CHAPTER 7

THE DRIVE FOR OBJECTIVITY

How the iron rule enforces objectivity in scientific argument while allowing pervasive subjectivity in scientific reasoning (the iron rule's second and third innovations)

OVER THE WALLS of a moonlit Spanish cemetery, one night in the summer of 1868, climbed two figures with larceny on their minds. Where better to pick up a few surplus bones? Their owners had no further use for them, and the gravediggers had moved them aside to make room for new clients. The pair found what they were looking for "tumbled in confusion and half buried in the grass." They made their selection and then their getaway.

Had the grave robbers been caught in the act, the authorities in the small Aragonese town of Ayerbe would have been rather surprised to find that they had in their custody the town doctor, Justo Ramón Casasús, along with his 16-year-old son. Dr. Casasús was desperate. His son, failing at school and perpetually in trouble with the authorities, was set on living the bohemian life of an artist. The doctor's outlandish idea was to use the specimens from the cemetery both as artistic subjects and as objects of instruction: his son would, sketching the bones, learn the rudiments of osteology and develop a newfound enthusiasm for a medical career.

By the time the son, Santiago Ramón y Cajal, collected the Nobel

Prize in Medicine in 1906, it seemed safe to say that the father's strategy had been a considered success. Cajal received the prize for discovering that the brain is constituted of discrete cells—neurons—along which impulses travel from head to tail. This "neuron doctrine" was the beginning of modern neuroscience.

Like many Nobels, the 1906 award was shared. The other winner was the Italian Camillo Golgi, who had developed a staining technique that allowed the delicate structures of nerve tissue to be seen clearly under a microscope. Cajal had taken this technique, improved it, and then used his peerless powers of observation to discern the individual neurons and their interrelationships, producing in the course of his inquiries many striking illustrations of neuronal structure (Figure 7.1).

At the Nobel ceremony in Stockholm, Golgi was the first to give his acceptance speech. He rose to his feet and began to denounce Cajal. The

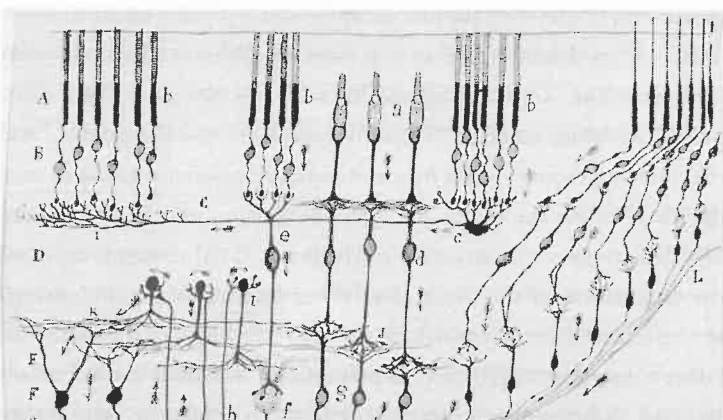

Figure 7.1. A drawing by Cajal of the cells making up the retina. At the top (layers A and B) are the light-sensitive cells; at the bottom (layer F) are the retina's output neurons. The signals travel in the direction of the arrows.

neuron doctrine, he argued, was wrong in every respect. Neurons were not in any sense isolated cells, but were joined to one another to form a great continuous network through which signals could flow in any direction. This was the "reticular theory" of the brain, an old idea that Cajal regarded as refuted by his meticulously made drawings—such as Figure 7.1, which shows a clear separation between neurons. But Golgi had his own drawings, which he displayed during his address, purporting to reveal the neural continuum that he described.

Cajal later repaid Golgi with withering scorn. He characterized the reticular theory as a lazy man's stratagem—"admirably convenient, since it did away with all need for the analytical effort involved in determining in each case the course through the gray matter followed by the nervous impulse." In his pursuit of the easy way out, Cajal further claimed, Golgi had presented images in support of the reticular theory that were "artificially distorted and falsified."

