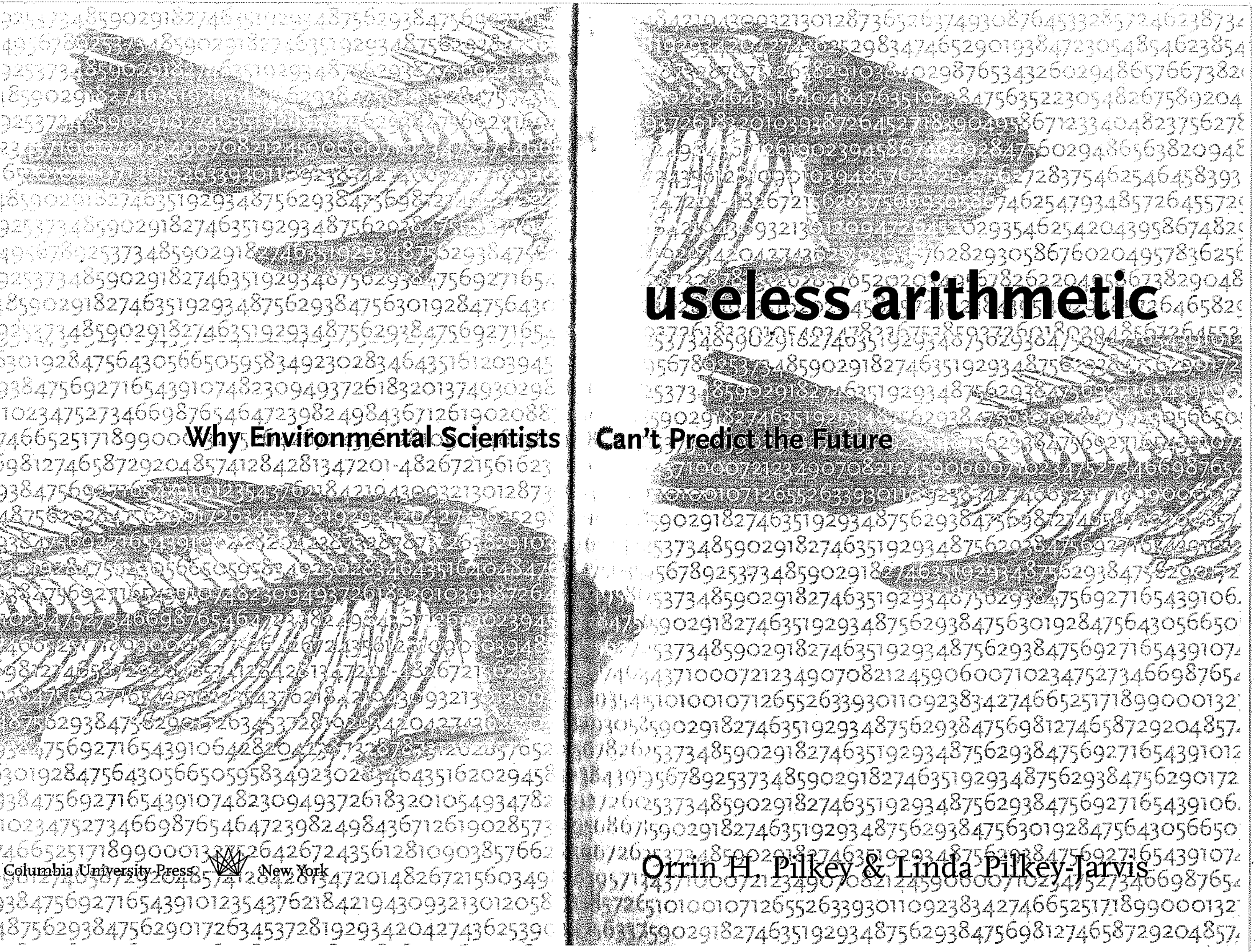

useless arithmetic

Why Environmental Scientists Can't Predict the Future

Orrin H. Pilkey & Linda Pilkey-Jarvis

Columbia University Press New York

Whenever I hear a fishery scientist proclaim that his analysis is rigorous, I am reminded about what John Kenneth Galbraith is reputed to have said once to a group of economists: that the prestige of mathematics has given economics rigor but, alas, also mortis.

—*Jim O'Malley, fishing industry representative and executive director of the East Coast Fisheries Federation*

chapter one

mathematical fishing

The Almighty Cod

More than five hundred years ago, fishers from Portugal and the Basque region of Spain began fishing the fabled Grand Banks of Canada. Although many species of fish were harvested from the seemingly inexhaustible stock, the most famous and valuable was the cod. Thousands of vessels sailed back to Spain and Portugal, from the New World to the Old, their holds jammed with barrels of salted cod. Codfish—*bacalao* in Spain and *bacalhau* in Portugal—became a food staple for the entire Iberian Peninsula. Salted cod achieved added importance because of the numerous meatless days imposed by the Catholic Church. Later, generations of North American children learned of the importance of another cod product, the foul-tasting cod liver oil valued (by parents) as a source of vitamin D.

The Grand Banks are on the Canadian continental shelf off Newfoundland (figure 1.1). Nearly 300 miles across, it is one of the widest continental shelves of the world. The banks cover an area of 110,000 square miles and consist of shallow submarine plateaus, 75 to 300 feet

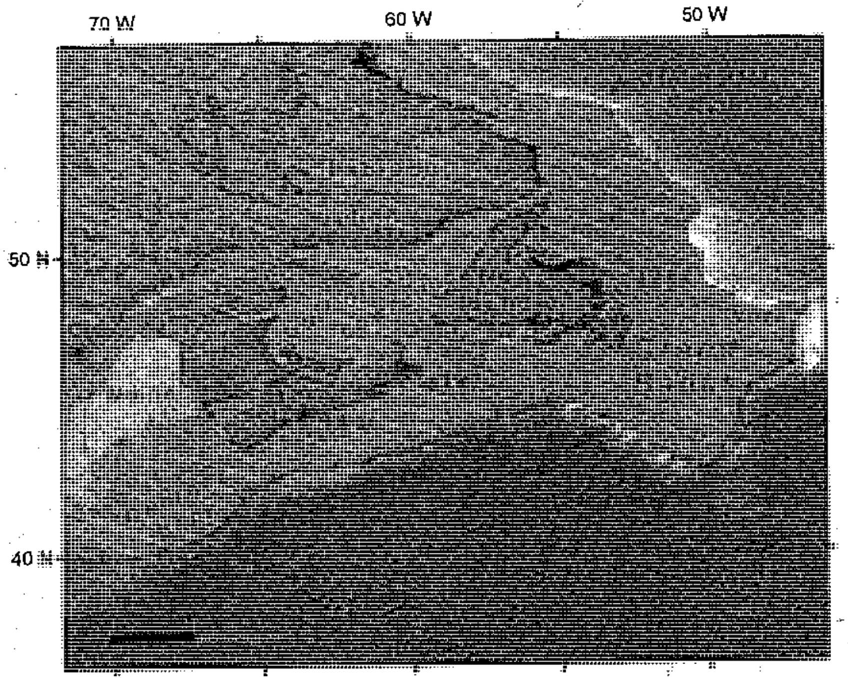

Figure 1.1 This physiographic diagram of a portion of the North American continental margin shows the Grand Banks and Georges Bank, both very important fishing grounds. In 1992 the cod fishery on the Grand Banks crashed as a result of overfishing, and it has not recovered since. Mathematical models must bear some of the blame for this failure of what may have been the world's richest fishery. Cod are still harvested from Georges Bank, but in much smaller numbers that in previous years. Map by David Lewis.

deep, separated by troughs that are 600 or more feet deep. The cold Labrador Current flows down from the north, to mix over the banks with the warm Gulf Stream coming up from the south. The resulting churned-up waters are rich in nutrients and support a huge marine ecosystem. Icebergs are commonly present, slowly drifting south, melting along the way. The winter storms on the banks are legendary, but the water never freezes over.

The Atlantic cod, *Gadus morhua* (figure 1.2), has always been the mainstay of the Grand Banks fishery. Perhaps 90 percent of the fish catch on the banks during the 1980s was cod. It is a tasty fish that can be salted or sun-dried and preserved for a long time, which was of particular importance in the days before refrigeration. Cod is often the fish used for fish-and-chips and for the McDonald's fish sandwich.

Cod have an olive green spotted back and a white belly, with a prominent, slightly curved back-to-front stripe along the side. Various shades of brown and even red may be present, depending upon the habitat. They are commonly two to three feet long and weigh five to ten pounds, although occasionally in the past individual fish "as big as a man," six feet long and two hundred pounds, were caught. They continue to grow during their entire lifetime.

Cod were once found in schools, sometimes miles across, in deep water in the winter and in shallower water in the summer. The Atlantic cod probably has a number of subpopulations, each following the same migration paths year after year. The Northern cod used to extend from off the tip of Labrador down to Cape Hatteras.

The cod eats just about anything, including the occasional unwary seabird resting on the rolling ocean surface. It is a fish that virtually swims with its mouth open, devouring clams, squid, mussels, echinoderms, jellyfish, sea squirts, worms, and other fish, including its own young. Its favorite fish is perhaps the capelin, a small plankton feeder that spawns in the summer on and near beaches. Capelin are probably responsible for the cod's migration to shallow water in the summertime. Many who have written about the demise of the Atlantic cod have noted the irony that a fish as greedy as the cod is being destroyed by humans, another of God's creatures with even greater greed.

Cod spawn between March and June, releasing eggs that float to the surface and become part of the plankton for ten weeks. When the larvae reach one inch in length, they swim back to the bottom. Each female cod releases between 2 million and 11 million eggs—a stupendous figure

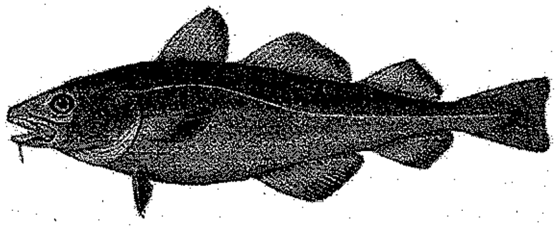

Figure 1.2 The Northern cod (*Gadus morhua*), shown here, was once the mainstay of the world's greatest fishing grounds, the Grand Banks of Canada. Misplaced confidence in mathematical models played a role in the demise of this fishery. Drawing courtesy of the National Oceanic and Atmospheric Administration; modified by Dave Lewis.

that gave rise to the poem (said to be written by an anonymous American) comparing the productivity of codfish and chickens:

The Codfish lays 10,000 eggs
The lowly hen but one;
But the codfish never cackles
To tell what she has done.
And so we scorn the codfish
While the humble hen we prize,
Which only goes to show you
That it pays to advertise.

For hundreds of years, Grand Banks fishers caught cod from small dories manned by one or two men, using herring-baited hooks. The boats were lowered from a mother ship each morning and gathered back in by nightfall. It was dangerous work immortalized by Winslow Homer's famous painting *Lost on the Grand Banks.* The seascape shows two forlorn fishers, separated from the mother ship, peering over the side of their dory in rough weather. Microsoft mogul Bill Gates purchased the painting in 1998 for $30 million. It was, by a factor of three, the highest price ever paid for an American painting.

Gradually, newer and more efficient fishing methods came along (figure 1.3), especially in recent decades. These include nearshore traps, used when cod come in to shallow water during the summer. Seines, or nets pulled into circular traps by small motor vessels, and untended drift nets are also both used on the Grand Banks. In some fisheries (not cod), drift nets can be as long as forty miles.

