INDUCTION, DEDUCTION, AND THE SCIENTIFIC METHOD

AN ECLECTIC OVERVIEW OF THE PRACTICE OF SCIENCE

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ABSTRACT: Science is a never-ending, always changing process through which we learn to know the material nature of the universe. Science does not deal with nonmaterial entities such as gods, for there is no way their existence can be either proved or disproved. No single, identifiable method applies to all branches of science; the only method, in fact, is whatever the scientist can use to find the solution to a problem. This includes induction, a form of logic that identifies similarities within a group of particulars, and deduction, a form of logic that identifies a particular by its resemblance to a set of accepted facts. Both forms of logic are aids to but not the solution of the scientist's problem.

Being a good scientist requires patience, perseverance, imagination, curiosity, and skepticism; the essence of science is to doubt without adequate proof. Science also requires knowing how to make and interpret observations (which presupposes a broad point of view), how to ask the right questions, how to theorize without getting lost in the details, and knowing when to do experiments and apply statistical tests. Recognition of one's work is desirable but should not be the primary goal, and publishing papers should be used primarily as a test of the scientist's ability to pursue good science.

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INTRODUCTION

In an essay entitled *Is the Scientific Paper a Fraud?* [1], Peter Medewar claimed that induction, in contrast to deduction, had no place in science. His implication of fraud was aimed, not at the paper's contents, but at how they were presented, and here he strongly implied that this presentation was an inductive process. Medewar was a great admirer of Karl Popper, a philosopher of science. In *The Logic of Scientific Discovery* [2], Popper rejected induction as a legitimate form of logic in the practice of science. To bolster his argument against induction in science, Medewar cited an unsuccessful attempt by John Stuart Mill to solve problems in sociology by induction, but neglected to mention Francis Bacon's contribution to the birth of modern science in the 17th century by the use of induction as a powerful alternative to Aristotelian and scholastic dogma.

Popper and Medewar argued vehemently for a method of scientific practice based on the so-called hypothetico-deductive system, the essence of which is the formulation of a hypothesis derived from a collection of facts, testing the hypothesis by trying to 'falsify' it, collecting more facts if 'falsification' fails, and repeating the falsification tests until either you and the hypothesis agree on a draw or one of you admits defeat. Medewar (1915–1987) shared the 1960 Nobel Prize in Medicine or Physiology with Sir Frank MacFarlane for their work on the mechanism of tolerance to acquired immunity. Karl Popper (1902–1994) was knighted by Queen Elizabeth II in 1965 and elected a Fellow of the Royal Society in 1976, so there's no question here about the kinds of minds we're dealing with.

It is perhaps not too hard to understand that a philosopher, even of science, could make judgments about any aspect and especially the methods of science, but what confuses me and I'm sure would confuse any graduate student or postdoc or, in fact, anyone with an inquiring mind is why someone like Medewar, a practicing scientist, and certainly no dummy, should get worked up enough about induction to write an essay excommunicating it from the scientific community. Is it really that wicked? Or useless? Should I, as a graduate student, watch my step to make sure I don't ever use induction in my research? Can I still become a scientist if I do? Should I be careful to use only deductive reasoning and not lift a finger to make my next advance into knowledge without first having formulated a hypothesis? What if I just want to ask a question?

Medewar's essay and Popper's philosophy of science are a good example of an idiosyncratic viewpoint about what science is and how it should be practiced. It is not my own point of view. The purpose of this essay is to make three main points that emerge from this difference. The first is that induction is an integral part of the practice of science and Propper and Medewar, therefore, in spite of their membership in the class of intellectual giants, are not only talking nonsense about induction having no place in science, but are committing a logical heresy by doing so. The next is that scientific methods such as hypothetico-deductive [1], Koch's postulates [3], or any other system based on rules of procedure or analysis, while they may be legitimate ways to practice science, are far from the only ways to do so. The final point is that certain features of the practice of science, theorizing, for example, are essential parts of *all* branches of science and far more important than searching for a non-existent only true "scientific method." Most of what I have to say should be seen only as a perspective of my own ideas about how we practice science, arising from my familiarity with the practical and theoretical methods of science, or having read or heard about or observed being used by other scientists. Most (if not all) of this has been said before but that doesn't matter. Each viewpoint, like each human being, is different and the differences can sometimes be more interesting than the similarities. A close friend and colleague, for example, disagrees with my definition of science! That's the point. There is no consensus, even among scientists, about exactly what science is and every viewpoint, therefore, can be at least potentially, valuable. It hardly needs saying that the views expressed in this essay are not necessarily those of the SSR or its Web site.

Before we get into the nitty-gritty of this essay, however, a small light touch may help to set the stage, especially since it serves very well as a pleasant example of what science can be all about. In a charming essay entitled *Can an Ape Tell a Joke?* [4], Vickie Hearn describes a problem-solving study in which a chimpanzee and an orangutan, housed separately, were each given a small hexagonal block of wood and an assortment of differently shaped openings into only one of which the block would fit. They knew they would be rewarded for making the right choice.

