Chapter 10: The Scientist

Let's conclude by turning our gaze inward. Knowing that science thrives on a diversity of styles and techniques, can we nevertheless identify dominant patterns of behavior, ethics, and motivations?

> "One thing I have learned in a long life: that all our science, measured against reality, is primitive and childlike -- and yet it is the most precious thing we have." [Einstein, 1879-1955]

> Isaac Newton [1642-1727], a man known more for his arrogance than for humility, said near the close of his life:
>
> "I do not know what I may appear to the world; but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me."

Scientists' Characteristics

The traits possessed by successful scientists are seldom examined systematically in college or graduate school. They are not the traits that one would choose in an idealized characterization of a scientist. Nor are they revealed by courses and tests. Most courses and tests emphasize temporary fact accumulation, a worthy but largely unnecessary acquisition in an age of ready access to reference information. Some personal characteristics are so pervasive among scientists that they appear to be essential for scientific success. Others are common, advantageous, but not essential.

Essential Characteristics

• **persistence**: This necessary characteristic encompasses traits such as dogged perseverance, patience, tenacity, thoroughness and singleness of purpose. Perhaps, attainment of a Ph.D. demonstrates persistence more than any other capability. As a musician friend told me, daily practice encounters peaks of surging progress and bogs of apparent stagnation. Both are transitory stages that must be outlasted; persistence is the bridge. For scientific success, persistence must continue beyond research and through publication.

> "Nothing in the world can take the place of persistence. Talent will not; nothing is more common than unsuccessful men with talent. Genius will not; unrewarded genius is almost a proverb. Education alone will not; the world is full of educated derelicts. Persistence and determination alone are omnipotent." [Hoenig, 1980]

> "Let me tell you the secret that has led me to the goal. My only strength resides in my tenacity." [Pasteur, 1822-1895,a]

Persistence is not always a virtue. One needs to recognize when to let go -- unlike the weasel, reduced to a skull, found with jaws still imbedded in the throat ruff of a living eagle. It is naive optimism to think that any problem can be solved just by working harder. If a problem is reaching diminishing returns, it should be abandoned. Perhaps the problem is posed wrongly:

> Matthiessen [1978] says that the Buddha “cried out in pity for a yogin by the river who had wasted twenty years of his human existence in learning how to walk on water, when the ferryman might have taken him across for a small coin.”

Persistence in a technically difficult experiment is commendable; persistence in investigating a discredited hypothesis is not. If advocacy of an opinion has become counterproductive, then adapt. Nevertheless, far more scientists have failed because of insufficient persistence than because of excessive persistence.

> “. . . let us run with patience the race that is set before us.” [Hebrews 12:1]

• **curiosity**: The desire to know more, an inquisitiveness that is not satisfied with surface explanations, is the ratchet of scientific progress.

> Jonas Salk [1990] said that he spent his life “reading the scriptures of nature. . . I began to tease out the logic of the magic that I was so impressed by.”

The scientist’s curiosity is not passive; it is an active embrace of nature:

> “I come down to the water to cool my eyes. But everywhere I look I see fire; that which isn’t flint is tinder, and the whole world sparks and flames.” [Dillard, 1974]

• **self-motivation:** Internal drive to work is a product of job enjoyment. Self-motivation is scarce in most types of jobs [Terkel, 1974], frequent in professions, and nearly universal among productive scientists. Single-minded drive undoubtedly increases effort, but self-motivation seems to have more impact than effort can account for. Self-motivated scientists, who may do only part-time research because of teaching or administrative responsibilities, can produce more than full-time scientists who have lost their internal drive (e.g., because management does not value their work).

Self-motivation can be overdone: I and many scientists whom I know are stress junkies, who are stimulated so much by ‘emergencies’ that they seem to create such situations even when a rapid pace is unnecessary. To a stress junkie, efficiency and productivity are additional sources of job satisfaction.