This method of cod fishing has been a particularly insidious and wasteful killer of Grand Banks fish. When the nets are lost or untended, large numbers of fish are caught by their gills as the net eventually sinks to the seafloor. Scavengers empty the net, which once again floats to the surface, fills with fish, sinks, and again returns to the surface after being emptied. The deadly cycle continues until the net disintegrates, which may take years if it is made of durable nylon.

But the biggest problem for the cod fishery on the Grand Banks was the fishing trawlers. These vessels drag nets shaped like giant bags behind them, scooping up everything in their path. Invention of the *otter trawl,* which uses chain weights to hold the net on the bottom and "doors" attached to the towing cables that keep the net open, was a major step in the evolution of trawls. The otter trawl makes it possible to drag nets over

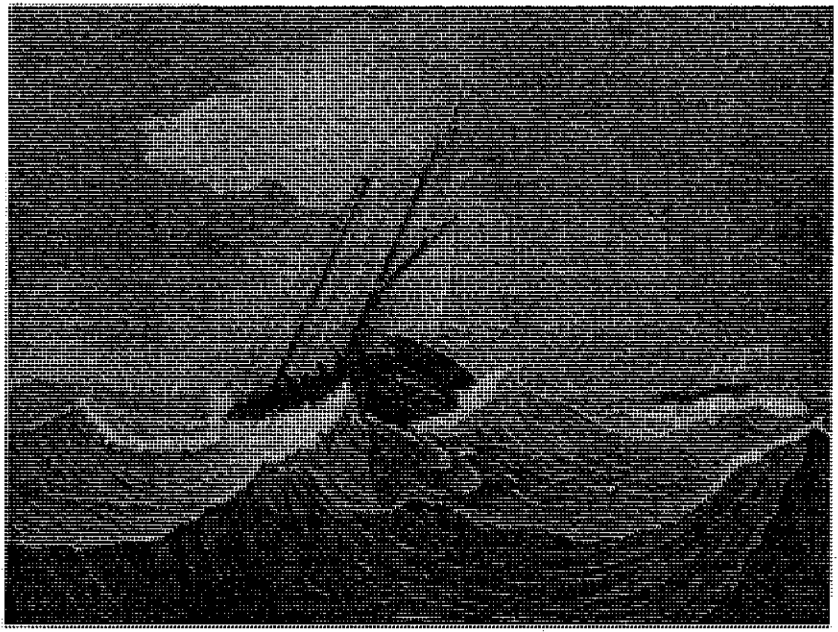

Figure 1.3 Hand-line fishing for cod in rough weather on the Georges Bank, from a painting by Paul E. Collins. It was a rugged life for those who went to sea in these cold, rough waters! Courtesy of the National Oceanic and Atmospheric Administration.

uneven bottoms. Later, the invention of electronic devices that could spot fish schools and guide the towing vessel in their direction added more efficiency to the fishery. And then other devices told the trawler skipper when the bag was full, preventing a premature retrieval or loss of the catch if an overfull bag broke while being hauled on board.

In the mid-1980s, rock hopper dredges came in. These are trawl nets with large, heavy wheels capable of rolling over almost any seafloor obstructions and preventing the net from being torn. Bottom creatures of all kinds, often with no food value, are captured or scraped away. These modern trawlers, if not regulated in some way, can take more fish than the fishery can sustain.

Overfishing or not, they can destroy the very environment needed for recruitment of the next generation of fish. Studies have shown that juvenile cod survive best in areas that have rough bottoms, hiding from predators behind and within the many nooks and crannies afforded by such a seafloor. Almost any seafloor irregularity can provide shelter—rocks, shells, ripple marks, mud patches, sponges, worm tubes, depressions excavated by fish and rays. Since an area equal to all of the world's continental shelves is trawled every two years, the habitats provided by an uneven seafloor disappear into geologic history.

The Grand Finale of the Grand Banks

With the benefit of impeccable hindsight, it is possible to watch with equal parts fascination and horror as an entire industry and ecosystem drives off a cliff. In 1968 the cod catch on the banks was 810,000 tons. The total cod catch from the Grand Banks, the Bay of Fundy, and the Gulf of St. Lawrence reached 1,900,000 tons! Pol Chantraine, in his book *The Last Codfish*, called it "senseless, wild over fishing." In 1992 the Grand Banks fishery collapsed; it was the biggest fishery disaster ever. The cod and flounder were no more; they had joined the haddock fishery that had already crashed in the 1950s and never recovered. Forty thousand jobs and a way of life disappeared as the world's most famous fishing grounds closed up. People, young and old, began to leave the remote fishing villages lining the shores of Newfoundland, hoping for greener pastures on the mainland. More than a decade later, the cod have still not recovered, perhaps because of the effect known as *dispensatio*, a lowering of reproduction rates that occurs when the density of a fish population is no longer sustainable. Among other things, dispensatio reflects the difficulty that widely dispersed fish have in finding one another for mating. So few cod are left that now there is a push to have the Northern cod declared an endangered species!

How could the Grand Banks, with all of its high visibility, go belly-up? Or, as writer Deborah MacKenzie in the 1995 *New Scientist* asks: "How could an advanced nation with an army of scientists allow one of the richest fisheries in the world to . . . be destroyed?" The *Boston Globe* noted that "after five centuries of abundance, the cod are gone from the Grand Banks of the North Atlantic, wiped out by a combination of scientific mismanagement, bureaucratic sloth and above all, almost incomprehensibly mindless greed." Richard Cashin, chairman of a task force looking into the collapse, characterized it as "a famine of biblical scale—a great destruction."

Overfishing, poor fishery science and management, reduction of capelin, the tragedy of the commons, pollution, climate change, seals (recently protected from hunting), and foreign fishers (especially the Spanish) constituted the usual suspects in the Grand Banks debacle. But MacKenzie believes, as do many other more or less neutral observers of the fishery scene, that the mathematical models used by the scientists to depict the health of the cod stock must also absorb much of the blame: "Press the experts harder and an additional culprit emerges—the scientific models used for estimating sustainable catches. According to these models, the Grand Banks should still be full of fish. Most experts admit the models are inaccurate. . . . In the meanwhile, the models which failed the Grand Banks are being used to govern fisheries around the world."

Just before the end of the fishery, politicians on either side of the Atlantic began sniping at each other. The premier of Newfoundland, Clyde Welks, suggested that the European Community's claim of legal fishing on the Grand Banks compared favorably to Saddam Hussein's claim of legal possession of Kuwait. Other Canadian officials characterized European Union fishery officials as pirates in a fish war. In turn, European Union spokespeople accused Canadians of conducting a politicized media campaign to blame European fishers for problems created by Canada's own mismanagement.

Accusations of international interference in local fisheries are not exactly unheard of in other parts of the oceans. In 1998 the *Shen Kno*, an 80-ton Taiwanese long-liner, was caught fishing within three miles of the shore of Somalia. The skipper was fined $3 million and sentenced to amputation of his right hand and left foot. Several months later, the skipper steamed away with all of his limbs intact, but $300,000 poorer.

In 1969 the regional Grand Banks cod catch began the long downhill slide. By 1974 it was as low as 34,000 tons. In 1977 Canada extended its fishery control to 200 miles offshore, thus covering the entire Grand Banks except for two small areas called the "nose" and the "tail" of the banks. Canadian fishery scientists told the government that if appropriate catch limits were put in place, the catch should rise to 500,000 tons by the mid-1980s.

The *total allowable catch* (TAC), determined on the basis of estimates of the size of the cod stock, was set at 16 percent of the fish per year. According to the models, this size catch would allow the stock to gradually increase. In response to this good news, the government began to build up the fishing industry to prepare for the coming cod bonanza. In order to increase the economic efficiency of the fleet, tax breaks and subsidies were used to modernize existing vessels, build larger vessels, and rescue the foundering seafood companies. By the time the cod fishery collapsed, the subsidies were worth far more than the fish catch.

Almost every developed country, including the United States, did exactly the same thing when its 200-mile limit was declared. Each country viewed the declaration of offshore sovereignty as an opportunity to build up its struggling fishing industry. In the case of Canada, it was hoped that the change would revive the economies of Newfoundland and Nova Scotia, both relatively impoverished provinces. It was then that the problems began in earnest on the Grand Banks, and in most of the rest of the world's fisheries. Fisheries that were overfished by the hated foreign vessels now began to be overfished by domestic fishers. The fish population never got a rest and a chance to recover.

On the Grand Banks, the harvest by foreign ships was greatly reduced. The Spanish and Portuguese ships and those of other nationalities could still fish the nose and the tail of the banks (which harbored 5 percent to 10 percent of the total Grand Banks cod population), and also the cod banks on the nearby Flemish cap, all of which were beyond the 200-mile limit. In 1986 Canada refused to allow foreign ships to come into St. Johns, Newfoundland, for repairs and resupply, thereby adding another measure to reduce the Iberian invasion.

Despite enormous management and analysis efforts, something was going wrong. Cod stock estimates fell far short of the increases predicted by the models. The inshore fishers, which used small vessels less than 45 feet long, found that it was increasingly difficult to catch cod. They knew that the large offshore trawlers were taking too many fish, but complaints to the government, which was proud of its now prospering Grand Banks fishery, fell on deaf ears. The scientists seem to have ignored the inshore fishers as well.

The offshore fish being caught were smaller, and the fleet was catching them in a smaller and smaller area of the Grand Banks. Not to worry. It was well established, on the basis of hundreds of years of experience, that cod inexplicably disappeared from some sections of the banks from time to time. It was assumed that one of the well-known temporary shifts in fish migration patterns must be occurring.

As it turned out, the dense congregation of cod in small areas was in itself a sign of a depleted population. When only a small number of fish remain from a population that once roamed a large area, they will naturally gravitate to the best habitat, the one with the most food. When the population was large, competitive pressure for food made the fish scatter far and wide.

The estimates of the number of cod by the Canadian Department of Fisheries and Oceans (DFO), based on a random sampling survey of the banks, were much smaller than the estimates of the fishers, who did not fish randomly, but instead went to areas where the cod were congregating. One seafood company noted in 1990 that the scientific estimate of cod numbers was low because the sampling was not being done where the fish were! As far as the fishers were concerned, fishing had never been better.

During the last years of fishing leading up to the demise of the codfish, the TAC was partly based on a fish population estimate that was determined by splitting the difference between the fishers' population estimates and the estimates by DFO scientists. This calculation provided a number that made no one happy and that was indefensible scientifically. Ironically, the 1989 recommended catch of 125,000 tons was changed to 235,000 tons by fisheries minister John Crosbie, who declared the proposed 125,000-ton allowance to be so low it was "demented."

Crosbie was catastrophically wrong. In retrospect, it became clear that in the last year or two of fishing, 60 percent—not 16 percent—of the total fish stock was being removed. In January 1992 DFO scientists recommended a catch of 185,000 tons. In June 1992 they recommended that the cod fishery be closed down. Dogfish had replaced the cod

In a twist of irony, in July 2002 the same John Crosbie who had facilitated the demise of the cod fishery warned that shellfish, which replaced the cod as the main element of the Newfoundland fishing industry, were being "over fished and treated in an irresponsible manner." On the tenth anniversary of the cod collapse, Crosbie noted that provincial governments seem to have learned nothing from past mistakes. To his great credit, Crosbie seems to have learned a great deal.