The chimp examined every detail of the floor, walls, and ceiling; the openings and every side of the hexagonal block; smelled it, tasted it, and, after trying one opening after another, found one the block would fall into. The orangutan scratched his back with the block, and then sat with a far away look in his eyes for what seemed to the human observer like forever. He then put the block directly into the hexagonal opening.

Was the chimp an inductivist? Did the orangutan consider the problem, form a hypothesis, then test it? Which one was the scientist? Let's reserve the answer for the section below called THE SCIENTIFIC METHOD.

INDUCTION AND DEDUCTION

A commonly held idea of the distinction between these logical paths to knowledge is that induction is the formation of a generalization derived from examination of a set of particulars, while deduction is the identification of an unknown particular, drawn from its resemblance to a set of known facts. For example, if we examine enough feral cats we can generalize that feral cats are a rich sources of fleas (induction). If, like Robinson Crusoe, we come across footprints on the beach of a desert island, we can conclude from our knowledge of the human footprint that another human is or was on the island (deduction).

In fact, however, both terms can have more subtle meanings. Let's start with a look at their etymology and definitions.

Etymology

The etymology of these words does not seem to have any of the judgmental qualities attributed to them by Popper and Medewar. They come from the Latin verb *ducere*, to draw on or along, to pull or drag, to draw to oneself, to lead, and with the Latin propensity for prefixes. suffixes, and the modification of the verb itself, *ducere* has spawned an enormous population of derivatives [5]. Even with only the prefixes *in* and *de*, meaning 'in' and 'from,' respectively, both words may have many more than one meaning. Simply put, *to induce* could mean 'to lead or draw into, to infer, to persuade,' and *induction*, 'to lead to the conclusion that etc....' *To deduce* could mean 'to lead from, to draw from' and *deduction*, 'to draw a conclusion from etc....' The official lexicographic and practical definitions are not always much more distinctive.

Definitions

Induction. From *The Oxford English Dictionary* (*OED*); *to induce* (in relation to science and logic) means "to derive by reasoning, to lead to something as a conclusion, or inference, to suggest or imply," and *induction* "as the process of inferring a general law or principle from observation of particular instances." Another version is the "adducing (pulling together) of a number of separate facts, particulars, etc. especially for the purpose of proving a general statement."

My 1967 edition of the *Encyclopedia Britannica* (*E. Brit.*) gives two versions by John Stuart Mill: "the operation of discovering and proving general propositions" or "that operation of the mind by which we infer that what we know to be true in a particular case or cases will be true in all cases that resemble the former in certain assignable respects."

A paraphrase of Francis Bacon's view (also from the *E. Brit.*) is "a selective process of elimination among a number of alternative possibilities."

The *E. Brit.* in a separate entry defines *primary induction* as "the deliberate attempt to find more laws about the behavior of the thing that we can observe and so to draw the boundaries of natural possibility more narrowly" (that is, to look for a generalization about what we can observe), and *secondary induction* as "the attempt to incorporate the results of primary induction in an explanatory theory covering a large field of enquiry" (that is, to try to fit the generalization made by primary induction into a more comprehensive theory).

E. Mayr in his *Growth of Biologic Thought* [6] offers this definition: "inductivism claims that (we) can arrive at objective unbiased conclusions only by… recording, measuring, and describing what we encounter without any root hypothesis…."

Deduction. Sherlock Holmes' "Elementary, my dear Watson!" has made deduction common knowledge a more familiar feature than induction in problem solving. The OED definition of *to deduce* is "to show or hold a thing to be derived from etc..." or "to draw as a conclusion from something known or assumed, to infer"; *deduction* thus is "inference by reasoning from generals to particulars," or "the process of deducing from something known or assumed..."

Both terms define systems of logic the purpose of which is to solve problems, in the one case by looking for a general characteristic (generalization, conclusion, conjecture, supposition, inference, etc.) in a set or group of observations, in the other to identify a particular instance through its resemblance to a set or group of known instances or observations. Popper's ridicule of induction was based on the premise that induction requires the observation of *every* instance of a given phenomenon for the generalization to be true—an obvious impossibility; the fact that all known crows are black, for example, doesn't prove that no white crows exist. Of course it is ridiculous when looked at in this way, but what *really* matters is that most if not all crows *are* black, and even if a white one should show up and prove to be a crow and not another kind of bird, most crows would still be black. His argument can also be used to make deduction useless for it, too, is based on an incomplete set of known facts. Even if the identified fact resembles the members of the set, how can we be sure that *every* possible feature of either the unknown or the members of the set itself has been considered? As we will see in what follows, in many of the examples of the way science is practiced, induction is as much a part of this practice as is deduction or any system of logic that serves the purpose of advancing knowledge. Induction and deduction are two, usually different but never contradictory, approaches to problem solving. The problem must be solved by testing the validity of the conclusion or inference, etc. reached from either direction. Induction and deduction are thus valuable, often complementary, tools that facilitate problem solving.

