Scientific communication is highly stylized—far more stylized, in fact, than forms such as the literary essay. When we look back at the past, say to the 17th century, and trace technical expression forward, we find that what we are doing when we write is telling very condensed, extremely formalized "stories" to an equally particular audience. In most cases, we have learned to do this through imitation, another trial-and-error process. Consciously or otherwise (usually otherwise), we are employing strategies to convince the reader of our knowledge, competence, originality, and contribution. This seems a tall order, when put this way. It is both ordinary and magnificent. Perhaps the sense that all of this is going on helps make us the critical, scrutinizing, and often skeptical beings that we are. But it should also reconnect us with the reasons why we originally chose to do science, the wonder and fascination, the ambitions and desires, that propelled us in this direction. Writing is about these aspects of our lives, too. Scientists are also writers because science is a great presence in the world.

2. THE LANGUAGE OF SCIENCE: HISTORICAL REALITIES FOR READERS AND WRITERS

All that a scientist creates in a fact is the language in which he enunciates it. —HENRI POINCARÉ

Matters of History

As scientists, we are largely creatures of the contemporary. Unlike other areas of study, our training provides little on the history of our discipline or the development of our discourse. Does scientific language even *have* a real history? What would Newton or Laplace make of a recent article in *Physical Review Letters*? If Darwin's *Origin of Species* were submitted to a major scientific publisher today, what might be its chances of acceptance (or should we say, survival)?

These are not merely academic questions. Scientists communicate with each other in a professional dialect, one that has evolved. Language stands still for no one, and scientific language is no exception. If you doubt this, I urge you to read through articles in your own field written 50, 75, and 100 years ago: the differences from today will be both obvious and subtle. In fact, let me offer a few examples here.

> Anno: 1672. In the year 1666 . . . I procured me a triangular glass prism, to try therewith the celebrated phaenomena of colours. . . . It was at first a very pleasing divertissement, to view the vivid and intense colours produced thereby; but after a while . . . I became surprised to see them in

an oblong form; which, according to the received laws of refraction, I expected should have been circular.[1]

1760. Beside the horizontal division of the earth into strata, these strata are again divided and shattered by many perpendicular fissures, which are in some places few and narrow, but oftentimes many and of considerable width. There are also many instances, where a particular stratum shall have almost no fissures at all, though the strata both above and below it are considerably broken: this happens frequently in clay, probably on account of the softness of it.[2]

1868. The extent to which a country suffers denudation at the present time is to be measured by the amount of mineral matter removed from its surface and carried into the sea. An attentive examination of this subject is calculated to throw some light on the vexed question of the origin of valleys and also on the value of geological time.[3]

1965. In arid climates the rocks exposed to the blazing sun become intensely heated, and in consequence a thin outer shell expands and tends to pull away from the cooler layer a few centimeters within. Under perfectly dry conditions, however, the stresses so developed are insufficient to fracture fresh, massive rocks. Experiments leave no doubt about this.[4]

2014. Ancient saline fracture waters in the Precambrian continental subsurface, with groundwater residence times ranging from millions to billions of years, provide a previously underestimated source of H_2 for the terrestrial deep biosphere. Until now, little of the information on H_2 in these settings, accessed via underground research laboratories and mines, has been incorporated into global geochemical and biogeochemical models.[5]

1. I. Newton, "New Theory about Light and Colors," in *Isaac Newton's Papers and Letters on Natural Philosophy*, ed. I. B. Cohen (Cambridge, MA: Harvard University Press, 1958), 47. This is often acknowledged to be the first modern scientific paper.
2. J. Mitchell, "The Earth Composed of Regular and Uniform Strata," in *A Source Book in Geology, 1400–1900*, ed. K. F. Mather and S. L. Mason (Cambridge, MA: Harvard University Press, 1970), 84.
3. A. Geikie, "On Denudation Now in Progress," in Mather and Mason, *Source Book in Geology*, 523.
4. A. Holmes, *Principles of Physical Geology* (New York: Wiley, 1965), 248.
5. B. S. Lollar, T. C. Onstott, G. Lacrampe-Couloume, and C. J. Ballentine, "The Contribution of the Precambrian Continental Lithosphere to Global H_2 Production," *Nature* 516, no. 7531 (2014): 379.

