

A Canadian Tragedy¹

by Neil Frazer²

Like most newspaper columnists, Stephen Hume receives a lot of peculiar e-mail. Especially bizarre were the e-mails he received in response to his columns in the *Vancouver Sun* discussing recent research on sea lice. E-mailers urged him to investigate both Alexandra Morton, a biologist who studies whales and sea lice, and Alexandra's mother, a futurist author. They urged him to investigate Martin Krkošek, a recent PhD from the University of Alberta who also studies sea lice. They urged him to investigate the editorial board of *Science*, one of the two most-respected scientific journals in the world. They urged him to investigate the editorial board of *PLOS Biology*, another highly respected scientific journal.

To understand why Hume's correspondents were so provoked, it is helpful to read a copy of *Northern Aquaculture*, the self-proclaimed "Voice of Cold Water Aquaculture in North America." North America might be a bit of a stretch, but *Northern Aquaculture* is published in Victoria, and it is likely that most salmon farmers in B.C. read it. I suspect that many B.C. politicians and bureaucrats also read it. I'm a loyal subscriber for sentimental reasons: *Northern Aquaculture*'s editor, Peter Chettleburgh, once wrote a fine book about the marine parks of B.C., and I keep hoping he'll write another.

The January-February 2008 issue of *Northern Aquaculture* has three front-page stories. The first front-page story describes "a new campaign to bring aquaculture to the forefront in the thinking of federal politicians and their senior staff." The second front-page story, with headlines in bright-red type, tells us: "New study describes environmental benefits of marine net-pen systems." Reading on, we learn that two consultants have shown that salmon net-pens in Puget Sound have over a hundred different species of seaweed and marine life growing on their anchor lines. The study is made to sound important, but in fact, it wasn't published in a scientific journal, possibly because scientists are already aware that marine organisms grow on any structure that hasn't been coated with toxic paint. All the front-page stories were written by Quentin Dodd, who is listed on the masthead of *Northern Aquaculture* as a regular contributor. Quentin is a pleasant, older man who lives in Campbell River. He would be the first to admit that he doesn't understand much about science.

The third front-page story, also by Quentin Dodd, is headlined "Industry and government refute alarmist predictions in *Science* journal," and sub-titled, "Industry and government question credibility of peer-review process." The body of the article lists some criticisms from anonymous spokesmen for the salmon farming industry, and then informs us that the paper in *Science* was "roundly disputed within DFO³, particularly from (sic) Dr. Brian Riddell, as the department's leading scientist on the sea lice issue, and by senior research

¹ This essay may be freely copied. It was written May 2008, and revised November 2008.

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³ Fisheries and Oceans Canada is invariably referred to in Canada as DFO.

scientist Dr. Simon Jones.” The paper Quentin refers to as disputed by DFO is entitled “Declining wild salmon populations in relation to parasites from salmon farms” published December 14, 2007 in *Science*. Authors of the study are Martin Krkošek, Jennifer Ford, Alexandra Morton, Subhash Lele, Ransom Meyers and Mark Lewis.

Here are some excerpts from the remainder of the article in *Northern Aquaculture*:

“Jones [‘the DFO senior research scientist’] said in a brief statement to this writer that DFO scientists found the paper to be so seriously flawed and out of keeping with the department’s own scientific-study and run-monitoring findings, that the agency felt it needed to give considerable thought to how best to respond to it, not just in the short term but also in the longer term, perhaps with a scientific peer-reviewed paper of its own – which could take months not just to write but to appear.”

“Jones, who does dozens of reviews of scientific papers for well-regarded journals each year said he felt the paper was so flawed that it casts doubt on the peer-review process.”

“Riddell [‘DFO’s leading scientist on the sea lice issue’] told this writer that he believes the risk assessment in the paper is ‘overstated’ as a whole and ‘significantly overstated’ when it comes to the idea of a 99% collapse in four generations.”

“Riddell noted that the paper draws on salmon run assessments and other studies by the department going back to about 2001 and 2003; and he said that, contrary to suggestions in the report, salmon-run statistics in the Broughton show no clear pattern of ongoing major decline over that period.”

“Riddell stated strongly that a major pattern needed to be fully and scientifically demonstrated before a forecast of the kind put forward in the paper could be confidently claimed on a scientific basis.”