Unfortunately, as the cod became more difficult to catch, some Canadian trawlers moved off the continental shelf to deep water, up to a mile in depth, to catch grenadier, hake, and eel. Fishery researchers at Memorial University in Newfoundland reported in 2006 that five of these sought-after species were now endangered and close to extinction. Fishery scientist Jennifer Devine noted that deep, cold-water species take a very long time to mature and produce fewer young than their shallow-water counterparts; hence they are very vulnerable to overfishing.

Mixing Politics and Science

The DFO was the Canadian federal agency responsible for setting the TAC on the Grand Banks. It is accurate to say that in the case of the codfish debacle this agency made one of most important and far-reaching scientific blunders of the age. But, of course, it is an agency in a democratic political system, with all that entails in terms of political involvement in decisions that are said to be scientifically based. Politicians, responding to the fishers who elect them, put pressure on the fishery administrators, who then pressure the scientists to come up with a version of the truth appropriate for the situation at hand. In this sense, science and the mathematical models were used as a cover-up, or a *fig leaf,* for irresponsible actions of the Grand Banks fishery managers.

Fishery ministers in Canada, like their politically appointed brethren everywhere, must be pliable and willing to make compromises. After all, the lives of people and the careers of politicians are at stake in the decisions they make. In addition, it seems to be a universal truth that the fishing industry will always take a short-term view of the problem and can usually be depended upon to oppose cuts in TACs. Clearly, as fisheries scientist Michael Orbach has pointed out, the study of fisheries is a combined study of politics and conservation.

For the cod fishery, as for most of earth's surface systems, whether biological or geological, the complex interaction of huge numbers of parameters made mathematical modeling on a scale of predictive accuracy that would be useful to fishers a virtual impossibility. The interaction of fish with other fish, the roles of predators and prey, the cycle of the food used by larvae and adults, the vagaries of recruitment and mortality rates, the complex food chain, the oceanographic environment in turbulent areas of the ocean where two major ocean currents mix, climactic variations, the habitat loss caused by trawlers, and many other such parameters were poorly known. Even if all of these variables were precisely understood, no one can ever know the order and intensity with which they might occur.

As a substitute for modeling the whole ecosystem, fishery scientists usually focus their models on the particular species of fish with which they are concerned at the time. When a part of the system is modeled, the assumption is made that the rest of the ecosystem will behave as "expected." Nothing unusual is expected to happen in the ecosystem, but of course it inevitably does.

An example of the single-species-focus problem—one that was used on the Grand Banks cod—is the assessment of stock size made on the basis of age profiles of the fish population. Using the age distribution of individual fish in a population of fish, the mathematical model calculates the population size that would likely be responsible for the observed ages. The model tells a fishery manager the number of fish that survived to a catchable size for a given year, and from that number it calculates

the size of the cod stock. Knowing the stock size, the manager can then determine the TAC.

In arriving at the TAC, a number of assumptions have been made. One example is the age profile of a fish population (assuming you have an accurate age profile from field sampling), which is determined both by the number of fish that successfully reach maturity (recruitment) and the number of fish that die (mortality) from natural and fishing causes. But neither natural mortality nor fishing mortality is ever accurately known and, of course, each varies widely from year to year and from place to place. To get around this, a "reasonable" mortality rate is simply assumed.

James Wilson, a University of Maine fishery economist, notes that fishers have a strong distrust of government field sampling of various fish populations. There may be good reasons for this. In September 2002, the National Marine Fishery Service (NMFS) made a startling confession: Using the Research Vessel *Albatross,* NMFS had been studying fish populations for two years on the New England shelf, and all of its population size estimates were wrong. The problem was that the two trawling cables leading from the ship to the trawling net differed in length by six to eight feet. Normally, professional fishers make sure that there is no more than a two- to three-inch difference in cable length, since unequal cable lengths lead to erratic behavior of the trawling nets, including the complete closing of the net's opening in shallow water.

In an earlier test of the accuracy of the NMFS estimates, the government-owned R/V *Albatross* trawled side by side with a private fishing vessel that was using similar equipment. The catch size aboard the government vessel was about one fourth the size of that brought aboard the fisher. As it turns out, trawling is like fly-fishing in a mountain stream. There are those who catch many fish and those who catch only a few. There are lots of little tricks to hauling giant trawls (figure 1.4) that are not apparent to the untrained eye.

The mathematical models used in the assessment of the Grand Banks cod population also assumed that the size of the adult cod population had a direct bearing on the number of young fish that survived to adulthood each year. It was assumed that more adults meant more babies. The huge number of eggs produced by each spawning female cod probably provided a security blanket for belief in this assumption. Despite its intuitive correctness, the assumption was wrong because the factors in the natural environment that affect larvae are very different

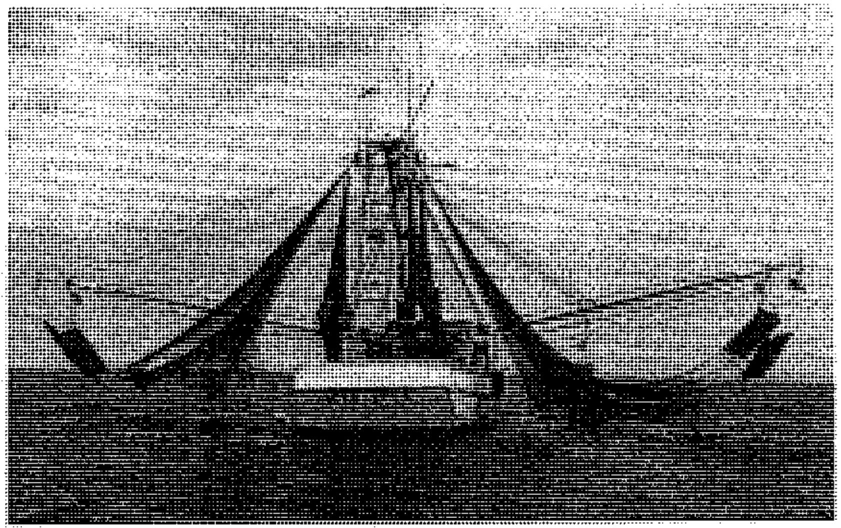

Figure 1.4 A typical shrimp trawler of the type used in both Atlantic and Gulf fisheries. Problems with trawling that affect the health of local fisheries include a large bycatch (especially in the shrimp industry) that is thrown over the side. In addition, trawling tends to smooth the seafloor and reduce habitats for some of the same fish sought by the trawlers. Photo courtesy of the National Marine Fisheries Service.

from the factors that affect later stages of development. For example, much depends on whether food is available to the larvae at the moment of hatching. This is an example of a very damaging *simplifying assumption* put into a mathematical model in order to bridge a gap in understanding of the system being modeled.

Daniel Pauly and Jay Maclean, in their 2003 book *In a Perfect Ocean,* note that population estimates are further complicated by fish that are discarded at sea, fish caught but unreported, and fish caught illegally. Discards are often juveniles or fish that are caught when another species is the target. During the 1980s *high grading* was a common practice. The more valuable large cod (greater than two feet long) were kept, and previously caught smaller cod were shoveled over the side. All these practices profoundly affected the modeling of population sizes needed for fishing management and also judgments concerning the health of the marine ecosystem.

The plot is thicker than just population size. Research by Oregon State marine ecologist Mark Hinson and a number of colleagues revealed that individual fish size is also a critical factor. It is well established that big fish produce more eggs, but what these workers found

in their 2004 research was that the larvae hatched from the eggs of large fish are much hardier than those derived from small fish. They refer to the big fish as *big old fat female fish,* or BOFFFs. The larvae from BOFFFs grow more rapidly and are more capable of withstanding periods of starvation than the larvae of smaller females. In addition, BOFFFs as a group have a longer time span over which they spawn relative to their smaller sisters, which maximizes the possibility of hatching at least a portion of the eggs and of having access to a good supply of food for the larvae. It is statistically more likely that small females will produce eggs at the wrong time, when the larvae find little food, and the survival rates will therefore be reduced.

Did anybody object to the Grand Banks models or the model results? In hindsight it appears that there was a storm of protest. The inshore fishermen took matters into their own hands, since their pleas to the government to stop the overfishing by the offshore trawlers went unheeded. They hired Derek Keats, a university professor from Memorial University, to evaluate DFO's published analysis of their cod numbers. The DFO fish stock numbers were off by as much as 100 percent, Keats said. In the mid-1980s one fishery expert characterized DFO's assessments of fish numbers as *non gratum anus rodentum,* or not worth a rat's ass.

The Canadian Committee on the Status of Endangered Wildlife in Canada (COSEWIC) also clashed with DFO over the fish stock numbers. Kim Bell, a fishery scientist working for COSEWIC, characterized the Atlantic cod population as a whole as endangered. Certainly an endangered species doesn't hold much potential as the basis of a fishing industry. DFO objected to this characterization.

Bill Doubleday, director of science for DFO since the mid-1980s, defended the fish number estimates of the agency by saying, "Unless you were sure you were right . . . you don't come to that conclusion [that the fish numbers were correct]. You said it was inconclusive."

Doubleday's statement says it all. It is the nature of good scientists to be skeptical, to be reserved, and to rarely assume that their numbers are absolute truths. For bureaucrats like Doubleday and politicos like fishery ministers, both under enormous pressure from every direction to keep an industry going, the constantly questioning nature of science provided the natural opening needed to ignore the warning signs of nature. Since the scientists aren't certain, why not go ahead and inflate the allowable fish catch? But Doubleday, a professional administrator of scientists, should have known better. He should have been shouting "the cod are dying" from the rooftops.

Charles Clover, in his book *The End of the Line* (2004), has few good things to say about the fishery scientists who oversaw the collapse of the Grand Banks fishery. The data on the size of the fish stocks were in the hands of a few secretive individuals, which made outside analysis of the quality of the data impossible. And when these same scientists produced the allowable catch numbers, they fell victim to the desire to bring good news to their politician bosses and to the fishing community. Good news was easy; bad news brought the roof down on them. It is a problem that plagues modeling in many specialties.

The Past and Future of Fish Modeling

Basing allowable catches on *maximum sustainable yield* (MSY) was the goal of DFO's hugely unsuccessful management of the cod on the Grand Banks of Newfoundland. Through the use of mathematical models, a level of fishing that could sustain itself indefinitely was sought. The concept of MSY is based on the assumptions that any species in the sea will each year produce a harvestable surplus and that if you take back to the dock that much and no more, you can keep harvesting it forever.

The concept of MSY was introduced in the late nineteenth century but reached its heyday in the 1930s. It was the governing concept of fishery science during the 1940s and 1950s and still finds widespread use in applied fishery science, although it sometimes appears under a pseudonym. The scientists who supported MSY believed that fish were the great integrators of the environment, the peak of the food chain, and they ignored the rest of the ecosystem.

MSY was a commonsense concept that brushed aside the old *doctrine of traditional limnology*, which took a more holistic view of marine ecology. These traditional scientists assumed that the fish were part of a living community within a larger ecosystem. Fish species interacted with one another and with other organisms, both plants and animals, in a very intricate and balanced process of feeding, growing, reproducing, and dying. The water circulation, storms, bottom sediment, and bottom shape all played some role. It was all very complex, too complex to model and come up with useful answers. Then along came MSY, and all this immensely complex system could be mathematically bypassed.