Even in so brief a selection, we are witness to a crucial evolution in scientific expression over the span of the modern era. We begin with Newton, who gives us a personal tour of his experiments and observations; he is there, with us, gesturing in the room. By the second excerpt, he is gone. The passive tense takes over and the object of interest ("strata") performs the action. Phenomena are now the main characters of the story. Because of this, the writing gains a more objective, formal tone. But as we continue toward the present, the writing undergoes a still more potent increase in density and reliance on terminology. By the final passage, nearly all literary or conversational touches are gone; any fluids of informality have been squeezed out. The style seems mechanical, Euclidian, even ceremonial (actually, it is none of these things, as we will see). Such is the direction our discourse has taken. How, then, did this process begin?

Where We Came From: The Beginnings of Modern Scientific Expression

Modern scientific writing in English began in the 17th century, with authors such as Francis Bacon, Robert Boyle, and Isaac Newton. This period was characterized by intense debates over the nature of language generally. At issue was the presumed power of words to control knowledge, as Bacon put it, to "force and overrule the understanding, throw all into confusion, and lead men away into numberless empty controversies and idle fancies." Bacon was thus the first to claim revolt against Elizabethan styles of writing (which, of course, included Shakespearean drama); these, he said, pulled a veil between the intellect and the world. To advance knowledge, especially "the new experimental philosophy," there was needed a simple, direct, and unadorned form of speech. This would lift the veil and provide "an equal number of words as of things."

Bacon's followers took his ideas very much to heart and made them a philosophical nucleus for the new Royal Society of London, the first scientific society in the English-speaking world. How closely did these men adhere to Baconian principles? Thomas Sprat, in his *History of the Royal Society* (first published 1667), gives us some idea:

> Who can behold, without indignation, how many mists and uncertainties, these specious tropes and figures have brought on our knowledge? . . . [We of the Society] have therefore been more rigorous in putting in execu-

tion the only remedy that can be found for this extravagance; and that has been a constant resolution, to reject all the amplifications, digressions, and swellings of style; to return back to the primitive purity and shortness . . . [to] a close, naked, natural way of speaking . . . to bring all things as near the mathematical plainness as they can; and preferring the language of artisans, countrymen, and merchants, before that of wits, or scholars.[6]

Those of the Royal Society were never more flowery than when denouncing the Elizabethans.

Yet a new style did emerge, by the end of the century. The society had established a journal (the *Transactions*), which mainly published lectures given during meetings. That the earliest scientific papers in English very often had to be read aloud in front of an audience did, eventually, impose certain changes in style and length. Newton's "New Theory about Light and Colors" (1672) helped set a standard. The paper was written as a letter to Henry Oldenburg, then president of the Royal Society, in a form to be read aloud to the membership. Newton's paper showed how effective it was to confine one's speech to a demonstration, a repeating in words of what was done in actions. Newton, meanwhile, had drawn on, and simplified, the writing of Robert Boyle, who, as it happens, may well have modeled his own discussions of chemical experiments on the essays of Montaigne.[7] Newton abbreviated the form to a sort of plot summary of events and findings, with himself, the "I," as narrator. And so, in some part, the scientific article has remained. The witnessing "I" was thus science's first storyteller. It was a way to "prove" rhetorically that the work had actually been done.

What happened thereafter, during the next three centuries, is a complex tale in itself. Different fields evolved somewhat separately, while sharing the article format and an overriding idea of what "scientific style" should be.[8] Yet literary elegance clearly had a place in science as recently as the end of the 19th century. Note, for example, a passage from the famous, aether-destroying paper by Michelson and Morely published in 1887:

6. T. Sprat, *The History of the Royal Society of London, for the improving of Natural Knowledge*, 4th ed. (London: J. Knapton, 1734), 112.

