish. Besides concerns over the sheer number of fish that could be caught at once and the possible damage inflicted on the fish by being compressed under the weight of the catch and dragged along the ocean floor, a major concern was by-catch: trawling could not distinguish between marketable fish and undesirable species or, worse, too-small individuals of desirable species, until the net had been hauled. Unlike line-fishing, unwanted fish could usually not be thrown back alive, and trawling was viewed as damaging to the future of the fisheries, killing small fish that would otherwise have grown into marketable individuals, and even worse, killing sexually immature fish that would otherwise have reproduced and given rise to thousands of marketable individuals.

An apparently simple course of action was for legislators to prohibit the sale of fish below a certain size particular to each species, and prohibit the use of nets with mesh smaller than a size that would let small, sexually immature fish pass through unharmed. Though it was the concerns of long-liner fishermen, who saw trawling as a threat to their livelihood, that had prompted the Royal Commissions of 1865 and 1884, by 1890 the concerns about overfishing had spread to include the trawler-owners themselves, who formed a group called the National Sea Fisheries Protection Association (NSFPA). At a meeting held at Fishmongers' Hall in July of 1890 to address "the capture and sale of immature trawl fish", the assembled audience of "representatives of the fishing industry

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[58] FF, 9. The strong editorial content of this note is unusual among M'Intosh's fishery notes, which even in his later years tended to be mainly dry and factual.

from all the principal parts of the United Kingdom" as well as scientists, members of Parliament and the Fishery Board for Scotland considered a resolution: "This conference considers it desirable that an official international conference of European maritime Powers should be held with the view of concluding a convention for the prohibition of the landing and sale of undersized flat fish within their jurisdictions."[59] The resolution was eventually passed unanimously, but only after a discussion period that made clear how difficult it would be to determine what constituted an "undersized flat fish."

Among those present were two of Britain's leading scientific authorities on fisheries biology in general and flat fish biology in particular, W.C. M'Intosh and J.T. Cunningham. M'Intosh spoke first, and the newspaper report of the meeting shows that he took pains to emphasize the complexities of the question, saying

> that immature fish might mean a very young fish, or fish that had not yet ripened the roe. It had been found in some cases that what was called an immature fish was really a mature fish. There was no question about the destruction of immature fish, but a good deal of difficulty would arise in dealing with this matter in connexion with legislation, because in considering these very young fish they had to remember the various modes by which such fish were captured. Opinions differed as to whether the little fish would live when thrown into the water after being in the trawl. At present the mode of trawling was not favourable to their survival. He very much doubted whether any close season could be made in the sea. A thorough investigation of the great fishing grounds off shore would, he held, be the first step to remedy many of the existing evils; but the well-known fishing grounds should be first examined. He could not altogether agree with those who took a gloomy view of the situation, thinking, on the contrary, that great hope should be afforded them by the enormous power of reproduction of food fishes and the extent of the ocean. He supported the resolution...[60]

[59] A1, 3 July 1890, newspaper clipping , page 4 of insert between album pages 45 and 46.
[60] A1, newspaper clipping, 3 July 1890.

The definition of "undersized" could mean simply small, or could mean sexually immature, and it was far from clear how size and sexual maturity were related. M'Intosh's support of the resolution seems to have been based on the idea that a large amount of scientific research must come before any definition of undersized was arrived at. Unlike what was rapidly becoming the vast majority of fishermen and concerned scientists and legislators, M'Intosh felt no sense of urgency towards immediate legislative action on behalf of the fisheries. Others might take "a gloomy view" and want to restrict trawling activities immediately before any further harm could be done, but M'Intosh's confidence in the resources of the sea meant that there was plenty of time for further research.

M'Intosh's growing frustration with the behaviour of regulatory bodies like the Fishery Board for Scotland, who in his view asked for scientific data on food fish but then acted despite what they were told, or on the basis of preliminary, poorly-designed studies rather than having the patience to wait for a complete answer, spurred his growing alienation from the fisheries biology community and caused him to embrace his own views ever more dogmatically, as will be seen in the next section. The comments made to the NSFPA meeting by J.T. Cunningham of the Marine Biological Association Laboratory must have made his blood boil. The newspaper report of the meeting, which M'Intosh or his sister Agnes clipped out and saved, describes how Cunningham

> pointed out that the size of the fish caught depended on the mesh of the net, which to prevent the catch of very small fish, should be enlarged. He thought that certain bays, like St. Andrews, and others which were not fished very much might be investigated by competent naturalists, and then closed altogether. They would thus form natural nurseries

> for the production of fish, which would not be caught until they got outside these waters.[61]

It is hard to say which aspect of Cunningham's statement would have been the greatest affront to M'Intosh. Perhaps the blithe suggestion that St. Andrews Bay (which had a thriving fishing industry which, along with caddying at the local golf courses, was the chief source of employment for many local men) and other little-fished bays be "closed altogether" and used as "natural nurseries for the production of fish"? M'Intosh might just as well have suggested that the fishermen of Plymouth Sound, who Cunningham knew and relied on for access to the fish he researched just as M'Intosh relied on some of the men and boats of St. Andrews, give up the whole of their inshore fishery. The very notion, too, of relatively small and shallow inshore bays serving as "natural nurseries" to supply the open ocean with fish was precisely the opposite of M'Intosh's view of how the ocean worked biologically. As he had told the Royal Institution audience the previous year, "it does not follow that the fishes of an enclosed bay [a footnote here reads, "For example, closed by a Fishery Order.] will increase of themselves...most of the large mature fishes are beyond the limits...Such bays, therefore, have to depend for their stock of fishes on the unprotected offshore. If by any chance the latter waters were depopulated the inshore would seriously suffer."[62] It was "the extent of the ocean", incomprehensible and largely inaccessible to man, that sustained food fish populations, and expecting the inshore bays, small enough and near enough to land to be easily accessed and influenced by man and as such particularly ill-suited for the protection of fish stocks, was simply backward. As much as either of these points, however, M'Intosh must surely have minded Cunningham's fairly simplistic suggestion that the allowed net

[61] A1, newspaper clipping, 3 July 1890.

mesh size should be enlarged to prevent the capture of undersized fish – not because this was a bad idea, since everyone at the meeting, M'Intosh included, agreed that "the destruction of immature fish" by trawlers needed to be addressed, but because by making his suggestion about mesh size Cunningham was in some sense begging the question of defining undersized, and was perhaps the only other man in the room besides M'Intosh who understood something of the complexity of this question – that is, who should have known better. Whether by setting a minimum size for landing and selling fish, or by setting a minimum size for the mesh of nets, the same problems relating to size and sexual maturity had to be addressed before informed action could be taken. M'Intosh had voiced this concern, and might have expected Cunningham to second it.

After the meeting, Cunningham undertook some field and laboratory research related to this question. The laboratory research was undertaken chiefly on the population of young flounders that Cunningham had received two months previously (see Chapter Four); for the field research he needed look no farther than the active fisheries of Plymouth, where fish were caught by a variety of different methods. Cunningham produced a Report to Council for the MBA entitled "The capture of undersized and immature flat fish by various modes of fishing actually followed."[63] In it, he compared the destructiveness to small plaice and soles of three techniques: inshore seining, shrimp trawling, and sea trawling. Seining, he found, captures very many small fish, and many small plaice of six to nine inches (smaller than the minimum size of ten inches for flat fish being proposed by the NSFPA) were sent to market. The same was true of the many small soles caught by shrimp trawling. Plaice and soles five inches or less in length were

[62] McIntosh, "The Life-history of a Marine Food-fish," 386.
[63] RC.

thrown back by seiners and shrimp trawlers; in both cases "the unmarketable fish are returned alive and uninjured."[64] In contrast to the seiners and shrimp trawlers, sea trawlers of 40 tonnes and over caught relatively few flatfish under ten inches in length; the difference, however, was that sea trawlers could not "return fish to the water alive and uninjured usually."[65] This finding was a partial confirmation of the wastefulness of trawling: though not as many too-small fish were being caught as was feared, those that were caught could not be returned alive to the sea.

It was Cunningham's examination of the Plymouth fish market in Sutton Harbour that made clear just how difficult was the question of protecting immature fish through limiting landing size, however. "I find," reported Cunningham, "that the big trawls take no plaice, soles, turbot, or brill which are too small to be marketable."[66] That said, upon examining some fish, he found that some twenty to twenty-four percent of the landed plaice were sexually immature, "but to exclude these it would be necessary to impose a limit of 14 inches, and that would mean throwing overboard 55 percent of the plaice now caught. The limit of 10 inches proposed by the Nat. Sea Fisheries Protection Assoc. would not affect the Plymouth trawlers much. The objection to it is that the fish thrown back would seldom survive."[67] In the case of soles sold at market, Cunningham found that fifty-four percent were mature males, eighteen percent of all soles were sexually immature, but only eight percent were under ten inches in length. As well, "a certain

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[64] RC, page E1.
[65] RC, E1.
[66] RC, E1.
[67] RC, E1.

proportion of immature turbot are taken by Plymouth trawlers, but none under 12 inches, the limit proposed; and the same is true of brill."[68]

The aim of prohibiting the landing and sale of fish under a particular size was to protect sexually immature fish from being killed before they were able to reproduce, and Cunningham found two problems. Firstly, as it had been long suspected, too-small fish caught in a sea-trawl net can usually not be thrown back alive. Secondly, and which was much more troubling and interesting from a scientific standpoint, for a given minimum length, there will be some sexually immature fish who are larger than the length. Setting the minimum length at a level that will protect all or nearly all sexually immature fish would lead to a massive reduction – perhaps more than half in the case of plaice[69] – in the number of fish that can be taken to market at all, and will mean throwing overboard or not catching many sexually mature fish in order to protect the immature ones. There is not, that is, some magic size at which fish become sexually mature, and length is a poor predictor of sexual maturity. Because of these difficulties, Cunningham concluded his report by recommending that trawling be prohibited altogether in British territorial waters, that a minimum marketable size be imposed on flat fish caught by other means than trawling, and that the eastern fishing grounds of the North Sea be closed "by international convention."[70] In the long paper resulting from these inquiries, published in the Association's *Journal* as "The immature fish question," Cunningham restated these

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[68] RC, E1-E2.

[69] In the long article published in 1893 that resulted from these inquiries, on "The Immature Fish Question," Cunningham reported that on a three-day trawling trip off of Cornwall, "32 per cent. of the merry soles caught were under the proposed limit [of 11 inches, though none were sexually immature]. Merry soles form a very important part of the total catch of a trawler fishing out of Plymouth, and they fetch a very good price. There would be the strongest opposition on the part of the Plymouth trawlers to a proposal that they should be compelled to throw away 32 per cent. of the merry soles they catch." (Cunningham, "The Immature Fish Question," 66.)