THE SCIENTIFIC METHOD

In spite of what I have said so far, is there a particular method we can call *THE* scientific method? To answer this question it is essential that we first ask another question: what do we mean by science? The word comes from Latin *scire*, "to know," and *scire* comes from an earlier Latin root meaning "to cut through," i.e. to take apart, to analyze. But science is more than just knowing by analysis. Science is a process of learning to know the nature of everything in the material world, from atoms to the most complex of living organisms and inanimate objects. Nonmaterial things, like gods, whose existence can be neither confirmed nor disproved, are excluded, for science deals only with those elements of the universe that can be shown, at least potentially, to exist. Science, therefore, is never-ending and always changing. Although its goal is knowledge, it is more than and different from knowledge itself, for knowledge is its product not its essence. Its essence is to doubt without adequate proof. Science is the offspring of philosophy, and differs from it mainly in the methods used in learning to know.

As with almost all systems of classification, we can't draw a sharp distinction between science, as defined here, and other forms of scholarship as sources of knowledge such as the *OED*, *Grove's Dictionary of Music and Musicians*, the *Dickson Baseball Dictionary*, etc. or even history, for example. In many respects, history is a science but it is poorly endowed with or even lacks the ability to predict, one of the important things that separates science from other forms of learning.

In all respects science is logically incompatible with the belief in a nonmaterial intelligent entity that controls the universe and is called God [7, 8], yet many scientists, especially among the chemists and physicists but even among some biologists have such a religious belief. I can think of only three resolutions of this paradox. The scientist's God either is not an intelligent entity or has no control over the universe. The second is to accept the concept of science as defined here with a part of one's mind and that of God with another, with an impermeable barrier between the two parts. The third is either not to be a scientist or not to believe in God, i.e. to be an atheist, or euphemistically, a nonbeliever since among many people 'atheist' is a dirty word. The funny thing about these solutions is that they all work! The troublemakers are the zealots, i.e. the proponents of Intelligent Design on the one hand, and the Russian communists' idiotic attempt to prohibit religion on the other.

A firm distinction between the so-called *hard* and *soft* sciences, e.g. physics and sociology, can also not be made simply because it is easier to test reality in some more than in other sciences. Science itself, therefore, answers our question with a simple but firm *No*. There cannot be one method that every kind of scientific study—in the field, the library, or the laboratory—must follow; the things scientists are curious about differ too much from one another for all of them to be studied according to the same or any set of rules or algorithms.

Medewar's caricature of the scientific paper boils down to a matter of beating a dead horse. He labels it inductivist because the authors often present their results without comment and reserve interpretation of them for the Discussion. In the first place, this isn't induction. Even Bacon, its chief proponent, saw induction mainly as a way to separate particulars from one another into groups of similarities. This is exactly what the taxonomist does. But even if it were induction (the meaning of which seems to depend on who defines it), what's unscientific about saying, "Here's what we found. How would you interpret them? Then we'll tell you what we think"? Shouldn't a scientific paper be at least as much fun to read as a good detective story? There are plenty of things wrong with the way many scientific papers are written (freight-train adjectives, misplaced clauses, redundancies, mistakes in grammar and/or syntax, teleologisms, etc.), but presenting the results without comment is not one of them. I have a hunch that Medewar was not a lover of who-done-its!

The only true scientific method is to use whatever tools we can to make observations, ask and answer questions, solve problems, test a theory, etc., and it doesn't matter whether we use induction, deduction, or any other kind of reasoning to do so; it would be a heresy to deny the validity of *any* method that helps us learn to know. Induction, in fact, resembles what prose was to Molierre's bourgeois gentleman [9]. We use some form of induction in almost every kind of scientific endeavor: no matter how it is defined, induction amounts to making and collating observations. This was Francis Bacon's great contribution to science, i.e. induction as a path to knowledge through *direct* observation of nature [10].

Let's come back now to the chimp and the orangutan. Were both scientists? Yes. Was the orangutan more so than the chimp? No. He was only different. Who can say which was better? Are Mayr's contributions to evolution through ornithology [11] less valuable than Dobzhansky's through genetics [12]? Was Vesalius less a scientist than Mendel because he described human anatomy while Mendel did experiments? Both made observations, one by dissection and the other by making hybrids. Both increased our knowledge of the natural world. Yes, some are better than others; it's how the game is played. We can't all play the violin like Heifetz, and the likes of a Copernicus, a Newton, a Darwin, and an Einstein don't make the headlines every day. But we can all be scientists.

BEING A SCIENTIST

Just as it takes more than talent alone to become a professional musician, it takes patience, perseverance, curiosity, imagination, and skepticism to become a good scientist. These qualities come more as gifts that can be refined by practice rather than ones that can be learned. Certain features of the practice of science, however, can and should be learned for they apply to every kind of science with equal force. We'll look at them one by one, even though in the real world they are inseparable and can be fully grasped only as parts of a whole system.

Making Observations

Observations are the meat and potatoes of science. We start a research project with observations made either in the field, the library, or the laboratory. How these observations are collected, classified, interpreted, and used as the basis of theorizing (from a hunch to a eureka) is, more or less, what science is about. Regardless of our objective, having an open, unbiased mind with no preconceptions about what we are looking for is a distinct advantage as we start our search. Medewar derided this mindset as part of his indictment of induction, but his prejudice made him miss its significance. The trick is to have this mindset *simultaneously* with one oriented toward the specific goal of our investigation, so that, while searching for information to satisfy our goal, we are also on the alert for the unexpected, our antennae always ready for the odd-ball fact that may even change the course of our investigation. But even if we start making observations with no specific goal except to learn more about a given subject, a glimmer of meaning in the form of a generalization, sometimes even a eureka, will emerge from the mass of facts and we now look back on all of them from a different point of view.