7. These connections are discussed in J. Paradis, "Montaigne, Boyle, and the Essay of Experience," in *One Science: Essays in Science and Literature*, ed. G. Levine (Madison: University of Wisconsin Press, 1987), 59–91.

8. See my own "Notes for a History of Scientific Discourse," chap. 2 in *The Scientific Voice* (New York: Guilford, 1996).

If the earth were a transparent body, it might perhaps be conceded, in view of the experiments just cited, that the inter-molecular aether was at rest in space, notwithstanding the motion of the earth in its orbit; but we have no right to extend the conclusion from these experiments to opaque bodies. . . . [And] as Lorentz aptly remarks: "Quoi qu'il en soit, on fera bien, à mon avis, de ne pas se laisser guider, dans une question aussi importante, par des considérations sur le degré de probabilitié ou de simplicité de l'une ou de l'autre hypothèse."[9]

This, indeed, seems a long way from the likes of today's article on superstring theory or quantum chromodynamics. When was the last time you read (or wrote) a paper stating "we have no right to extend our conclusion" or quoting French? What would a contemporary editor do to such a passage?

Yet there is much else that has remained in place. Don't we still propose hypotheses in order to confirm or destroy them? Don't we cite the competition, or our immediate predecessors, in a manner that supports our approach and conclusions? Of course we do, though in more formalized fashion. What, then, of the Newtonian "I" and its fate over time? Was it really killed off, forced into extinction by a more objective style? In reality, no. Both rhetorical approaches have existed side by side, and even together, down through the centuries, up to the present, though, again, in stylized form:

Anno: 1775. I cannot, at this distance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real issue of it. . . . If, however, I had not happened . . . to have had a lighted candle before me, I should probably never have made the trial.[10]

1903. The results of the investigation of radio-active minerals . . . led M. Curie and myself to endeavour to extract a new radio-active body

9. A. A. Michelson and E. W. Morely, "On the Relative Motion of the Earth and the Luminiferous Aether," *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, 5th ser., December 1887, 450, reprinted in W. F. Magie, ed., *A Source Book in Physics* (Cambridge, MA: Harvard University Press, 1965), 369–377. The French translates as, "Whatever the case, with respect to a question of such importance, one would do well in my opinion not to be swayed by considerations regarding the degree of probability or simplicity of one or another hypothesis."

10. J. Priestley, "Of Dephlogisticated Air, and of the Constitution of the Atmosphere," in *A Source Book in Chemistry, 1400–1900*, ed. H. M. Leicester and H. S. Klickstein (Cambridge, MA: Harvard University Press, 1952), 120.

from pitchblende. Our method of procedure could only be based on radio-activity, as we know of no other property of the hypothetical substance. The following is the method pursued for a research based on this property.[11]

1999. We first searched for neurons exhibiting a relatively high rate of spontaneous activity when the animal's eyes were closed. Next we characterized the orientation tuning properties of these neurons and selected the neurons with sharp tuning preference and robust response. We chose orientation tuning . . . because the majority of neurons in cat striate cortex are tuned for the orientation of bars or gratings.[12]

If the confessional "I" has turned into the royal scientific "we," the first-person point of view is still an important element in our efforts at persuasion. Yes, our language has tended to exchange tasteful tweed first for gray flannel and then bleached lab coat. Yes, we no longer write for someone who might be interested in an artful, novelistic type of narrative. But note how Priestley's confession of serendipity ("If, however, I had not happened . . . to have had a lighted candle . . .") changes for the Curies, who are "led" by "results" to perform their experiments, and how, in the final example, "neurons" are the principal performers within a symphony of choices conducted by the "we." The tales we tell are, by nature, still based on techniques whose goal it is to gain agreement and cooperation.