> Volcanologist Maurice Krafft, who was later killed by Unzen Volcano, said “ I would say that if one truly specializes in explosive volcanoes then it’s not worth contributing towards retirement, and that if one makes it to retirement it’s a little suspicious. It means that he really didn’t do his job conscientiously.” [Williams and Montaigne, 2001]

Productivity has become a cliché of the business world, but productivity is not just a national or industrial goal. It is a personal skill. Computer expertise and efficient fact finding are tangible forms of individual scientific productivity; more essential and less tangible aspects are problem-solving ability, quantitative reasoning, and self-motivation. Quantity of publications is the most commonly used measure of productivity [Maddox, 1993]. Its virtues are simplicity and objectivity, but scientific impact does not depend on number of publications.

> “Every man, every civilization, has gone forward because of its engagement with what it has set itself to do. The personal commitment of a man to his skill, the intellectual commitment and the emotional commitment working together as one, has made the Ascent of Man.” [Bronowski, 1973]

- **focus:** Focus is the ability to spot the crux among a morass of detail and then stay concentrated on it, without being distracted or sidetracked. Focus assures that the target receives all of the attention needed. Lack of focus is evidenced by tendencies toward incompleteness, inefficiency, overlooked significant details, grasshopper science, and panic reaction to setbacks.

> Thanks to physicist Richard Feynman [1985], I now associate focus with chocolate pudding. During a period of his life when he went out to dinner frequently, a waiter asked him what he would like for dessert. Suddenly he considered how much of his life was wasted in thinking about that trivial question, so he decided that henceforth the answer would always be chocolate pudding! Focus does not tolerate needless distractions.

- **balance between skepticism and receptivity:** A critical attitude is essential; all data and interpretations must be evaluated rather than simply accepted. Yet it is equally essential to achieve a balance between skepticism and receptivity: willingness to propose speculative hypotheses that may be proved wrong, tempered by ability to weed out the incorrect hypotheses. One must be receptive to novel concepts or results, rather than greeting the new with a ‘fight-or-flight’ reaction of dismissive criticism. The critical filter that rejects everything as insufficiently proved robs science both of joy and of raw materials for progress.

This balance is manifest also by a blend of optimism and realism. Optimism and enthusiasm for new ideas are contagious and powerful, if accompanied not by a casual confidence that effort alone will find a solution, but by a problem-solving mentality and preparation for potential obstacles.

Common Characteristics

Many prospective scientists think that love of science and high intelligence are the two primary characteristics needed to permit them to be successful scientists. This idealized picture of science can lead to disillusionment or worse. Love of science and high intelligence are neither necessary nor sufficient, though they are the springboard of most scientific careers.

- **fascination with the beauty of nature:** We may not use words such as ‘beauty of nature’; we may try (at least outwardly) to maintain the myth of objectivity. Yet we revel in the elegance and wonder of the natural world, and we choose occupations that further our opportunities for appreciation of it.

> “I am among those who think that science has great beauty. . . A scientist in his laboratory is not only a technician but also a child placed in front of natural phenomena which impresses him like a fairy tale.” [Marie Curie, 1937]
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> Konrad Lorenz [1962] described the prerequisites to success in the field of animal behavior as follows:
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> “To really understand animals and their behavior you must have an esthetic appreciation of an animal’s beauty. This endows you with the patience to look at them long enough to see something. Without that joy in just looking, not even a yogi would have the patience. But combined with this purely esthetic characteristic, you must have an analytical mind.”

- **love of science:** Love of science is a greater spur to productivity than any manager can offer. Love of science, love of discovery, and enthusiasm for science are contagious; they are nurtured by scientific interactions. Most scientists are inclined to be somewhat forgiving of weaknesses in those colleagues who passionately love science.

If you were beginning a career again, would you pick the same type of work? The answer to this question was 'yes' for 86-91% of physical scientists, 82-83% of lawyers and journalists, 41-52% of those in skilled trades (printers, autoworkers, and steelworkers), and only 16-21% of those in unskilled trades (assembly-line steelworkers and autoworkers) [Blauner, 1960]. Jobs with the highest levels of worker satisfaction are those that are esteemed by society, that allow both personal control of decisions and unsupervised work, and that involve teams [Blauner, 1960]. Scientific careers provide all of these.