For example, Steno, a 17th century anatomist [10], while dissecting the head of a Great White shark, was struck by the resemblance of the shark's front teeth to a common Maltese fossil that the local people called 'tongues of stone.' Steno eventually identified them as fossils of shark teeth [13]. This observation, in turn, led later to the recognition of similar evidence hidden in sedimentary rocks [13], and this eventually, through a long series of related discoveries, to Darwin's *Origin of Species* [14].

Another example, which also brings out the importance of our point of view and of asking the right question (see below), comes from the science of paleontology. Like the other sciences that rely heavily on description (e.g. archeology, sociology, linguistics, even astronomy), the principal source of paleontologic information comes from the collection of fossils. This evidence, however, is full of gaps in the record of how species, genera, families, and so on evolved. Eldredge and Gould, by seeing these gaps as positive evidence of how things changed rather than as ignorance about how they changed, formulated the theory of punctuated equilibrium [15]. Its essence is that a significant element in the evolution of life forms consists of long periods of stasis in form and function punctuated by relatively short but very active periods of change, too rapid for fossils to be deposited. Not all biologists agree with this theory, but many do. It is typical of the way gathering information by observations alone (induction) can help the way we learn to know things.

Point of View

Human history is loaded with instances of how a particular point of view influences or, in fact, determined it. In science, the importance of our viewpoint when examining information of any kind cannot be too strongly emphasized, for *how* we look at a thing determines *what* we see.

The most forceful example, for it so clearly violated common sense, was Copernicus' theory of heliocentrism. He did not see anything that had escaped the notice of millions of people for thousands of years; he simply saw what they'd been looking at from a different point of view, and by doing so explained the movements of the planets, the daily rotation of the earth, and the seasonal changes in the patterns of the constellations (zodiac).

Before William Harvey, the absence of a visible connection between arteries and veins prevented people from even imagining that the blood could circulate and, therefore, also made them fail to see the heart as a pump. Harvey, however, by *observing* it in a living creature (a poor pig, tied down with its chest cut open) saw it as a muscular pump, and by measuring the volume of its ventricles and calculating the number of beats per unit of time, he concluded that the blood *must* circulate, even though he didn't know how it got from the arteries to the veins [16].

It had virtually always (in modern times, of course) been assumed that the vertebrate oocyte grew to maturity as a passive occupant of the ovarian follicle, but Andy Nalbandov[1] saw it otherwise. The follicular cells undergo a metamorphic process called luteinization[2] (i.e. formation of the corpus luteum[3]) normally almost only after the oocyte leaves the follicle at ovulation. Nalbandov saw this as evidence that the oocyte prevents luteinization. To test this viewpoint he and his co-workers removed the oocytes from preovulatory rabbit follicles[4] and found indeed that the follicle cells luteinized. This finding was the progenitor of a series of further studies that supported the idea of the oocyte as an active, indeed essential, factor in the physiology of its own and its follicle's development [17] and, among other things, led me to write a theoretical paper on the role of yolk in the evolution of mammalian viviparity [18]. The absence of yolk in the mammalian oocyte, in fact, changed my own viewpoint of how rep-

[1] *Nalbandov, Andrew V.* (1912-1986). Andy Nalbandov was one of L.E. Casida's grad students in the School of Agriculture during the late 1930s at the same time that I was one of R.K. Meyer's grad students in Zoology at the University of Wisconsin. Andy spent most of his career at the University of Illinois, Urbana/Champaign and was one of the founding members of the Society for the Study of Reproduction. We disagreed about almost everything in reproduction but remained friends.

[2] *Luteinization* is a term applied to a metamorphosis of the cells of the post-ovulatory ovarian follicle resulting in the formation of the corpus luteum, hence the name.

[3] *Corpus Luteum* (pl.: *corpora lutea*) is an organelle formed from the post-ovulatory ovarian follicle in most vertebrates. Its secretion of progesterone is essential for the establishment and, in some species, the maintenance of pregnancy in mammals.

[4] *Female rabbits* during the breeding season remain in constant oestrus (heat) and their ovaries always contain a crop of preovulatory follicles; these are ovulated only in response to coitus.

tiles may have evolved from amphibians, and mammals from reptiles [18].

The idea of geologic change with time and of the evolution of different forms of life existed at least two centuries before Darwin wrote *Origin of Species*, but it was the industrial revolution and Malthus' writings about the relation between population growth and food production that sparked a point of view leading Darwin to theorize that natural selection was the driving force of evolution.

Our point of view is by no means always valuable in our search for knowledge. Columbus, for example, was so fixed on the idea that he could reach the Far East by sailing west from Europe that when he did touch land his fixed viewpoint prevented him from seeing that the land and its people in no way resembled those Marco Polo had described. Even worse, he sailed around Cuba only enough to convince himself that it was the peninsula Marco Polo had described. As a result, he never knew that he had discovered the New World [19].