Role of Education

In fact, there are many techniques that we, as scientists, commonly use in our writing to convince our readers. I will go over some of these in a moment; but for now, a different point needs to be emphasized. It is this: changes in technical expression over time are not due, as is so often believed, to armies of editors, eager to tame an ever more rebellious cohort of thinkers. Language evolves because of a host of factors, not all of them well

11. M. Curie, "Radio-active Substances," in Leicester and Klickstein, *Source Book in Chemistry*, 522.

12. M. Tsodyks, T. Kenet, A. Grinvald, and A. Arieli, "Linking Spontaneous Activity of Single Cortical Neurons and the Underlying Functional Architecture," *Science* 286, no. 5446 (1999): 1722.

identified, and very few of them planned. As the examples above suggest, individual scientists, editors, publishers, and institutions all have played and continue to play a role. Manuals on writing and usage do, too; as noted in chapter 1, these have often been attempts to bring down the gavel and compel order, always without success. The task, in some sense, appears at once heroic and impossible. In another sense, however, it is merely part of the process.

Until quite recently, roughly the last 75 years or so, modern scientists were educated to acquire the skills of good research *and* good writing. In the 18th and 19th centuries especially, scientists were regularly among the most eloquent authors of the day (one thinks of Sir Charles Lyell or Thomas Huxley in England, for example). This was due to the type of classical education then in effect, whereby all students at the middle and upper levels studied authors of the Greco-Latin tradition, as well as grammar and rhetoric, and the works of the most successful modern thinkers and writers as well. Learning to write in a variety of styles was part of this education, and a demanding part at that.

During the past century, such training has given way to one that is far more specialist in design, far less interested in the study of rhetoric. Again, the reasons for such a change are many and cannot be explained by a few pretty thoughts or ugly phrases (e.g., "Little science has become Big science"). The background required to train in a research discipline has broadened and deepened enormously. Learning complex methods and acquiring a huge technical vocabulary (a form of language learning) are but two of many requirements not faced by our predecessors.

We can think of it another way. The amount of research performed and published today is many orders of magnitude greater than even 50 years ago (for most of the 20th century, the volume of scientific literature doubled every 15 years).[13] As this has happened, the length of the average article has tended to shrink and its style has become ever more jargon-rich: think of the papers published in *Science* or *Nature* (e.g., the last excerpt). These two journals, the most international in all of science, provide wonderful examples of what has happened to technical literature. In one way, these periodicals are holdovers from the past—they publish news and research from a wide array of disciplines. Yet in every other sense, they are leaders in language specialization. Their papers are commonly under five

13. See D. J. de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).

pages and very challenging or impossible to understand (even when well written) except to practitioners. In style, they read like ingots of terminology, and in content, they are often models of pioneering, influential work.

Why is any of this important? Because the past offers perspective and guidance. It reveals that, as scientists, we have become disconnected from the study of language, including our specific discourse, such that writing is no longer a core part of our training. Thus, the real job of a guide like this one is itself historical, that is, to help put back some of this knowledge and awareness. The past also shows that technical language is dynamic, changing. We need to write and speak in the stylistic idiom of our time, not as our scientific grandparents did generations ago. Our best teachers are our contemporaries, our colleagues (broadly defined) extending back a few decades at most. Whether we take writing classes or no, we ultimately learn to express ourselves, in our specialist contributions, from our peers, and that we can improve our communication skills through self-directed effort, instead of betting against chance and looking for figures in the carpet.

Scientific Rhetoric: An Instructive Analysis of a Notable Paper

For most of the modern era, science and literature openly employed many of the same techniques to persuade their readers. What about today? To answer this, I'd like to take a brief look at a scientific paper of some renown, published in the journal *Nature*, put it under the microscope, and point up some of the rhetoric it employs. We begin with the first few sections.