I learned how atypical the scientist's job satisfaction is when I told my half-brother, who was an insurance salesman, that I love my work; he laughed and told me not to bullshit him. Sometimes the exhilaration with science is so overpowering that I break out in a silly grin. Then I remember, consciously or unconsciously, the scientist's distrust of sentimentality. I transform the grin into a knowing smile and dryly remark, "It's a dirty job, but somebody has to do it; don't they?"

- **above-average intelligence**: This characteristic is almost essential, but a scientist with only average intelligence can succeed by excelling in the other traits of scientists. Genius is not required. Among those with an IQ > 120, IQ shows little relation to either scientific innovation or productivity [Simonton, 1988]. Genius without the other needed qualities is insufficient for scientific success.

> Srinivasa Ramanujan was a mathematics genius in 19th-century India. He was rich enough to receive a high school education, a few books, and live as a scholar. Yet for most of his life he was completely cut off from virtually all mathematics literature and knowledge. He worked on his own, and mathematicians are still deciphering and applying his work [Gleick, 1992d]. How much more could he have accomplished as part of the science community? How many geniuses never see a book?

Most of us equate IQ scores with intelligence, but IQ scores predict success in school, not in life. Career and family success is forecast more successfully with tests that model constructive thinking, problem solving, and persuasion, with and without emotional distractions. In contrast, IQ tests evaluate specific types of verbal and mathematical ability. They do not evaluate how well these will be applied to the often ambiguous and open-ended problems of real life, where ability to react to crises and manage one's emotions are just as essential as IQ [Goleman, 1992a].

- **imagination**: Imagination is necessary for insight and even for the everyday problem solving that is intrinsic to most science. Almost all scientists are unusually imaginative, but the unimaginative can produce valuable science in the form of careful hypothesis testing. Individuals who have imagination but lack a critical attitude can be cranks; they cannot be scientists. When imagination is combined with both will and a vision of what is achievable, the result can be formidable: "We choose to go to the moon" [J. F. Kennedy, 1960 speech].

- **desire to improve**: "Boredom could be an important stimulus to evolution among the animals" [Calvin, 1986], because it leads to trials of a variety of different behaviors. Like curiosity, dissatisfaction with the *status quo* certainly is a stimulus to scientific progress. This dissatisfaction is manifested by boredom, the appeal of the mysterious, and the desire to improve circumstances.

> "The most beautiful experience we can have is the mysterious. It is the fundamental emotion that stands at the cradle of true art and true science." [Einstein, 1879-1955]

The desire to improve encompasses both oneself and one's environment:

> "How thankful I should be to fate, if I could find but one path which, generations after me, might be trodden by fellow members of my species." [Lorenz, 1962]

- **aggressiveness:** Aggressive scientists tend to be highly successful and productive. Science is an obstacle course of puzzles, experimental problems, and bureaucratic hurdles, and success requires an aggressive unwillingness to be stopped by such obstacles. I am cautious, however, about interactions with aggressive scientists, as most of them seem to have trouble finding a balance between ethics and aggressiveness. Ethical barriers are not just problems to be overcome, and other scientists are not just tools to be used for furthering one's progress.

Style determines whether aggressiveness is an asset. For example, we see quite different aggressive styles every day on the highway: some drivers fight the traffic, whereas others go with the flow much of the time, while anticipating congestion and seizing opportunities.

- **self-confidence:** Self-confidence fosters a willingness to face challenges and a constructive optimism, relatively free of worries about the opinions of others and about whether the problem can be solved. Both self-motivation and self-confidence are needed if one is to lead a scientific discipline into new productive directions, rather than just following along with the majority. Self-confidence inspires acceptance of one's opinions by others, in spite of scientists' claims that they are influenced only by the evidence, not by the presentation.

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Scientists are subject to many of the same fears as most people. They fear mediocrity, completing a life of science only to conclude that they had little or no significant impact on science. They fear humiliation, being proved wrong in print or, worse yet, being shown to have made some mistake that 'no real scientist should make.' They fear that someone else will get the credit for their discoveries. They fear that they cannot keep up with the pace of science and are being left behind [Sindermann, 1987].

Perhaps instead they should fear that they have lost proportion: that they are sacrificing too much of their personal life to science, that they have abandoned some ethical values because those values hampered achievement of scientific objectives.