Root-Bernstein's *Discovering* [20] has many more such examples. Luck or serendipity is often given the credit for a discovery, even a eureka, but the fact is that the discoverer saw something that others could have seen but didn't. The discoverer possessed a mindset flexible enough to change when confronted with an oddity, and curious enough to ask the right question: "what's it doing there?" Luck helps, but we must be awake when it's offered. This means cultivating as broad and flexible a point of view as possible [21].

Asking the Right Question

With observations as the meat and potatoes of science, it's obvious that our appetite and hunger for them come from our curiosity, our never-ending desire to know. This is why we begin most (if not all) research projects with a question and, therefore, why it is so important that we ask the right one. Troy Duster, I think, expressed this better than I can in an article in *Science* [22]: "The procedures for answering any inquiry into the empirical world determine the scientific legitimacy of claims to validity and reliable knowledge, but the prior question will always be: Why that particular question? The first principle of knowledge construction is, therefore, which question gets asked in the research enterprise?" He then discusses the dangers of asking the wrong question, using as examples quotations concerning relationships between race and susceptibility to diseases, responsiveness to therapeutic drugs, criminal behavior, and warns against the conveniences of DNA databases as primary or sole sources of answers to such questions.

An excellent example of the difference between asking the wrong question and asking the right one is "Which came first, the chicken or the egg?" Because the question arises from a point of view limited to the chicken and its egg there is no rational way of answering it, but from the viewpoint of evolution, however, there is, i.e. "At what stage in the evolution of the vertebrates did the precursor of the cleidoic[5] egg appear?" We have shifted our focus from the finished product to its origin, and now have a reasonable chance of finding an answer.

If the proponents of Intelligent Design could also shift their focus to *origins* before asking how complex designs in nature occur, our kids could learn how we acquire knowledge without interference from creationists [8].

Fleming didn't look at his moldy cultures and ask, "How can I get rid of these pesky molds?" Instead, he asked, "Why are there no bacteria near the molds?" and penicillin was conceived. Copernicus saw more than just the sun and earth. Before him, the movements of the planets and the seasonal changes in fixed stars were explained only by a crazy-quilt patchwork of guesses. But, by shifting his point of view from the earth to the sun and asking, "Which of us is moving?" he showed us our solar system. Harvey was equally ingenious; by looking at a beating heart as a muscle, and asking "Is it behaving like my biceps when I flex and then extend my arm?" he gave us our circulatory system. If Columbus had asked, "How can I be *sure* Cuba is a peninsula?" and had tried to sail all the way around it, he would have known he was nowhere near the Far East.

Theorizing

Making observations of any given subject will inevitably generate a question, a hunch, even a theory about a secret hidden somewhere in the mass of findings. Darwin, as the naturalist aboard the *Beagle*[6], in the course of collecting samples of fossils, and of plant and animal life at each port of call, began to wonder about the meaning of some of them—such as evidence of marine animals in sediments on or near mountaintops. The simplest response to such curiosity is to accumulate more observations, perhaps more specialized ones or more oriented toward comparisons. Darwin continued making observations at home, in-

[5] *Cleidoic egg:* contains all the nutrients, liquid, mRNAs, and proteins necessary for a zygote to complete embryogenesis enclosed in a protective shell outside the body of its mother. All bird and most reptile eggs are cleidoic.

[6] The *Beagle* was a ship of the Royal Navy assigned to a circumnavigation exploration of the globe that lasted for five years.

cluding the breeding of pigeons, dogs, farm animals, etc. and eventually was led to theorize that the secret of biodiversity was evolution through natural selection—a good example, incidentally, of the use of induction in science.

The production of milk by mammary glands helped to distinguish mammals from other vertebrates, but further observations told us that pigeons also made "milk," although the 'milk' was made of different components and in their crops, not in mammary glands. Like mammalian milk, however, pigeon 'milk' was used to feed their young. Scientists later found that some species of fish and amphibia produce skin exudates on which their young feed. Since prolactin, a pituitary hormone, was already known to stimulate milk production in both mammals and pigeons, this knowledge led to speculation that prolactin may be involved in other forms of maternal behavior. As a result of research to follow up such ideas, we now know that prolactin is responsible not only for the skin exudates in fish and amphibia, it is a necessary, although not always sufficient luteotropin[7] in several mammalian species, it induces broodiness in birds, maintains the brooding of young in the skin pouch of the sea horse, and is a crucial agent in the control of kidney function in fish that migrate to spawn either from marine to fresh water or vice versa; these findings have led to theories about the evolution of prolactin as a hormone intimately connected with post-fertilization reproduction, especially the expression and regulation of maternal behavior in vertebrates [23].