In the end, it *was* too good to be true. Doubt began to creep into the minds of even the most loyal MSY supporters starting in the 1960s.

The practical application of the models was proving impossible, and fish stocks declined. As far as scientists are concerned, the end of MSY came in 1977, when P. A. Larkin, a Canadian fisheries biologist, wrote his famous marine fisheries paper titled "An Epitaph for the Concept of Maximum Sustainable Yield." He ended the article with a genuine epitaph:

MSY
1930's–1970's

Here lies the concept MSY
It advocated yields too high
And didn't spell out how to slice the pie
We bury it with the best of wishes
Especially on behalf of fishes
We don't know yet what will take its place
But hope it's as good for the human race.

Larkin pointed out four major shortcomings of MSY:

- **Fishing to the MSY creates the possibility of population collapse.** If one fishes right to the limits of the MSY for a species, most of the fish that are caught will be young and first-time spawners, because this class of fish is the healthiest and has the lowest mortality rate. The problem is that first-time spawners don't produce the best eggs; they're not as good as BOFFFs. In addition, if only one age group is spawning, a failure in egg hatching or larval survival could lead to a calamitous population failure. Putting it another way, a population of fish that is being harvested at maximum sustainable yield is much more unstable than an unfished population. Once a population collapses, it may stay low for a long time, such as the cod or haddock on the Grand Banks.
- **Fishing to the MSY may reduce genetic variability in a fish population.** Most fishes can be divided into subpopulations, all of which may have different ideal MSYs. Salmon subpopulations spawn in different rivers. Cod subpopulations, of which at least a dozen are recognized, migrate to different shallow-water areas during the summer. Some of the subpopulations recover from fishing pressure more slowly than others. Consequently, if a population is fished at or near the MSY, subpopulations that reproduce the slowest will be hit hardest. The danger is that the remaining fish available for catch will all be from one subpopulation. Under ideal circumstances, in order to preserve essential genetic

variability within a healthy fish population, the MSY should be based on the harvestability of the most vulnerable subpopulation.

- **The MSY does not accommodate the interactions among the species of organisms that constitute an entire aquatic community.** Fish species have very complex interrelationships, the classic example being the cod-mackerel-herring saga. Herring eat cod eggs and mackerel eat herring. If the mackerel are depleted by fishing, herring become more abundant and eat more cod eggs, to the detriment of the cod population. Another example is the growing Atlantic squid fishery that has resulted in an important and possibly damaging reduction of food supply for porpoises. The doctrine of traditional limnology was right; one species should not be managed out of context with other fish species.
- **The MSY concept is completely irrelevant for recreational fishers** (figure 1.5).

Larkin's paper and poetry came too late. The simple and appealing concept of MSY was already deeply entrenched in the world of fishing politics. The fishery scientists who believed in MSY had been too successful in selling the idea.

The U.S. fisheries are governed by a related concept called *optimum yield* or *optimum social yield*, as defined by the Magnuson-Stevens Fishery Conservation and Management Act of 1976: "Conservation and management measures shall prevent over fishing while delivering optimum yield from each fishery on a continuing basis. Optimum yield is the maximum sustainable yield modified by any relevant economic, social or ecological factors."

The term "over fishing" is not defined in the Magnuson-Stevens Act, and the definition of optimum yield is so vague that it could justify any level of fishing, even one exceeding the MSY. At least the act recognizes that the industry is also made up of people and not just fish. Modeling the aquatic ecosystem is hugely complex, and throwing in economic and social factors as well only increases complexity. However, recent modifications to the Magnuson-Stevens Act require not only that fishing pressure be reduced on an overfished resource but also that at the same time the stock must increase.

Today, no matter how it is phrased, governments keep asking scientists for advice on catch limits, or the MSY. Basically, our politicians have decided that catch control is the best and easiest way to control fisheries. If that is what government wants and pays its researchers to study, then that is what we will get, despite the fact that it is increasingly obvious

Figure 1.5 Recreational fishing in the surf zone off Cape Hatteras, North Carolina. The impact of recreational fishing on fish populations is often difficult, if not impossible, to determine. Photo courtesy of the National Atmospheric and Oceanic Administration.

that the huge complexities in "fish population science" extend far beyond simply establishing catch limits.

Some fishery scientists argue that it would be easier in a practical sense to use an effort-control approach. Fishing effort would be regulated by controlling the size of the boats, the size of the fishing area, and the dates within which fishing can occur. An example of this approach is the oyster fishery in Chesapeake Bay, which requires using hand-operated dredges from small skipjack sailing vessels. But there doesn't appear to be much difference between catch control and effort control. Both must adhere to some sort of sustained yield principle if the fishery is to survive. As it turns out, the oyster fishery in Chesapeake Bay is in catastrophic decline, as illustrated by a reduction in harvest from millions of bushels annually in the 1880s to 3,800 bushels in 1999.

Still another management method is the establishment of no-fishing zones, which allow stock recovery, especially among the BOFFFs, which is necessary to reestablish a healthy fish population.

Fisheries in the United States are managed by eight regional fishery management councils, made up mostly of politically appointed nonscientists who represent various interests (e.g., commercial fishing, recreational fishing, tourism, and state government). As is so often the case with citizen councils, individuals on the council view themselves as lobbyists for their particular constituency. In 2003 and 2004, respectively, the Pew Charitable Trust and the U.S. Commission on Ocean Policy agreed that the regional management councils will not be able to solve the overfishing problem. The problem, as stated in a Pew Ocean Science Series report, is that "most council members [around 80 percent] are affiliated with or reflect commercial and recreational fishing interests. Virtually none comes from the conservation world or the public at large."

When evidence that a fishery is in danger is presented, acquired from the results of a recognized model analysis, regional management councils are more likely to respond than if raw numbers are placed before them. NMFS scientists argue that since models are alleged to provide a long view, this is one way to jar the councils off a very short-term, economically driven view of a fish stock. Perhaps it also has something to do with the mystique of models and the mystery and apparent sophistication of mathematics.

An example of a recent model "victory" is the use of the *virtual population analysis* (VPA) model to convince the South Atlantic Regional Fishery Management Council of the need to regulate and eventually close the red porgy fishery. The red porgy is both a commercial and a recreational fish whose numbers and size are in decline. Recruitment is down 95 percent.

VPA requires knowledge of the number of fish caught from a single-year class each year for several years, as well as the mortality rate from fishing and natural causes. All of these numbers are difficult to come by. Mortality rates of a population under extreme stress, as in the case of the red porgy, are especially vague. Whether right or wrong, the mathematical model was used to demonstrate what was already quite obvious from catch and fish size numbers. But in this case, a precise or quantitative prediction should have been neither needed nor expected—the qualitative indication that the species was in deep trouble should have sufficed. In spite of the fact that the red porgy is believed to be the most overfished species in the South Atlantic and in spite of the VPA analysis, it still took five years of debate on the council to limit fishing of the species. And even then, the North Carolina representative objected to the decision.

Certainly it would be difficult to claim that the use of mathematical models in fishery science has resulted in a stable and healthy world fishery. In fact, the world's fisheries are deteriorating, and mathematical modeling efforts are reaching a peak. Two-thirds of the marine stocks in the Atlantic and Pacific oceans (including cod, shark, lobster, and shrimp) are either gone, overfished, in strong decline, or being exploited to the maximum extent possible. But models aren't the only villains. How much of this deteriorating situation is attributable to models and how much to politics is difficult to determine. In developing countries, chaos often substitutes for management, and even perfectly functioning mathematical models would make little difference.

There are some positive signs indicating that at least in some fishery research scientist circles, mostly in the academy, *"what if" modeling* has arrived to act as a guide to fishery management. Such qualitative models are used to seek general guidance for management and not specific defined numbers such as TAC or MSY. "What if" modeling provides one way of evaluating alternative approaches to solving a fishery problem. What if the fishery is closed? What if fishing is permitted only on a certain area of the continental shelf or during a certain season? What if mesh size of nets is increased? Decreased? In this kind of model it's not necessary that all the assumptions behind the model be completely understood or that all the parameters that affect the ecosystem be included, so long as the most important ones are taken into account. General questions are asked and general answers are received. A high degree of accuracy is not expected.

Another approach that can bypass or minimize the use of mathematical models altogether, one that seems to be particularly favored by those concerned with U.S. Pacific fisheries, is *adaptive management*—essentially "Give it a try and adapt as time goes by." This is particularly suited for the management of marine reserves or no-fishing zones. For example, once a marine reserve is designated, study it and see if BOFFFs increase. Observe what this does for the population that is being fished outside of the reserve. Adaptive management approaches could involve moving, enlarging, or shrinking the marine reserve, or even making it a seasonal reserve or a reserve for certain species only. The marine reserve approach may be the only way to encourage increases in the numbers of BOFFFs.

Most fishery scientists seem to recognize that Larkin was right in his criticisms of MSY, and there is widespread agreement that accurate mathematical modeling of the complex marine ecosystem for fishery purposes is probably impossible. Yet mathematical modeling to come up with an allowable catch number continues to be the mainstay of fishery management. We believe the quantitative mathematical models actually used in fishery management fall into two categories.

Category 1: Modeling Blindfolded. Fishery modeling is done by nonbiologists or those biologists who are deeply ensconced in the political system, where their hand is forced in the direction of finding the politically acceptable and most optimistic answer. It is a fact of life that the basic researchers who formulate the models are usually not the ones who actually apply them in the chaotic tangle of special interests in a democratic society. The models are often applied and released to the public without explanation or discussion of the uncertainties.

Such blind or rote application of models, whatever the reason, is the problem addressed by Raymond Beverton and Sidney Holt, authors of a once widely used mathematical model that bears their names, using fish age profiles to calculate the population of a fish species. Both expressed concern about the simplistic way that their model and other models are actually applied by fish managers. According to Beverton, "There is a strong inverse relationship between the growth of fisheries science and the effectiveness with which it is applied." In Deborah MacKenzie's *New Scientist* article, Holt is quoted as noting: "It has been extremely difficult to dissuade fisheries biologists from applying simple formulas like recipes and getting half-baked answers."

Category 2: Models as Fig Leaves, Shields, and Clubs. Peter Aldrich, an NMFS modeling expert and model realist, argues nonetheless that models have proved to be very useful because of their value in winning converts for reducing catch levels to save a fish species. Models have a reputation as the state-of-the-art, sophisticated approach to solving the problem of the dying American fishing industry. They give all interests something to hang their hats on, something to use by way of explanation to disappointed constituents, something to hide behind, something to use as a club. Certainly, the argument goes, the use of a mathematical model to reduce fishing pressure on a species, even if the model is wrong, is better than the alternative of having to sort through some tabulated raw field data accompanied by the opinion of an "expert," only to be refuted by the opinion of another "expert."

Models can also serve as strong insulators, protecting agency scientists and fishery managers from direct attack by politicians who are anxious to please the unhappy fishers among their constituents.

As we move into the twenty-first century, we are not even close to accurate quantitative modeling of any significant portion of the marine ecosystem. Experience in regulating fishing and the catastrophe of the Grand Banks are probably getting us closer to good fish management, but it's questionable whether the knowledge gained from quantitative mathematical models is helping in that regard. Single-species models can't work and protect the entire ecosystem, but single-species models are really all we use. Just like the studies at Yucca Mountain (chapter 3), it seems as though the more we know about fisheries, the less we know. Each step in the direction of understanding ecosystems reveals more and more complexities, and in any complex system in nature we can never obtain quantitative modeling answers at the level of accuracy that society needs.