[70] RC, E2.

recommendations and reasoned that prohibiting beam trawling in territorial waters "would be no greater difficulty…than in preventing the capture of salmon in illegal nets, which has been done for years."[71]

Clearly, Cunningham did not share M'Intosh's willingness to delay legislative action until science had provided a definitive answer; for Cunningham, the limits to knowledge about the sea's resources were reason for caution, not optimism. To both men, these limits were motivation to continue their work. In papers published in the few years previously (see Chapter Four), Cunningham had seemed hesitant to make any concrete recommendations. However, in 1893 yet another government inquiry, a seventeen-member Select Committee on Sea Fisheries headed by Edward Marjoribanks, had been ordered, this time not to determine whether a problem actually existed, as had been the task of previous committees, but instead

> to consider the expediency of adopting measures for the preservation and improvement of the Sea Fisheries in the seas around the British Islands, including the prohibition of the capture, landing, or sale of undersized sea fish, the prohibition of the sale or possession of certain sea fish during the periods when their capture is forbidden, the fixing of close seasons, the prohibition or regulation of certain methods of fishing, the protection of defined areas, and other like regulations, international or otherwise.[72]

This Committee had, as compared to the Royal Commissions that preceded it, the disadvantage that Select Committee meetings were held during fishing season, and required all witnesses to come to Westminster to give their testimony. On the other hand, they

> had an advantage over any previous fishery inquiry in the fact that they have laid before them the statistics which have of recent years been collected by

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[71] Cunningham, "The Immature Fish Question," 74.
[72] "Report from the Select Committee on Sea Fisheries, Together with the Proceedings of the Committee, Minutes of Evidence, Appendix and Index," *House of Commons Parliamentary Papers* XV (1893-1894): ii.

> the Board of Trade, and the statistics compiled by the officers of the Scottish Fishery Board; and they have also had the evidence founded on the observations of the scientific experts employed by the Marine Biological Association, and by the Scottish Fishery Board. Indeed, it may be almost said that this is the first fishery inquiry in which the more important complaints have been founded, not merely on the statements and the ideas of rival classes of fishermen, but upon facts and studies.[73]

Scientists had, it is clear, unseated fishermen as the authorities on fish and fisheries; they were able to provide "facts and studies," not mere "statements and ideas." As it turned out, however, this time around the facts and studies and statements and ideas were not opposed. Though the Committee found no danger with regard to the herring and roundfish fisheries, when it came to the flat fish of the North Sea it mattered not who they asked,

> whether trawlers or linesmen, whether smack owners or fishermen, whether scientific experts or statisticians, there seems to be no doubt that a considerable diminution has occurred amongst the more valuable classes of flat fish, especially among soles and plaice, and that this diminution must be attributed to over-fishing by trawlers in certain localities.[74]

The answer might seem to be minimum size limits, and sets of suggested sizes had been provided by the National Sea Fisheries Protection Association as well as the Marine Biological Association (whose numbers were somewhat higher than the NSFPA's, but came paired with an oddly noncommittal suggestion that the limit in practice should probably be lower).[75] The Committee rejected both sets on the grounds that minimum size limits would cause "great hardship to many of the poorer fishermen who fish near

[73] "Report from the Select Committee," iii.
[74] "Report from the Select Committee," iv.
[75] Calderwood, Cunningham, and Holt all gave evidence on behalf of the MBA; Calderwood proposed the size limits, and all three emphasized the promise of marine fish hatcheries as a way out of the overfishing problem.

the shore in the smaller class of boats,"[76] and instead suggested a set of limits much lower than even the NSFPA's, but matching limits already in place in the other North Sea nations of France, Belgium, and Denmark, and suggested that the several North Sea nations should "secure the adoption of uniform limits of size and other matters."[77] The Committee's report was good news for the cause of international fisheries cooperation, but on the question of fish size and sexual maturity, the adoption of the very generous Continental numbers must have seemed like a lot of wasted work to the NSFPA and the MBA. Legislation resulting from the Select Committee's report, and setting minimum catch sizes at eight inches for sole and plaice and ten inches for turbot and brill, was passed as the Sea Fisheries Act of 1895.[78]

As was made clear by the MBA's uncertain recommendation to the Select Committee, the question of fish size and maturity was far from a closed one. In St. Andrews, meanwhile, M'Intosh and his colleague Masterman were undertaking their own studies into the question, which in 1897 they presented at length in *British Marine Food-Fishes*, along with a sharp critique of Cunningham's approach. The sixth chapter of *British Marine Food-Fishes*, entitled "The Rate of Growth of Food-Fishes", begins in typical M'Intosh fashion by observing that "The importance of this subject from an economic point of view would be difficult to exaggerate, and at the same time it is remarkable how very little is definitely known in connection with it."[79] Though the authors initially grant that the phenomenon of growth in all organisms is "held by most

[76] "Report from the Select Committee," iv.
[77] "Report from the Select Committee," iv.
[78] "Bill to Amend Fisheries Acts," *House of Commons Parlimentary Papers* III (1895).
[79] McIntosh and Masterman, *British Marine Food-Fishes*, 97.

zoologists to be either the direct effect of, or intimately connected with, cell-division"[80] – that is, that growth is a function of the number of cells and their rate of division – the discussion very rapidly takes an interesting turn. Just as M'Intosh had argued earlier in the book, and would shortly argue far more strongly in *The Resources of the Sea*, that there was a fundamental disanalogy between man's influence on land-dwelling organisms and his influence on the inhabitants of the sea, he argues here that "The problem of the growth of fishes is...very different in many ways from that of the growth of quadrupeds and birds."[81] This difference is again chiefly a result of the different environment in which fishes live:

> the fish as an organism shows itself to be more directly susceptible to the influence of its environment. Thus, as mentioned below, the period of incubation can be altered at will between very wide limits by simply varying the temperature, whereas one would hardly expect to alter the period of mammalian gestation, or even of avian incubation, except within extremely narrow limits.[82]

This distinction between the relatively self-contained, environment-indifferent development undergone by mammals and birds and the highly environment-dependent development of fishes was an important one for M'Intosh's embryological thinking. With the beehive of activity surrounding cell biology, especially on the Continent, and the ongoing interest in the theory of germ layers in describing the events of development, not to mention the fascination with embryos as the key to uncovering the evolutionary history of vertebrates, embryology was focusing less and less on the embryo in its environmental context. M'Intosh instead conceived of fish development as a highly

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[80] McIntosh and Masterman, *British Marine Food-Fishes*, 97.
[81] McIntosh and Masterman, *British Marine Food-Fishes*, 98.
[82] McIntosh and Masterman, *British Marine Food-Fishes*, 98.

localized and context-dependent process, which perhaps put him out of step with embryological trends but which was a natural outcome of his commitment to a practical, fisheries-oriented investigation into the life-histories of economically important fish.

The disanalogy between mammalian and piscine growth led Masterman to a surprising conclusion, which appeared in the Thirteenth Annual Fishery Board for Scotland Report and was quoted at some length in *British Marine Food-Fishes.* In this report, Masterman first presents the disanalogy as a matter of empirical obviousness:

> In the mammalia there is a definite duration of growth in bulk and of life quite apart from the environment, and either period can only be altered by continued action of the environmental factors through many generations....[T]here is no proof of the hypothesis that the individual fish ceases to grow at any period of its life; on the contrary, there are considerations which point to the other view, i.e., that a fish continues to grow throughout its life.[83]

Mammals and fish differ fundamentally, in that in mammals there is a distinct period of growth, defined by factors internal to the organism and alterable only by "continued action of the environmental factors through many generations" – that is, through evolution. Fish instead undergo continuous growth throughout their lives, and unlike mammals are far more influenced by their environment. The surprising conclusion is that this disanalogy in pattern of growth extends to a further disanalogy relating to man's influence of the two types of organisms, such that "if a mammalian or avian species be subjected to the destroying agency of man, there follows a *diminution in numbers*; whereas, if a fish be subjected to like conditions a *reduction in the size of the individuals*

[83] A.T. Masterman, "Rate of Growth of Marine Food-Fishes", 13th Annual Report, Fishery Board for Scotland, 289, quoted in McIntosh and Masterman, *British Marine Food-Fishes*, 98.

is the immediate result."[84] *British Marine Food-Fishes* offers some explanation for this conclusion, which depends in part on an observation similar to Cunningham's at Plymouth, that size is not a perfect predictor of sexual maturity, so that in a population of fish there will be found "every gradation, from those maturing from [sic] the first time at [let us suppose]6 inches and those maturing at 10 inches."[85] Under heavy fishing pressure, those fish who mature at the smallest sizes will be those most likely to reproduce, "and their offspring will have the same tendency to commence breeding at the earlier age and shorter size. The first effect will therefore be that of reducing the average size of the fish."[86] Fishing pressure, then, is a kind of selective pressure that favours those individuals who reproduce at a smaller size, and their offspring, who resemble their parents in the small size at which they mature. One of the unusual features of the St. Andrews approach to fisheries biology was this view of the fish stocks not as passive individuals being taken from the sea like marbles from a jar, but as biological populations that would respond to fishing pressure in sometimes surprising ways.[87]

If it was a surprising conclusion that fishing pressure would lead, not to a reduction in numbers as hunting pressure would for mammals or birds, but instead to a reduction in the size of individuals, then the conclusion that followed closely upon it was nothing less than startling. Beçause of the unusually close relationship between fish

[84] McIntosh and Masterman, *British Marine Food-Fishes*, 98.
[85] McIntosh and Masterman, *British Marine Food-Fishes*, 99.
[86] McIntosh and Masterman, *British Marine Food-Fishes*, 99.
[87] In a 1907 paper, mainly disapproving of the work done by the MBA and the "Proposed International Scheme", M'Intosh returned to this theme, suggesting that fish become accustomed to a particular mode of fishing and learn to avoid it, saying that "[a] change from trawls to fixed nets, or to bait, might upset the conclusions [that the fish population has been greatly diminished by trawling] by the discovery of numerous fishes." He saw as a goal for future fisheries work the task of "demonstrating the effective nature of sudden changes in the method of capture – *e.g.* the substitution of Anemones for Mussels, of Cuttle-fishes for Herrings, of Lobworms for Scallops, and of the alternation of gill-nets with tempting bait of

biology and the environment, M'Intosh and Masterman conclude that "the second effect of over-fishing a district will be that of *multiplying the numbers*."[88] The authors' italics reveal that they recognise the extent to which this conclusion is counterintuitive. Fishing pressure, on this view, would have to be not just somewhat but utterly unlike removing marbles from a jar.

