ESSAY

Observation, Hypothesis-Testing, and Discovery in Oceanography

BY THOMAS R. ANDERSON

Discovery lies at the very heart of scientific endeavor. It heralds progress, the march of science in uncovering the mysteries of the world that surrounds us. But how do discoveries come about? "By observation, of course" is the simple answer, nature being an open book with new discoveries beckoning at every call. This attitude, the curiosity and desire to observe nature in her original form, turned naturalists into explorers. In the early days of oceanography, these brave souls travelled far and wide in search of exotic creatures and plants, marvelling at their beauty and diversity. Yet today, such a cavalier approach is likely to be frowned upon, particularly by the philosophers of science. They emphasize the importance of hypothesis-driven research, in which questions are formulated and then subject to test by experiment. Observation is subordinate. In this article I will elaborate these issues and argue that, despite the criticisms of the philosophers, observation is an essential prerequisite to identifying and understanding the complex patterns and trends of variability in the ocean. It is therefore central to the progress of oceanography today, complemented by hypothesis-driven studies focusing on cause and effect.

EARLY EXPLORATION AND INDUCTION

The Challenger Expedition of 1872 to 1876 is often proclaimed as the birth of oceanography as an organized discipline. Led by the naturalist Charles Wyville Thomson, it had the aim of "examination of the physical and biological conditions of the deep sea throughout the great ocean basins" (Thomson and Murray, 1885, p. 1). Lasting three and a half years and covering 68,690 miles, this pioneering voyage gave Thomson the ideal opportunity to explore the deep sea, which he described as "the land of promise for the naturalist, the only remaining region where there were endless novelties of extraordinary interest ready to the hand which had the means of gathering them" (Thomson, 1874, p. 49). The first systematic record of currents, temperatures, and depths of the world's oceans was made on this expedition, as well as the discovery of more than 4000 new species in net trawls. A set of fifty volumes, the *Challenger Reports*, was completed in 1895, documenting the hydrography, botany, zoology and bottom sediments encountered throughout the voyage. Thomson's predecessor, Edward Forbes, summed up the spirit of adventure and discovery in these early oceanographers: "Beneath the waves, there are many dominions yet to be visited, and kingdoms to be discovered; and he who venturously brings up from the abyss enough of their inhabitants to display the physiognomy of the country, will taste that cup of delight, the sweetness of whose draught those only who have made a discovery know" (Forbes and Godwin-Austen, 1859, p. 11).

The early days of oceanography thus had a strong emphasis on the collection and cataloguing of facts, from which generalizations and the development of theory could arise. This process is known as induction, the scientific method

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championed by the seventeenth century philosopher Francis Bacon. Science is then a critical and analytical activity, as summed up by Karl Pearson: “The hard and stony path of classifying facts and reasoning upon them is the only way to ascertain truth” (Pearson, 1892, p. 20). This view of scientific method was prominent among both philosophers and scientists until the early part of the twentieth century. Since then, various distinguished philosophers, notably Bertrand Russell and Karl Popper, as well as many practicing scientists and in particular the immunologist Peter Medawar (who was also an accomplished philosopher), have emphasized instead the virtue of hypothesis-driven research. Induction has been criticized on a number of grounds. First, inductive arguments are subject to uncertainty (i.e., can never be proven, and so cannot be justified by logic). For example, if we assert inductively that flamingos are pink on the basis of millions of observations, this does not mean that all flamingos, present and future, are or will be necessarily pink. Second, induction, at least in its simplest form, supposedly starts with unprejudiced, innocent observation of facts. But, as it is fashionable to claim, observations are themselves based on ideas or theories, i.e., they are “theory-laden.” Popper emphasized this point by beginning a lecture to a group of physics students by saying: “Take a pencil and paper; carefully observe, and write down what you have observed!” The students asked in turn what he wanted them to observe, Popper concluding that the instruction “Observe!” is absurd (Popper, 1963, p. 46). Third, it can be argued that collections of observations cannot lead to knowledge unless they are linked together, usually inferring causal mechanism, requiring creativity and imagination rather than simple deduction. Realizing this, the philosopher William Whewell advocated an advanced form of induction utilizing conceptions akin to hypotheses to aid discovery: “Before the inductive truth is detected, the facts are there, but they are many and unconnected. The conception which the discoverer applies to them gives them connection and unity” (Whewell, 1840, p.42). The American philosopher George Gale emphasized the importance of addressing cause, labelling science based on generalizations of data “cookbook science,” and proposing that only if we go on to show why regularities exist can we progress to “explanatory science” (Gale, 1979, p. 65). Without addressing cause, Gale likened science to following recipes in a cookbook.