> We wish to suggest a structure for the salt of deoxyribose nucleic acid (D.N.A.). This structure has novel features which are of considerable biological interest.
>
> A structure for nucleic acid has already been proposed by Pauling and Corey. They kindly made their manuscript available to us in advance of publication. Their model consists of three intertwined chains, with the phosphates near the fibre axis, and the bases on the outside. In our opinion, this structure is unsatisfactory for two reasons: (1) We believe that the material which gives the X-ray diagrams is the salt, not the free acid. Without the acidic hydrogen atoms it is not clear what forces would hold the structure together, especially as the negatively charged phosphates near the axis will repel each other. (2) Some of the van der Waals distances appear to be too small.

> Another three-chain structure has also been suggested by Fraser (in the press). In his model the phosphates are on the outside and the bases on the inside, linked together by hydrogen bonds. This structure as described is rather ill-defined, and for this reason we shall not comment on it.
>
> We wish to put forward a radically different structure for the salt of deoxyribose nucleic acid. This structure has two helical chains each coiled round the same axis (see diagram). We have made the usual chemical assumptions, namely that each chain consists of phosphate di-ester groups joining β-D-deoxyribofuranose residues with 3′,5′ linkages. The two chains (but not their bases) are related by a dyad perpendicular to the fibre axis. Both chains follow right-handed helices, but owing to the dyad the sequences of the atoms in the two chains run in opposite directions. Each chain loosely resembles Furberg's model No. 1; that is, the bases are on the inside of the helix and the phosphates on the outside. . . .
>
> The novel feature of the structure is the manner in which the two chains are held together by the purine and pyrimidine bases. The planes of the bases are perpendicular to the fibre axis. They are joined together in pairs, a single base from one chain being hydrogen-bonded to a single base from the other chain, so that the two lie side by side with identical *z*-co-ordinates. One of the pair must be a purine and the other a pyrimidine for bonding to occur. The hydrogen bonds are made as follows: purine position 1 to pyrimidine position 1; purine position 6 to pyrimidine position 6.
>
> If it is assumed that the bases only occur in the structure in the most plausible tautomeric forms (that is, with the keto rather than the enol configurations) it is found that only specific pairs of bases can bond together. These pairs are: adenine (purine) with thymine (pyrimidine), and guanine (purine) with cytosine (pyrimidine).
>
> In other words, if an adenine forms one member of a pair, on either chain, then on these assumptions the other member must be thymine; similarly for guanine and cytosine. The sequence of bases on a single chain does not appear to be restricted in any way. However, if only specific pairs of bases can be formed, it follows that if the sequence of bases on one chain is given, then the sequence on the other chain is automatically determined. . . .
>
> The previously published X-ray data on deoxyribose nucleic acid are insufficient for a rigorous test of our structure. So far as we can tell, it is roughly compatible with the experimental data, but it must be regarded

as unproved until it has been checked against more exact results. Some of these are given in the following communications. . . .

It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.[14]

One of the most epochal papers in all of 20th-century science, Watson and Crick's "A Structure for Deoxybribose Nucleic Acid" defies nearly every major rule you are likely to find in manuals on scientific writing. It does not follow the IMRAD (introduction, methods, results, and discussion) structure. It is descriptive, not analytical. It contains many "unnecessary" phrases ("in other words," "so far as we can tell,"), vague words ("ill-defined," "loosely"), and redundancies (the first sentence is repeated at the beginning of the fourth paragraph). There are even grammatical errors (frequent use of the unrestricted "which" instead of "that"). Its paragraph form is uneven and improper. It has not included or even consulted the relevant experimental data (and admits this!). And finally—but this is far from a complete list—it has no real conclusion.

Yet it works. We can agree that, despite all its unforgivable blunders, the article is persuasive and effective. Why? First of all, it answers quite well the five fundamental questions essential to good scientific storytelling:

1. *What* did you (and your coauthors) do?
2. *Why* did you do it?
3. *How* did you do it?
4. What did you *find*?
5. What does it *mean*?