Society seeks an answer through fish mathematical models, but it can never get that answer from them. Turning away from the fishery models, however, may be akin to reversing a high-speed freight train that's rolling downhill. It won't be easy.

Prediction is very difficult, especially if it's about the future.

—*Neils Bohr, Nobel Prize–winning physicist*

chapter two

mathematical models

escaping from reality

War by the Numbers

During World War II, military mathematical modeling, or *operational research*, became a critical tool for analyzing the war experience. One of the more successful applications of mathematical models resulted in a large increase in the sinking of U-boats by the British navy, after studies suggested new tactics and new settings for depth charges. Operational research was also responsible for suggesting that large convoys of merchant ships were safer than small convoys, the opposite of contemporary military thinking on the subject.

However, the low point of the military mathematical model may have come and gone during the Vietnam War, when modeling the battlefield proved difficult and disastrous. Robert McNamara, one of the ten Ford Motor Company whiz kids, instituted a numbers-only mentality in the management of everything from industry to the World Bank. It may have worked quite well at Ford where the whiz kids, hired by Henry Ford II, turned around a foundering company. But this mentality applied to war was another matter.

McNamara, today best known for the fiasco he helped to create in Vietnam, emphasized numbers, costs, and efficiency, while downplaying the role of human intuition. Once when a White House aide said that the war was doomed to failure, McNamara reportedly responded: "Where is your data? Give me something I can put in a computer. Don't give me your poetry." Twenty years after the war was over, Mr. McNamara admitted that his approach to managing the war was "terribly wrong."

Allain Enthoven, now a chaired professor at Stanford University, was the chief whiz kid and systems analyst for McNamara. The McNamara and Enthoven approach to managing war was cold as a fish, quantitative, impersonal, objective, and lacking in intuition and common sense. Events proved that these *rational modelers* had a fatal flaw: they were unable to admit failure.

One infamous part of quantitative warfare in Vietnam was the notoriously inaccurate enemy body count, considered a measure of success in the war. The body count for remote air and artillery strikes was mathematically modeled to determine how many people would be killed by a certain tonnage and type of explosives and the number of napalm canisters, taking into account the terrain, the vegetation, and the density of people on the ground, among other factors. In closer combat involving infantry units, individual combatants tracked the body count. It was a mathematical model vulnerable to manipulation because evaluations, promotions, commendations, and decorations for officers and noncoms were at stake, depending on the results.

As we have already seen, models may be far from objective when human choices and politics play a part in the process. Arriving at high body counts in Vietnam perhaps was easier than going against the grain with more accurate counts, just as going against the grain of an assumed robust cod population on the Grand Banks by reporting more realistic figures proved difficult for fisheries managers. Eventually all dead bodies became enemy dead bodies—"If it's dead and Vietnamese, it's VC" was the gruesome saying of the times.

The body-count modeling problems are obvious, especially in hindsight. They provide important lessons for all quantitative mathematical modeling.

- *Political objectives polluted the models.* The perception that the war was being won was important in order to sustain support back home.

- *The wrong question was asked.* The body count was not a good measure of success of the American army against a highly motivated, disciplined peasant army.
- *No one looked back.* The veracity of the modeling effort should have been confirmed by field checks.

Models, Models, and Models

A *mathematical model* is a description of a process or a prediction about the end result of a process, expressed as an equation or equations. A model is a numerical analogue—a set of equations that describes the relationships between parameters that control a process. In this book we talk mostly about mathematical models that are said to describe or predict with useful accuracy something about large-scale processes on the surface of the earth. This includes both physical and biological processes. All of the model dominions in this book are *applied models,* or societal relevant models used for engineering, policy, financial, or management purposes.

Quantitative mathematical models are predictive models that answer the questions *where, when,* and *how much.* Where will the invasive plant spread next? When will an artificial beach disappear? How much will the sea level rise in the next century?

By contrast, *qualitative mathematical models* are used to predict directions and magnitudes. For example, is a plant likely to be invasive? Will sea level rise or fall? Will the available fish for harvest be large or small? Will the global climate warm or cool? These models also seek the answer to the questions *why, how,* and *what if.* Why is plant species X invasive, while plant species Y is not? How will the nourished beach disappear, and what mechanisms are likely to be responsible for beach loss? What if trawling for a fish species is halted and only long-lining for the species is allowed? What if rainfall increases at Yucca Mountain's proposed nuclear waste storage site?

The distinction between quantitative and qualitative models is a critical one. The principal message in this volume is that quantitative models predicting the outcome of natural processes on the surface of the earth don't work. On the other hand, qualitative models, when applied correctly, can be valuable tools for understanding those processes.

There are a number of other categories of models as well, sometimes rather vaguely defined. *Statistical models* are those based on statistical studies of past events for the purpose of estimating the probabilistic future behavior of the system. This type of modeling is often used in the social and health sciences. The insurance industry, for example, determines premiums based on statistical models of health data. *Simulations* mimic an event to determine what might transpire. For example, hurricanes, floods, and landslides are often simulated, as are nuclear weapon explosions, battles, and damage to spacecraft in orbit.

Quantitative models may be categorized as either analytical or numerical. *Analytical models* involve simple equations that can be solved rather readily, perhaps using only paper and pencil. *Numerical models* are much more complex, may involve differential equations, and are often solved with complex computer codes.

Determining *model sensitivity* is a method used to resolve the relative importance of the various factors that make a process work. Various components of the equations are changed to see if the outcome of the model changes. Is wave height more important than wave angle or grain size of the sand on a beach in determining sand transport in the surf zone? An important parameter will make a big difference in the final answer and an unimportant factor won't make much difference. *It is important, however, to recognize that the sensitivity of the parameter in the equation is what is being determined, not the sensitivity of the parameter in nature.* If the model is well founded, determining the sensitivity of various parameters is a valid exercise. If the model is wrong or if it is a poor representation of reality, determining the sensitivity of an individual parameter in the model is a meaningless pursuit.

Another distinction between qualitative and quantitative models is the kind of answer that a model provides. If the answer is a single number, the model is quantitative. For example, a quantitative model might predict that the global atmospheric temperature will rise by 3 degrees Centigrade, plus or minus 1 degree, over the next century, whereas a qualitative model might predict that the temperature will continue to increase over the next century, with a possibility that the rate of temperature rise will accelerate. In another example, quantitative modeling is the prediction that because of sea-level rise, the shoreline will retreat 170 feet, plus or minus 30 feet, over the next century. The qualitative equivalent might be a prediction that the shoreline will continue to retreat and probably the rate of retreat will accelerate over the next century. Whether the path to an answer is analytical or numerical, a quantitative answer comes from a quantitative model. The same goes for qualitative models.

In a qualitative model, because one is determining only the direction of a process or the basic mechanics behind a process, only the most important variables need to be considered. Because of the omission of minor processes, the results of all qualitative models may be imprecise or wrong to some degree, but that does not matter so long as the qualitative question at hand can be reasonably answered. Quantitative models require a great deal more accuracy, and to make an accurate prediction a process must be completely understood. All variables of any importance, including feedbacks, must be accounted for if the model is to answer the question at hand.

The actual model may be expressed in one or several relatively simple equations (see appendix), but the calculations using these equations that apply to a large area of the earth's surface through time may be very complex. The method of calculation required for the application of a model is known as the *computer code.*

A single computer simulation of a natural process over time and space may involve hundreds of lines of equations. Imagine the fifteen-year effort involving a small army of specialists that Microsoft went through to develop the word-processing program used in typing the drafts of this page. Millions of dollars were spent in debugging Microsoft Word, yet as anyone who uses a word processor knows, bugs still exist, albeit mostly very minor ones. Programs behind the models that we discuss in this volume have for the most part not been through a detailed quality assurance program. So the question always exists: does the software or computer code actually model what the authors say it models? Programmers know that inevitably there will be many bugs; the hope is that they will all be minor ones.

In chapter 3 we deal with a complex super model, actually based on hundreds of models, to predict the fate of nuclear waste stored at Yucca Mountain, Nevada. These computer codes must describe hundreds of physical, biological, and chemical events that occur over long periods of time over a wide area of the earth's surface. The potential for computer code error is vast, and it is very difficult to evaluate.

A good modeling approach is to "*open-source*" the codes for any and all who are interested. In a recent controversy concerning the shape of the global warming curve over time, however, the scientists who came up with the curve refused to allow others to inspect their computer code. As a result, a pall of suspicion has fallen over their results.

Equally crucial is providing a list of all important assumptions behind models—but this can be tricky. For example, one might say that the assumed average wave height in a mathematical model to predict sand transport is six feet. But the story behind that assumption is more complex. To fully understand the average wave height number, one must accept the following sub-assumptions:

- All waves come from the same direction.
- All waves are of the same height.
- Future wave conditions will be the same as those in the past.

Naomi Oreskes, science historian and modeling philosopher of the University of California at San Diego, uses Lord Kelvin to provide an illustration of the hazards created in earlier times by the drive for quantification. William Thomson, otherwise known as Lord Kelvin of Kelvin temperature scale fame (figure 2.1), was one of the leading physicists of the latter half of the nineteenth century. More than 100 years ago, in Lord Kelvin's time, there was much uncertainty about the earth's age. This was before the onset of techniques to determine ages by rates of decay of radioactive elements. Estimates by geologists ranged from 100 million years to hundreds of billions of years, but most geologists, more or less correctly, thought that the age must be on the order of a few billion years. Current thinking is 4.5 billion years. To come to their conclusions, the geologists used a *conceptual model* based on observation, past history, and experience, spiced with a dose of intuition. A conceptual model is a qualitative one in which the description or prediction can be expressed as written or spoken words or by technical drawings or even cartoons. The model provides an explanation for how something works—the rules behind some process.

The conceptual model that provided a qualitative age estimate was based on the *Principle of Uniformitarianism,* which holds that the present is the key to the past. It is assumed that the processes that mold and shape the surface of the earth today must have worked the same way in the past. Judging from the rate at which streams, blowing wind, and glaciers remove and deposit sediment today, and the frequency of volcanic eruptions, the geologists calculated, in extremely rough form, that it must have taken billions of years for the earth to come to its present state.

Lord Kelvin, unconvinced by such a crude approach, obtained an age of 98 million years, on the assumption that the earth had started out as a molten body and had been cooling ever since. This determination could be shown using a simple mathematical model, which could be calculated by hand. Kelvin's method was a quantitative and precise way to get at the

Figure 2.1 Physicist William Thomson, otherwise known as Lord Kelvin, estimated the age of the earth to be 93 million years, on the assumption that the planet began in a molten state. The simple model he used to calculate the age was valid, but the underlying assumption was wrong. Geologists using conceptual models correctly determined that the planet was much older, but Lord Kelvin's age estimate remained credible (until the role of radioactive elements was discovered) for a few years, an early example of the quantitative trumping the qualitative. Photo from answers.com.

earth's age. And since its basis was a principle of physics (cooling rate), the results were widely accepted. Lord Kelvin declared much of geologic thinking about fossils, stratigraphy, and earth history to be invalid. He also cast doubt on Darwin's theory of evolution because, according to his concept, the earth had been at its current temperature for only a short time span, too short for evolution to operate.