At bottom, Cajal accused Golgi of not looking properly at his own specimens, of allowing his preconceptions and psychic needs to interfere with his vision, of failing to present an objective account of what lay before him. Cajal considered himself, by contrast, to be a scrupulous scientific reporter. "Objectivity was at once the guiding and the unifying theme . . . for his career-spanning defense of the neuron doctrine," write the historians Lorraine Daston and Peter Galison. His depictions of the structure of the brain, Cajal thought, captured the true nature of the thing as surely as had his long-ago drawings of the stolen bones, of which he wrote, recording the success of his father's unorthodox program of persuasion, "Thirsting for the objective and the concrete, I seized eagerly the fragment of solid reality which [they] presented to me." That same sense of the objective and the concrete, he believed, had won him his Nobel Prize—and his contest with Golgi, as the neuron theory of the brain came to replace the reticular theory.

Cajal intended the barbs he fired at Golgi to sting, and they were able to do so because Golgi was no less committed than Cajal to objectivity in the presentation of scientific findings: he claimed that his own diagrams were "exactly prepared according to nature."

This clash of Nobels shows just how important objectivity is to scientists as a shared goal and hence as a communal standard against which a competitor's work can be measured and found deficient. It is not just Cajal and Golgi: everywhere you look, for a scientist to call a colleague's work objective is to bestow upon it high praise, while to deny its objectivity is to call into question its scientific worth.

But can this be anything more than talk if scientists' reasoning is essentially subjective? It can be far more than that. There is room for objectivity in science because it is limited to playing a rather peculiar and constrained—though crucial—role in regulating the procedural consensus imposed by the iron rule. The fashioning of that role constitutes what I have called modern science's second and third great innovations. To better understand scientific objectivity and its self-imposed limits, let's go back to a quintessential case of subjectivity, embarking one last time with Eddington on his expedition to test Einstein's theory of general relativity by observing the 1919 total eclipse of the sun.

THREE TELESCOPES, you will recall, were trained on the eclipse. The instrument on the African island of Príncipe barely saw the sun through the clouds. The two telescopes in Brazil gave conflicting testimony: one, the 4-inch telescope, saw light bent roughly as Einstein predicted, but the other—the Brazilian astrographic telescope—saw the bending predicted by Newtonian physics. Eddington discounted the results from the Brazilian astrographic on the grounds that its photographs were out of focus, while lavishing great care on his own equally obscure Príncipe photographs, teasing a pro-Einstein result out of the murk. He then convinced various British scientific luminaries to endorse his conclusion that the observation of the eclipse was a spectacular confirmation of Einstein's new theory. The astrographic anomaly was largely forgotten.

Eddington's behavior was not fraudulent, but his reasoning was partial and self-regarding. Enchanted by the elegance of Einstein's theory and intent on closing the rift between British and German science prised open by the Great War, he hungered for an Einsteinian triumph. So he was quick to conclude that the Brazilian astrographic had malfunctioned hopelessly even as he treated the badly compromised photos he had himself taken in Príncipe as imperfect yet highly informative. In his own eyes, he was reasoning his way toward truth, but

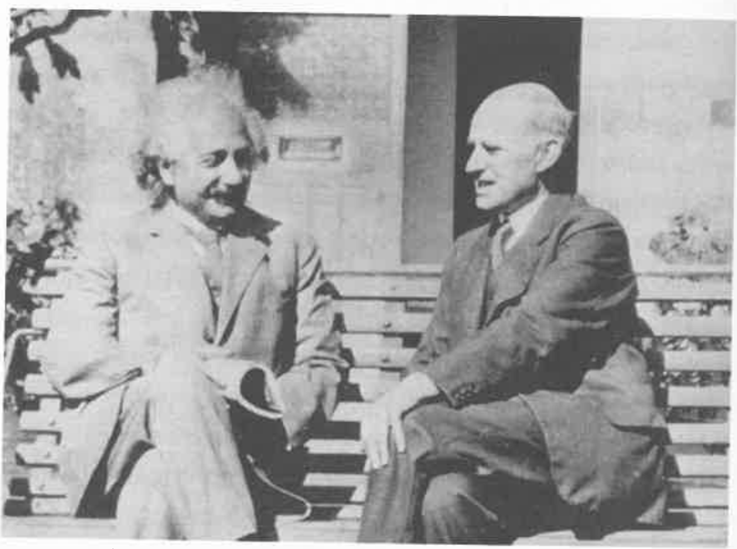

Figure 7.2. Albert Einstein and Arthur Eddington enjoy a quiet moment together at the University of Cambridge Observatory in 1930.

the plausibility rankings that went into his reasoning—in particular, his estimate of the probability of a systematic Brazilian astrographic breakdown—were twisted by his hopes and expectations for what that truth might be.