This is as good a time as any to point out that M'Intosh and his St. Andrews colleagues were wrong about overfishing. M'Intosh overestimated the extent of the sea's reproductive resources in the face of accelerating fishing pressure, and did not and perhaps could not have predicted the extent to which the "boundless reserve-margins of the sea" that sustained the areas used for fishing would come under human influence due to the ever-increasing range of the steam-trawlers and the eventual advent of factory freezer-trawlers in the mid-twentieth century. Toward the end of his career, too, M'Intosh became more and more dogmatic in his denial of human influence on food-fish populations, as will be seen in the next section.

However, M'Intosh and Masterman had some quite sophisticated scientific reasons to reach their startling conclusion. The main reason had to do with the almost incomprehensible fecundity of marine fish, the incredible overproduction of eggs and fry that were so essential to the "complex cycle" of food and feeding that M'Intosh had identified in the marine realm. M'Intosh and Masterman argued that fishing pressure had the effect of freeing up more food for those fishes that remained: "It is a well-known fact that a far greater number of fry are annually produced than will ever attain sexual maturity, and the numbers surviving in any given district till sexual maturity will bear a

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various kinds. Few appreciate the revelations made by such a change of method." FRP, item 5, W.C. M'Intosh, "Scientific Work in the Sea-Fisheries", *The Zoologist* (1907), 2.

constant relation to the amount of nutriment supplied by that area."[89] A population of fishes, they argue, is really made up of several different age and size groups making use of the same food: immature individuals, newly mature individuals, maximally mature individuals, and old individuals "past their maximum reproductive capacity" (see Figure 14).

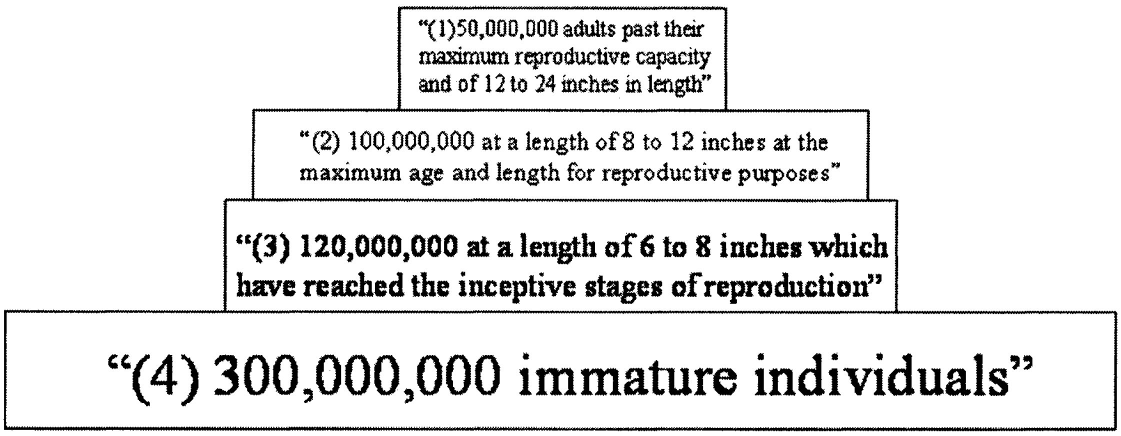

Figure 14. Diagrammatic representation of McIntosh and Masterman's four fish subpopulations. Group 1 contains the oldest and largest individuals, but the fewest total individuals. There are six times as many fish in group 4 as in group 1. Text from McIntosh and Masterman, 100.

Fishing tends to remove members of groups (1) and (2) from the population, since they comprise the largest fish. With these fish gone, M'Intosh and Masterman argued, "the supporting powers of the district remaining the same, a much greater quantity of (3) and their offspring included in (4) will be able to find subsistence and will survive."[90] Nor was their argument purely theoretical, as they attest that "[t]he results at which we have arrived here are found in nature. One of the most obvious effects of over-fishing a district is that there results a great number of small-sized individuals of the particular species affected." This peculiarity of fish population biology, which made fish so different from

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[88] McIntosh and Masterman, *British Marine Food-Fishes*, 100. italics in original.
[89] McIntosh and Masterman, *British Marine Food-Fishes*, 99.
[90] McIntosh and Masterman, *British Marine Food-Fishes, 100.*

land and marine mammals and birds, was presented as a built-in response to the pressure of predation, one that prevented fish from becoming extinct:

> The species is subjected to risks of extinction and the counteracting effect is a reactive production of a greater number of smaller individuals, so that extinction may be prevented by multiplying the chances of a sufficient number reaching sexual maturity. The want of this ready re-activity to changed surroundings in mammals helps to render the whales and seals liable to extinction at the hands of man.[91]

Not only are fish much more influenced by the environment around them when it comes to growth than are mammals and birds, but they are also uniquely blessed with a "ready re-activity" by virtue of their incredible fecundity, such that they are able to resist extinction in ways unavailable to the other animals hunted by humans.

Fishes' extreme environmental sensitivity when it came to growth and development was, for the St. Andrews workers, a major epistemological barrier when it came to the scientific study of the relationship between size and maturity in fish, a question whose answer had major significance for the regulation of the fisheries. Factors such as available nutrition, temperature, injury and disease, and "what we may term the 'individual tendency'"[92] – that is, naturally-occurring variations within a population – all played a role, and muddied the waters considerably for anyone trying to solve the size/maturity problem. "These factors…form such a formidable array of tendencies, all active under natural conditions and all working to the same end, namely, diversity of growth, that they might well appear to make the rate of growth of fishes an insoluble problem, but we do not think matters will be so hopeless as this if carefully

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[91] McIntosh and Masterman, *British Marine Food-Fishes*, 100.
[92] McIntosh and Masterman, *British Marine Food-Fishes*, 104.

considered,"[93] M'Intosh and Masterman remarked. Not insoluble, but not at all easy: in many ways the size/maturity question revealed the limits of the marine research laboratory with its tanks full of real or homemade seawater. Finding rules for growth and maturity of food fishes that would guide legislation meant finding these rules not for an experimental fish population in a laboratory tank setting, but for naturally-occurring fish populations in their ocean environment. The reliance of some fish biologists on the analogy between artificial and natural conditions would simply not cut it for M'Intosh and Masterman, for two main reasons. Firstly, the extreme sensitivity of fish growth to environmental conditions meant that any failure to very closely mimic natural conditions in a laboratory setting could easily produce misleading results. Secondly, in the field, but not in the lab, natural selection in the form of the "destructive tendencies" of starvation and predation was always operating on fish populations, such that "the same destructive tendencies still act upon every generation to weed out the maxima and the minima to the preservation of the mean, so that after the early stages the largest or smallest are continually removed, the mean only surviving."[94] In the case of fishes, mimicking nature in the laboratory would require a complete recreation of the natural environment, predators, food, space, temperature, and all – a task akin to making a map so complete that it occupies the same amount of space as the area being mapped, rendering the map useless. The constant, small-scale action of natural selection, arguably the hardest factor to mimic in the laboratory, was in the eyes of the St. Andrews workers probably the most important, "for it follows from the same, that if one takes a number of fish-eggs or young larvae, *already varying in slight degree* as regards size, and places them in tanks in

[93] McIntosh and Masterman, *British Marine Food-Fishes*, 104.
[94] McIntosh and Masterman, *British Marine Food-Fishes*, 105.

artificial conditions – such as easy access to food, and immunity from actual foes; then, all the slight variations in size tend to be emphasized, and in course of time fishes of very various sizes will result."[95]

M'Intosh and Masterman's emphasis on the likelihood of important disanalogy between artificial and natural conditions when it came to studying growth was not shared by all fish biologists, and they were not slow to criticize other workers who were not so worried.

> The method of rearing young fish in confinement and measuring them periodically must be condemned once and for all as absolutely useless; for, although the temperature may be registered, the conditions of nutrition and the exclusion of the check caused by natural selection upon 'individual tendencies' make all results obtained in this way abnormal and useless. This method has been resorted to by Mr Cunningham at Plymouth and by some other workers....it will be manifest that there are strong objections (which cannot be lightly dismissed as 'theoretical') to conclusions deduced from such experiments. This being the case the growth of fishes under natural conditions must be studied in another way.[96]

M'Intosh and Masterman forcefully rejected Cunningham's method of studying growth in favour of their own method, which involved catching sample populations of fish ("the more the better"[97]), estimating their ages based on knowledge of the species' spawning period, and measuring their sizes to determine a mean size for each age group. If these mean sizes per age group are compared with the known, species-specific maximum and minimum sizes for sexual maturity, then average ages and sizes at sexual maturity can be determined (M'Intosh and Masterman arrived at the finding that most food fish mature

[95] McIntosh and Masterman, *British Marine Food-Fishes*, 105.
[96] McIntosh and Masterman, *British Marine Food-Fishes*, 105-106.
[97] McIntosh and Masterman, *British Marine Food-Fishes*, 106.

during their third year, or during the second year for males, which was quite similar to Cunningham's findings for flounders, which he then generalized to flat fish as a group). The only way to be sure, however, was by examining the sexual organs of each individual fish to determine whether they are mature, and M'Intosh and Masterman ultimately rejected the usefulness (to science, at least) of making connections between size and sexual maturity. "Some observers have attempted to apply these terms," they write of maturity and immaturity, "as if they were synonymous with 'fully-grown,' but from what has been said above, the latter term is meaningless and even misleading as applied, at any rate, to the vast majority of fishes."[98] Answering the size/maturity question was, as M'Intosh and Masterman recognised, epistemologically problematic: it may even have suffered from a kind of fishy Uncertainty Principle. By raising fish from very young stages in the laboratory, as Cunningham had done, it was possible to be certain about their age, but by raising them in the laboratory under conditions that differed in many ways from their natural surroundings, it was impossible to be certain whether the size they attained at a given age was representative of wild fish, or was an artifact of their growing up under laboratory conditions. By catching fish from the sea, as the St. Andrews workers advocated (and as Cunningham did as well – for instance, by estimating the ages of numerous young flatfish sent to him from Grimsby by Holt),[99] it was possible to be certain that their size was representative of fish growth under natural conditions – but it was impossible to be certain about their actual age, which was basically estimated based on their size. The act of observing the ages and sizes of fish,

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[98] McIntosh and Masterman, *British Marine Food-Fishes*, 107.
[99] J. T. Cunningham, "Report on the Probable Ages of Young Fish collected by Mr. Holt in the North Sea," *Journal of the Marine Biological Association of the United Kingdom* 2 (1891-1892). Plaice from the North

under the conditions available to workers at the Plymouth and St. Andrews marine laboratories, made it impossible to be certain of the relationship between age and size, and so significantly clouded the pressing practical question of size and sexual maturity.[100]

This fundamental difficulty in studying fish, which M'Intosh recognised more strongly than most, paired with his arguments against proceeding by analogy from what was already known about land-dwellers and mammals to what was not known about fish, did not however lead him to conclude that any of the practical and scientific questions associated with food fish were unsolvable. Access to these answers would require lengthy and careful experimentation, and responsible regulation of the fisheries would require legislators to wait, perhaps for a long time, for scientists to deliver these answers to them. This was supposed to be the arrangement between M'Intosh and the Fishery Board for Scotland when the St. Andrews marine station was opened in 1884, in response to the

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Sea were known to be much larger than plaice from the English Channel, which further complicated the question of age, size, and sexual maturity.