For question 1, Watson and Crick say they worked on the structure of DNA. They then tell us why (question 2): it has "considerable biological interest" and hasn't been solved by anyone else, other models being "unsatisfactory." Their answer to question 3 is less clear, and we have to deduce it through the logic they provide regarding the details of their structure, as well as the strong suggestion that they used X-ray data from crystallized DNA fibers. Their paper, however, was published in 1953, a fact that accounts for some of the stylistic differences from today's discourse (an interesting exercise: rewrite the paper so that it sounds more contemporary to the 21st century). Today, reviewers would likely demand, with reason, that the authors be more explicit about their data and methods. As for question 4, the answer is the model itself, which is described and also shown in the paper's single figure, that of the double helix—an image, we might note, whose simple elegance has helped it become nothing short of an icon for the entire field of genetics. Finally, Watson and Crick tell us the deeper meaning (question 5) of the structure in the last sentence, rather coyly, we might note, but distinctly.

14. J. D. Watson and F. H. C. Crick, "A Structure for Deoxyribose Nucleic Acid," *Nature* 171, no. 4356 (1952): 737–738.

There is another way to look at all of this. We can consider what the article actually *does*, step-by-step. It has a flow that moves through the following stages: (1) a declaration of its importance and novelty; (2) a statement that its problem has not been solved, even by some worthy competitors; (3) discussion of its findings (paragraphs 4–7); (4) qualification of these findings, as not yet complete or final (need for further work); and (5) specification and elevation of the importance declared at the beginning. What type of "story" does this offer? A tale full of understated sound and fury. The authors tell us that there is a rush to unravel the "grail" of DNA, that all important alternatives appear doomed, and that they alone have happened upon the rightful path. Watson and Crick then reveal their discovery, build a castle out of detailed descriptions, and set their flags flying. A final fanfare announces that their victory holds yet a greater triumph than first imagined. Like a well-crafted story, the argument follows an hourglass structure overall: it begins with the general, moves into the ever more specific, and then, at the end, expands back outward into the general.

Now, let us look a little closer. Note that the paper begins with short, concise sentences, which evolve into longer ones as the authors begin to describe their model. This progression, in fact, matches the main effort of the paper, which is to "build" the model in words, through specific details. The use of brevity at the beginning moves the story along more quickly, and us with it. Shorter sentences at the end provide a touch of subtle drama and a more memorable finale, so to speak. We, as readers, are therefore treated as if we deserve to see only the best, most privileged information; we are given the authors' full confidence.

Notice, too, the use of "we." This also happens at the beginning and the end. Why not in the middle? Because this is where the main subject of the narrative becomes the model and its structure. We can envision this as a video that starts out with the authors in frame, then pans to the model, moves in for a close-up, pulls out, then shifts back to the authors. In the article itself, this happens so smoothly that we don't perceive it, though it is a true rhetorical technique, such "invisibility" being the sign of a good paper.

Finally, the paper employs a form of logic that is speculative, yet, at the same time, as ordinary in scientific communication as the comma. We see it in the form "if we assume . . . we then find . . ." (paragraphs 6 and 7). Speculation, say many writing guides, has no place in technical writing except perhaps in certain arm-waving theoretical communications. But they are wrong. The posing of scenarios, possibilities, and alternatives is a venerable and valuable type of reasoning in science. The "if . . . then" technique acts like a postulate and is part of the rhetorical toolbox of the competent scientific writer, past and present. We see with Watson and Crick that it is used to propose what they think is actually the case, but for which they lacked the data to firmly claim in a publication.

At this point, you might feel we've gone far enough with our analysis of this brief, two-page paper—indeed, maybe a little too far. We've made Watson and Crick's article appear no less complex than a protein molecule or fusion reactor. Yet the truth is that a full rhetorical study of "A Structure for Deoxybribose Nucleic Acid" would easily fill an entire monograph. Wouldn't such an analysis be wasteful for our purposes? No, not really; just unnecessary. From the above, it's obvious that a great deal is going on in the average scientific paper. The surface may seem calm and composed, like a windless lake, yet only a few drops beneath the microscope are enough to reveal a world of strategy, claims-making, and enlistment.