All this is, of course, just to repeat and to underscore the lesson learned from the collapse of the Great Method Debate: scientific reasoning is inflected always and everywhere with subjectivity. There is no place, apparently, for what the iron rule demands, an argument that is purely objective. Or is there?

Eddington and his collaborators reported the outcome of their expedition in a scientific paper with the informative if unwieldy title "A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations made at the Total Eclipse of May 29, 1919," which was published in a scientific journal, an official repository of research, called the *Philosophical Transactions of the Royal Society* (Figure 7.3).

Leaf through the pages of the eclipse paper, and you will find in the serried ranks of figures and careful calculations plenty that looks purely objective. In Figure 7.4, for example, the names and positions of the brighter stars surrounding the eclipsed sun are listed, along with Einstein's predictions for the apparent shift in the stars' positions. Even Eddington's fiercest critics will grant the accuracy and the impartiality of the information thereby presented. Those were the stars; those were their positions; and the Einsteinian predictions were calculated without error.

Or turn back to the second of the two tables that I extracted from the eclipse paper in Figure 2.3. There you have a faithful record of the 18 photographs taken by the Brazilian astrographic telescope, along with a calculation of the gravitational bending implied by each photo. The number at the bottom right-hand corner summarizes the results: the photographs seem to show that the "bending number" is 0.86, which is

IX. *A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations made at the Total Eclipse of May* 29, 1919.

By Sir F. W. DYSON, *F.R.S., Astronomer Royal, Prof.* A. S. EDDINGTON, *F.R.S., and Mr.* C. DAVIDSON.

(*Communicated by the Joint Permanent Eclipse Committee.*)

Received October 30,—Read November 6, 1919.

[PLATE 1.]

CONTENTS.

I. PURPOSE OF THE EXPEDITIONS.

1. THE purpose of the expeditions was to determine what effect, if any, is produced by a gravitational field on the path of a ray of light traversing it. Apart from possible surprises, there appeared to be three alternatives, which it was especially desired to discriminate between—

(1) The path is uninfluenced by gravitation.

(2) The energy or mass of light is subject to gravitation in the same way as ordinary matter. If the law of gravitation is strictly the Newtonian law, this leads to an apparent displacement of a star close to the sun's limb amounting to 0″·87 outwards.

(3) The course of a ray of light is in accordance with EINSTEIN's generalised relativity theory. This leads to an apparent displacement of a star at the limb amounting to 1″·75 outwards.

Figure 7.3. A scientific paper: the opening of Eddington's report on the eclipse. (Because the Astronomer Royal Frank Dyson was the nominal head of the project, his name appears first.)

almost exactly what Newtonian physics predicts. Again, no one disputes that the number is calculated fairly and correctly.

There are outbreaks of objectivity throughout the eclipse paper, then. What about subjectivity? Eddington and his team, as you know, chose to put aside the Brazilian astrographic data, which allowed them to draw their dramatic conclusion:

No.	Names.	Photog. Mag.	Co-ordinates. Unit = 50′.		Gravitational displacement.			
					Sobral.		Principe.	
			x.	y.	x.	y.	x.	y.
		m.			″	″	″	″
1	B.D., 21°, 641	7·0	+0·026	−0·200	−1·31	+0·20	−1·04	+0·09
2	Piazzi, IV, 82	5·8	+1·079	−0·328	+0·85	−0·09	+1·02	−0·16
3	κ^2 Tauri	5·5	+0·348	+0·360	−0·12	+0·87	−0·28	+0·81
4	κ^1 Tauri	4·5	+0·334	+0·472	−0·10	+0·73	−0·21	+0·70
5	Piazzi, IV, 61	6·0	−0·160	−1·107	−0·31	−0·43	−0·31	−0·38
6	υ Tauri	4·5	+0·587	+1·099	+0·04	+0·40	+0·01	+0·41
7	B.D., 20°, 741	7·0	−0·707	−0·864	−0·38	−0·20	−0·35	−0·17
8	B.D., 20°, 740	7·0	−0·727	−1·040	−0·33	−0·22	−0·29	−0·20
9	Piazzi, IV, 53	7·0	−0·483	−1·303	−0·26	−0·30	−0·26	−0·27
10	72 Tauri	5·5	+0·860	+1·321	+0·09	+0·32	+0·07	+0·34
11	66 Tauri	5·5	−1·261	−0.160	−0.32	+0·02	−0·30	+0.01
12	53 Tauri	5·5	−1·311	−0·918	−0·28	−0·10	−0·26	−0·09
13	B.D., 22°, 688	8·0	+0·089	+1·007	−0·17	+0·40	−0·14	+0·39

Figure 7.4. Objectivity epitomized: the names of stars surrounding the eclipsed sun, their positions (under "Co-ordinates"), and—under "Gravitational displacement"—Einstein's predictions for the apparent shift in their positions, when viewed both from the Brazilian location (Sobral) and from the African island of Príncipe.