[100] Work done not long thereafter at the Marine Biological Association Laboratory by J. Stuart Thomson provided one way out of this dilemma: Stuart Thomson established that sea fish deposit the material of their scales in annual rings, such that counting the rings in a section taken from particular scales gave a direct indication of the fish's age. Stuart Thomson traced this idea back to Leeuwenhoek; more recently, it had been suggested of the carp by Hoffbauer, a German fisheries biologist and marine station researcher, in papers published in 1899 and 1901. However, this discovery was not entirely free of the Fishy Uncertainty Principle either: Stuart Thomson had to conclude that "It is almost impossible to acquire direct proof of this hypothesis, the conditions of life in tank and aquarium being so unlike the natural haunts, yet even with this, I have already mentioned that in the case of a whiting which lived from shortly after hatching for thirteen and a quarter months in a tank, the number of growth lines formed on the scales during that period roughly agreed (after allowing for a slower scale growth under captive conditions) with the number of growth-lines in the scales from sea whiting calculated to be about the same age." (J. Stuart Thomson, "The Periodic Growth of Scales in Gadidae as an Index of Age," *Journal of the Marine Biological Association of the United Kingdom* 7 (1904-1906): 105.) How rough that agreement could be, how exactly to allow for slower scale growth in captivity, and how to calculate the wild fishes' age: the Uncertainty Principle strikes again. The promise of something like certainty was appealing, and the idea of annual rings in fish scales became popular, and was later funded by the International Council for the Exploration of the Sea (on which more later), until D'Arcy Wentworth Thompson presented a stinging criticism of the idea in 1910, at least as it applied to herring, saying that "scale rings were a physical phenomenon and that, while for a group of herring the mean number of rings was probably an accurate indication of the age of the group, the number of rings was subject to individual variation and thus not reliable for determining the age of individual fish." (Helen M. Rozwadowski, *The Sea Knows No Boundaries: A Century of Marine Science under ICES*, (Copenhagen: International Council for the Exploration of the Sea, in association with University of Washington Press, 2002), 82.)

recommendations of the Royal Commission of 1884, in which M'Intosh was a main participant. However, the FBS moved quickly, on the basis of what M'Intosh believed to be insufficient and poorly-obtained evidence, to close more and more areas to trawling. M'Intosh was incensed, and in the following years unleashed a series of published criticisms of the FBS' actions and investigative methods.

Limits to Influence II: M'Intosh's Critiques of the Mainstream

In his "Brief Sketch of the Scottish Fisheries" covering 1882-1892 and read at the 1892 meeting of the British Association for the Advancement of Science in Edinburgh, M'Intosh painted a picture of the gradual coming to knowledge that had taken place in British fisheries biology: "During the decade a notable feature has been the increase in our knowledge of the development and habits of British food-fishes. Previous to 1884 the life-history of scarcely a single marine food-fish had been studied...the field was almost untouched in Britain."[101] This period of ignorance was brought to an end, averred M'Intosh, by appointment in 1883 of a Royal Commission, on which M'Intosh sat, to investigate the effects of trawling. Seeing that "no reliable statistics could be produced to show whether the captures of sea fishes, other than herrings, were diminishing or increasing in Scottish waters" and that "no data were available for the support of the statement that the fisheries were everywhere diminishing", Commission Chairman Lord Dalhousie, along with the Fishery Board for Scotland established the St. Andrews Marine Laboratory in 1884. The Fishery Board had benefited from the Commission's report, which had recommended that the Board be given the power "to make bye-laws for the

[101] FRP, item 2, W. C. M'Intosh, "A Brief Sketch of the Scottish Fisheries, Chiefly in their Scientific Aspects, During the Past Decade, 1882-1892," 11.

regulation or suspension of beam-trawling, or any other mode of fishing within territorial waters".[102] When these powers were officially granted in 1886 with the passing of the Sea Fisheries (Scotland) Amendment Bill, the FBS moved quickly to close several inshore fishing areas, St. Andrews Bay, Aberdeen Bay, and the Firth of Forth, to trawling in order to perform more research using their newly-obtained steamer, the *Garland.* Aberdeen Bay was reopened shortly thereafter, but in 1886 the closed areas of the other two bays were enlarged, and the area from shore to three miles out in Moray Firth was closed for research reasons. The Firth of Clyde was closed in 1889, along with the entire three-mile limit along the Scottish coastline, and in 1891 the whole of Moray Firth was ordered closed.[103]

At the time of M'Intosh's speech to the British Association, the FBS had been doing experimental trawling on the closed St. Andrews Bay for six years and had produced a great deal of data. In his address, he mildly pointed out a number of limitations of the FBS statistics, including the early failure to include measurements of fish, no trawls at all during stretches of a month or much more, and other procedural problems. M'Intosh then compared the Board's numbers to a second data set from the year previous to the closures,

> which were made either for purposes of trade or for scientific experiment, but in every case under the direct supervision of myself. Thus, in 1884 four hauls of the trawl (which was over 50 feet) were made by powerful iron ships belonging to the General Steam Fishing Company, Granton; two by a fishing boat with the ordinary trawl (30 feet) then in use in St Andrews Bay; and fourteen by the 'Medusa'[ ], the little steam vessel from the Granton

[102] 1885 Royal Commission Report, quoted in FRP, "A Brief Sketch of the Scottish Fisheries," 5.
[103] McIntosh, *The Resources of the Sea*, 57.

> Laboratory, with a small-meshed trawl having a beam of 10 feet.[104]

M'Intosh's data set for 1884, which were reflective of both commercial and research-oriented trawling procedures, revealed to him that prior to the experimental closure of the Bay, it "contained a very considerable number of fishes."[105] In the years following the closure, his analysis of the FBS data revealed "that the captures of fishes in subsequent years followed the usual variations observed in all such operations in the sea. The closure of the Bay has not been followed by the growth of teeming multitudes of flat and round fishes within its limit,"[106] though he acknowledged that the larger fish would not be found in the inshore bays anyway, as they tended to migrate offshore. M'Intosh ended the speech by crediting the many scientific advances found in the pages of the FBS reports, by endorsing the minimum size limits recently resolved by the NSFPA, and by calling for more research facilities as well as the enrollment of fishermen in collecting statistics: "every fishing boat, trawler, crab-boat, and shrimper should furnish returns, on arrival in port or by other means, of the extent and nature of the catch, the position in the chart, the nature of the ground, weather, and other particulars. This would be no hardship, as a rule, since the blank forms could be filled up on the homeward journey from the fishing ground."[107]

In an address "On the Scientific Experiments to Test the Effects of Trawling in the Waters of Scotland" given to the International Congress of Zoology at Cambridge in 1898 that was published in the conference proceedings and in pamphlet form, M'Intosh stepped up his criticisms further, presenting a reanalysis of the FBS' own data and

[104] FRP, "A Brief Sketch of the Scottish Fisheries," 8.
[105] FRP, "A Brief Sketch of the Scottish Fisheries," 10.
[106] FRP, "A Brief Sketch of the Scottish Fisheries," 10.

concluding that "[they] show no gradual accumulation of fishes due to the closure...The idea that – because the first five years of the decade had a higher average than the second five years – diminution had ensued is shown to rest on insecure data."[108] To these methodological criticisms he added his own views on the uselessness of closing inshore bays in order to protect marine fish stocks: "The results, further, show that the mere closure of the three-mile limit has little or no effect on the fishes, invertebrates and plants of the area. The distribution of life in the ocean is on too great a scale to be affected by a measure so minute."[109] In a lecture given on April 6, 1898, in Aberdeen's Town and Country Hall, which the *Dundee Advertiser* reported was attended by "a very large audience, largely representative of those engaged in the fishing industry",[110] of whom the *Peterhead Sentinel* noticed that "the trawling interest was largely represented",[111] M'Intosh reiterated his criticisms of the FBS' research methods, concluded that the closures were not based on "satisfactory data",[112] and advanced his confidence in "the powers of recuperation inherent in the ocean."[113]

By the end of the decade, M'Intosh had adopted an even more harshly critical tone towards the FBS' trawling experiments and bay closures. "After operations of more than twelve years' duration," he asked in *The Resources of the Sea*, "can it be said to-day that the closure of the areas of the Forth and St Andrews Bay or any other area has been followed by a notable increase in the numbers and in the size of the food-fishes therein?",

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[107] FRP, "A Brief Sketch of the Scottish Fisheries," 15.
[108] PWCM, item 12, "Extracted from the Proceedings of the International Congress of Zoology, Cambridge, 1898. On the Scientific Experiments to Test the Effects of Trawling in the Waters of Scotland," 171.
[109] PWCM, "On the Scientific Experiments," 171.
[110] A1, newspaper clipping marked "Dundee Advertiser April 7th 1898," "East Coast Fishing Areas. Interesting Lecture by Professor M'Intosh," 84.
[111] A1, newspaper clipping marked "Peterhead Sentinel Apr 9th 1898," 85.
[112] A1, "East Coast Fishing Areas," 84.
[113] A1, "Peterhead Sentinel Apr 9th 1898," 85.

hot on the heels of a snarky footnote opining that "Those who read the Fishery Boards reports of to-day would imagine that the whole Trawling experiments and their adjuncts were the creations of the Board itself, whereas they were carefully discussed and mapped out by the Trawling Commission and especially looked after by Lord Dalhousie."[114] In undertaking these trawling experiments, the FBS had "possibly from lack of funds, deviated from the fundamental advice given" by Lord Dalhousie, purchasing a too-small, fairweather boat with an insufficiently large trawl rig, a set-up that "more or less crippled the experiments, especially as the work has only been conducted by day, and as a rule by a statistical, not a fishery expert, on board." [115] They had set themselves up to get unrepresentative results, M'Intosh was afraid.