Having read this article in *Northern Aquaculture* it is easier to understand what agitated Hume’s emailers. After all, DFO’s “leading scientist on the sea lice issue” and a DFO “senior research scientist” have both condemned the Krkošek paper in *Science*. How could it have been allowed to appear? And what is *Science* anyway, a salmon farmer might ask. It’s certainly not seen on most newsstands. Is it published by American environmentalists, or financed by Alaskans trying to destroy B.C.’s farmed salmon industry? Why would an article faulting salmon farming gets so much publicity when two DFO scientists, designated experts on sea lice, agree that it’s fundamentally unsound? Didn’t they say that the article was so bad that it calls into question the peer-review process?

But *Northern Aquaculture* is not alone in its condemnation of the article in *Science*. No less an authority than Pacific Salmon Forum, chaired by John Fraser, has pronounced on it. Here is the important part of what Fraser said in his press release of December 18, 2007:

“Since 2005 the Forum has commissioned some \$2.5 million in field and laboratory research, most of it focused on the Broughton Archipelago involving more than a dozen of the leading scientists in Canada. This research, which is taking place under the guidance of a Science Advisory Committee composed of many of Canada's leading fish biologists, will not be complete until the end of 2008, at which time its overall findings will be peer reviewed and made public.”

“However, interim findings from this research, to be released in early January 2008, do not support the Krkošek prediction of rapidly declining pink and chum salmon stocks in the Broughton. The marine survival of pink salmon to the Glendale River, the region's major producing river for pinks has been equal or better than the survival rates for pinks in other coastal watersheds where there are no salmon farms. Pink salmon returns in the other Broughton watershed were as good as or better than those that occurred in 2005. All the field researchers noted that over 80 percent of the wild salmon smolts migrating out of the Broughton in the spring of 2007 had no lice whatsoever.”

There you have it: John Fraser, Queen's Counsel, Order of Canada, former federal cabinet minister, backed by “a dozen of the leading scientists in Canada,” says there is no evidence for a decline of pink salmon in the Broughton Archipelago⁴.

Several years ago, in Port Hardy, a little town near the north end of Vancouver Island, the mechanic working on the engine of my boat raised the subject of sea lice. “Alexandra Morton,” he explained “is not a scientist because she does not have a PhD.” One of his friends read that in *Northern Aquaculture*, I suppose. Obviously, nobody in Canada's DFO had taken the trouble to explain to *Northern Aquaculture* that a PhD isn't essential to being a scientist. Later, as we pattered out of Hardy Bay into Queen Charlotte Strait, I wondered how science had become a spectator sport in B.C.

In 2005 Marty Krkošek, Mark Lewis and John Volpe published an elegant paper⁵ in *Proceedings of the Royal Society B* (an internationally-respected U.K. journal that also accepts only a small fraction of papers submitted to it), showing that a salmon farm in the Broughton Archipelago caused elevated levels of sea lice larvae at distances up to 35 km. The method of the study was to use migrating pink and chum as sentinel fish, and invert their infection levels for the ratio of farm-origin larvae to background larvae. Shortly after this paper was published I visited a friend named Terry, who works for the B.C. government in aquaculture management in Courtenay. Terry assured me that the Krkošek *Proc. B.* paper had been refuted by a Professor Alistair McVicar from Scotland. Terry was certain that

⁴ As one of the founding mandates of the Pacific Salmon Forum is “to increase public confidence in fisheries management generally, and aquaculture in particular, in the marine environment,” Fraser's dozen leading scientists appear to have earned their \$2.5 million in grants.

⁵ Krkošek, M., Lewis, M.A., Volpe, J.P. 2005. Transmission dynamics of parasitic sea lice from farm to wild salmon. *Proceedings of the Royal Society, B-Biological Sciences* **272**: 689–696.

McVicar's comment had also been published in *Proc. B*. In fact, McVicar's comment was never published in a scientific journal; it was published on the web site of the B.C. Salmon Farmer's Association. Terry can be forgiven for his error because that website led readers to imagine that McVicar's comment had been published. What McVicar's comment mainly showed was that he had no understanding—not even a little—of the mathematics used in the Krkošek *Proc. B*. article. McVicar was a retiree from Scotland who had been brought to Canada by DFO.

The reporter Quentin Dodd, the mechanic in Port Hardy, the aquaculture specialist in Courtenay, and the former cabinet minister who heads Pacific Salmon Forum, have a lot in common with most readers of *Northern Aquaculture*. They are all non-scientists, and they all believe that government scientists such as Riddell and Jones would not mislead them. It's easy to understand why. The late Bill Ricker, a fisheries scientist who worked for the federal government in B.C., was so highly respected that the American Fisheries Society named an annual award after him. It's called the "W.E. Ricker Resource Conservation Award." Perhaps the American Fisheries Society was aware that federal fisheries science in Canada was reorganized in 1978 to muzzle outspoken scientists like Ricker, and that they were unlikely to see another like him for many years⁶.