> Thus the results of the expeditions to Sobral [Brazil] and Príncipe can leave little doubt that a deflection of light takes place in the neighborhood of the sun and that it is of the amount demanded by Einstein's generalized theory of relativity.

The subjectivity in the paper must manifest itself, then, in Eddington's argument that the Brazilian astrographic was functioning so badly that its measurements should in effect be ignored.

Comb the paper looking for that argument, however, and you'll come up empty-handed. It is nowhere to be found. Eddington does tell his readers that the astrographic images were "diffused and apparently out of focus." He then speculates on the cause ("the unequal expansion of the [telescope's external] mirror through the sun's heat"), and he is very clear that the images are to be given "much less weight"—indeed, by the end of the paper, apparently no weight whatsoever.

But there is something essential missing from this chain of reasoning. Even if Eddington is correct that the "diffusion" is caused by the distortion of the mirror, it is, as he acknowledges, "difficult to say whether this caused a real change of scale in the resulting photographs or merely blurred the images." A change of scale would result in a systematic error, possibly yielding a misleadingly low value for the bending angle, but mere blurring would not. To justify his disregard for the Brazilian photos, then, Eddington would have to share his reasons for believing that there was in fact a scale change. He does nothing of the sort.

The eclipse paper is, as a consequence, peculiarly deficient as a piece of rhetoric (although Eddington made up for that behind the scenes). At the same time and for the same reason, it maintains not only a semblance of but a genuine claim to objectivity. There is little in it that can be contested: not the measurements, not the calculations, not the observations about the clarity or blurriness of various sets of plates, not the speculations about the cause of the blurriness and its possible consequences (you can hardly argue with a mere speculation), and not the logic that says that data from a broken instrument ought to be given little or no weight. The objectivity is not quite complete—the bare claim that the astrographic telescope yielded no useful information can certainly be disputed—but it is close; certainly, the subjective considerations that we suppose pushed Eddington to draw that conclusion, running from the beauty of relativity to the ugliness of postwar politics, are entirely absent.

It is the iron rule that demanded objectivity of Eddington's paper, as it does of all scientific communications. Everything subjective in a scientific argument, the rule says, must be eliminated. Directly in the firing line are plausibility rankings, scientists' estimates of the likelihoods of various important assumptions. If a plausibility ranking does not reflect a broad scientific consensus or an obvious conclusion from incontrovertible premises, it needs to go. Eddington could not, then, simply say that he was confident that the Brazilian astrographic suffered a change of scale (as we may surmise he believed). He would be permitted to give his reasons for thinking there had been a change of scale, but only if they were based substantially on something other than personal opinion or guesswork. That was not the case here; he had no independent evidence that there was not simply a loss of focus, a "mere blurring." He was therefore forced to leave the argument hanging.

So it is in general: when a scientific paper is written, the grounds of many of the experimenter's crucial assumptions, being partially or wholly subjective, are cut away. What is left are only observation reports, statements of theories and other assumptions, and derivations that connect the two. Consequently, arguments appearing in official scientific venues—such as Eddington's published argument for ignoring the Brazilian astrographic—are characteristically incomplete, perfunctory, or oddly blunted. Strange though it may seem, that is what the iron rule's insistence on objectivity entails.

I call this process in which subjectivity is excised from scientific argument "sterilization" in honor of the great sterilizer Pasteur, who understood that the appearance of objectivity is as important as the real thing. My compact formulations of the iron rule typically do not make the demand for sterilization explicit. But it is there.

Those archenemies of the scientific method, the radical subjectivists, do not dispute the existence, indeed the preeminence, of a norm of objectivity in science. But they take it to be pure propaganda. The iron rule may stipulate that scientific argument should be sterilized, cleansed of all subjectivity; the reality of scientific reasoning is, however, the usual human story: bias, contextuality, the whole teeming ecosystem of the human mind with its desires and fears, its antagonisms and allegiances, its need to please and its will to believe.

