

Misplaced Concreteness: Prospects and Problems with Interdisciplinary Approaches to the Fisheries

Joseph E. Taylor III, Department of History, Iowa State University

Abstract: In an age of increasing complexity, simple answers no longer suffice, but the demand for greater sophistication is itself at odds with an era of growing specialization. Researchers must thus incorporate information from specialists in other fields. This trend in interdisciplinary research contains hidden challenges, however, including a necessary familiarity with the development, ideas, major questions, and dominant institutions of those disciplines.

Keywords: history, interdisciplinarity, fisheries, El Niño, science

Fishery history is still largely mare incognita. Few academic scholars venture into these waters, and most who do quickly depart. Harold Innis, Dean Allard, and Art McEvoy each wrote one very important study of a North American fishery and then nothing more.¹ Their peers judged these works excellent, yet the authors still fled. Having myself decided to stick with the fisheries, I wonder about this exodus and why American humanities cannot sustain interest in the fisheries. I doubt it is intrinsic to North American culture since Canadians have created an enviable community of fishery scholars within the humanities.² Some apathy may result from an intersection of professional demands and cultural resistance. In academic environments shaped by publish or perish and lorded by colleagues who forget the very existence, let alone significance, of extra-terrestrial beings such as fishers and sailors, there is no incentive to stick with a marginalized topic. I suspect, though, that the complexity of fishery issues is another deterrent.

One thing distinguishing every outstanding fishery history is an effort to embrace the tangle of issues surrounding fisheries. Innis's study of cod, Allard's work on the U.S. Fish Commission, and McEvoy's book on California reveal complex relations between nature and culture and science and politics and economy. These are not just outstanding fishery histories; they are some of the best examples we have of interdisciplinary research. Each draws upon a full range of humanities, social sciences, and sciences. Their intellectual dividends have been great--but also daunting and exhausting. When I began my work on the salmon fisheries, I was actually intimidated by what McEvoy had done, and I had great doubts whether I could do something

similar. I pressed on only because I cared greatly about the subject, but I am not sure how many colleagues would do similarly.

I soon discovered what my predecessors already knew: fisheries blur boundaries. The material blends with the cultural; the social blends with the economic and political. At such moments researchers face vexing analytical problems, and their greatest challenge might be to resist common sense. Take overfishing, for example. When fish vanish, we zero in on human predation. This is understandable. Fishers are the most conspicuous threat to fish, and they irresistibly draw our attention. But in fixating on fishing, we can end up committing what philosopher Alfred North Whitehead called "The Fallacy of Misplaced Concreteness." By focusing on a definite set of factors, he said, we can "arrive at a variety of important truths relating to our experience of nature." In the fisheries, we see huge fleets, sprawling nets, and piles of fish, and we decide this is too much. *Overfishing* thus helps define a problem with vanishing fish, but it can also overdetermine our answer by obscuring our attention to other issues. In this case, that which seems straight forward and tempts easy conclusion ends up containing both more and less than meets the eye. The research challenge is to recognize this trap and resist snap answers.³

At such moments, researchers will discover both the trials and rewards of interdisciplinary research. Rejecting simple answers leads toward adopting a much broader perspective, often by relying on experts from other disciplines. This is the essence of interdisciplinary research, but it contains hidden bargains. To seek answers from other

disciplines yet maintain intellectual independence, researchers must be able weigh evidence critically. That means--here is the hidden cost--they have to master the history and language of those disciplines well enough to understand them *on their terms*. Hard experience has taught me that this is a recipe for confusion, frustration, and ibuprofen. It has also been responsible for some of the biggest breakthroughs in fishery research. Thus what I want to do is begin with an anecdotal account of my personal experiences researching questions about El Niño, and then to abstract from them to several general but important lessons about interdisciplinary research.