Most upsettingly of all, M'Intosh was convinced that after the initial impetus from Lord Dalhousie and the powers granted through legislation brought about by the Royal Commission report, the FBS was not actually closing areas in order to research them, nor was it closing areas for their protection based on the results of earlier research. They were neither asking Nature nor acting on answers received from Nature. "The principle of which the closure was originally applied was that systematic and careful examination of these areas should be carried out for a considerable time. But such was impossible in the circumstances – especially of time and ship,"[116] wrote M'Intosh. When unexpected results had come in, showing that the number of fish captured was *less* when the waters were closed than they had been before, the FBS had not interpreted this as a clear answer from Nature, nor as an indication that their sampling methods were flawed, but instead concluded that trawling in the still-open areas was to blame, and closed those areas as

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[114] McIntosh, *Resources of the Sea*, 54-55.
[115] McIntosh, *Resources of the Sea*, 56.

well. The FBS was a body with both access to and influence over the marine food-fishes: access by virtue of their scientists and their research vessel, as well as through the information they received from affiliated bodies such as the St. Andrews Marine Laboratory, and influence by virtue of their powers to close areas to trawling. M'Intosh was upset that the FBS appeared to be using its influence without the benefit of its access, and doing so while claiming otherwise: "No one will question the right of the Government to take such a step in the interests of the fishing-population – on philanthropic, social, or even on political grounds; but if such a step were taken on the basis of the scientific evidence furnished by the Fishery Board, then it would appear that the premises (and here all matters of fact are included) do not warrant the conclusion."[117] The mild critiques of the FBS' methods that he had presented seven years earlier were now scathing: the FBS had made a "serious misapprehension"[118] based on work flawed seriously enough to have "more or less crippled the experiments."[119]

For the 1902 meeting of the British Association held at Belfast, M'Intosh prepared the sequel to his 1892 BA address, calling it "A Second Decade, 1893-1903. British Fisheries' Investigations and the International Scheme".[120] Like the 1892 installment, however, the ground covered by this address began in 1883 with the "comparative ignorance respecting the life-histories of the sea-fishes"[121] in Britain, the scientific beginnings courtesy of the Royal Commission trawling experiments, and the founding of the M'Intosh's St. Andrews laboratory by the FBS. Twelve pages of

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[116] McIntosh, *Resources of the Sea*, 58.
[117] McIntosh, *Resources of the Sea*, 58.
[118] McIntosh, *Resources of the Sea*, 58.
[119] McIntosh, *Resources of the Sea*, 56.
[120] FRP, item 4, W. C. McIntosh, "A Second Decade, 1893-1903. British Fisheries' Investigations and the International Scheme," transcript of address to British Association at Belfast, 1902.
[121] FRP, "A Second Decade," 3.

criticisms of the FBS' research methods followed: they had generated a set of observations "which could be compared with nothing which had been done previously, which was then being done, or which would be done in future";[122] they had chosen a poor ship and staffed it with clerical workers "whose duty was mechanically to fill in printed forms",[123] rather than with "[a]n experienced observer," the kind of worker who "could not fail to have gradually appreciated the complex conditions under which fishes appeared and disappeared;"[124] their "rough-and-ready methods of judging a question so complex [were] of little importance;"[125] they had made further closures without any compelling scientific reasons to do so; and worst of all, that their investigations "were founded on an assumption, which has never been proved, viz., that the sea has been ruinously impoverished by over-fishing, and that it was only necessary to enclose areas to witness rapid recuperation of the fishes when thus unmolested."[126] Such hard language might be understandable in a lecture to Aberdeen trawlermen, or even in a fisheries biology-oriented book like *The Resources of the Sea*, which was described by the *British Medical Journal* as being "neither more nor less than an indictment of the policy of the Fishery Board for Scotland in closing certain extensive waters surrounding North Britain against the access of trawlers,"[127] but this relentless attack was delivered to the mainstream scientific audience of the British Association. What's more, it was a lengthy attack on fisheries research and area closures that almost entirely predated the stated

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[122] FRP, "A Second Decade," 5.
[123] FRP, "A Second Decade," 6.
[124] FRP, "A Second Decade," 7.
[125] FRP, "A Second Decade," 11.
[126] FRP, "A Second Decade," 12.
[127] A1, p. 4 of insert between pages [113] and [114], review of *The Resources of the Sea* in the *British Medical Journal*. This positive review of *The Resources of the Sea* states that "...though the pages of the *British Medical Journal* are not the place for an elaborate analysis of its contents, yet the fish supply of the

range of the article. M'Intosh's 1902 address to the BA, along with the generally negative critical response by fisheries scientists to *The Resources of the Sea*, is a fairly clear marker of his departure from the mainstream of scientific opinion regarding the regulation of the fisheries.

M'Intosh's biographer, his great-nephew A.E. Gunther, ascribes M'Intosh's movement "into the wilderness" after 1898 to the discontinuation of government funding to the St. Andrews Laboratory in July of 1896 (four months before the laboratory's reopening in a new building with new, private funding as the Gatty Marine Laboratory), which "created a resentment which coloured his future attitude to the board and to its work."[128] It is likely that the end of the financial and official relationship between M'Intosh and the FBS made it easier for M'Intosh to criticize their policies, but he was already presenting these criticisms (to the British Association, what's more), albeit less harshly, several years before losing the £100/year contributed by the government to the operating expenses of the St. Andrews laboratory. His twelve-page criticism of FBS research and policies from before 1893 in a lecture intended to address the decade after 1893 can certainly be seen as the irrational outcome of an obsession with the Board, but it was more than that. The choices made, the conclusions reached, and the assumptions held by the FBS, whether proven or not, were the same assumptions that motivated a new target of M'Intosh's: representatives of the "Impoverishment of the Sea" view of food fish stocks, and proponents of the "International Scheme" of largely hydrographical (that is, focusing on physicochemical factors such as wind, weather, temperature, and salinity

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country is of so great importance in relation to dietetic problems that it is desirable to acquaint the medical profession with its contents."

[128] A. E. Gunther, 108.

of the sea as important influences on fish and fish stocks) fisheries research. Criticizing his new opponents began by undermining his previous one.

In response to *The Resources of the Sea*, and perhaps also in response to the daily newspapers' extensive coverage of the book's reassuring message,[129] the *Marine Biological Association Journal* in 1900 published the influential essay "The Impoverishment of the Sea" by Walter Garstang, Cunningham's successor as Naturalist of the Marine Biological Association Laboratory. Beginning with an assertion of the importance of physicochemical factors on the breeding of sea fish, Garstang then invoked M'Intosh 's "remarkable book" – which adjective he did not intend as a compliment. In contrast to M'Intosh's views on the inefficacy of man's influence in the face of the numerous other predators that menace food fish, Garstang had "no hesitation in affirming that, as regards the relative influence of the various destructive agencies upon the death-rate of flat fishes, the destruction directly effected by man far exceeds the destruction wrought by other enemies."[130] Whatever results Garstang looked at – official, unofficial, large-scale, or local – he found a "melancholy unanimity": bottomfish catches, when judged by his measure of catch per unit of fishing effort (smack-equivalents), were falling.[131] It had become a matter of "established fact that the bottom fisheries are not only exhaustible, but in rapid and continuous process of exhaustion;" in the greatest danger were the "more valuable flat fishes, plaice and prime fish" like sole and turbot.[132] Some sixty pages of detailed figures on fish catches and fishing effort followed, with occasional refutations of particular claims of M'Intosh's. Garstang's avalanche of data,

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[129] see A. E. Gunther, 111.
[130] Walter Garstang, "The Impoverishment of the Sea," *Journal of the Marine Biological Association of the United Kingdom* 6 (1900-1903), 3.
[131] Garstang, 8.

paired with his new measure of fish abundance that could be standardized across changing levels of fishing effort, made for a compelling and influential paper.

M'Intosh took up the gauntlet thrown by Garstang, seizing upon the title of Garstang's essay and of his own 1899 book to present the views contained in each as opposite schools of thought in fisheries biology -- which quite arguably they were. In a 1907 article in *The Zoologist* on "Scientific Work in the Sea-Fisheries", M'Intosh referred to "the antagonistic views, *viz.* – on the one hand, of the 'Resources of the Sea', and, on the other, of the 'Impoverishment of the Sea', which is really a revival of the old doubts and fears."[133] St. Andrews was the home base of the "Resources" view, and Plymouth was home to the contrary point of view. M'Intosh heaped scorn upon "the fisheries' work of the Marine Biological Association, a body which more or less identified itself with the 'Impoverishment of the Sea':"[134]

> From its workers, therefore, with their new and unequalled opportunities, we looked for substantial proof of the soundness of their position, more especially when it is stated that "facts have been obtained upon which a proper understanding of the yield of the sea must in future be based" – a pregnant sentence, which apparently dispenses with all previous observations at home and abroad. This statement appears to derive its origin, not from laborious surveys of the fish-fauna of the southern half of the North Sea – both practically and scientifically studied in the adult and young conditions – but from certain experiments with marked Plaice. The marking of Plaice has long been carried out by the Fishery Board in Scotland without important results…[135]

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[132] Garstang, 8.
[133] FRP, "Scientific Work in the Sea-Fisheries," 2.
[134] FRP, "Scientific Work in the Sea-Fisheries," 6.
[135] FRP, "Scientific Work in the Sea-Fisheries," 7.

If anything, M'Intosh's dislike of the Plymouth workers had grown since his and Masterman's disapproval of Cunningham's work on fish growth ten years previously. This dislike was probably then further inflamed by the heavy involvement of the Marine Biological Association in the new International Scheme of fisheries research, an initiative driven by Scandinavian fisheries biologists, many of whom were interested in hydrographical research. Indeed, his scorn for the MBA, registered on the pages of *The Zoologist*, had been delivered in the second of two lectures at the Royal Institution on the occasion of the publication of five years' worth of research made under the auspices of the International Scheme.[136]

The formation of the International Council for the Exploration of the Sea in 1902, as a result of annual international conferences held beginning in 1899,[137] raised M'Intosh's ire for two reasons: its great expense to the participating governments, which could instead have been supporting more fisheries research at home; and its heavy emphasis on hydrography, the physical conditions of ocean life, at the expense of zoological work. Unlike the Plymouth Laboratory after 1891, which diversified when H. N. Dickson was hired on as Assistant to the Director and undertook meteorological and physical observations of the weather and water of Plymouth, the St. Andrews laboratory had remained focused on biological work. The Marine Biological Association and its workers became heavily involved with the ICES work, and so became doubly damned in M'Intosh's view: for believing in the "impoverishment of the sea", and for embracing the hydrographical study of the sea represented by ICES.

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[136] A. E. Gunther, 132.

[137] On the history of international fisheries research and the establishment and work of ICES, see Rozwadowski; Adams, 111-112; Lee, 28-41; Jens Smed, "On the Foundation of ICES: A Look Behind the Scenes at the Events in Britain," *Buckland Occasional Papers* 2 (1996).