When the neuroscientists Cajal and Golgi scrutinized the same nerve tissue, each observed just what his own theory of the brain predicted and lambasted the other for gross failures of objectivity. Such pontification seems to amount to little more than "You don't see what I want you to see." The philosopher Karl Popper claimed that evidence could be interpreted using an indisputable rule of logic; in fact, what gets "falsified" turns out to depend on plausibility rankings, subjective estimates of various crucial assumptions' likelihoods.

The iron rule, then, must be a cover-up, a ruse that dresses up as objective something—scientific deliberation—that is anything but. According to the subjectivist philosopher of science Paul Feyerabend:

> There is hardly any difference between the members of a "primitive" tribe who defend their laws because they are the laws of the gods . . . and a rationalist who appeals to "objective" standards, except that the former know what they are doing while the latter does not.

What, according to subjectivists like Feyerabend, is the function of the iron rule? To make scientists feel good. To make the nations that fund science feel good. To make Western civilization feel good at having overcome an atavistic mélange of emotion, partiality, and prejudice to attain an exalted kind of knowledge that is purified of its human origins.

In 1936, Stalin oversaw the introduction of a new constitution in the USSR, guaranteeing freedom of speech, freedom of the press, freedom of assembly, and freedom to demonstrate in the streets. By 1937, his regime was executing perhaps one thousand people every day. Sometimes the finest words are nothing more than words. So it is, according to the more radical of the subjectivists, with the iron rule. It is the Soviet constitution, the Marlboro Man, *Arbeit macht frei*.

The subjectivists see the moral bankruptcy of the iron rule, the gulf between science's ideology and its reality, in two apparently contradictory sentences:

> The ideology: Scientific argument deals only with the objective implications of empirical tests.

> The reality: Scientific reasoning relies essentially on the subjective interpretation of empirical tests.

But there is no contradiction and little conflict. As far as the iron rule is concerned, *argument* and *reasoning* are two quite separate things.

Reasoning is what scientists do in their heads to get from the outcomes of tests to opinions, convictions, and plans of action. It is how they make up their minds whether some theory is surely false, likely true, or still up in the air. It is how they decide whether some research program is staid, foolish, or risky but bold. It is how they determine for themselves whether a therapy or experimental procedure is reliable, hopeless, or simply unproved. Vital to such thinking are plausibility rankings, which supply the auxiliary assumptions on which all scientific reasoning is based. It is because plausibility rankings are essentially subjective that scientific reasoning is essentially subjective.

Scientific argument, by contrast, in the sense that matters to the iron rule, is what appears in science's official channels for broadcasting research, namely, the scientific journals and conference presentations. It is only in such venues, on the printed page and the projected slide, that objectivity is required.

There are, of course, many other places in which scientists argue: in their lab meetings, in their conversations over a beer after a hard day's research, and in their public but unofficial communications, such as television interviews, popular books, and talks at public libraries and corporate retreats. The iron rule does not require objectivity in any of these endeavors. When I say that the rule is concerned exclusively with scientific argument, then, I use that term in a narrow sense—shorthand, in effect, for "official scientific argument."

Official scientific argument excludes not only scientists' informal public communication but also their private thoughts. That resolves the tension between the essential subjectivity of science and the iron rule. Reasoning and argument coexist peacefully in science: one in the minds of scientists, drawing on their judgments, feelings, and inclinations; the other in science's designated organs of communication, which because of sterilization is quite devoid of such things.

A call for objectivity is not in itself unique to modern science; mathematical inquiry, for example, makes the same demand. What distinguishes science from other like-minded pursuits is, first, that it asks for objectivity only in official publications and not in private reasoning, and second, that in pursuit of the objective ideal, it will dismember an argument and throw away essential parts, just as Eddington omitted from his paper any reason to think that the distortion of the astrographic telescope's mirror led to a change of scale. Not objectivity, then, but the singular kind of objectivity achieved by sterilization, is special to science.

THE SUBJECTIVIST CRITIQUE of scientific objectivity as propaganda supposes that the primary function of a scientific paper is to assemble an argument, to make a case, to provide reasons to accept or to reject a hypothesis. The assumption is natural enough: scientific papers often take on the form of arguments, and indeed I have called this form of communication "scientific argument." But it is not at all correct. Scientific papers have two important functions. Neither requires that they articulate genuine arguments. Both benefit immensely from the process of sterilization.