This begs a question: are good writers aware of the techniques they employ? The answer is almost always yes and no. Yes, because good writers very often plot out or experiment with the logical course of their narrative. No, because many specific rhetorical techniques are used intuitively; they have been learned and internalized by attentive reading of the literature. Effective writers are those who have an inner ear for what sounds right, what is persuasive at each turn of a discussion. Being aware of even a few such techniques, and how to acquire them, will provide the scientist with a powerful instrument for his or her expressive work.

Grammar: Facts and Fallacies

Questions of history and language change bring us, happily or unhappily, to the subject of grammar. What is to be said about it? Look again at the

first set of examples given in this chapter: notice that, as the number of technical terms has grown, the grammar of the sentences has become more simplified. This is not an accident. Consider the following:

> A comprehensive overview of quality control in DNA would include a discussion of DNA polymerase fidelity and postreplicative mismatch correction and would also consider the damage-responsive cell-cycle checkpoints and the signal transduction systems that lead to cellular effects.[15]

Now replace each technical term or phrase with an ellipsis:

> A comprehensive overview of . . . would include a discussion of . . . and . . . and would also consider the . . . and the . . . that lead to cellular effects.

Finally, with a bit more distillation, we get

> A comprehensive overview of . . . would include a discussion of . . . and would also consider . . .

Once the terminological smoke clears, the average scientific sentence today emerges as fairly elementary. Perform the same exercise on an entire paragraph in a recent article—on your own writing even. I guarantee it will be revealing, and helpful. Indeed, the exercise can be valuable in pointing out unneeded complexity (the above sentence, for example, could certainly be helped by enumeration).

I ask that you neither let the subject of grammar cripple you with concern nor float your intentions too high. Good grammar alone does not a writer make; bad grammar, if only occasional, does not destroy one. On the other hand, to communicate effectively, you have to be able to produce a legible sentence. This goes (almost) without saying. But being hyperconscious of possible mistakes can reduce your progress to a glacial melt. Perfectionism makes for unforgiving standards, whether you are a writer or reviewer.

Grammar is mainly about rules and formulae. It necessarily involves a rule-driven, mechanistic view of language use. It is the zero law of the communication process: necessary but way far of sufficient. "Proper usage," on the other hand, is, in reality, a different subject. Authors of technical

15. T. Lindahl and R. D. Wood, "Quality Control by DNA Repair," *Science* 286, no. 5446 (1999): 1898.

writing manuals are often in riot gear over whether to change "prior to" to "before," "perform" to "do," and so forth. Such efforts are not about grammar; they are about brutal simplification as a standard. They are largely ineffectual and, worse, irrelevant. In many cases, they represent pet peeves of individuals. They are important only when they belong to journal editors and, to a lesser degree, reviewers. In general, scientific discourse rumbles on, fortissimo, more productive than ever, without paying much attention to these constabulary proclamations.

Does this mean "standards" are out the window? Is science groaning under the weight of its own fatty, acidic verbiage? Hardly. Yes, there is poor writing in abundance (as in other fields); and indeed, it is important to do something about it. But implying that a few dozen ironclad rules will improve scientific expression overall is tantamount to believing that knowledge of the periodic table automatically makes you a chemist.

Thus, my advice is this. If you feel shaky about your ability to generate a correct sentence, to the point where it slows or even blocks your writing, then by all means focus some initial effort in this arena. Study a grammar text for a month or two, or more. Do this while reading the literature of your field with an occasional eye to evaluate its grammatical aspects. Gain a degree of confidence here; lessen your concerns. Learn the basics before proceeding on to the delights, challenges, and deeper humiliations of professional writing.

What It Means to Write or Speak Well in Science

When it comes to putting words on paper, the scientist has a certain advantage. What, possibly, could this be (don't all scientists *hate* to write)? Simply this: at a basic level, scientists have a choice generally denied to other disciplines. They can be purely functional writers—ordinary engineers of the word—or they can strive for higher levels of eloquence, even creativity, within the bounds of acceptable formality.