Alongside the shaky qualitative conceptual models of the struggling field geologists of his day, Lord Kelvin's number, derived from a valid mathematical model, seemed to be a precise and reproducible thing of beauty. Combined with his forceful personality, Lord Kelvin's declaration plunged geology into a virtual dark age that held back progress in both earth science and evolutionary theory for a few years.

But Lord Kelvin was wrong, and it was the discovery of the continuous production of heat by the decay of radioactive elements in the earth's upper layers that finally countered his idea. The present temperature of the earth was not derived from the cooling down of a molten body. Instead, because heat is generated in the crust by radioactive decay of a number of elements, including uranium, the earth has steadily maintained its current temperature for a very long time. Otherwise, we would be looking toward a very cold earth on the not-too-distant horizon. Interestingly, Lord Kelvin's age of the earth is still supported by a number of creationists in their battles with modern earth science.

Lord Kelvin's model was an early example of a quantitative model trumping a qualitative one, a common problem even today. His model of the rate of cooling was perfectly valid—that is, the principles of physics he applied were correct. The cooling of the earth is not a complex process, and a quantitative model can successfully describe it. His mistakes were the underlying assumption of a molten beginning of the earth and the failure to understand the importance of radioactive decay as a source of heat in the earth's crust. The important lesson here is that no model can overcome a series of bad assumptions.

In hindsight, it is hard to see a way that Lord Kelvin could have guessed the truth. His was a *situational bias*, the phenomenon by which our thinking is so obscured by our present state of knowledge and known conditions and observed trends that we are blinded to the future. It is hard to get out of one's own cocoon.

Still another conceptual model of the age of the earth dominated Western thought for more than 1,000 years. It was the biblical chronology model, which began with the publication of *Chronologia* in A.D. 212 by a priest and former Roman soldier named Julius Africanus. The "chronologists" were trying to determine the date of the Second Coming of Christ, in order to understand when the thousand-year reign revealed in the book of Revelation would begin. The widely held assumption at the time was that the Second Coming would occur 6,000 years after the earth was formed. Thus the age of the earth was needed in order to determine the date. The 6,000-year assumption was based on two sources of information. The first was Elijah's prophecy in the Jewish Talmud that the earth would last 6,000 years. The second was the belief that each day in the seven days of creation described in Genesis was in reality 1,000 years and that Christ would return for the seventh day of rest.

Africanus totaled "known" time spans of biblical lives and events, starting with Adam. The Septuagint version of the Hebrew Bible was the source of the data on the early part of the earth's existence, and it revealed that Adam lived for 930 years, Noah for 950, Moses for 120, and Abraham for 175. Adding up all the life spans, Africanus concluded that Christ was born 5,500 years after the formation of the earth and that he would return in the year A.D. 500.

Subsequent chronologists, including Martin Luther, adjusted the date of the Second Coming by a process we would now call *model tweaking*. According to Jack Repcheck's fascinating account of this in *The Man Who Found Time*, "the chronologists [that followed Africanus] were consistent in putting off [the Second Coming] until a couple of hundred years after their own deaths." As is often the case in some modern modeling endeavors, so much uncertainty existed in the original numbers that tweaking was carried out without raising questions of credibility.

The last and perhaps most famous chronologist was James Ussher, the Calvinist archbishop of Armaugh (Ireland), who pronounced in a 2,000-page book published in 1650 that creation of the earth started at noon on Sunday, October 23, 4004 B.C. By his reckoning, Christ should have returned around October 23, 1996.

The age of the earth according to geologists is much greater than Ussher's reckoning. The conceptual model of the chronologists failed for a number of reasons. Like Lord Kelvin's model of a cooling earth, the methodology of the model was reasonable enough, but the underlying assumption was unsound. Counting biblical generations and events is a valid approach (assuming that everything was recorded accurately in the Bible), but the assumption that Adam came along when the earth began has no basis in science.

Faith-based assumptions and conceptual modeling are clearly immiscible. But we will demonstrate that applied mathematical modeling is at times no less biased, skewed, or slanted by political correctness, advocacy, or economic interests than the biblical slant of the chronologists.

Fast-forwarding to the late twentieth century, we confront another celebrated failure of quantitative modeling. It began with the 1972 publication of *Limits to Growth*, a book commissioned by the Club of Rome. The club, a secretive think tank started by a distinguished British research chemist and a successful Italian industrialist in 1966, today consists of around a hundred economists, businesspeople, scientists, and government officials from fifty-two countries on six continents. The club's book famously predicted that within the coming hundred years, there would be widespread natural resource shortages and economic collapses. The authors warned that unless immediate action was taken to control population and pollution, we would not be able to turn the situation around. This doomsday prediction was based on a mathematical model known as the *pessimist model*. Unlike the simple analytical model applied by Lord Kelvin, this was a more complex model called World III and requiring extensive computer calculations. The document argued that population growth and pollution from industrial expansion were leading to total exhaustion of natural resources and massive environmental destruction. It predicted that catastrophes would begin by the year 2000.

There were many problems with the model. It treated the earth's mineral reserves as fixed and unchanging. This decidedly static view of economics and unhistorical understanding of human creativity held that we would run out of oil according to a time schedule calculated from what was then known about reserves and production methods. It ignored the possibility of additional major oil discoveries, advances in petroleum exploration and extraction technology, and the possible contributions of nuclear, solar, or wind energy sources. The model also assumed that food production per unit of land area would remain steady.

Oreskes notes: "In effect [earth] scientists treat the systems they are modeling as though the systems were static. This is not to say that the modelers believe the systems are static—no earth scientist could imagine any system as truly static. Nonetheless scientists often imbed stasis into their models."

University of Manitoba professor Vaclav Smil summed up his view by noting that the *Limits to Growth* report "pretended to capture the intricate [global] interactions of population, economy, natural resources, industrial production and environmental pollution with less than 150 lines of simple equations using dubious assumptions to tie together sweeping categories of meaningless variables."

The problems with the model went beyond the huge technical weaknesses. It was an example of an *advocacy model*. A Club of Rome official stated shortly after the predictions were released that the idea was "to get a message across, and to make people aware of the impending crisis." In other words, the model outcome had been determined before the model was run. Finding the truth according to a preconceived opinion or philosophy is a common flaw in applied mathematical modeling. And it is very similar to finding truth that matches one's religious faith.

The *optimist model* emerged in a 1976 book titled *The Next 200 Years*, by Herman Kahn and others. This volume presented a view of the future that could be briefly stated as "necessity is the mother of all invention." Kahn basically argued that when the need for more food arises, better technology will save the day. When the price of oil soars out of sight, other sources of energy will come to the fore. This model is a qualitative conceptual model, based simply on a number of scenarios devised by the authors.

Both the pessimist model and the optimist model were derived from the same database. The difference is in the assumptions made and in the personal views of the modelers. *Personal view models* are those that are slanted to prove the belief of the modeler.

Ideally, comparing model results with a real-world situation, a process known as *calibration or validation*, tests a model. That is, an attempt is made to "predict" an event that has already occurred using the model in question. For example one could *hindcast* the cod failure on the Grand Banks.

However, one successful calibration or one successful prediction does not mean the next attempt at calibration will also pass muster. As Naomi Oreskes argues, successful reproduction of an event in a complex natural system is no guarantee that the model will accurately predict or describe the next such event. In fact, she argues that most likely it won't make a successful subsequent prediction. Calibration may show that a model fails to reproduce a situation, but the converse is not always true. Leonard Konikow and John Bredehoeft, geologists with the U.S. Geological Survey, made the same point in a famous 1992 paper titled "Ground-Water Models Cannot Be Validated." The Konikow and Bredehoeft paper received the Meinzer award from the Geological Society of America, but their paper and the views of Oreskes seem to have had minimal impact in the modeling community. Model calibration and validation are alive and well.

In some types of modeling, a second calibration, known as verification, is carried out. The model is first calibrated with one set of events and then verified with a second set. It could work like this: The model is tweaked so it successfully predicts shoreline erosion along a stretch of coast that occurred between 1950 and 1970. The tweaked model is then verified by application to the known erosion rate between 1970 and 1990. If it successfully predicts the erosion rate between 1970 and 1990, the model is said to be verified and can be used to predict the future.

Perhaps the single most important reason that quantitative predictive mathematical models of natural processes on the earth don't work and can't work has to do with *ordering complexity*. Interactions among the numerous components of a complex system occur in unpredictable and unexpected sequences. In a complex natural process, the various parameters that run it may kick in at various times, intensities, and directions, or they may operate for various time spans. Chapters 5 and 6 provide examples of this phenomenon, with lists of dozens of parameters that may affect the natural processes of shoreline erosion and longshore transport of beach sand, respectively. Parameter after parameter kicks in and out—who knows when, where, and for how long. Complicating things even more are positive and negative feedbacks.

Complexity is a big thing in today's modeling world. There are circulating newsletters, books, technical journals, societies, scientific meetings, and branches of funding agencies that are concerned almost exclusively with complexity. The term is formally defined in a number of (complex) ways, but we will stick with the (relatively) simple explanatory description in the previous paragraph.

William Sherden is a marketing consultant, a Stanford University professor, and the author of *The Fortune Sellers*, a book that provides a skeptical view of stock market forecasting. He notes that complex systems are so highly interconnected with numerous positive and negative feedback loops that they often have counterintuitive cause-and-effect results, as when the "addition of a new highway to alleviate a traffic jam causes the traffic jam to become worse," a *negative feedback*. The rich getting richer and the poor getting poorer are both examples of *positive feedbacks*.

Global warming could lead to melting of the Arctic Ocean ice cover, leading to increased evaporation of ocean water, leading to more precipitation in the Arctic region. Increased snowfall leads to increased accumulation of ice leads to a new ice age. Thus, global warming leads to global cooling, a negative feedback of global proportions.

One reason why earth systems are complex has to do with the relationships between the variables that make a system work. A *linear relationship* is one in which variables increase or decrease at a uniform rate—a straight line on a graph. Most relationships between parameters in a complex earth surface process, however, are *nonlinear relationships*. As one variable changes, another may change exponentially. What complicates the relationships between the numerous parameters that control any natural process even more is the fact that a number of them may change simultaneously as a natural process unfolds. A relationship that may be linear in isolation may be nonlinear in the context of simultaneous changes in other parameters. The reality of any natural process on the earth's surface is a convoluted bird's nest of interrelationships. Complexity reigns, and that is the beauty of the natural world. An example is Yucca Mountain (chapter 3), where as the downward rate of water flow increases through the rocks, the volume of water transported increases disproportionately. This is a positive feedback and a nonlinear relationship.

In classic physics, by contrast, the systems being dealt with are usually not complex, in the sense used here. Modeling in physics is labeled *determinism*, in that prediction of events is possible. Thus we are successful in prediction of the future positions of the planets, the times and dates of eclipses, the rate at which radioactive elements decay, and the time it will take a ball to roll down an inclined plane.

The *New York Times* on June 7, 2004, noted: "In New York City sunrise will be at 5:25 a.m. Eastern time on Tuesday, and Venus is to begin leaving the solar disc at 7:06 a.m., when the sun is 17 degrees above the horizon. The planet's final contact with the sun's edge should occur about 7:26 a.m. when the sun is 20 degrees high. There will be another transit on June 6, 2012. After that, the next ones will occur in 2117 and 2125." What a contrast to prediction of events in complex systems like beaches, global climate, fisheries, rivers, the stock market, and invasive plants!

The same predictive success is possible in the engineering design of bridges and elevated water tanks. The laws of physics apply well to steel and concrete. Plus, humanity has accrued a great deal of experience with these materials to sharpen predictions.