* * *

> "What is then the quality which enables some men to achieve great things in scientific research? For greatest achievements men must have genius -- that elusive quality that so often passes unrecognized, while high ability receives reward and praise. But for achievement genius is not enough, and, for all but the greatest achievements, not necessary. What does appear essential for real achievement in scientific research is a combination of qualities, by no means frequent, but commoner than is genius. It

> seems that these qualities are clarity of mind, a combination of imagination and caution, of receptivity and skepticism, of patience and thoroughness and of ability to finalize, of intellectual honesty, of a love of discovery of new knowledge and understanding, and of singleness of purpose. Of these the most important is the love of discovery of new knowledge and understanding. If any young readers, contemplating scientific research as a profession, do not feel this love . . . scientific research is not for them." [Freedman, 1950]

> "What can I wish to the youth of my country who devote themselves to science? *Firstly, gradualness*. About this most important condition of fruitful scientific work I never can speak without emotion. Gradualness, gradualness and gradualness. Learn the ABC of science before you try to ascend to its summit. Never begin the subsequent without mastering the preceding . . . Do not become the archivists of facts. Try to penetrate the secret of their occurrence, persistently search for the laws which govern them. *Secondly, modesty*. Never think that you already know all. However highly you are appraised, always have the courage to say of yourself – I am ignorant. Do not allow haughtiness to take you in possession. Due to that you will be obstinate where it is necessary to agree, you will refuse useful advice and friendly help, you will lose the standard of objectiveness. *Thirdly, passion*. Remember that science demands from a man all his life. If you had two lives that would be not enough for you. Be passionate in your work and your searchings." [Pavlov, 1936]

* * *

These generalizations concerning characteristics of scientists are subjective, based on my and others' personal observations. In contrast, Rushton [1988] summarizes the results of several objective statistical analyses as follows:

> "Scientists differed from nonscientists in showing high general intellectual curiosity at an early age and in being low in sociability. . . Eminent researchers [were] . . . more dominant, self sufficient, and motivated toward intellectual success. . . In summary, the impression that emerges of the successful research scientist is that of a person less sociable than average, serious, intelligent, aggressive, dominant, achievement oriented, and independent."

* * *

Cooperation or Competition?

Both cooperation and competition are integral aspects of scientific interaction. Joint projects combine diverse, specialized expertise to promote research success. For many scientists, competition provides a motivation to excel. This drive to win is particularly effective for those researchers who can pace themselves, putting out a burst of extra effort on those occasions when it can make the decisive difference between being a discover and being a confirmer of others' discoveries.

The choice between scientific cooperation and competition is a daily one, involving conscious or unconscious decisions on style of interactions with scientific peers. Most scientists simplify this decision-making by adopting a strategy that provides the decision. Perhaps the strategy is to cooperate with all other scientists; perhaps it is to compete with everyone over everything. More likely, the individual always cooperates with a few selected scientists and competes with others. Whatever viable strategy is selected, we should recognize its consequences.

The survival value of successful competition is almost an axiom of evolutionary theory. Why, then, has cooperation survived evolutionary pressure, in humans as well as in many other species? Kinship theory is the usual explanation. According to kinship theory, a genetically influenced strategy such as cooperation is evolutionarily viable if it helps a substantial portion of one's gene pool to survive and reproduce, even if the cooperator dies. A classic example is the sterile worker honeybee, which commits suicide by stinging. Altruism of parents for offspring is easy to explain, but kinship theory also successfully predicts that altruism would be high among all members of an immediate family and present throughout an inbred tribe. Sacrifice for an unrelated tribe member may improve future treatment of one's children by tribe members.

Modified kinship theory can account for many manifestations of cooperation and competition among scientists. An *us/them* perspective can be developed among members of a company, university, or research group. Thus a member of a National Science Foundation proposal-review panel must leave the room whenever a proposal from their home institution is under discussion. Here the health or reputation of an institution is an analogue for genetic survival. Similarly, a clique of scientists with the same opinion on a scientific issue may cooperate to help defeat a competing theory.