Having finished with the FBS in his Belfast address of 1902, M'Intosh turned to the International Scheme. Proponents of hydrographical research, he hinted, had, "perhaps, in the fisheries' problems a convenient opportunity"[138] to have governments fund their work; this work could easily be carried out in naval vessels, rather than on dedicated fisheries research vessels, "without burdening the Fisheries' Scheme with so heavy an expenditure and from which really so little can be gained."[139] The ICES work amounted to "pigmy meas[ur]es in the North Sea" which were "of little moment in the interests of the fisheries."[140] Instead, M'Intosh urged more work on the biology of food-fishes, their distribution and abundance, the effects of different classes of fishing vessels, all to be administered by government agencies in England, Scotland, and Ireland, each of which would be responsible for running "[o]ne, or at most two, Marine Laboratories, efficiently supported and energetically administered...The great International Laboratory proposed in the scheme would have a proneness to be out of touch with the several areas and their special fisheries."[141] M'Intosh's type of fisheries biology and his small marine laboratory, both of which had been at the forefront of fisheries work in Britain for the previous two decades, appeared to be becoming unfashionable internationally and so in danger of losing influence in Britain. But, as M'Intosh reminded his audience at every occasion, Britain had come very late to fisheries biology; by jumping into the International Scheme, British science and government were giving up on it too soon, giving their money and enthusiasm away to the latest Continental enthusiasm –

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[138] FRP, "A Second Decade," 17.
[139] FRP, "A Second Decade," 17.
[140] FRP, "A Second Decade," 21.
[141] FRP, "A Second Decade," 27. The ICES Central Laboratory was located in Kristiania, Norway. It opened in late 1902 and closed a mere six years later when its chief administrator and booster, Fridtjof Nansen, was appointed to be the Norwegian ambassador to London. (Rozwadowski,, 46-47.)

hydrographical research – and forgetting about the answers that marine zoology was still in the process of providing.

In his 1907 attack on the disciples of "The Impoverishment of the Sea" and the International Scheme in *The Zoologist*, M'Intosh took pains to show that he was not alone in his criticisms. H. M. Kyle had recently spoken out against the assumption of overfishing in a paper "On the Statistics of the Sea-Fisheries in the Countries of Northern Europe." He and a Mr. Archer had "assert[ed]", wrote M'Intosh, "that the average catch per boat is insufficient to prove over-fishing.[142] Kyle, who in 1903 had been appointed as the Biological Secretary to the Council,[143] had worked with new statistics generated by the International Scheme and found, reported M'Intosh, that "these show that there is no decrease in the total quantities over this very wide field, though it is possible the average size of certain adults may be reduced, yet the intermediate stages of the Plaice and the Haddock have increased."[144] This finding was very much in line with the model that M'Intosh and Masterman had presented in *British Marine Food-Fishes* several years previously. M'Intosh also directs the reader's attention to two other detractors of the International Scheme, Professor Noel Paton in his 1902 article "On the Proposed International Scheme of North Sea Investigations" and M'Intosh's own colleague and sometime coauthor Dr. A.T. Masterman, in his lecture to the Royal Society of Edinburgh in March of 1903. Having lined up his allies, M'Intosh again launched into an attack of the International Scheme's hydrographical emphasis:

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[142] FRP, "Scientific Work in the Sea-Fisheries," 2.
[143] Rozwadowski, 51. In the early years of ICES, the question of how to define and detect overfishing consumed the work of a committee formed by Garstang, the Overfishing Committee.
[144] FRP, "Scientific Work in the Sea-Fisheries," 15.

> The consideration of the results of these surveys, indeed, does not lead us to expect a solution of fisheries' problems from the hydrographers, however much we may appreciate their skilful and patient, though expensive, labours in other respects....Dr. H.M. Kyle, a trained marine zoologist, plainly says that neither the temperature-charts nor those for salinity exhibit a true parallelism with the biological phenomena, and suggests weekly instead of monthly data...[145]

Besides the ongoing expense involved in supporting the largely irrelevant measurements being made by the International Scheme researchers, M'Intosh by 1907 had fresh reason for alarm. The hydrographic hegemony had extended beyond ICES to encompass the object of his previous ire, the Fishery Board for Scotland, and to extend beyond the FBS' policy-makers to encompass its scientific staff. A large report had appeared in 1905; having read it, reported M'Intosh, "surprise was felt on finding that about three-fourths of it consisted of hydrographical work (part of which has already been published elsewhere)..." and recently "[a] second Report of a purely hydrographical nature has just appeared, as if to emphasize the surprise in connection with the first."[146] Hydrography had seemingly ceased to be merely a Continental enthusiasm, taken up by a worried British government and insecure scientists like Garstang and Thompson[147] out of

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[145] FRP, "Scientific Work in the Sea-Fisheries", 4-6.

[146] FRP, "Scientific Work in the Sea-Fisheries", 11-12.

[147] M'Intosh and Thompson despised one another, as described wonderfully by A.E. Gunther. Thompson had been appointed to the Chair of Biology at Dundee in 1884. Dundee had, like Manchester and other fast-growing British industrial cities, recently acquired a university college. Despite the distaste of old, elitist St. Andrews University for the new, accessible Dundee school, in order to continue growing in the areas of science and medicine it was decided, in large part by M'Intosh, that the two schools should establish a joint medical school. He would not, of course, stoop to teach natural history at Dundee, so Thompson was hired for that purpose. When an article in *The Scotsman* suggested that Dundee was the superior school for teaching and research opportunities, M'Intosh was enraged and wrote to Thompson, asking him to set the record straight – in effect, asking Thompson of Dundee to tell *The Scotsman* that St. Andrews was superior to Dundee. Thompson was understandably annoyed. Three decades of slights from M'Intosh followed: referring to Thompson only as "my junior colleague at Dundee", not allowing him to give science examinations to students at Dundee, but instead requiring them to travel to St. Andrews, blocking nominations Thompson tried to make for honourary degrees, and so on (Gunther, 181-182). When it came to speaking on the International Scheme, of which Thompson was an enthusiastic proponent,

eagerness to join the International Scheme and be part of the new wave of fisheries research. It had become the norm even in domestic fisheries work, and that left M'Intosh and the body of work he had contributed to British fisheries regulation appreciated, but irrelevant.

Conclusion: Into the Wilderness, Indeed

Particularly in the years of his retirement following the First World War, M'Intosh appeared to have become willfully blind to evidence of the decreasing fish stocks. In his formal letter of retirement to the Court of St. Andrews University, which summarizes his main contributions to Natural History at St. Andrews and particularly to fisheries research in the St. Andrews and later Gatty Marine Laboratories, M'Intosh wrote that "The work accomplished since the opening must speak for itself; though it may be mentioned, in passing, that the safety of the Sea-Fisheries of the country was first clearly shown at St. Andrews."[148] His fury at the continued government investment in the fisheries equivalent of Big Science did not abate: A.E. Gunther writes that in the early 1920s, "[t]he £70,000 a year that the International Council absorbed, when a few hundred would restore life to the Gatty, continued to arouse his ire, as did its neglect after the war."[149] A letter to his nephew, Robert Gunther, dated 20 July 1921 and reproduced in A.E. Gunther's biography of M'Intosh, synthesizes his dislike of Thompson, the International Scheme, and the Fishery Board for Scotland in four sentences of vitriol:

M'Intosh was sure to tell the Royal Institution audience that Thompson's contributions to the ICES research volume had already been published elsewhere previously (A. E. Gunther, 132).

148 OR, 5.

149 A. E. Gunther, 143.

> D'Arcy T. is of a different mould from Garstang….I am inclined to think it a scandal that for many years he draws a larger salary (in addition to his St. A. Chair) for making almost useless statistics, no doubt mainly collated by assistants in pay of the Scot. F. Board. The whole Internat. Explor. of the Sea is little less than a farce. The work should be done by the scientific staff of each nation.[150]

Nor was it only the fisheries science community and the government that were responsible for what M'Intosh believed more and more every year to be the erroneous belief that the fish stocks were dwindling. In his notebook entry for 25 August 1924, M'Intosh blamed the fishermen, not the lack of fish, for the inactivity of the St. Andrews fishing fleet:

> The men (fishermen) aver that the 'sweep' nets ruined the Bay, & many boats have stopped work and been sold – the men taking to other occupations. Yet it is said by…fishermen that there are plenty of plaice in the bay – tho' small – 8-11 in….There seems to be a want of applications to the fishing industry & disappointment at the limited catches. Dabs are not scarce. So far as can be observed it is not an absence of fishes but want of steady application.[151]

M'Intosh's earlier emphasis on the predatory activities of birds, whales, and the like became more strongly presented as well, in the introduction to a 1927 pamphletization of fifty years' worth of his St. Andrews fishery notes published in *The Fishing News*:

> If [the purported absence of food-fishes from St. Andrews Bay] were the case, the first to suffer from the supposed impoverishment would be the multitudes of seals, whales, sea-birds, sharks and predatory fishes of all kinds, the sum-total of whose levy on the food-fishes probably equals, if not far exceeds, the depredations of man in every clime.[152]

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[150] Letter, WCM to Robert Gunther, 20 July 1921, quoted in A.E. Gunther, 127.
[151] NB, 220-221. M'Intosh formally retired in 1917, but requested that he be allowed to continue work at the Gatty Marine Lab and the Museum "so long as strength lasts" (OR, 6.).
[152] FF, [3].

The final two pages of his notebooks are titled "Resources of the Sea", and appear to be a list, gleaned between 1927 and 1929 from the pages of *The Fishing News*, of captures of large fish in British waters. "[A] giant Turbot weighing 37½ lbs was caught on home grounds & brought to Grimsby market," M'Intosh recorded in December 1927, and on the same day "a plaice captured by the trawler "Naoon Padraig" in S. Irish waters wt. 11¾ lbs.". The next year, "Brixham fish smack 'Lance' landed at Brixham a phenomenal catch of turbot and rays on Sat (21st?) from trawling on the English Channel grounds…Largest catch in post war days."[153]

M'Intosh had settled into dogmatism during the years of his retirement, and it is tempting to say that he had settled in long before that. But dogmatism, at least in its early stages, is not necessarily the same thing as irrationality. In advancing arguments on behalf of his doctrine of *The Resources of the Sea*, M'Intosh showed remarkable sophistication in his thinking about fish. An emphasis on the importance of local environmental conditions, paired with an awareness of the incredible vastness of the sea; an understanding of the difficulty of using laboratory results to make claims about biological processes such as rate of growth in nature, paired with a consciousness of the challenges of making revealing and comparable experiments on regions of the sea itself; and a general skepticism about man's confidence in his ability to access, understand, and influence the sea based on the assumption that the sea was not importantly different from land or fresh water – these were M'Intosh's strengths, if too much conviction was his weakness. His journey from the centre to the periphery of opinion when it came to

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[153] NB, 223-224.

overfishing – or rather, the centre's journey away from him as he stood still over several decades – is a fascinating one.