One function of scientific papers is to serve as moves in the scientific game. Sterilization, by putting restrictions on the form that these moves can take, helps to make the rules of the game as clear and simple as possible, thereby bolstering modern science's procedural consensus. A sterilized paper contains only the outcomes of empirical tests and demonstrations that some theoretical cohort or other either explains or fails to explain those outcomes (or ascribes to them a certain probability). The shallow causal conception of explanation, known to and agreed upon by all scientists, determines what satisfies these criteria. Thus, the rules for writing a legitimate paper are as plain as the rules of the game of chess. Every scientist can trust that even though their colleagues may question the quality of their moves, they will not challenge their fundamental permissibility. Secure in this knowledge, they may throw themselves wholly, utterly into the contest.

The other function of scientific papers is archival: they constitute a permanent record of scientists' empirical testing. Sterilization puts these archives in a codified form that is useful to readers who may share few of the preconceptions of a paper's author. A sterilized paper, containing only the outcomes of empirical tests and the explanatory relations between those outcomes and one or more theoretical cohorts, is like a construction kit that other scientists can use to build their own arguments. To the eclipse paper, for example, add a high plausibility ranking for the assumption that the Brazilian astrographic suffered a change of scale, underreporting the bending angle, and you have a powerful argument for Einstein's theory of relativity. Add, by contrast, a high plausibility ranking for the telescope's suffering only from a lack of focus, and you will conclude that it provided useful if unrefined information suggesting a Newtonian bending angle, thus yielding overall an ambivalent case for Einstein, with different sets of measurements pulling in different directions.

Every reader, then, pours their own plausibility rankings onto the desiccated framework of a scientific article, bringing it to life and drawing conclusions accordingly—conclusions that are saturated with subjectivity and that consequently differ from scientist to scientist. Each epoch, each research group, each individual scientist will interpret the scientific literature, and so the significance of the amassed scientific evidence, in their own way.

Early on there will be doubt and disagreement, but as the evidence accumulates, it will emerge that whatever plausibility rankings you bring to the findings, one theory is far better than the rest at explaining all that has been observed. The meticulous preservation of data in its sterilized form smooths and speeds this process of Baconian convergence.

The archival function of sterilized papers is, in short, to capture evidence in a kind of presupposition-free suspended animation ready for the use of future generations. You'd rather read a novel than the labels on carefully organized freezer bags. That is why scientific writing—in journals and conference proceedings—is as dull and respectable as writing about the inner workings of science is delectable.

THE SCIENTIFIC JOURNAL in which Eddington's eclipse report was published—the *Philosophical Transactions of the Royal Society*—was established in 1665 and is generally considered the oldest publication of its kind. From the very beginning, the *Philosophical Transactions* took on the character of an archive of objective fact, a "vast pile of experiments," in the words of Thomas Sprat, one of the first members of the Royal Society, who in 1667 characterized its purpose thus:

> The Society has reduced its principal observations, into one common stock, and laid them up in public registers, to be nakedly transmitted to the next generation of men.

That official, public scientific writing should be concerned exclusively with naked fact, stripped of the opinions and untainted by the aims of the author—that is the ideal of sterilization, the "drive to objectivity" of this chapter's title, present as you can now see from science's earliest days.

The rhetoric of objectivity has evolved. During Sprat's time—the first decades of the Royal Society's existence—authors emphasized their presence in the laboratory, using an active, first-person narrative to communicate what was seen to occur. They painted the scene with numerous details of only passing relevance, striving to give their readers a sense that they themselves were there alongside the experimenter, seeing the phenomena with their own eyes. In a characteristic report, the physicist Robert Boyle wrote:

> We took a slender and very curiously blown cylinder of glass, of nearly three foot in length, and whose bore had in diameter a quarter of an inch, wanting a hair's breadth; this pipe, being hermetically sealed at one end was, at the other, filled with quicksilver . . .

I spare you the rest. As the historian Peter Dear, who quotes this passage, aptly observes, such writing conveys with great force "the actuality of a discrete event."