To understand why I became involved with El Niño research in the first place--and why it became so important to my work, it is first necessary to understand something about the way people have remembered the Pacific salmon fisheries. The modern salmon crisis has long been scripted in two diametric patterns, as either comedy or tragedy. Government officials have usually invoked the former, trumpeting the promises of opportunity and technology, while historians have tended towards the latter, fixating on failure and oppression. In both cases, the chosen narrative shaped both questions and answers about the past, and this includes social scientists and scientists as well, since everyone presents their research in some form of narrative. Another way of stating my big point here is that telling tales is never a purely inductive exercise.⁴ All sorts of biases shape our stories--even before they are written. In the case of salmon, this has led to many rather similar histories about society harming nature or the promise of science and technology to ameliorate the vices of society. I raise this point because it very clearly shaped the way I initially approached the history of Pacific salmon fisheries. My head already contained a narrative of events as I started to read the primary sources. Like most scholars who tell stories about salmon, I already knew what had happened.

And then I had one of those epiphanies that made me want to smack my forehead for not realizing it sooner. In 1991 I had published an article on the Nestucca River fisheries. A year later I returned to an episode in the article involving a sudden downturn, a moment that had seemed like a clear case of overfishing for the last seventy years. When considered in isolation, the Nestucca in 1926 did look like a case of greedy fishers gone too far. Spawning habitat was still relatively intact, but the number of nets in the river had grown inexorably. When runs declined precipitously, anglers blamed

local netters and persuaded Oregon legislators to prohibit gillnets. I had focused on the class and spatial aspects of this episode, conceding claims of overfishing, but when I looked again the local story no longer made sense. The 1926 harvest had plummeted all along the coast from Vancouver Island to California. Attributing that decline to synchronous overfishing did not seem likely. Something much bigger must have been at work, something like an ocean.⁵

In a wild, completely speculative fashion, I wondered if there had been an El Niño in 1926, but I had zero clue how to substantiate my hunch. I did a literature review and found a surprising number of articles on the mechanics of El Niño and something called the Southern Oscillation; other articles discussed the biological consequences of El Niño. Then I discovered a series of articles by the late William Quinn, of Oregon State University. Quinn, Victor Neal, and Santiago Atunéz de Mayolo had listed and ranked the severity of every El Niño during the last 450 years. My first reaction was gratitude; my second was to peruse the lists. Sure enough, there had been an El Niño in 1925-1926. Even better, Quinn ranked it as "very strong," on the same order of intensity as the global event in 1983-1984. I thought, "how convenient." Harvests in 1926 had all the same markings of the 1983-1984 event. I broadened my review of harvest and El Niño records and found correlations between bad years and El Niños in 1877, 1899, and 1957. I started dreaming of major historical revisions.⁶

I now call this my infatuation period with interdisciplinary research. My initial phase was pure enthusiasm. This interdisciplinary stuff was great--and easy too. My case was made. The records proved it. I was deeply into a case of misplaced concreteness. Yet nothing was quite what it seemed, and flaws soon emerged as my familiarity with the records grew. Quinn had ranked all El Niños in four categories: weak, moderate, strong, or very strong. When I reflected on these phrases, I began to wonder what "very strong" really meant. What really was the difference between "very strong" and just plain "strong," and between those and "moderate" El Niños? I sensed a lot of imprecision in this terminology, and I needed clarification. I wrote to Quinn but got no answer. When I tried again I learned that he was very ill and then dead. I briefly toyed with dropping the line of inquiry, but that lasted maybe an hour because I figured that El Niños had happened in the past and that at least some of them may have had a major, if episodic, impact on regional salmon runs.

I had no idea where to turn for help, however, so at first I just plowed ahead alone. I went back to the Southern Oscillation (SO) and El Niño literature. I tried to absorb the sprawling interpretations of atmospheric scientists and physical and biological oceanographers. I spent two months reading everything I could find, but I discovered that even experts were struggling to understand El Niño. Only a couple articles limned the impact of earlier major El Niños in 1877 and 1925, and no one was asking the sort of historical questions about salmon that interested me. It was then I realized with a shudder that I might have to do this research from scratch. By late-1993 I was seriously adrift and in need of help. It was at this point that I wandered through the University of Washington's School of Fisheries trying to locate help. Jim Anderson pushed me to Tom Quinn; Quinn steered me to Ray Hilborn; for some reason Hilborn sent me to Kai Lee; and Lee said, "You need to talk to Wooster." "Who?", I asked. "Warren Wooster," he replied. "If you've got a question about El Niño, he's your guy. He knows everything about that stuff."