Concluding Remarks

The closing decades of the nineteenth century were early days for the interaction of British biology with industry and government, and the beginning of organized investigations of economically important fish and fisheries by British biologists. Shortly after the century turned, there occurred an acceleration in both the amount of interaction and the extent of organization between biology and the fisheries in Britain, which in light of the research presented in this thesis can be seen as an elaboration of the work done during the 1880s and 1890s. In 1902, Britain joined with the other coastal nations of Europe in the establishment of the International Council for the Exploration of the Sea (ICES), a sophisticated, systematic, and large-scale study of the coastal waters of Europe, undertaken based in large part on the "idea that science should provide the basis for rational exploitation of fish stocks,"[1] an idea that several European nations, Britain included, had been pursuing independently for some time. Meanwhile, in Britain, the rapid development at the provincial universities of "Economic Biology," with the foundation in 1904 of the Association of Economic Biologists and in 1905 of the *Journal of Economic Biology* began a period of greatly increased government involvement with biology, chiefly as it pertained to agriculture.[2]

But if Britain fell in line with and came to resemble more closely its continental neighbours in the years following 1902, it had been rather a misfit in the preceding decades. From the virtual absence of scientific involvement with the fisheries decried by the likes of Lankester to what Helen Rozwadowski characterized in her history of ICES as the "piecemeal" development of marine biology in the United Kingdom "as a result of

[1] Rozwadowski, 3.

a patchwork of private, industry, and university support,"[3] British biology when it came to the sea had its own, rapidly developing character in the years prior to ICES. Continental governments were much more generous to their marine scientists and stations than was the tight-fisted British government; this scarcity of money meant that the fishing industry was a larger source of support for the Plymouth station in particular than for any Continental station, which made even more pressing the need to generate practical results. Reflecting in 1910 on the pasts and presents of the many biological stations of Europe, Charles Kofoid found in Britain "a relatively large absorption of the funds and activities of the British stations in scientific fisheries work."[4] The emphasis on fisheries work at the Plymouth station resulted from "the fisheries having had from the start a large interest in its support and direction;"[5] and at St. Andrews and elsewhere in Scotland "the economic factor [was] helpful or dominant in the foundation of the stations."[6]

In the absence of an overarching governmental plan or even reliable sources of financial support, those engaged in marine fisheries biology at the newly-opened British stations were to a large extent playing it by ear, combining scientific and practical work as closely as possible and on a limited budget. In every aspect of their practice, naturalists like Cunningham and M'Intosh worked simultaneously towards scientific and practical goals; in doing so, they were helping to strengthen the connections between British biology and the fortunes of the nation. In Chapter Five I discussed how the establishment of a specimen supply service by the Marine Biological Association helped

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[2] Alison Kraft, "Pragmatism, Patronage and Politics in English Biology: The Rise and Fall of Economic Biology 1904–1920," *Journal of the History of Biology* 37 (2004), 218.
[3] Rozwadowski, 26.
[4] Kofoid, 144.
[5] Kofoid, 144.
[6] Kofoid, 145.

to make the Association and its Plymouth station indispensable to British biology, a laboratory course dissecting type specimens, many of them marine, having become the gateway to advanced education and careers in biology. Similarly, through its involvement with research relevant to the management of the nation's fisheries, British biology was making itself indispensable to the governance of the nation, a position that was solidly established by the Edwardian period, but begun in the decades before at the marine stations in particular. Cunningham was in many ways the embodiment of the earliest form of this connection and commitment between British biology and the aims of government and industry, and making sense of his work reveals why and how this connection was upheld and strengthened.

In different ways, M'Intosh embodied the link between the fisheries and marine science as well. His view on the "resources of the sea", presented in Chapter Six, was a remarkable feat of connecting the two fields of marine biology and the sea fisheries. M'Intosh used remarkably sophisticated and creative scientific arguments to present an answer to a question of enormous importance to the fishing industry and anyone interested in the fishing industry, but it was the way he went about it that is most revealing of the nature of the connection being cultivated. Going against the method of arguing by analogy that had been so widely used in the preceding decades, M'Intosh established a set of disanalogies: between hunting or fishing on land and fishing in the oceans, and between the basic biology and conditions of life characteristic of land, air, and freshwater organisms and the organisms living in the vast, unknowable sea. The sea fisheries and the sea sciences needed one another: they were in the same boat, so to speak, far from land and the rules that held there.

One of the most effective ways in which the connection between scientific and practical work on sea fish was expressed was by Cunningham: "Both as an Organism and as a Commodity." This apparent double nature was remarkably complex; in its close attention to the details of getting, keeping, and studying food fish, my study reveals the multiple significances that these fish held for those who worked with them. Fish were organisms; within that category, they were organisms as individuals and as species, organisms in development and evolution. Individual fish could be normal or abnormal in appearance; if abnormal, they could be merely abnormal or genuinely monstrous. As organisms, fish could be the population living wild in the sea, or they could be the population dwelling in laboratory tanks; the fish dwelling in the laboratory tanks might be an experimental group living under intentionally artificial conditions of life, or as a control group or specimen source group living some semblance of a natural life. Fish were commodities; as such, they were both a natural resource and an economic resource, and as an economic resource they were the object of commercial consumption, exchanged for money, as well as an object of literal consumption, eaten for nourishment. As objects of commercial consumption, fish were both luxury goods and cheap, mass-produced coarse goods; as food, they were both a treat and a staple. They were caught and exchanged for money as food; within a smaller economy, fish and other marine organisms were caught and exchanged for money as specimens for scientific study, commodified on a smaller scale. Cunningham and M'Intosh both studied "hybrid" flatfish specimens (Chapter Three), and Cunningham and several of his contemporaries studied what were known as "double" flatfish (Chapter Four); in a sense, though, every fish they studied was a hybrid and a double. Scientists like Cunningham and others in the

MBA were able to see double, and fish became organisms and commodities both. Those involved in the fishing industry, similarly, became able to and interested in seeing their commodity as an organism as well, one which might need protection or cultivation in order to withstand the intensity of their fishing efforts. Members of regulatory bodies like the Fishery Board for Scotland also held such a hybrid view of fish; and in their enthusiastic visiting of fisheries exhibitions and reading the popular works produced by Cunningham and M'Intosh, the general public as well accepted that scientific understanding of one of their most popular foods was not just a reasonable aim, but an important and interesting one.

Nor was the hybrid organism/commodity view of fish only or entirely brought about because of marine biology's association with the fishing industry. Changes within the way living things were being subjected to scientific study, and in particular changes away from a field collecting-oriented, descriptive natural history mode of work and towards a more laboratory centred, experimental biology which were occurring in Europe and America during the late nineteenth century might also have played a role. In embryology in particular, the emphasis of work was shifting from the descriptive and evolutionary movements into a powerful experimental trend, beginning in Germany with the work of Wilhelm Roux, but quickly spreading throughout Europe and across the Channel and the Atlantic via Roux's reputation, publications, and journal, and by way of the Naples Zoological Station. The Naples Station had been established in 1873 by Anton Dohrn, originally in an effort to give zoologists, and especially those zoologists interested in embryos, better access to supplies of the marine invertebrates and fish that were believed likely to hold the secrets to vertebrate evolution. By virtue of its wide range of prominent international visitors, work done and techniques used at Naples could spread rapidly throughout the Western biological community, and experimental embryology was no exception.

Unlike descriptive natural history, some of which could be done on preserved specimens, experimental biology always required live specimens. Keeping these specimens alive long enough under controlled conditions to perform the experimental manipulation and for results to appear was a difficult task, and the very high likelihood of experimental failure, most often by way of the death of some or all of the experimental subjects, meant that a large number of these subjects had to be procured if there was to be any chance of getting meaningful results. In Chapter Three, I described how Cunningham faced and dealt with this constant need for more specimens, whether in the constant supply of young flounders needed for his colouration experiments as well as for his descriptive studies of early development in fish, which required the fertilised eggs to stay alive for at least a few days until they reached the stage of development of interest, at which point they could be preserved and sketched. The progress of his work relied critically on keeping a steady march of specimens coming through the laboratory door from the sea beyond. The fish specimens Cunningham needed were like coal for a boiler: they were organisms to be studied, yes, but at the same time they were a commodity for which suppliers had to be found. He found his suppliers within the local fishing community, and to a lesser extent among the local amateur naturalists, and constructed and participated in many specimen supply chains, connecting the laboratory to the sea by way of himself, his assistant William Roach, and the local fishermen with their boats. Their use as scientific specimens gave the fish another hybrid identity as both organism and commodity: Cunningham had to procure a supply of fish to keep up with the demand his work was generating, while the fishermen who became essential to supplying this commodity to Cunningham had to approach the fish as organisms, learning to collect eggs and milt and perform artificial fertilisation of fish at sea.

The MBA itself got involved in this hybrid commodity/organism approach to specimens with the introduction of its specimen supply service, discussed in Chapter Five. Providing a catalogue of readily available organisms and recasting an earlier survey of Plymouth Sound as a special orders catalogue, the MBA offered up its Plymouth Laboratory, and the specimen supply chains that had been built up there, as a link that would serve the inland marine science workers and teachers in a way much like the railways had functioned in the growth of the white fisheries described in Chapter One. The specimens would be landed at Plymouth and delivered to the Laboratory along the established supply lines; some specimens would be consumed by the work done in the Laboratory itself, but a significant proportion would be sent inland to feed the hunger for specimens to be consumed in the many schools, universities, and laboratories away from the coast. There were forces within marine biology itself, then, that already inclined some of its practitioners towards a view of fish that included elements of both organism and commodity, albeit within the far smaller-scale economy of specimen supply and demand. Perhaps this trend allowed the connection between marine biology and the fishing industry to be more easily established, and for each to recognise the other as a useful ally with at least some shared goals.