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HYPOTHESIS, IMAGINATION, AND UNIVERSAL LAWS

Hypothesis-driven research, the alternative to induction, supposedly begins with a new idea conjured from the imagination, which is then the subject of critical analysis via observation and experiment. The resulting theories (a theory is a hypothesis or group of hypotheses that have withstood critical empirical testing) are described by Popper (1959, p. 59) as “nets cast to catch what we call ‘the world’: to rationalize, to explain, and to master it.” Well-publicized hypothesis-driven discoveries often originate as flashes of inspiration, a classic example being the discovery of the benzene ring in chemistry, postulated by Friedrich Kekulé in 1865. In an address to the German Chemical Society during 1890 Kekulé recollected how, in a half-waking dream as he slumbered before the fire, he had seen atoms “gambolling” before his eyes and thereby conceived that molecules of aromatic substances are formed of chains of atoms coiled in a ring. Continuing his lecture, he went on to proclaim “Let us learn to dream, gentlemen, then perhaps we shall find the truth” (Japp, 1898, p.100). It was only some time after Kekulé’s discovery that chemists were able to establish the theoretical stability of the ring structure with certainty. Another example, this time from the realm of physics, is the "plum-pudding" model of the atom (e.g., Eisberg and Resnick 1974, p.95), proposed by John Joseph Thomson in 1898. In this model, clearly a conjecture of the imagination, electrons are scattered in a spherical sea of positive charge, like plums in a pudding. The model was experimentally tested and disproved in 1911 by Ernest Rutherford, leading to the current theory that positive charge resides in the nuclei of atoms. In each case, the discovery was an act of mind, and the necessary facts for confirmation obtained after the hypothesis was conjectured.

The recent philosophical literature, with its emphasis on hypothesis, has been heavily influenced by the progress of physics and chemistry throughout history. There has been extensive analysis of, for example, how Copernican astronomy replaced the Ptolemaic system, the downfall of the phlogiston system of combustion, and the development of Newtonian mechanics and its subsequent modification by Einstein. It appears that the supreme task of the physicist is to arrive at universal elementary laws. Hypothesis, in conjunction with controlled experiment, may indeed be the primary means of making progress for this purpose, with induction being of little or no use. If, for example, one carefully kept a notebook full of the experiences encountered in the course of daily life (i.e., simply collected observations), this would probably be of little or no value to physicists at all. As Toulmin (1953) has pointed out, discoveries in the physical sciences consist in the introduction of fresh ways of looking at phenomena and in the application of new modes of representation, rather than in finding new generalizations.

Marine systems are complex and consequently, unlike the confined environment of a physics laboratory, it is inherently difficult to undertake controlled experiments at sea in which only a single factor is varied in the face of all the other variables at Mother Nature's command. Repeatability, a hallmark of ideal scientific method, is likewise a real problem. Hypothesis testing does nevertheless have a major role to play in oceanography, as in other scientific disciplines, providing a powerful tool for investigating cause and effect. One cannot after all simply observe the vertical velocities associated with coastal upwelling, that copepods grow best on a mixed diet, or that marine phytoplankton are the dominant source of methyl iodide to the atmosphere. A good example of the value of conjecture is provided by the "iron hypothesis" of John Martin, in which he proposed that iron deficiency limits phytoplankton growth in the nutrient-rich waters of the Antarctic and subarctic Pacific oceans (Martin and Fitzwater, 1988). Various iron fertilization experiments were subsequently undertaken to test this hypothesis, giving rise to artificially stimulated blooms of chlorophyll.

It should be noted that even the early oceanographers were not entirely devoid of hypotheses when planning their expeditions. For example, Edward Forbes is remembered for his azoic hypothesis, which stated that at depths greater than 300 fathoms (600 m) the sea bottom became a desolate wasteland, devoid of life, due to the immense pressure exerted by the water column above and lack of light. The bottom dredging work of the Challenger Expedition was able to test and disprove this hypothesis.