For scientists facing the decision of cooperation or competition with a fellow scientist, kinship theory is not a particularly useful guide. A more helpful perspective is provided by the concept of an evolutionarily stable cooperation/competition strategy. Evolution of a cooperation/competition strategy, like other genetic and behavioral evolutions, is successful only if it fulfills three conditions [Axelrod and Hamilton, 1981]:

- **initial viability**. The strategy must be able to begin by gaining an initial foothold against established strategies.
- **robustness**. Once established, the strategy must be able to survive repeated encounters with many other types of strategy.
- **stability**. Once established, the strategy must be able to resist encroachment by any new strategy.

Axelrod and Hamilton [1981] evaluated these three criteria for many potential cooperative/competitive strategies by means of the simple game of Prisoner's Dilemma [Rapoport and Chammah, 1965]. At each play of the game, two players simultaneously choose whether to cooperate or defect. Both players' payoffs depend on comparison of their responses:

My choice	Other's choice	My score	Explanation
cooperate	defect	0	**S**ucker's disadvantage
defect	defect	1	**N**o-win mutual defection
cooperate	cooperate	3	**R**eward for mutual cooperation
defect	cooperate	5	**C**ompetitive advantage

When the game ends after a certain number of plays (e.g., 200), one wants to have a higher score than the opponent. But even more crucial if the game is to be an analogue for real-life competition and cooperation, one seeks the highest average score of round-robin games among many individuals with potentially varied strategies.

The optimum strategy in Prisoner's Dilemma depends on both the score assignments and the number of plays against each opponent. The conclusions below hold as long as:

- $S < N < R < C$, i.e., my defection pays more than cooperation on any one encounter, and cooperation by the opponent pays more to me than his or her defection does;

- $R > (C+S)/2$, i.e., cooperation by both pays more than alternating exploitation; and

- I neither gain nor lose from my opponent's scoring (e.g., if I were to gain even partially from his gains, then continuous cooperation would be favored).

If one expects to play only a single round against a specific opponent, then the optimum strategy in Prisoner's Dilemma is to *always defect*. Similarly, in a population of individuals with no repeat encounters or within a species incapable of recognizing that an encounter is a repeat encounter, constant competition is favored over cooperation. More relevant to interactions among scientists, however, is the case of many repeat encounters where one remembers previous encounters with a given 'opponent'. It is this situation that Axelrod and Hamilton [1981] modeled by a computer round robin tournament, first among 14 entries and then among 62 entries of algorithm strategies submitted by a variety of people of different professions. Subsequent computer studies by various investigators simulated the process of biological evolution more closely, incorporating variables such as natural selection (higher birth rate among more successful strategies) and mutation.

In nearly all simulations, the winner was one of the simplest of strategies: *tit for tat*. *Tit for tat* cooperates on the first move, then on all subsequent moves duplicates the opponent's preceding move. Axelrod and Hamilton [1981] call *tit for tat* "a strategy of cooperation based on reciprocity." When *tit for tat* encounters a strategy of *all defect*, it gets burned on its first cooperative move but thereafter becomes a strategy of *all defect*, the only viable response to an *all defecter*. *Tit for tat* does much better against itself than *all defect* does against itself, and *tit for tat* also does much better against various other strategies, because mutual cooperation pays off more than mutual defection.

Axelrod and Hamilton [1981] prove that *tit for tat* meets the success criteria of initial viability, robustness, and stability for Prisoner's Dilemma, and they argue that *tit for tat* is also a successful evolutionary strategy in various species from human to microbe (its reactive element does not require a brain). Some of their examples are highly speculative, while others such as territoriality ring true. Individuals in adjacent territories develop stable boundaries ('cooperation'), but any attempt by one individual to encroach is met by aggression by the other. In contrast to this dominantly *tit for tat* behavior with the same individual, one-time encounters with encroaching strangers are consistently met by aggression (*all defect*).