So I trundled down to the Marine Affairs Building to see this guy named Wooster. I was so obsessed that I walked into his office, sat down, and spent five minutes gibbering on about El Niños and the SO and salmon runs and 1926 and how confused I was and nobody was asking the questions I wanted answered. Wooster finally leaned over and asked, "Son, what's your name?" We spent the next five minutes introducing ourselves and explaining what we did. It took a while for Wooster to realize I was in history and not fisheries, but then he began to teach. He suggested some basic texts, showed me a NOAA website that graphically illustrated current sea surface temperatures across the Pacific basin, and briefly explained the history of ideas about El Niño and the SO. The one idea he kept going back to was how complex and contingent El Niños were, and how incomplete our knowledge was. Unbeknownst to me, I was asking some very complicated questions. Wooster warned me that I might not be able to answer all of them, but he steered me to historical databases on sea surface height and sea temperature that might help me gauge some events. He also encouraged me to come back when I had more ideas or questions.

That first meeting with Wooster taught me three critical lessons about interdisciplinary research. The first was that unmediated knowledge can be a dangerous thing. I had read a lot of theories about El Niños with no way to discriminate between authors or publications. Wooster spent a great deal

of time disabusing certain theories, qualifying others, and always reminding me that scientists were still learning how complex the interactions were between the oceans and atmosphere. The second lesson was the importance of personal contacts with other fields. Kai Lee was wrong on one point: Wooster did not know everything, but he often knew who did. Through Wooster I gained access to a huge cohort of experts on ocean and atmospheric climate. Wooster and David Fluharty schooled me on El Niño and the SO. Robert Francis and Steven Hare showed me drafts of a major paper on long-term fluctuations in the North Pacific fisheries. Michael Wallace and Nathan Mantua essentially gave me private tutorials on atmospheric engines. None of this would have happened without having first established a rapport with Warren Wooster.

The third lesson from that first meeting was that I might have to accept a lot of uncertainty, that I might not be able to answer all my questions, but that I could pursue them. I had both the ability and critical help necessary to make sense of other fields. It is this last point that I stress endlessly to my students. Scholars cannot dabble in other disciplines. They must go at it whole hog, or they will surely make mistakes that their peers will spot. Another way of saying this is that complex subjects are sure tickets to hard labor--one of the factors that may dissuade historians from the fisheries. Mastering a second, third, or fourth discipline is never easy, but it is much easier with help from within. Hilborn, Wooster, Francis, and Mantua were indispensable to my education. Without them, I would have made many errors and overstatements; with them, it was fun to find materials, generate ideas, and refine answers.

Yet I need to play Polonius for a moment and add a caveat. Guides are essential, but they can hinder as well as help. In recent years scholars have grown skeptical of objective truth. We are increasingly willing to admit that bias is intrinsic to social knowledge. Private ambition, community norms, and institutional agendas shape both the questions we ask and the interpretations we adopt, and most academic disciplines are riven by politics, especially the sciences. Yet for all our awareness, researchers in the humanities and social sciences too often still accept science at face value. Overcredulity can become another trap when establishing close links with experts, so while familiarity is important, it is best not to get too close. Remember that allies also grind axes, and be as critical of their advice as you would a colleague in your own field. The best prophylactic is to read widely, and be well

versed on the big debates in a discipline. Know the history of its development, ideas, major questions, and dominant institutions. In all endeavors be an educated and discriminating spectator. In other words, "To thine own self be true."⁷

This is not easy or comforting. For ten years I tried to measure my own ideas and my guides' counsels. During that period I mined many archives, and 150 years' of regional newspapers, government reports, and scholarly journals. For my labors I got a lot of conflicting information. I had experts tell me that dams posed no threat to salmon, that there was no difference between wild and hatchery fish, that El Niño and decadal warming trends were statistical anomalies, and that oceans *or* dams *or* fishers *or* Indians *or* loggers *or* grazers *or* irrigators *or* urbanites *or* environmentalists were the real problems. Every expert had an opinion, and no theory went uncontested. Yet as absurd as some claims seemed, sorting through them was far from easy because each came from a sincere researcher with solid credentials, and every explanation held at least a grain of truth. The more I listened, the murkier the past appeared.