THE IMPORTANCE OF PATTERN

A glance at the scientific literature shows that the human capability for pattern recognition is deeply embedded in scientific practice (Ziman, 1978). Unlike physics and chemistry, oceanography is not about discovering universal laws. But instead, identifying and elaborating pattern is of greatest value, induction playing a central role. The ocean (climate) system can be characterized by modes that can be thought of as naturally occurring patterns of variability, with each pattern exhibiting unique spatial characteristics but typically vague temporal characteristics (Mantua et al., 2002). Examples include the El Niño Southern Oscillation (ENSO), the North Atlantic Oscillation (NAO), and the Antarctic Circumpolar Wave (ACW). Study of these patterns leads to suggestions of possible interconnections between otherwise unrelated facts, providing the stimulus for ideas about what might or might not be important in terms of causal relationships. Examples include relationships among the biogeography of zooplankton in the Northeast Atlantic, sea surface temperature, and the NAO (Beaugrand and Reid, 2003), and the effect of the ACW on ecological interactions among ice, krill, and penguins in the Southern Ocean (Fraser and Hofmann, 2003).

Patterns in the ocean are often complex and subtle, and so a great deal of effort needs to go into "fact-gathering." Fortunately, the last two decades has seen a revolution in the ability to observe the oceans on a global scale, driven in large part by the Tropical Ocean Global Atmosphere Study (TOGA) and the World Ocean Circulation Experiment (WOCE) (Gould, 2003). These and other programs have led to the synthesis of various global *in situ* and satellite-derived data sets, such as current directions and speeds, ocean color, nutrients, and CO_2. An observational presence in the ocean is ongoing under the general umbrella of the Global Ocean Observing System (GOOS), which includes an ENSO observing system in the tropical Pacific and an array of drifting, profiling floats as part of the Argo program. A promising development of recent times is the use of autonomous platforms, equipped with a variety of sensors, providing cost-effective monitoring capability (Perry and Rudnick, 2003). Initiatives such as the Ocean Research Interactive Observatory Networks (ORION) are paving the way to obtain more and better data for processes that are rare and episodic, or that involve fluctuations or trends over many years. The ORION project involves a combination of permanent moorings placed at strategic locations across the world's oceans, a network of seafloor cables, and coastal observatories. Such an intense effort will provide the means of detecting and forecasting ocean components of climate variability.

Scientists need to use their experience and intuition to greatest effect when deciding upon what types of observations to make, bearing in mind that discoveries are often a result of new connections among facts. Diversity of measurements is important. If, for example, biogeochemical cycles are of interest, then independent measurements of as many processes in the budgets as possible are desirable. Concentrated interdisciplinary studies, an ongoing strength of the marine scientific community, are of the greatest benefit in this respect. A fine example is the Joint Global Ocean Flux Study (JGOFS), which had as its aims to study on a global scale the processes controlling the time-varying fluxes of carbon and associated biogenic elements in the ocean, and predict their response to anthropogenic perturbation. The North Atlantic Bloom Experiment, part of JGOFS, typified the cooperative spirit of marine scientists, combining U.S., British, German, Canadian, and Dutch ships. A series of stations along 20°W were occupied during 1989, and much learned about the factors controlling plankton community structure, the spring bloom, and drawdown of CO_2 (Koeve and Ducklow, 2001).

Patterns in the ocean are in a continual state of change, and so long-term records of relevant parameters are essential (Karl and Winn, 1991). Perhaps the jewels in the crown of oceanographic research during recent decades have been the JGOFS time-series stations, such as the Bermuda Atlantic Time Series and the Hawaiian Ocean Time series (HOT), that combine continuous records over many years and intensive sampling of numerous variables of interest. Various important and unexpected discoveries have ensued, such as the switch from the nitrogen-limited regime at HOT that prevailed before the 1980s to a phosphorus-limited regime in recent years (Karl, 1999).

FACTS MATTER

In order to elucidate pattern, there is an ongoing need to establish basic "facts," such as the size and direction of currents, and the range and diversity of plant and animal life. Unlike physics, a notebook full of observations, such as the *Challenger Reports*, is now a good starting point as a source of knowledge. Pattern and process can only be established in context of this information. It has, however, been suggested that the end is in sight for science, in the sense that all the major discoveries may soon have been made (e.g., Glass, 1971; Horgan, 1998). Is fact-gathering nearing completion? Not in oceanography. Much of the ocean, in particular the murky deep waters, remains largely unexplored. All manner of weird and wonderful animals inhabit this zone, such as the various predatory fish whose names alone are testament to their spectacular nature, including nibblers (e.g., hatchetfish and lanternfish), stalkers (e.g., the dragonfish), ambushers (e.g., the anglerfish) and hunters (e.g., the fangtooth fish) (Robison, 1978). Countless other deep-sea and benthic animals as yet await our discovery. The decline in funding for taxonomy (Godfray, 2002) is particularly worrying in this respect.