Engineering design and prediction always have a large *safety factor* to allow for human error and to assure that structural safety predictions will be right. Designs are intended to last a certain length of time, to withstand a certain wind velocity or an earthquake of a specified magnitude.

Modeling in any system that results in a single answer (right or wrong) without any indication of the possible range of error in the answer is a *deterministic approach*. In most applied quantitative modeling of earth processes, the results should be *probabilistic* to express the uncertainties involved. That is, the answers should have an error bar or a plus or minus expression of the possible range within which the correct answer must lie.

It is a catch-22 situation. Modelers view error bars as a valid response to critics of quantitative mathematical models, but you can't determine accurate error bars for a prediction without having the same level of model accuracy that is needed to get accurate deterministic answers. Furthermore, an invalid model doesn't provide a valid answer, whether you use error bars or not.

The *error envelope*, or *cone of uncertainty*, on a predicted hurricane path (figure 2.2) is an example of a very useful error bar for quantitative mathematical models. The National Hurricane Center and the Weather Channel produce maps showing an ever-widening funnel of possible storm impact areas in the direction of storm movement. The funnel is centered on the most likely predicted line of storm movement for as much as three days in advance. Most people in hurricane-prone areas probably have an intuitive feeling about the accuracy of hurricane model predictions because in the past many have stocked up with extra food supplies and/or evacuated their homes only to find that the sun is shining and a gentle breeze is blowing on the predicted day of storm arrival.

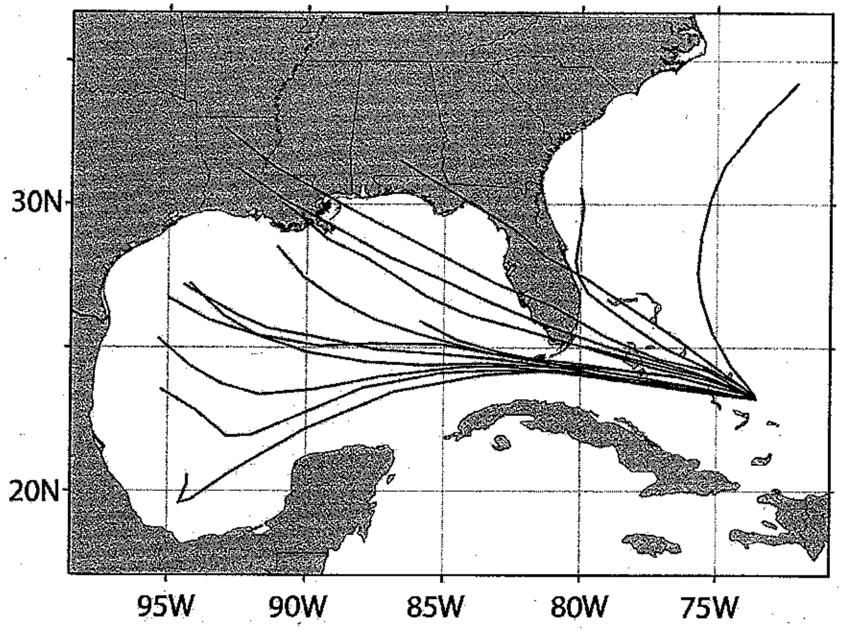

Figure 2.2 Modeled hurricane tracks for Hurricane Rita in 2005. The tracks form a cone of uncertainty, which, while frustrating to coastal dwellers, is a straightforward way to represent the uncertainties of hurricane track predictions. Diagram from Colorado State University Department of Atmospheric Sciences.

Meteorologists are up front about the uncertainties of their hurricane path predictions, which are high, even though the models have an excellent statistical or experience base. There must have been much gnashing of teeth at the Hurricane Center when Hurricane Dennis (1999), located off Cape Hatteras, halted and then reversed its path and began moving south, a most unusual path. Teeth gnashing of an even higher order must have occurred when Hurricane Ivan (2004) passed across the Gulf of Mexico shoreline, through the state of Virginia, and then made a wide southerly arc out into the atmosphere over the Atlantic Ocean. Eventually the remnants of Ivan returned to the Gulf of Mexico and crossed the Gulf shoreline for a second time!

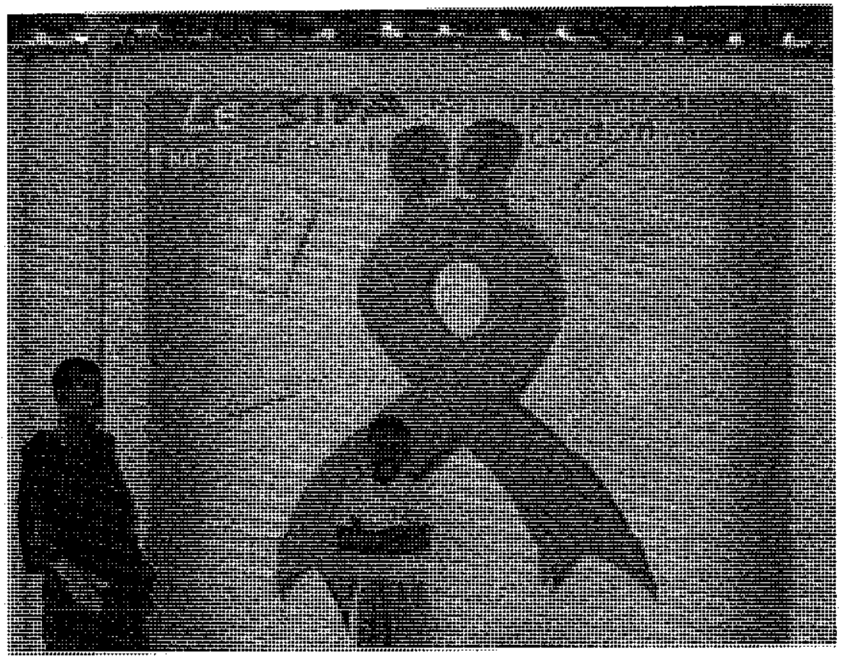

Figure 2.3 AIDS education poster in Mali, Africa. Countries in Africa with extensive education campaigns are having some success in holding down HIV numbers. Mathematical modeling by the United Nations of the extent of this societal catastrophe in Africa illustrates the problem of introduction of a sympathy bias into the numbers. Photo courtesy of the United Nations.

Mathematical models can be used to boost causes, both bad and good. A troublesome example of *good cause modeling* is the prediction and monitoring of the spread of HIV/AIDS around the world, especially in Africa, where the disease is taking its worst toll (figure 2.3). UNAIDS, a sub-agency of the United Nations World Health Organization, takes the responsibility for tracking the disease, which it does in large part through the use of mathematical models. UNAIDS now claims that 30 million Africans suffer from the disease. Rian Malan, a descendant of the Malans who instituted apartheid in South Africa, author, anti-apartheid activist, and now an investigative South African reporter, argued in a startling article in the December 14, 2003, *Sunday Telegraph* that the UN models may have distorted the extent of the AIDS epidemic in Africa.

Quantitative mathematical models are universally used to keep track of and to predict the future courses of diseases. But, of course, models require extensive *ground-truthing*, or *field-checking*. In most of southern Africa, record keeping is poor to nonexistent, and with the exception of South Africa there simply is no dependable real-world information to run checks on model results. UNAIDS predicted (in hindsight) that 250,000 South Africans died of AIDS in 1999. This figure was determined by the use of the *Epimodal Model*, the same model that was used to predict AIDS deaths all over Africa. Although the number who died of AIDS is unknown, according to Malan it is accurately established that 375,000 South Africans died of all causes in 1999. The number of AIDS victims predicted by Epimodal is far too large a proportion (two-thirds) of the total deaths. Other public health scientists, using the *ASSA 600 model* (Actuarial Association of South Africa), predicted (again in hindsight) that 143,000 South Africans died of AIDS in 1999. In 2001 the "much advanced" *ASSA 2000* model concluded that there must have been 92,000 AIDS deaths. A run of the new *MRC model* (Medical Research Council) came up with 153,000 deaths in 2001–2002 from AIDS in South Africa.

This is not to negate the importance of AIDS modeling, for the disease is a dreadful global plague that kills before middle age and has left orphans by the many thousands. Indeed, there are real difficulties in determining AIDS death rates because the weakened immune system can result in death from a number of other causes. In addition, doctors may not report the involvement of HIV in order to spare stigmatization of relatives or to prevent invalidation of insurance policies.

Robert Guest, in his book *Shackled Continent* (2004), argues that two kinds of orphans are produced by AIDS—the young and the old. In most African societies the elderly expect their children to care for them in their final years. Instead, the old ones are caring for their dying children and then inheriting their grandchildren. The tragic AIDS-related 1995 death of Nelson Mandela's son brought home to South Africans that no one is safe from the disease. In Durban, South Africa, where the disease is particularly widespread, there are now 600 funerals per weekend compared to 120 five years ago, and graves are being "recycled." And the worst may be yet to come, as the disease appears still to be on the increase in Africa.

But the experience in South Africa suggests that the AIDS disaster may not be as far advanced as previously assumed by the United Nations. Certainly this is a point worth considering, because research on other, more ravaging diseases in Africa, such as malaria, is said to be underfunded because of the anticipated AIDS calamity.

Malaria experts say that 900,000 deaths from malaria occur every year in sub-Saharan Africa. Seventy percent of the dead are children under five years of age. Where did the numbers come from? The estimates of both of these dreadful diseases in Southern Africa suffer from the same lack of local health records. The models have a poor database.

The possibility that a true global human disaster is just around the corner unfortunately provides an unparalleled opportunity for modeling that jacks up the numbers to draw attention and funding. Failure to make a simple reality check allowed the results to become accepted "facts." The apparent sophistication of the models dampened criticism, as did the huge outpouring of sympathy for the afflicted.

In this case the models are probably perfectly good, but the answers they come up with are problematic because:

- the database was poor;
- the models were polluted by a huge "sympathy" bias;
- no one looked back.

Reporter Malan noted: "They told me that AIDS had claimed 250,000 lives in South Africa in 1999 and I kept saying this can't possibly be true. What followed was very ugly—ruined dinner parties, broken friendships, ridicule from those who knew better, bitter fights with my wife. After a year or so she put her foot down. 'Choose,' she said. 'AIDS or me.'" He dropped the subject for more than a year but couldn't resist returning to the question, presumably with his wife's reluctant approval. Malan discovered the going-with-the-grain truth about models, that modeling results are easier to live with if they follow preconceived or politically correct notions.

Sometimes the results from mathematical models are pushed aside by public opinion for reasons bearing no relationship to the veracity of the models. *Epidemiology models* provide standard and widely applied methods to evaluate the causes of human illnesses. *Relative risk* is determined by comparing one population that is affected by something (cigarette smoking, coffee drinking, polluted water consumption, and so on) with the general population. The models must take into account a complex array of *confounders* that could affect the results, such as age, sex, economic status, race, location, allergies, and nationality, among others. Still, the results of these data-rich *statistical models* are widely accepted. Statistical models are based on the assumption that past behavior of a system is a guide to future behavior.

The secondhand smoke (SHS) problem is an example of the public's refusal to accept model results because the results are unpopular.

This is buttressed, of course, by the fact that health problems related to smoking are indisputable and widely recognized and that public tolerance for SHS has rapidly diminished in recent years. Imagine the response of a crowd in an elevator when the big guy in the opposite corner lights up a cigarette!