R.K. Meyer[8], who at the University of Wisconsin taught me how to become a scientist, used to say half seriously, "I don't theorize; I just experiment and collect facts." However, every one of his facts was both the offspring and the parent of a theory. It doesn't matter whether it comes as a question, a hunch, a hypothesis, or a theory as important as or at least similar in kind to Copernicus' heliocentricism; theorizing is what moves science forward. Theory is more important than the facts, Einstein told Heisenberg, because theory tells us what the facts mean [20]. Theory is the most powerful tool through which we acquire knowledge, but like any heavy-duty tool it must be handled with care and used with discretion.

Like making observations and asking the right question, theorizing can't be separated from our point of view. It is here that its value is most seriously threatened, mainly in three ways. A good theory tends to block our ability, even our urge, to find a better one. Of course, the better a theory is the greater is this danger. A theory can lead to a fixed point of view, several examples of which I've mentioned above. The failure of two labs to discover that the pituitary secretes prolactin autonomously is an especially pertinent example, which I will describe in detail below under *Experimentation* since it is also applicable there. Becoming emotionally attached to one's theory as though to a beloved possession is extremely dangerous, for it can even lead to the temptation to fudge the data. Faith in the validity of a good theory, however, is another story (see below).

The theory (or finding) that questions authority. Proponents of such a theory (which Kuhn would call the basis of scientific revolution [24]) should have strong stomachs, stout hearts, and temperaments aggressive enough to enjoy a good fight. Eldredge and Gould and their theory of punctuated equilibrium [15] are a good example. The scientific community tends to be hostile to attacks on authority; whether the theory persists depends on the effectiveness of the theory's few supporters in keeping it alive. There is both good and bad news behind the hostile reception. The good news is that the resistance of scientists to the questioning of authority (a peculiar aspect of our skepticism) protects us from the Lysenkos.[9] The bad news is that the benefits of a superior theory may be delayed for a very long time, to say nothing of the effect of such hostility on its proponents.

Copernicus was lucky. He died without knowing that Tycho Brahe, a contemporary authority of astronomy, never accepted his theory or that Galileo suffered for defending it, or that it was almost one hundred years before his theory found general acceptance. Marcelino Sautuola, a Spanish archeologist, died in disgrace; he was accused in 1875 of forging the cave paintings of animals and hunts by primitive humans when the prevailing dogma was that the cave man was incapable of intellectual creativity [25]. Even Darwin's theory wasn't accepted by most biologists until the

[7] A *luteotropin* is any substance that directly or otherwise increases the secretion of progesterone by the CL or placenta.

[8] *Roland Kenneth Meyer* earned a Ph.D. in Zoology under Frederick Hisaw at the University of Wisconsin and returned to its Zoology Department in the fall of 1935 as its specialist in endocrinology and reproduction when Hisaw moved to Harvard. He inherited two grad students left over from Hisaw, but I was his first full-time grad student. He was a tough but dearly beloved teacher and researcher. The University of Wisconsin's Department of Zoology has recently initiated a lecture series in his honor.

[9] *Trofim Lysenko* was a Russian agronomist who persuaded Stalin in the 1930s that only the environment, not genes, determined the quality and productivity of agricultural products. He blocked the development of genetics in the Soviet Union for more than twenty years.

synthesis of genetics and evolution in the mid 1930s [6, 12], and some scientists even now do not believe in evolution.

In the mid 1920s, Cecelia Payne-Gaposchkin, as a graduate student in astronomy at Harvard, found that her spectroscopic observations of the sun meant that hydrogen, not iron, was the most common element in the universe. She described this finding in her Ph.D. thesis, but it was criticized by Henry Norris Russell, the world's authority on stellar spectroscopy as "clearly impossible." Several years later Russell published his own confirmation of her data but without acknowledging her priority. As a graduate student she did not feel up to defending herself against an authority like Russell and so added a remark that her data were "probably not real" to her Ph.D. thesis. She went on, however, to a successful career in astronomy. She was the first woman to become a full professor at Harvard, and even received the Henry Norris Russell award for her achievements a few years before she died in 1979 [26].

Defending the controversial theory or finding. The keystone of Popper's logic of scientific discovery [2] is 'falsification,' that is, testing the validity of a theory (or finding) by how well it withstands attempts to "falsify' (i.e. disprove) it. The term itself is distasteful (at least to me) since it implies *fake* or *counterfeit*, but that's beside the point. In the first place, there is nothing new about his premise. As I said in defining science, its essence is to doubt without adequate proof, and the scientist's gift of skepticism sees to that. But in the second and even more important place, disproving a theory is not the only test of its validity. Any theory or finding, especially if controversial, always rests on a fine knife edge, balanced by the weight of disproving evidence on one side and corroborating evidence on the other. To claim that only one of these is the *only* true test of validity is, to put it very simply, not true [27].

It is a truism that nothing is sadder than the murder of a beautiful theory by a nasty little fact. This old chestnut, however, leaves out the question: How do we know the fact *is* a fact? Copernicus could not explain the movements of the planets as well as did Ptolemy but his theory explained most of the available facts much better than did Ptolemy's, and he did not abandon it. He had assumed that the planets' orbits were perfect circles. Kepler later found them to be elliptical, and Copernicus' faith in his theory was justified. Harvey's faith in his theory was similarly justified when capillaries were discovered.