A little of this persists in Eddington's eclipse report from one hundred years ago. It relates a number of inessential narrative details, such as the names of the ships on which Eddington and his collaborators sailed and the island of Príncipe's dominant vegetation and prevailing climate ("very moist, but not unhealthy"). Contemporary scientific writing, by contrast, proclaims its objectivity through its ruthless exclusion of any such elaborations. Its color is gray, its nakedness clinical.

More significant than changes in style over the centuries are changes in the standards for content. The methods of statistical analysis in Eddington's eclipse report, although sound, are somewhat informal and are not consistently applied. More important still, the paper asserts forthrightly that the Brazilian astrographic measurements "are of much less weight" than the others, a remark that amounts to the oblique declaration of a plausibility ranking. Practices vary among disciplines and journals, but you would be far less likely to find these things in a scientific article today. The long-term trend is toward ever more thorough sterilization, toward methods of presentation in which the outcomes of empirical tests are exposed directly to the reader, apparently free of authorial intercession.

New ideas and new technologies have ever held out hope of fully realizing the objective ideal. With the advent of photography in the nineteenth century came the prospect that the subjectivity inherent in using words or drawings to capture what is observed might be overcome. Figure out how to photograph clearly the fine structure of the brain, for example, and Cajal's and Golgi's feuding becomes beside the point; everyone will see for themselves how neurons connect. Surely a camera shows the world as it really is, independently of the scientist's mind or eye?

At the same time, the development of formal statistical methods promised unblemished objectivity in argument. A simple mathematical formula would take the raw data as input and supply as output, without any human interference or evaluation, a judgment as to whether or not a scientific claim was credible enough to be published—whether it would be acceptable to assert in scientific print that smoking causes cancer, that the Higgs boson exists, that human activity is causing global warming.

Neither of these promises has been entirely fulfilled. Even the most objective statistical techniques leave some choices up to the scientist, and these choices, it has become increasingly clear, can be gamed to illuminate the data from the most favorable (or publishable) angle. As for photographs, the obstacles to achieving the objective ideal are crisply illustrated by the scientific study of snowflakes.

To discern the geometry of a snowflake, you need a microscope and good cold temperatures—the canonical six-sided flake forms only below −15 degrees Celsius, or 5 degrees Fahrenheit. Work on examining and depicting snowflakes began seriously in the seventeenth century in Robert Hooke's famous compendium of microscopic observations and continued through the next two hundred years as better microscopes

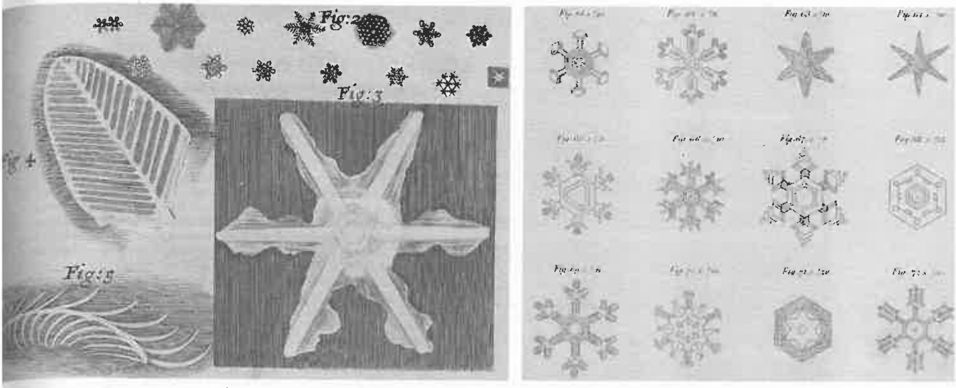

Figure 7.5. Snowflake forms drawn by Robert Hooke (1665), left; by William Scoresby (1820), right.

resulted in ever more intricate illustrations of their six-sided symmetry (Figure 7.5).

Whereas Hooke's flakes are a little uncouth, the resplendent forms drawn by the Arctic explorer William Scoresby in 1820 exhibit exemplary symmetry. Scoresby saw God imparting his own perfection to his creation:

> The particular and endless modifications of similar classes of crystals, can only be referred to the will and pleasure of the Great First Cause, whose works, even the most minute and evanescent . . . are altogether admirable.

Are snowflakes really quite so immaculate? Scoresby drew them that way, but other snowflake scientists' reports might prompt second thoughts.