In 1992 the EPA released its famous report classifying SHS as a class A carcinogen. In this example of *politically correct modeling*, the EPA announced that secondhand smoke was responsible for approximately 3,000 lung cancer deaths each year in nonsmoking adults. On the other hand, in 1998, on the basis of a study involving people from six European countries, the World Health Organization reported no significant cancer risk from SHS.

The EPA report has come under intense criticism. It was based on a *meta-analysis*, a summary of 33 previous investigations, mostly epidemiological mathematical model studies, by others, mostly non-EPA scientists. The number of studies actually used was reduced to 11, yielding a risk factor of 1.19. A risk factor of 3 or 4 is usually required before the EPA considers something a risk to humans. The EPA had announced the 3,000 annual American cancer deaths figure before the study was completed, and when the study did not back up the numbers, it doubled the statistical margin of error to come up with something close to 3,000 in order to save the day. The EPA increased the size of the error bars, thus "enclosing" the number 3,000 between the plus and minus extremes of the prediction.

In 1998 federal judge William Osteen declared the EPA study to be null and void. He noted there was evidence in the record that the EPA had cherry-picked its data (chose only the most favorable studies) and the agency was "publicly committed to a conclusion [3,000 lung cancer deaths from SHS] before the research had begun." In a 2003 speech, Michael Crichton, author of *Jurassic Park* and the highly controversial anti-global-warming novel *State of Fear*, called the EPA study "openly fraudulent science." EPA administrator Carol Browner responded to the judge's 92-page scolding of the agency that "the American people certainly recognize that exposure to SHS brings a whole host of health problems" (probably because of the EPA campaign against SHS). "Consensus trumps science," says Crichton.

SHS opponents routinely claim that the models prove a strong cancer health risk. Not true. Such dishonesty is accepted by our society because the cause (prohibition of SHS) has become a moral issue, not a scientific one. In addition, there are real and significant health problems associated with SHS, including heart disease, pneumonia, and bronchitis, especially among children and asthma sufferers.

Useless Arithmetic on Wall Street

Howard Kurtz and William Sherden, along with many others who have written about stock market prophecy, give innumerable examples of erroneous stock market predictions' being presented with great confidence, the aftermath of which produces no loss of prestige to the failed analyst. Phillip Tetlock in his recent book, *Political Judgment,* shows with statistical analyses that experts in general, and experts on the stock market in particular, are no better than educated non-experts at predicting the future.

Hope springs eternal, however, and market prediction is a field that is becoming ever more quantitative. In 1997 the Nobel Prize for economics was awarded to Myron Scholes and Robert Merton, who, collaborating with economist Fischer Black (who died in 1995), developed a mathematical model for stock market derivatives. Black and Scholes derived the original equation, and Merton is said to have improved it in such a way as to make the model applicable in the real world of Wall Street. The equation involved four variables: duration of an option, prices, interest rates, and market volatility.

The Nobel Prize that year was controversial from the start, although few doubted the genius of the equation. The controversy arose over the question of whether helping rich people get richer was elevating mankind in the sense of Alfred Nobel's original intentions. The next year, possibly in atonement for such insensitivity, the Nobel Prize Committee voted for Professor Amartya Sen, known best for his work on the causes of famine, poverty, and social inequality. The Nobel Prize Committee said Sen "has restored an ethical dimension to the discussion of vital economic problems."

The Black-Scholes equation was widely adopted to calculate the value of options in complex derivative dealings. Derivatives are financial instruments that have absolutely no value on their own, but instead "derive" their value from other assets. The use of derivatives reduces risk and uncertainty in profit, for example, the risk of unexpected price fluctuations. There are derivatives that are contracts or obligations for future delivery, called futures, and there are derivatives that give an opportunity (but not an obligation) to buy or sell at an established price, called options.

The Nobel laureates Scholes and Merton were founding partners in the now infamous Long-Term Capital Management (LTCM) hedge fund that helped fuel the explosion of derivatives trading on Wall Street. At its height, LTCM was the darling of Wall Street, a monetary fund comprising a dream team of Nobel Prize–winning founders and complex financial models who seemingly had developed a clean, highly rational way to earn high returns with little risk, using models and supercomputers to identify investments. Some book titles give clues to the fate of LTCM. The story is told in *Too Big to Fail,* by Kevin Dowd, and in *When Genius Failed,* by Roger Lowenstein. In 1998 the fund that was "too big to fail" suffered catastrophic losses that threatened the stability of money markets worldwide. Lowenstein said the cause was "the disease of perfect belief."

According to Lawrence Summers, former secretary of the U.S. Treasury, "The efficient market hypothesis is the most remarkable error in the history of economic theory." Yet two underlying assumptions behind LCTM's market models were

- that markets are always liquid (e.g., you can always sell an asset at a reasonable price); and
- that markets are efficient and they tend toward equilibrium.

For four years, starting in 1994, LTCM showed incredible returns of about 40 percent per year. Stephen Rhodes (a pseudonym) notes that with about 100 employees, LCTM made more money than McDonald's global hamburger business. All this money and not a useful product in sight.

In the global economy today, international markets are closely linked. A trend in one nation's market can quickly spread to the next. The demise of the LTCM hedge fund began on August 17, 1998. Russia defaulted on its debt, and the worldwide financial markets lost their logical order. Investors fled to more-secure investments, and the firm lost about $3.6 billion in five weeks. On one single day, the firm lost $550 million. The collapse of the hedge fund brought little sympathy from the American media. "We're So Rich, We Can Be Dumb," headlined the *San Francisco Chronicle.*

The collapse of Long-Term Capital Management threatened to create a panic on Wall Street, since many major banks had lent it and other such funds huge sums of money. Almost 50 percent of the world's top banks were involved in rescuing the hedge fund. The consortium gave LTCM $3.6 billion in exchange for 90 percent of the firm. Shareholders retained a 10 percent holding, valued at $400 million, and the dream team kept their jobs. Unfortunately, as some in the media have noted, by sparing shareholders, creditors, and fund managers some of the pain of the loss, we seem likely to see a repeat of the behavior that produced the crisis in the first place.

Economic models applied to the stock market do not work because human emotion and action are unpredictable. Is it not obvious that the stock market is not predictable? It shouldn't surprise us that panic, overconfidence, underconfidence, fraud, ignorance, success, and all kinds of other aspects of human nature control the market.

The lesson to be learned from the Nobel Prize–winning equation and its application by LCTM was forcefully expressed by financial guru and founder of Numa Financial Systems, Ltd., Stephen Eckett, who said, "I regard the Black-Scholes model as one of the most dangerous inventions of the twentieth century. This is not to blame Black and Scholes obviously: the danger is always in the application. But what happened was that one single equation—and mathematically the model is simple—seemed to offer the possibility of quickly understanding and controlling derivatives risk. This encouraged thousands of banks to employ bright mathematicians who had little knowledge of the financial markets but nonetheless started furiously programming their spreadsheets on which billions of dollars were gambled."

Cathy Minehan, president of the Boston Fed, is quoted as saying, while introducing a behavioral economist, "All our models and forecasts say we will have a better second half. But we said that last year. Now don't get me wrong. Mathematical models are wonderful tools. Standard economics would argue that people are better off with more options. But behavioral economics argues that people behave less like mathematical models than like—well, people."

The scandalous bankruptcy at the Enron Corporation holds modeling lessons as well. Economist Keith Cooley describes one of the ways that Enron was able to jack up its apparent profits: "At the heart of the so-called innovative trading at Enron was an accounting rule. When Enron agreed to supply power to a company or municipality at a fixed-price contract, it made projections on the level of future prices and the likely profit over the lifetime of the contract. Under the accounting rule in question, it was then able to report that profit as soon as the contract was signed rather than booking the gains over time."

The problems with Enron's prediction of profits under newly acquired contracts were

- the models were " undisclosed";
- the predictions were always highly optimistic;
- credence was provided through approval by an "independent" accounting firm (the firm, Arthur Anderson, lent the model results an air of credibility, but it had to sell itself off in pieces when the scandal unfolded);
- no one looked back.

A Look Back

Modeling equations are sometimes modified and altered (*tweaked* or *tuned*) until the model correctly "predicts" an already known natural event. Frequently in the modeling literature, however, this is considered a model application or prediction. Although the model may have reproduced something in nature, it is a jimmied equation, one that was adjusted bit by bit to fit a single event or to arrive in the approved range. According to Peter Haff, Duke University model critic, model philosopher, and physicist turned geologist, this approach is better termed *model development*. Haff notes that modeling when the outcome is already known is not the same as a true model prediction before an event occurs. It is in no sense a model prediction.

A good tweaked model example is the modeling of the artificial floods that took place in the Grand Canyon in 1996. Water was purposely released from the Glen Canyon Dam in Colorado to imitate the floods that occurred before the dam smoothed out the peaks in flow volumes (figure 2.4). The problem was that the dam had smoothed out floods and had also trapped almost all the sand coming down the river. In addition, the river was expected to become more difficult and dangerous for rafters because mounds of sediment ranging up to boulders in size, brought to the river by flooding tributaries, were staying in one place, piling up and creating dangerous rapids. Normally, the floods flattened out these rock piles.

The hope for this experiment was that the floods would leave behind new sandbars, just like the old floods once did. Sand would be derived from the stream channel, and the new sandbars would provide much-needed new campsites for river rafters and stream habitats for native fish species that are fast disappearing.

The 1996 experimental flood, probably the first of many to come (a second release was carried out in 2004), didn't completely succeed (politicians declared it a success, but scientists had a different view). Few new sandbars were formed. After the fact, however, geologists were

Figure 2.4 An artificial flood in progress. Water is being released at a high rate from the Glen Canyon Dam in an attempt to provide additional sandbars for river boaters in Grand Canyon. Predictive models of the sandbar configuration that resulted from the water release were unsuccessful. Photo courtesy of the U.S. Geological Survey.

able to tweak the model and come up with the same sandbar configuration on paper that actually remained on the canyon floor after the flood. The modelers confidently suggested that the model would now be useful to predict what will happen in planned future water releases from the dam. Tempting as it might be to believe that the model was now valid, agreement between the model and a single event is not an indication of model validity in a complex system, as noted by Naomi Oreskes. The modelers had developed a new model by tweaking the old one, but it probably won't predict sandbar formation in the next flood. It probably won't be even close.

Today's scientists have substituted mathematics for experiments, and they wander off through equation after equation and eventually build a structure which has no relation to reality.

—*Nikola Tesla, inventor and electrical engineer extraordinaire, 1934*

chapter three

yucca mountain

a million years of certainty

Waste Disposal: A Troubled History

The development and use of nuclear technology began in the early 1940s. Americans grandly entered into a nuclear age that ended a world war and promised permanent supplies of cheap energy. Many cultural images from this time linger with us today: dancing the atomic boogie, drinking atomic cocktails, building backyard bomb shelters, and practicing "duck and cover" drills in schools. Even today the mushroom-shaped cloud remains the high school symbol for the "Bombers" of Richland, Washington, located near the Hanford nuclear plant.

Over time, our perception of the bright promise dimmed as the hazards and by-products of nuclear use became apparent. Some nuclear waste products produce radiation that persists for long periods of time; other waste products can be used to make nuclear weapons. The antinuclear movement has been a vocal, visible presence in the United States for decades. Government failures are largely responsible for the current high level of skepticism that the American public holds for our regulation