Richard Feynman's and Murray Gelman's theory of beta decay went against accepted concepts of the interactions between the atomic nucleus and the electrons. One paper in particular, by a physicist Feynman respected highly, disproved their theory; but Feynman waited patiently, and the so-called disproving data turned out to be erroneous [28].

A somewhat related instance concerns the case of the oddball fact. Ernest Hooten, the professor of physical anthropology at Harvard, severely and in public criticized Sherwood Washburn, his former graduate student and now a well-established evolutionary anthropologist, for failing even to mention Piltdown man (*Eoanthropus dawsonii*) in a 1950 paper summarizing his ideas about human evolution. Washburn's point was that there was something too queer about Piltdown for him to fit with any of the other known human fossils. Washburn felt intuitively that Piltdown didn't belong [29]. It takes guts to take such a scientific gamble. To Hooten, a fact is a fact and we have no right to decide, without a justifiable reason, to exclude one from our data. However, in the end, Washburn was proved to be correct. Four years later, Piltdown was shown to be a hoax.

The point of these and many similar examples is that if a theory explains most of what is known better than any other, a discrepant fact will not kill it unless it *is* indeed a fact; even then it may turn out not to be discrepant. The key to high quality theorizing is "Don't get lost in the details" [20]. A good theory, at whatever level of revolutionary content, will rarely if ever explain everything. Even Darwin knew that natural selection did not explain all of evolution. The principle of theorizing is not necessarily to explain *every* piece of information in a particular field of knowledge. It is intended to explain what is known about the subject better and more comprehensively than any other has done so far. Its validity as knowledge depends on its future history.

Eurekas. When Archimedes leaped from his bath shouting, "I found it!" in Greek, he couldn't know that scientists would love to use his shout to express their delight in discovering one of nature's secrets. I think of eurekas only as very exciting, big discoveries, but there is no harm in thinking of them quantitatively as well, e.g., from barely audible whispers to shouts loud enough to wake the dead.

The high decibel eureka occupies a special place in theorizing. No matter what form it comes in, it functions eventually as the basis of a very good theory pointing to what seem like unlimited possibilities for exploration. No one knows how such eurekas occur and it wouldn't make much difference if we did, for they can't be learned, and are as rare and exciting as being dealt a straight flush in poker. In the almost seventy years since as a graduate student I took my first step into the world of science, I have had only two genuine eurekas and consider myself lucky to have had that many! But just as poker is fun even when we lose, practicing science is also and for much the same reasons. Another of R.K. Meyer's favorite sayings was that it's those rare little triumphs of discovery that make science worthwhile, because 99% of being a researcher is drudgery.

Experimentation

It's a common misconception, even among some scientists, that science means doing nothing but experiments. How can a paleontologist or an archeologist do an experiment? Experimentation, of course, is a very useful tool especially for chemists, physicists, and biologists, but it is not the only tool that even these scientists use. Experimentation is a way of making observations under controlled conditions, so the value of such observations is no greater than that of observations made in the field (including the astronomer's) or in the library, provided that the conditions under which the observations were made can unquestionably be identified and compared with one another.

Let's take as an example the prolactin experiments I used to emphasize the importance of point of view. By the late 1930s, biologists knew that a pituitary hormone stimulated the corpus luteum (CL) to secrete progesterone, but didn't know which hormone or how its secretion was regulated. Because mechanical stimulation of the cervix (or sterile coitus) of a rat in heat induced the secretion of this hormone (which in 1940 turned out to be prolactin), Alel Westman and Dora Jacobsohn, two Swedish endocrinologists, theorized that a neural reflex explained the effect of cervical stimulation. To test this, they cut the connection between the central nervous system (CNS) and the pituitary, but when they then stimulated the rat's cervix, they were surprised to find that the pituitary still secreted the as yet unidentified prolactin. They could not explain why.

Leon Desclin, a Belgian endocrinologist could. To him, their experiment meant that the pituitary secreted prolactin independently of the CNS, but in response to the estrogens secreted when the rat was in heat. To test his explanation, Desclin transplanted the rat's pituitary beneath the kidney capsule, together with a pellet of stilbestrol, a synthetic estrogen. Sure enough! The denervated pituitary, when exposed to enough estrogen, secreted prolactin.

Westman, Jacobsohn, and Desclin were not clumsy, bumbling amateurs or novices in science, yet they overlooked one of the most important and obvious requirements of good experimentation. When we test the hypothetical solution to a problem by changing the conditions of the problem itself, we must always be aware that *any* of the changes from normal, even a purely technical one, may account for our results rather than the particular change we used to test our hypothesis. In both experiments, the researchers' point of view was that the pituitary secreted prolactin *only* in response to an external stimulus. Westman and Jacobsohn, therefore, did not consider it necessary to see if denervation alone would induce the secretion of prolactin. Desclin fell into the same trap. It never occurred to any of them that the secretion of prolactin could be autonomous. It did occur to John Everett[10] and he proved it by repeating Desclin's experiment without the estrogen and got the same result [30].