Outside of science, functional writing is commonly looked upon as dull and unskillful. In most fields, there are expectations (or hopes) of grace, color, style. Particularly in the arts, in history or literature, language is expected to call attention to itself, to at least strive now and then for some obvious sign of craft, cleverness, or felicitous phrasing. Even in business or sociology, material with the aesthetic quality of cement is viewed as lacking in something. Not so in science. We can be as flat and gray as we like and not be judged ill for it. Functional writers, in fact, compose the great majority of successfully published scientists. This does not mean that such writing is always good—but a significant portion *is* competent: readable, informative, and adequately organized. Moreover, few writers are bad (or good) all the time; varieties of competence tend to exist in any single piece of writing. Functional communication, at a proficient level, is very much something to strive for in science. Indeed, as we've already indicated, it is essential for good science to be done.

There is something else here, too. Nonfiction authors frequently come up against a number of fundamental questions. William Zinsser, for one, has laid these out nicely, showing how they include (but are not limited to) the likes of, How am I going to address the reader? (Reporter? Provider of information? Average man or woman?) What pronoun and tense am I going to use? What style? (Impersonal reportorial? Personal but formal? Personal and casual?) What attitude am I going to take toward the material? (Involved? Detached? Judgmental? Ironic? Amused?) . . . Who will my readers be? What sorts of publication venues might be interested in my work? How much will I get paid?

Nearly all such questions are irrelevant in science. We simply don't need to worry about them. They've already been answered, in large part, by history. Such is a benefit to professional discourse in almost any field, but much more so in science. Again, this does *not* mean that our speech is simple and unchallenging, not in the least. We have our own problems to solve that other nonfiction writers lack: how to translate data into words; how to describe experiments so that they might be repeated; how to use illustrations; which colleagues to cite or challenge.

But can we really choose to be eloquent writers in science? Assuredly we can. Here is where aspects such as refined organization, use of transitions, sentence rhythm and length, and strategic employment of rhetorical technique come in. An entire chapter of this book is devoted to showing how some of these aspects can be used creatively. The trouble is that such creativity needs to be subtle in science. It reveals itself most often in an occasional manner and in background elements that direct and propel the argument, but quietly. It may therefore go largely unrecognized by a majority of readers. This is the risk you run in crafting beautiful science: only editors, writers such as yourself, and teachers of writing are likely to appreciate what you've done, at least at first. In the long run, of course, you are also in jeopardy of being used as a model for others.

I began this chapter with the contemporary scientific author, and it makes sense to end this way. History proves that, as writers and speakers, we are immediate contributors to the evolution of scientific discourse. Every article or proposal or report that we produce, every word we put to paper, is an event within the flow of this evolution. Scientific language continues to change, as it must. We are its creators and metabolizers. But even more, we are its primary medium: through our efforts to create and exchange knowledge, this language is made real and alive. To write and speak well, whether functionally or eloquently, is to take responsibility for history, for knowledge, for oneself as a scholar.

3. READING WELL: THE FIRST STEP TO WRITING WELL

The only demand I make of my reader is that he should devote his whole life to reading my works. —JAMES JOYCE

The Concept of Authorial Ear

Good musicians and skilled writers have something in common. They both have developed an ear for what sounds right and what does not. When faced with a work of music or text, the well-tuned ear can listen for the movement of notes and words, certainly, but it will be equally alert to patterns of sense, to elements of order and logic and how they move within the work. Having this type of skill is no small thing. But it is no mystery either. It involves being attentive to the medium in particular ways. It means, for a scientific author, being able to detect what feels awkward in a sentence like this:

> In higher plants, flowering—the transition from vegetative to reproductive growth phase—is controlled via several interacting pathways influenced by both endogenous factors and environmental conditions.

This is the opening to a published paper in a prominent journal. It is not a wonderful sentence, but it is comprehensible. Suppose, however, it were written like this:

> Flowering in higher plants is defined as the transition from vegetative to reproductive growth phase. This change is controlled by several interact-