What readers of *Northern Aquaculture* didn't learn from the articles by Quentin Dodd, probably because Quentin didn't know, is that most working scientists would regard the comments of Riddell and Jones as peculiar in the extreme. Scientists seldom comment publicly on the work of other scientists in their field. On the rare occasions when they do, it is usually because the editor of a scientific journal has asked them to do so for the benefit of other scientists, which is not the case here. Given the critical nature of Riddell and Jones' comments, and given that those comments were not for other scientists, it is fair to ask whether they are qualified to comment.

Scientific qualifications are difficult for the public to decipher because scientists don't label themselves the way medical doctors do. Medicine has committees of experts that certify doctors as competent in particular areas, and such doctors are said to be board-certified. Most people understand that a radiologist isn't qualified to criticize the work of a neurologist, and vice-versa. Unfortunately, if you want to evaluate the expertise of a research scientist, you really need to read his published papers. Those papers aren't readily accessible and they tend to be written in technical language that is difficult for non-scientists to understand.

To find peer-reviewed scientific papers, you go to something like ISI "Web of Science" and do a search. (Unfortunately "Web of Science" isn't free. You have to be at an institution that subscribes to it, and the subscription is expensive.) My search under "Riddell BE" and "Riddell B" turned up nineteen research papers published between 1981 and 2008, on four of which Riddell is first author. Riddell has about 631 citations and an h-index of 10, meaning that more than 10 of Riddell's papers have been cited 10 times. The most recent paper on which he is first author was published in 1991.

⁶ See: Hutchings, J.A., C. Walters, & R.L. Haedrich. 1997. Is scientific inquiry compatible with government information control? *Canadian Journal of Fisheries and Aquatic Sciences* 54:1198–1210.

Nineteen papers in twenty-seven years puts Riddell near the bottom of the heap among university scientists, although that level of productivity is respectable for a government scientist-manager of his age. The fact that he hasn't published a first-author paper for seventeen years indicates that he is much more of a manager than a scientist. More relevant is that none of Riddell's papers treat sea lice, or any other host-parasite system—reading his papers, you would never guess that mathematical ecology exists.

The important point is this: No scientific journal, or committee of experts would seek Riddell's opinion on the paper of Krkošek et al. in *Science*. When the editors of *Science* wanted commentary on the Krkošek et al. paper, they asked Ray Hilborn, an ecologist at the University of Washington. A search in "Web of Science" for "Hilborn R" excluding "Hilborn RC" turns up 138 papers in fisheries and ecology. Hilborn has 2882 citations and an h-index of 27. He's also the co-author of an acclaimed textbook, *The Ecological Detective*. When Hilborn was asked to comment on the paper of Krkošek et al. he independently re-analyzed their data and came to the same conclusions that they did. I am not telling you anything here that Riddell doesn't know. So the only interesting question is: *Why did Riddell open himself to ridicule by making such comments?*

With that question in mind, we examine the work of Simon Jones. Using "Web of Science" to search for papers by "Jones SRM" turns up 47 papers, with 508 citations, including self-citations, and an h-index of 11. This is not bad for a government scientist of Jones' age. He has four first-author papers concerning sea lice, and I've listed those in the Appendix. Let's take a look at them to see if they qualify him to pronounce on the Krkošek paper in *Science*, or any of the earlier papers by Krkošek and his co-workers.

Paper 1 is a study in which juvenile pink and chum salmon were infected with sea lice in a laboratory. The study finds that both fish developed strong immune responses, pinks more than chums. This is the kind of laboratory science in which Jones is well qualified by training and experience.

Paper 2 is a study of sea lice on threespine sticklebacks in the Broughton Archipelago. The authors collected over a thousand sticklebacks, which held more than nineteen thousand sea lice. The average number of sea lice per stickleback was lower in areas of lower salinity. Oddly, the data were not examined to see whether sticklebacks sampled near salmon farms had more lice than sticklebacks sampled distant from farms—in fact, the map in the paper shows no salmon farms at all. (To understand the significance of that omission, imagine a study of lung cancer risk factors in which cancer patients are asked detailed questions about diet, but are not asked whether they smoke.) The really striking thing about the data presented in this paper is that none of the nineteen thousand lice had eggs, so it is clear that lice were not reproducing on the sticklebacks. This is hugely important because DFO had been hoping to blame sticklebacks for the elevated levels of early-stage lice on juvenile salmon migrating past salmon farms. Instead of pointing out the significance of the lack of eggs, the paper states "Sticklebacks appear to serve as temporary hosts, suggesting a role of this host in the epizootiology of *L. salmonis*." An innocent reader is thus invited to conclude that lice survive on sticklebacks, then cause epidemics on wild juvenile salmon. It is much

more parsimonious to suppose that lice survive over the winter on the millions of farmed salmon now present in the study area.