The twin, and linked, preoccupations of my studies of Cunningham and M'Intosh and the fish on which they made their careers have been access and control: getting and keeping, influencing and using. Both in the marine science and the fishing industry of the British late nineteenth century, access and control were the fundamental issues facing the participants. Chapter One described the remarkable improvements made by the fishing and related industries in accessing food fish in unprecedented numbers. There were two dimensions to this access. On the one hand, fishermen became much better at catching and landing many more fish, through the use of sailing and steam trawlers as well as by operating in fleets; they were able to access the fish in the ocean and bring them to land. On the other, the nation's several railways and the rules under which they could co-operate made it possible for the large inland population to gain access to fish; once this access became possible, demand grew rapidly and was nearly impossible to exceed. Neither dimension of access to fish could have existed without the other, and together they formed a sort of positive feedback loop that led to the spectacular growth of sea fishing in Britain, such that it could truly be called an industry. This same spectacular growth, this scaling-up of production and demand and access, made the control of the fish into an issue that concerned the fishermen and regulators of Britain: what could be done to exert an influence, or to restrict the negative influence of human activity, on the nation's sea fish stocks? Should the government step in and make rules against unchecked trawling? Should the trawlers regulate themselves? Was trawling exerting a negative influence at all? Was human influence even possible over the ocean and its inhabitants?

Science, and marine biology in particular, could offer to provide answers to the questions of control and influence haunting the nation's sea fisheries. Marine biology, argued proponents like Lankester and Playfair, was in a special position to provide answers far more trustworthy and satisfying than the anecdotal fishermen's reports used by the several Royal Commissions that had been appointed since the trawling industry had first provoked significant concerns. Marine scientists could get the answers because they had the access: access to the existing body of scientific knowledge about sea fish, and access to the methods to get more of this knowledge; more objective than any obtained by interviews with fishermen, this knowledge could be got by scientists from interviews, as it were, with nature. Just as the work of the fishermen allowed the inland population to gain access to sea fish as an important food source, the work of marine scientists could allow those involved in running and regulating the sea fisheries to gain access to new and reliable information about sea fish, information that would help them exert their influence for the benefit of the fish stocks on which the industry, economy, and food supply relied.

Throughout this study, I have used the notion of supply chains of various kinds: here, the trawlers and railways supplied fish as food for the British people inland; the scientists, granted funds to operate coastal laboratories that furthered their privileged access to the sea fish, supplied information to satisfy the demands of government regulators and stakeholders within the fishing industry. At the Marine Biological Association Laboratory in Plymouth, we saw several other supply chains in action, albeit operating within smaller economies. Cunningham established chains of suppliers to keep him in specimens; the MBA's specimen supply service allowed inland researchers and teachers of marine biology access to the specimens available around Plymouth; Cunningham's own research outputs were expected to supply articles to fill the pages of the Journal of the Marine Biological Association. Cunningham's struggles to exert control over his specimens by bringing them inside to live in laboratory tanks were not just for the benefit of his own work, but served to shore up the various kinds of supply chains in which he and the MBA had involved themselves.

These struggles were, as well, part of a broader scheme to involve marine biology in the far larger supply chain of commercial fishing by literally supplying fish – not just information about fish, but the fish themselves – to the British fishing industry and its customers by establishing fish hatcheries along Britain's coasts. This broader scheme will be the subject of my future work, expanding on both the institutional history of British biology and the rapidly-increasing importance of biology as the twentieth century approached. The work of this thesis has been to show how, at a number of different levels and areas of practice, scientific and practical work were intertwined in the early British marine fisheries biological work done by two fortuitously placed naturalists. As both biology and the fisheries in Britain experienced a simultaneous increase in size and importance, the two areas became more involved with one another, each finding reasons to be more aware of the questions and problems faced by the other.

Otto Mayr's call to explore the relationship between these two sets of aims – whether we call them science and industry, science and technology, pure and applied science, or, as I have in this study in keeping with the terms used by the participants, scientific and practical work – not in order to determine some universal finding, but instead to see how the relations are articulated and practiced at a particular place and time, is an important one, no worse the wear for being thirty years old. To a large extent, the shift to discussing "technoscience" as it is manifested at select historical moments has made use of the same set of values, and added a broader outlook that encourages us to look at the materials and apparatus, the fish and tanks of science, as much as the biologist trying to study one by the use of the other. This thesis has presented both the public articulations of the relationship between scientific and practical work during the late nineteenth century, and the workplace realities of that commitment to connecting the two.

There is work to be done. Two biologists essential to connecting the early stages of British marine fisheries biology with the national and international integration that characterized the first years of the next century, D'Arcy Wentworth Thompson of the University of Aberdeen (and later St. Andrews) and W. A. Herdman of the University of Liverpool,[7] have appeared only occasionally in this thesis, but carried significant weight when it came to arguing for and demonstrating the connections between biology and the fisheries. Herdman's example would also illuminate another as yet unexplored dimension of this story: the role of local control and the fisheries initiatives of the several County Councils of England.

My thesis has shown the relationship between the station naturalists and the local fishing community, mediated by fishermen-assistants; more attention to the fishing communities themselves and their involvement with practical and scientific education and public lectures, as well as their attitudes toward the specimen-seeking (and status-seeking) biologists, and the biologists' attitudes toward the fishermen, would be a rich area of work. Another group of stakeholders, the general public who attended fisheries exhibitions and lectures in droves, read popular scientific works like Cunningham's *Marketable Marine Fishes* and M'Intosh's *The Resources of the Sea*, ate fish and chips,

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[7] Herdman is discussed briefly by Kraft, and is the subject of a section of June Jones, *Science, Utility and the "Second City of Empire": The Sciences and Especially the Medical Sciences at Liverpool University 1881–1925*, (Ph.D. thesis, The University of Manchester Institute of Science and Technology (UMIST), 1989), cited in Kraft.

and worried about the decline of the fish populations, are worthy of further investigation, particularly in light of the more general anxieties over the possibility of decline and degeneration that characterized British society – and was reflected in British biology – of the late century.[8]

The same questions and worries that motivated scientists, legislators, fishermen, and consumers alike into associating with one another in the late nineteenth century are still with us today. Are we taking too many fish from the sea? How can we know? Can science help? Is it worth paying for? Cunningham's and M'Intosh's careers by the sea, different in their situations but similar in their difficulties, studies, and publications, were the result of these questions being asked and answered in favour of biology, simultaneously scientific and practical biology that promised to strengthen Britain's vital links to the sea that surrounds it.

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[8] See Lester, 87, and references.

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Appendix 1

J.T. Cunningham's contributions to the *Journal of the Marine Biological Association of the United Kingdom*

Year	Title	Type	Pages
1888 (number 2)	Preliminary Inquiries at Plymouth into the Marine Fauna and the Ova of Fishes	Article	194-201
(total number of pages: 266)	Some Notes on Plymouth Fishes:		
	The Habits of the Cuckoo or Boar-fish	Notes and Memoranda	243-245
	The Breeding of the Conger	Notes and Memoranda	245-247
	The Spawn of the Pilchard	Notes and Memoranda	247-248
	Reproductive Organs of the common Sole	Notes and Memoranda	248-250
1889-1890 (Volume I New Series)	Studies on the Reproduction and Development of Teleostean Fishes occurring in the Neighborhood of Plymouth (with Pls. I-VI)	Article	10 to 54
(total number of pages: 472)	The Vernacular Names of Common Fishes	Notes and Memoranda	92-94
	Tealia tuberculata, Cocks: a Study in Synonymy (with Pl. XIX)	Article	205-210
	Anchovies in the English Channel (with an illustration in the text)	Article	328-339
	in Notes on Recent Experiments relating to the Growth and Rearing of Food-fish at the Laboratory:		
	II. The Rearing of Larval Fish	Article	370-375
	Colour changes in *Cottus Bubalis*	Notes and Memoranda	458-459
1891-1892 (Volume 2 new series)	On the Reproduction and Development of the Conger	Article	16-42
(total number of pages: 404)	On Some Larval Stages of Fishes (with Pls. III and IV)	Article	68-74
	Ray's Bream	Notes and Memoranda	78

	The Egg and Larva of *Callionymus lyra* (with Pl. V)	Article	89-90
	The Rate of Growth of some Sea Fishes, and their Distribution at Different Ages	Article	95-118
	On the Development of *Palinurus vulgaris*, the Rock Lobster or Sea Cray-fish (with Pls. VIII and IX)	Article	141-150
	The Reproduction and Growth of Pilchard (with Pl. X)	Article	151-157
	The Distribution of *Crystallogobius Nilssonii*	Article	158
	Saphenia mirabilis, Haeckel	Notes and Memoranda	194
	Pleurophyllidia Loveni, Bergh.	Notes and Memoranda	194-195
	Breeding of Fish in the Aquarium	Notes and Memoranda	195-196
	On a Species of Siphonophore observed at Plymouth	Article	212-215
	On the rate of Growth of some Sea Fishes, and the Age and Size at which they begin to Breed	Article	222-264
	Ichthyological Contributions	Article	325-332
	Report on the Probable Ages of Young Fish collected by Mr. Holt in the North Sea	Article	344-362
	Year-old Pilchards	Notes and Memoranda	398
	Muggiaea atlantica	Notes and Memoranda	398-399
1893-1895 (Volume 3 new series)	The Immature Fish Question	Article	54-77
(total number of pages: 458)	Researches on the Coloration of the Skins of Flat Fishes	Article	111-118
	The Life History of the Pilchard	Article	148-153
	The Ovaries of Fishes	Article	154-165
	Aphia pellucida, Day (Latrunculus pellucidus, Collett).	Notes and Memoranda	166
	Specialised Organs seen in Action	Notes and Memoranda	166-167

	Growth of Fishes in Aquarium	Notes and Memoranda	167-168
	Rearing of Fish-larvae	Notes and Memoranda	168
	Young Stages of *Zeugopterus punctatus*	Article	202-205
	Experiments of the Rearing of Fish Larvae in the Season of 1894	Article	206-207
	Fishery Publications in the United States	Article	236-245
	Experiments and Observations made at the Plymouth Laboratory:	Multi-Section Article	
	I. Diagnostic Characters in Flat Fishes		247-258
	II. The Development of the Egg in Flat Fishes and Pipe-fishes		258-271
	III. A Piebald Plaice		271-272
	IV. Growth and Distribution of Young Food-fishes		272-274
	V. Notes on Rare or Interesting Specimens		274-277
	The Larva of the Eel	Article	278-287
	The Migration of the Anchovy	Article	300-303
1895-1897 (Volume 4, new series)	North Sea Investigations	Article	10 to 47
(total number of pages: 425)	Additional Evidence on the Influence of Light in producing Pigments on the Lower Side of Flat-Fishes	Article	53-59
	On a Specimen of Leptocephalus Morrisii	Notes and Memoranda	73-74
	The Reproductive Maturity of the Common Eel	Article	87-89
	North Sea Investigations (continued).	Article	97-143
	Recent Reports of Fishery Authorities -- The Scottish, Newfoundland, and United States Reports	Article	203-218
	Physical and Biological Conditions in the North Sea	Article	233-263

	On the Peculiarities of Plaice from different Fishing Grounds	Article	315-359