During one particularly brutal winter freeze in 1855, the British meteorologist James Glaisher sketched snowflakes and handed them on to an illustrator to provide the finishing touches. The drawings were lovely, each flake flawless in its own six-sided way. As Glaisher remarked, however, the ideal was not perceived but rather inferred: the sketches had none of it. The illustrator had assumed the symmetry of the flakes in order to round out the finished pictures. That a drawing is made from life, then, does not preclude its being permeated by presupposition; in this case, the regular proportions represented on the page reflected the illustrator's convictions better than they reflected the world.

Photography promised to eliminate these subjectivities. There would be no interpretation, no opportunity for artistic idealization, only light traveling directly from flake to photographic plate to paper to the reader's eye.

The Vermont farmer for the job was Wilson Bentley. Extreme cold, a bellows camera, his mother's microscope, and a finely etched aesthetic sense—these were all he needed to begin to capture and photograph snowflakes in 1885 at the age of 19. By the time of his death from pneumonia in 1931, he had framed thousands of images; of these, over 2,000 were to be published in his book *Snow Crystals*. Figure 7.6 (left) shows a sample of his work; as you can see, the flakes are as perfectly proportioned, as symmetrical, as in Scoresby's drawings.

Such perfection made the German meteorologist Gustav Hellmann angry. Hellmann had spent years reconstructing the structure of snowflakes by hand from brief glimpses under a microscope, like Glaisher's assistant using the principle of symmetry to fill out missing parts. Eventually, he turned to the new technology of photography: in 1892, he and his collaborator Richard Neuhauss began to make images of flakes using an advanced camera. What they saw was quite different from the forms captured in Bentley's photos. Hellmann and Neuhauss's flakes had their own rough beauty, but perfect symmetry was vanishingly rare (Figure 7.6, right). Bentley, they concluded, had

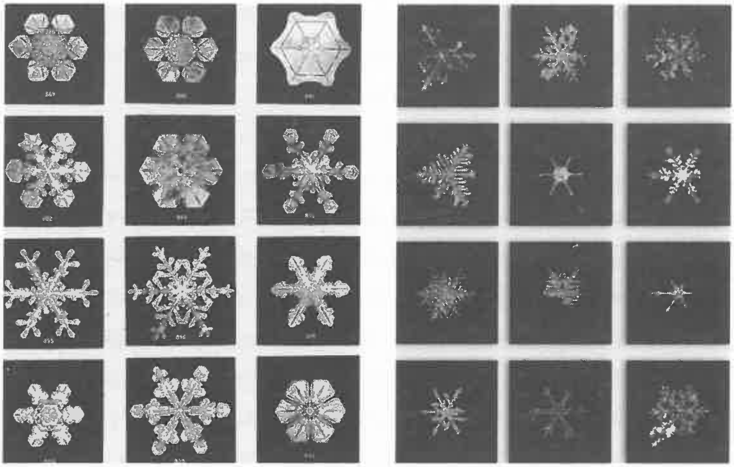

Figure 7.6. Snowflakes photographed by Wilson Bentley (1901), on the left; on the right, snowflakes photographed by Doug and Mike Starn, showing the pervasive irregularities observed by Hellmann and Neuhauss.

touched up his images to "correct" their imperfections. Worse, Neuhauss later wrote:

> In many images Bentley did not limit himself to "improving" the outlines; he let his knife play deep inside the heart of the crystals, so that arbitrary figures emerged.

Bentley's crystals were, in other words, ice sculptures of his own design.

The feud continued for decades. We don't know whether Neuhauss's worst suspicions were correct, but it seems clear that, at the very least, Bentley was not averse to a little post-processing, "the old-school version of Photoshop," as contemporary snowflake photographer Ken Libbrecht, a professor of physics at Caltech, has put it. Certainly, Bentley's depiction of the natural world is rather misleading; Libbrecht has found that only one in a thousand snowflakes has perfect six-sided symmetry. Even photography, it seems, allows space for a scientist's editorial inclinations; even the camera has a point of view.

The story of snowflakes shows as well as any that the iron rule's ideal of objectivity for arguments appearing in official scientific communications is only ever partially achieved. But it is not merely a pretense, not simply propaganda. The presentation of scientific evidence and argument may fall short of perfect objectivity while still largely performing its underlying functions: to archive for future generations the outcomes of tests and their explanatory relations to theory and to channel scientists' energy and attention away from opinion, persuasion, and invective—directing it instead toward the production of exquisitely detailed empirical fact.