Paper 3 is a laboratory study in which the authors experimentally infected sticklebacks with sea lice, hoping that the sea lice would reproduce on the sticklebacks. No luck. This work confirms results in paper 2: sticklebacks can't be responsible for elevated early-stage lice on juvenile salmon—it is more likely that sticklebacks act as a sink for lice—but the paper carefully refrains from pointing that out. In the discussion section of the paper the authors contradict their own data by stating: “No evidence generated in this study refuted the hypothesis that *L. salmonis*, although commonly referred to as the salmon louse, parasitizes and subsequently develops on the threespine stickleback.”

Paper 4 analyzes data analogous to those of paper 2, except that the sampled fish are juvenile pink and chum salmon instead of sticklebacks. As in paper 2, the map of the study area shows no salmon farms, and no effort was made to look for a farm effect by comparing infection levels of fish sampled near farms with fish sampled distant from farms. My earlier remark about lung-cancer research also applies here.

None of Jones' papers show any familiarity with the techniques, called meta-population analysis⁷, needed to tease out the relation between salmon farming and declines of wild salmon. There are scientists in Canada who are good with those techniques—Ransom Myers (now deceased) at Dalhousie University in Halifax was a master of them, as is his student Jennifer Ford. Randall Peterman at Simon Fraser University in Vancouver is also very experienced in that area.

A few years ago, I attended a symposium at which Jones presented the data in papers 2 & 4. After his presentation Jones admitted that his data would have much more value if similar data were gathered from an area without salmon farms—what scientists call a control—but that there were no plans to gather such data. In other words, DFO had no plan to do real science. The low point of the meeting (for me) was during the question period when Jones remarked that he didn't think sea lice increased the mortality of juvenile salmon. Mortality is a technical term for the reciprocal of life expectancy, so what Jones was saying is that a one-gram pink salmon with adult sea lice on it has the same life expectancy as a one-gram pink salmon with no lice. This is much like saying that a human with weasels clamped to his back has the same life expectancy as he would without the weasels—it's just stupid. Jones had a big smile on his face when he said it to the audience, which consisted mainly of salmon farmers and bureaucrats such as my friend Terry, the B.C. government aquaculture coordinator. I felt embarrassed for Jones, the way you feel embarrassed when a co-worker is humiliated.

Neither Riddell nor Jones is a fool. They both know that when you increase the density of fish in an area by farming salmon all year long, parasites of salmon are going to proliferate. They both know that sea lice larvae drift in and out of salmon net cages. They know that sea lice from salmon farms are going to infect wild juvenile salmon. They know that a pink

⁷ The trick is to scale stock data so that parameters have the same meaning for each stock, then combine stocks to reduce noise.

salmon weighing half a gram with an adult sea louse on it is less likely to survive. They might *hope* that there is no population-level effect, meaning that increased lice-induced mortality of juveniles is compensated by reduced mortality at another stage of life. However, what Krkošek et al. (2007) showed in their *Science* paper, is that there is a population-level effect: pink salmon stocks exposed to salmon farms have reduced population-level growth rates compared to stocks not so exposed. The effect isn't uniform in the Broughton Archipelago—the Glendale River has a spawning channel that increases egg-to-fry survival—but some stocks there appear to be headed for extinction. The results in the Krkošek *Science* paper are consistent with results for salmon farming in other countries, as shown in a recent paper by Jennifer Ford and Ransom Myers in *PLOS Biology*⁸.

The methods used by Krkošek et al. in their *Science* paper aren't fundamentally new, although they've advanced a bit since Riddell was in graduate school. Riddell knows that if he were still a working scientist instead of a manager, and DFO were different, he might have been a co-author of the paper. If I were in his shoes, I'd feel bad about that. I'd feel even worse if I felt obliged to scorn that paper for readers of *Northern Aquaculture*.