Tit for tat does have two weaknesses. First, a single accidental defection between two *tit for tat* players initiates an endless, destructive sequence of mutual defections. Second, a *tit for tat* population can be invaded temporarily by persistent cooperators. An alternative strategy – *win-stay, lose-shift* – copes with these situations more successfully [Nowak and Sigmund, 1993]. This strategy repeats its former move if it was rewarded by a high score (opponent's cooperation); otherwise, it changes its move. The strength of this strategy stems from the fact that cooperation by the opponent is more beneficial than their defection. *Win-stay, lose-shift* quickly corrects mistakes, and it exploits chronic cooperators.

It's incredible that we scientists make decisions – sometimes difficult, sometimes emotion-laden – based on strategies similar to those used by some single-celled organisms. Success of *tit for tat* and *win-stay, lose-shift* in computer games of Prisoner's Dilemma does not imply that these strategies are appropriate guides for interactions with fellow scientists. Experience shows that the extremes of total cooperation and total competition are also viable for some scientists, although the 'hawks' do take advantage of the 'doves'. Some doves react to being repeatedly taken advantage of by becoming either bitter or hawkish. *Tit for tat* seems like a more mature reaction to being exploited than does rejection of all cooperation.

Which strategy is best for science? Both cooperation and competition are stimulating to scientific productivity, and in different individuals either appears to be able to give job satisfaction by fulfilling personal needs. Communication of scientific ideas is clearly a win-win, or non-zero-sum game [Wright, 2000]. On the other hand, academic science is being forced into an overly competitive mode by the increasing emphasis on publication records for both funding and promotion decisions [Maddox, 1993]. Personally, I enjoy cooperation more and I unconsciously seem to use *tit for tat*, achieving cooperation most of the time without the sucker's disadvantage.

> "And though I have the gift of prophecy, and understand all mysteries, and all knowledge; and though I have all faith, so that I could remove mountains, and have not charity, I am nothing." [1 Corinthians 13]

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Science Ethics

Personal and professional ethics are not distinguishable; all ethics are personal. A scientist must make ethical decisions with care, not only because they affect self image but also because, as Sindermann [1987] has pointed out, scientific reputations are fragile.

Some rules of scientific ethics are universal, and others are subjective. All require personal judgment. Not all of the ethical opinions that follow can claim consensus. Another perspective, and one which has been subjected to much wider review, is given in the excellent pamphlet, "On Being a Scientist" [Committee on the Conduct of Science, 1989]. Scientists are not democratic; most insist on deciding personally whether a rule warrants following, rather than accepting the majority vote. Imagine yourself, for example, in the following situations; what would your decision be in each case?

Research project:

- You have just completed a study on the effect of X on Y. Nineteen of the twenty data points exhibit a very close relationship between X and Y, but something seems to be wrong with one data point: it is far different from the pattern. Should you omit it from your publication entirely, include it and explain that you consider it to be anomalous, or include it just like the other data?
- In your publication you cite relevant studies by others. Should you devote just as much discussion to studies that are inconsistent with your conclusions as to studies that are consistent?
- You have reached an insight inspired by reading a preprint, a pre-publication copy of a scientific article. Should you immediately publish the idea, giving credit to the preprint?
- For the paper that you are writing, should you include as authors people who have made useful suggestions? People who did only 5% of the work? People who did substantial work but disagree with your analysis or conclusions?
- Your graduate student has selected, carried out, and written up a project. You provided funding and guidance. What should the authorship be?

Research-related issues:

- Is it OK to make a personal copy of software, if you cannot afford to purchase it? Is it OK to buy one copy of a program, then install it on all of the computers in your lab?
- You are filling out a travel expense form. It forbids claiming an item (e.g. tips) that you consider to be a legitimate expense, or you failed to get a receipt for some item and you are not allowed re-

imbursement without a receipt. Should you increase some other category by an equivalent amount? Should you claim full per diem when your actual daily expenses were substantially less?

- You are writing the budget for a proposal. Knowing that the funding agency routinely cuts budgets by 10-30%, should you pad the proposal budget by 20%?
- A funding agency has announced that it seeks proposals on some subject. You are doing work on a quite similar subject. Should you submit a proposal, recasting your work in terms of the desired research? If funded, is it OK to continue in the original research area rather than focusing entirely on the area desired by the funding agency?
- In submitting a proposal, you know that including one subproject will substantially increase the chances of proposal funding but that the subproject is not really viable. Should you include it anyway? Should you say that you will accomplish more than you realistically expect to achieve?