So how did I handle conflicting advice? Historian Theodore Porter has a great phrase for the modern trend of quantification: he calls it a "strategy of irresponsibility." Trust in numbers, he says, allows authorities seemingly to shift responsibility for their actions to outside experts, yet in the end politicians still make the decisions. The same is true for interdisciplinary research. I relied on experts to help understand material reality, but in the end I alone had to decide who made sense. In each case, though, I tried to acknowledge such contests because uncertainty itself seemed to be part of the story, as did the tendency of politicians to embrace some strategies despite their uncertainties while suppressing others because of their uncertainties. I made a lot of this angle, but I still had to make some conclusions about what had happened to salmon in the material world. To do that, I did have to choose among competing claims.⁸

This actually turned out to be easier than I feared, but only because I could escape another trap. The morsels of truth I previously mentioned proved crucial because complexity itself was a key. In the case of El Niño, the evidence for a broad but incidental warming event such as a major El Niño was quite persuasive in 1877, the 1890s, and 1926. Other possible events listed in Quinn's publications did not have the sort of supporting local evidence that made these other dates persuasive.⁹ Combined with other evidence, my research suggested that the

salmon crisis was in fact very old with many contributing factors, yet the literature and political rhetoric ran in the other direction, focusing almost exclusively on single issues in the present. Rather than follow my predecessors and choose among competing claims, as if I could possibly isolate blame, I tried to lay out all of the claims and show where each made sense but was also simplistic. Out of this came a very different narrative, one in which the salmon crisis was neither linear nor singular, nor did it contain unambiguous villains or simple solutions. Out of this came another insight about how American political discourse simplifies problems into binary choices. My interdisciplinary approach to a fishing decline in 1926 helped me realize that the very way Americans talk about environmental issues has also been part of the problem.¹⁰

My experiences with interdisciplinary research have forced me to grow comfortable with ambiguity, and because of that I now tend to emulate a scale when I talk, always weighing factors on both sides of an issue. Thus as I talk about interdisciplinarity, I want to give equal time to both the costs and opportunities. My research led me through many perspectives, with many conclusions. As I grow familiar with each discipline, I learn to discriminate between the useful and the loopy. I have also adjusted to the reality of uncertainty in scientific research. There are many costs to this approach. The expenditure of time and energy has been significant, the confusion distressing, the complexity frustrating. These are real problems, but studies like Arthur McEvoy's *The Fisherman's Problem* reveal why it should be encouraged. McEvoy helped demonstrate that the science used to manage fisheries was itself embedded in multiple environments of resource, institutional, and economic politics. The consequences for people and nature cannot be understood apart from these issues, but understanding all of the factors requires a lot of hard work. Interdisciplinary research takes a lot of dedication and sweat, but the intellectual rewards are great.

To be bluntly honest, the social opportunities and costs are equally great. The way I learned to do interdisciplinary research resulted in a very different narrative. My history of the salmon crisis is unavoidably messier than past tellings, but I think it's also more interesting. Others disagree, and although I've received praise from my peers, I have also been reviled from all quarters. Some fishers accuse me of treason; some industrialists suspect I'm a radical environmentalist and an

opponent of progress; some environmentalists think I'm an apologist for capitalistic destruction; and one author on salmon sneers that I'm a "social constructionist." The professional rewards are gratifying, but friction has been an intrinsic element of that bargain. I do not know how many others would wish this bumpy ride, but I hope at least a few will. There are still huge opportunities and many ambitious projects in the fisheries, and vast stores of information are collecting dust in state and federal archives, just waiting for adventurous researchers.

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³ A. N. Whitehead, *Science and the Modern World*, New York: Free Press, 58, 33-59 *passim*, 1967.

⁴ W. Cronon, "A Place for Stories: Nature, History, and Narrative," *Journal of American History* 78(4), 1347-76, 1992.

⁵ J. E. Taylor III, "For the Love of It: A Short History of Commercial Fishing in Pacific City, Oregon," *Pacific Northwest Quarterly* 82(1), 22-32, 1991.

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⁹ Joseph E. Taylor III, "El Niño and Vanishing Salmon: Culture, Nature, History, and the Politics of Blame," *Western Historical Quarterly* 29(4) 437-57, 1998.

¹⁰ Joseph E. Taylor III, *Making Salmon: An Environmental History of the Northwest Fisheries Crisis*, Seattle: University of Washington press, 1999.