Ever-improving technology continues to open our eyes to new worlds of discovery. New types of measurements are always important. For example, in recent decades various developments have led to revelations of hitherto unimaginable microbial life in the ocean. In 1988, Sally Chisholm discovered the cyanobacterium *Prochlorococcus* using flow cytometry (Chisholm et al., 1988), a method in which cells pass through a laser beam, and analysis of the resulting light scattering allows identification of different plankton groups in terms of size and pigment content. *Prochlorococcus* has abundances of up to 20,000 cells per drop of seawater, leading Richard Barber of the Duke University Marine Laboratory to ponder that, "It's hard to believe we'd overlooked something so important for so long" (quoted from Nadir, 2003, p. 27). Molecular techniques, such as cloning and analyzing RNA sequences, have similarly uncovered new horizons. A good example is the discovery that *Archaea* (a group of microbes characterized by unusual genetic and molecular structures distinct from other forms of life) are widespread throughout the ocean, rather than being restricted to anaerobic sediments, hydrothermal vents, and highly saline, landlocked seas (DeLong, 1992).

CONCLUDING REMARKS

Observation and hypothesis play complementary roles in the progress of oceanography today. The former leads to the discovery of pattern, providing the context for focussed, hypothesis-driven, process studies. There is no single methodology in science. What, then, of the philosophers' criticisms of the inductive method? First, just because induction cannot be fully justified in logic does not mean that it is not of great service to scientists, who realize that knowledge is always subject to possible revision in the light of new evidence. Second, regarding the theory-ladenness of observation, it is true that we cannot simply "browse over the field of nature like cows at pasture" (Medawar, 1969, p. 29). Oceanography is not about randomly observing anything and everything in an ad hoc manner, but instead requires a systematic and considered approach. It is "focused exploration" (Roger Larson, University of Rhode Island, Narragansett, Rhode Island, personal communication, 2004). But by "focused," this does not necessarily mean hypothesis-driven. As the philosopher Ian Hacking has pointed out, there is a difference between having some idea (i.e., using one's intuition) about the general properties of a system thus indicating the types of observations that are likely to be useful, and making observations in response to a precise conjecture about the phenomena under scrutiny (Hacking, 1983). Third, Whewell's assertion that creativity, in essence intuition or hypothesis, is required in order to elucidate pattern is valid, but his advanced method of induction nevertheless begins with observation. A similar view of scientific method was held by Einstein, who emphasized that "All knowledge of reality starts from experience and ends in it" (Einstein, 1935, p.133), and also "New theories are first of all necessary when we encounter new facts which cannot be 'explained' by existing theories" (Einstein, quoted in Musser, 2004, p. 88). Closer to home, the great physical oceanographer Henry Stommel wrote: "The chief source of ideas in oceanography comes, I think, from new observations. … Most theories are about observations that have already been made" (Stommel, 1989, p.49). Establishing cause is the ultimate scientific prize, but it is often new and exciting observations that lead us to conjecture hypotheses for this purpose.

I agree with Peter Medawar that having ideas (i.e., asking the right questions) is the scientist's greatest accomplishment, whereas, although important and exacting, the working out of these ideas is but a lesser occupation (Medawar, 1967). However there is still so much that we do not understand about the oceans that it is often difficult to know what are the proper questions to ask (McNutt, 2002). Crucially, it is observations, along with the study of pattern, that are the fountain of many ideas and discoveries in oceanography. This view is counter to much of the contemporary literature on the philosophy of science which, in my opinion, puts too much emphasis on hypothesis-testing as the quintessential scientific activity. "So what?" I hear you say—most scientists pay little or no attention to the arguments of armchair philosophers. Ocean science is seriously expensive. The inductive approach, unlike hypothesis-driven science, often cannot be neatly packaged into clearly achievable goals with specific targets to be delivered within a fixed time frame. It therefore behoves scientists to be able to elaborate their arguments as to why such science is not just worthwhile, but essential. I have presented the case here that large-scale observational programs, with emphasis on diversity of measurements and expansive coverage in both space and time, are necessary in order to understand and predict the complex dynamics of ocean systems. Targeted, hypothesis-driven studies are of course also important. Thankfully, oceanography as a discipline is flourishing. Large international, multidisciplinary programs are very much to the fore. The vision of a network of ocean observatories moves steadily closer to becoming reality. Ongoing improvements in technology are enhancing the range of measurements that are carried out. And the time-series stations continue unabated. The effort is great, but entirely worthwhile in order to unlock the many secrets of the mighty ocean systems that influence and sustain life on our planet.

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