When professionals such as Riddell and Jones make public statements they know are unscientific, it is because they are afraid of something—afraid of losing their research budgets, afraid of losing their jobs, afraid of losing their pensions. In an organization like DFO, I suppose some of them are so used to the fear that they don't notice it anymore. But some of them notice. Brent Hargreaves collected the sea lice data analyzed in Jones' papers about the Broughton Archipelago. If you ask Hargreaves why he hasn't looked for the salmon farm effect in his data he might say something like this: "Every time Gordon Hartman goes to pick up his check, it is 25% less than it should be." Gordon Hartman is a scientist who worked for DFO when the Aluminum Company of Canada wanted water from the Nechako River for its smelter. When the bureaucrats asked Hartman how much water could be taken out of the Nechako without damaging its famous runs of chinook salmon, he thought they really wanted to know.

The quotes from Riddell and Jones printed in *Northern Aquaculture*, remind me of how the U.S went to war in Iraq: experts in positions of public trust said things they knew were improbable because they supposed that if they didn't, someone else would be found to say them. Colin Powell, then Secretary of State, told the United Nations General Assembly that Iraq had weapons of mass destruction. George Tenet, then head of the U.S. Central Intelligence Agency, confirmed that Iraq had bought yellowcake in Africa. Powell and Tenet are now on full pension, and the United States is in the sixth year of a war that has taken the lives of half a million Iraqis at a projected cost of \$35,000 per U.S. family.

The lesson I take from Riddell and Jones, no less than from Powell and Tenet, is that individual action matters. Powell and Tenet were not required to be heroes in order to save their nation. All they had to do was to be willing to live on smaller pensions. I find it difficult not to think of both Riddell and Jones as tragic figures, and I regard Canada's

⁸ Ford, J.S. and R.A. Myers. 2008. A global assessment of salmon aquaculture impacts on wild salmonids. *PLoS Biology* 6(2):e33.

attempted deceit of its citizens regarding the effects of salmon aquaculture on wild salmon as being just as tragic as the Iraq war. Governments have been deceiving the governed since the beginning of time, but they get away with it only when government employees go along. If global leadership in aquaculture is more likely to be won by facing problems and solving them, rather than denying them, then Canadian salmon farmers are also poorly served by denials. In any case, aiding denial by telling salmon farmers what they want to hear does not seem to me to be work that a scientist would willingly choose—if he had a choice.

In order to understand Riddell and Jones' employer (DFO) it is useful to read an article by M.C. Healey⁹ published in 1997. I've come to think of this article as the Healey Doctrine, because of its forthrightness in stating DFO's position. Healey writes: "Although stock conservation is an important objective, it is by no means the minister [of Fisheries and Oceans] only concern, and sometimes not even his or her primary concern." Healey points out that publicly funded institutions that fail to serve the interests of the political system tend to be short lived. He also questions the assumption that better data and analyses lead to better policy. As evidence for this view he points out that estimates of oil and gas reserves are often influenced by energy policy, and—without a trace of irony—that fishery policy influences stock assessments. Regrettably, this is true at least some of the time, but most scientists regard such things as mistakes that want correction. An institution that accepts them as part of its culture cannot be regarded as scientific.

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November 12, 2008

Appendix. Some first-author sea lice publications of Simon Jones

1. Jones S.R.M., M.D. Fast, S.C. Johnson & D.B. Groman. (2007) Differential rejection of salmon lice by pink and chum salmon: disease consequences and expression of proinflammatory genes. *Diseases of Aquatic Organisms* **75**:229-238.
2. Jones, S.R.M., G. Prosperi-Porta, E. Kim, P. Callow & N.B. Hargreaves. (2006) The occurrence of *Lepeophtheirus salmonis* and *Caligus clemensi* (Copepoda: Caligidae) on three-spine stickleback *Gasterosteus aculeatus* in coastal British Columbia. *Journal of Parasitology* **92**:473–480.
3. Jones S., E. Kim & S. Dawe. (2006) Experimental infections with *Lepeophtheirus salmonis* (Krøyer) on threespine sticklebacks *Gasterosteus aculeatus* L., and juvenile Pacific salmon, *Oncorhynchus spp.* *Journal of Fish Diseases* **29**:489–495.
4. Jones, S.R.M. & N.B. Hargreaves. (2007) The abundance and distribution of *Lepeophtheirus salmonis* (Copepoda: caligidae) on pink (*Oncorhynchus gorbuscha*) and chum (*O. keta*) salmon in coastal British Columbia. *Journal of Parasitology* **93**:1324–1331.

⁹ Healey, M.C. 1997. The interplay of policy, politics and science, *Canadian Journal of Fisheries and Aquatic Sciences*, **54**: 1427–1429.