A little later, Everett discovered that if he removed a rat's pituitary completely and transplanted it to the kidney, the rat's CL would maintain a high level of progesterone secretion for months [31]. In the normal, intact rat, the CL secretes progesterone for no more than four weeks. This discovery broadened our understanding of the physiology of the CL enormously. It also emphasizes my remarks on 'the failed experiment' (see next subheading).

In descriptive sciences like astronomy, paleontology, archeology, etc. where it is impossible to design experimental changes in the environment being studied, computer modeling is often used as a form of experimentation, and lab studies of age, size, and composition of collected specimens usually are thought of as experiments. In any case, the validity of any form of experimentation depends on whether we ask the right question, our point of view, the accuracy of our observations, and the appropriateness of our controls.

The failed experiment. We have a good question we want answered or a good hypothesis to test; our experiment was well designed (at least we think so) and gremlins did not infect our materials and methods, but the results did not answer our question or support our hypothesis. The failed experiment can be very depressing especially if we are a grad student or a postdoc—but even well-seasoned researchers are not immune to its effect. Nevertheless, the failed experiment should have the opposite effect.

For example, let's look again at the Westman and Jacobsohn experiment. Their theory of a neural reflex was correct, but their failure was due to an incorrect

[10] *John Everett* spent most of his scientific career in the Department of Anatomy at Duke University in Durham, N.C. He was a highly respected researcher; a pioneer in the role of progesterone in ovulation and in the control of the pituitary by the CNS.

point of view. The failure itself should have told them to look for a better one, but it was Everett who looked and found it. And how much more interesting his discovery turned out to be than what Westman and Jacobsohn hoped to find! *That* is why the failed experiment should cause elation not depression. Being a researcher is, after all, playing a never-ending game of solving fascinating puzzles; the failed experiment tells us that the excitement of the game is not over, and off we go again eager for another chance to win.

Perhaps it takes experience to react positively to the disappointment of the failed experiment, but it is worth waiting for it to develop.

Publishing

Aside from how publishing papers helps establish us in our careers by increasing our chances of being funded, and aside, also, from the sheer fun of telling our families, colleagues, and friends about our discoveries, publishing the results of each of our studies is a critical test of its validity. There can be a great difference between our opinion and the opinions of friends, colleagues, and all those strangers out there about our study—and we know this! We know that when we sit down to write our paper we must convince the readers that our findings are genuine and our interpretation of them is correct. In the act of satisfying these obligations with the appropriate words we subject the way our study was done and *how* we arrived at our conclusions to the ideal test of its validity. Regardless of any other purpose in writing a paper, *this* is its indispensable one. Unfortunately, not every scientist, when writing a paper, seems to know this!

Statistics

Statistical tests have only one purpose in science: to relieve our anxiety about whether what we have discovered means anything. We can't logically decide whether something is eternally true; e.g. David Hume remarked that because the sun has risen every day so far there's no absolute certainty that it will rise tomorrow. All our judgments about the validity of a discovery and our ability to predict the result of a given procedure, therefore, are a matter of probabilities, and we have a variety of statistical tests we can use to calculate the probabilities. I am far from an expert in this department. During almost my entire career in experimental science I got by with calculating a standard error and using either the chi-square or Student *t*-test, *where necessary*. The last two words summarize my next statement: that probabilities are part of everything we deal with is not the same as saying that *all* items of information must be subjected to a statistical test of their probability. Statistical tests are too often treated as if science were a religion and a statistical test a required ritual in its practice. That's nonsense. Statistical tests should be used only as aids in resolving an uncertainty about whether a difference between one condition and another is important.

Recognition

The desire for recognition affects the greats as well as you and me. Newton and Leibnitz fought bitterly over who invented calculus; Darwin's reaction to the letter asking his opinion about Wallace's theory of natural selection is a classic instance of the ubiquity of the power of recognition.

The question, therefore, isn't whether recognition is an inseparable part of being a scientist; the question is how much of a part is it?

It's a matter of our priorities. Recognition brings a variety of tempting rewards. Turning down an invitation to this or that symposium or to address a plenary session at an international meeting isn't always easy, to say nothing of major league stuff like the Nobel Prize, etc. With all of the varieties of recognition, how do we handle the flood of requests from grad students and postdocs to work with us? At what point do we have to choose between being a good researcher and accepting so many of its rewards that there is no room left for being any kind of a researcher? Richard Feynman said he didn't care about being recognized, and he may have really meant it, even though he had practically all the available honors including the Nobel, but he never stopped being a researcher and never had more than a handful of protégées in his lab.

When we've done good work and received little or no recognition, it hurts. We can easily imagine how Mendel must have felt when Nageli, his contemporary and an authority on inheritance, completely ignored his findings. Nonetheless, Mendel didn't stop experiments on how sweet pea flower colors are inherited.

We can also imagine that Aristarchus of Samos (2nd century BCE), who had the same idea as Copernicus, didn't enjoy being thought of as crazy (except perhaps by a few of his friends) by all his contemporaries.

In the end, it comes down to this: We become scientists because we want to learn how to solve at least a few of nature's infinite puzzles; if we are recognized for our accomplishments, so much the better. But desire for this recognition is not what made us become scientists. If it is, we're in the wrong profession.

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