Applied vs. basic research:

- Is it selfish or even ethical to carry out a government-funded basic research career, if you think that your research has absolutely no practical value?
- If your research has potential practical applications that you approve of, should you suggest that your employer get a patent or should you start an independent company that gets a patent?
- If your research has potential practical applications that you disapprove of, should you reveal them?

Every ethical decision must be weighed personally and subjectively. Before making a final decision on any ethical issue, it is worthwhile to consider the issue from the standpoint of Kohlberg's [1981, 1984] criterion for mature moral judgment: *does the judgment hold regardless of which position one occupies in the conflict?* It may be worth reviewing your decisions on the ethical questions above, this time pretending that you were a person affected by the decision rather than the one making the decision. Kohlberg's criterion sounds almost like a generalization of "Do unto others as you would have them do unto you." The habit of applying Kohlberg's criterion is analogous to the habit (or skill) of objectively evaluating the effect of data on various hypotheses, without regard for which hypothesis one favors [Kuhn et al., 1988].

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Verbal, if not always behavioral, unanimity prevails on three ethical issues: fraud, intellectual honesty and theft of ideas.

Fraud and falsification of data are so inimical to scientific method that almost never do scientists succumb to their lure of quick rewards. Even a single case of scientific fraud, when publicized, does unimaginable damage to the credibility of scientists in general, for the public cannot confirm our findings; they must trust them. Fraud also slows the advance of a scientific field, for experiments seldom are exactly replicated, and fraud is not suspected until all alternative explanations have been eliminated.

> "The scientific mind is usually helpless against a trained trickster. Because a man has mastered the intricacies of chemistry or physics is no qualification for him to deduce how the Chinese linking rings, for instance, seem to melt into each other, passing metal through solid metal. Only a knowledge of the rings themselves can reveal the secret. The scientific mind is trained to search for truth which is hidden in the mys-

> teries of nature -- not by the ingenuity of another human brain.” [Houdini, 1874-1926]

Intellectual honesty must be a goal of every scientist. As we saw in Chapter 6, people tend to ignore evidence that diverges from expectations. We must fight this tendency; continued awareness and evaluation of possible personal biases is the best weapon. Intellectual honesty requires that we remain alert to conflicts of interest, whenever we review proposals and manuscripts, and wherever objectivity and personal advancement clash. Intellectual honesty requires that we face weaknesses as well as strengths of data, hypotheses, and interpretations, without regard for their origin, invested effort, or potential impact on our beliefs.

“Thou shalt not steal,” and the currency of scientists is not money or objects but ideas. **Intellectual plagiarism**, the attempt to take credit for the ideas of others, is clearly unacceptable, but its boundaries are indefinite. Most scientists feel that:

- It is not OK to initiate a research project inspired by communications from another scientist in a letter, conversation, lab visit, or preprint, unless the other scientist has specifically encouraged you to do so. Ask permission, but weigh the other’s response to decide whether they really favor your jumping in or they simply feel obliged to say yes.

- It is OK to initiate a research project inspired by another scientist in a scientific talk for which abstracts are published. One should refrain from publishing a manuscript on this project, however, until the other scientist has published.

- It is not OK to let your research plans be affected in any way by either a proposal or a manuscript that you have been sent for review.

- It is OK to jump into a research area as soon as it has been published. The authors have no right to keep the field to themselves, and they do have a head start.

- When mentioning a previously published idea in a publication, reference the originator unless the idea has become common knowledge.

Intellectual plagiarism is more often suspected than deliberately practiced. Ideas frequently stem from interactions with others. In such cases, the combination of two perspectives deserves credit for development of the idea, not the person who first verbalizes it. Perhaps the idea is not even verbalized during the discussion, yet one of the individuals later ‘realizes’ the idea when solitarily thinking about the subject. Menard [1986], reviewing the formative days of the geological paradigm of plate tectonics, found that simultaneous ‘independent’ discoveries were remarkably common.

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Publication

> “To study, to finish, to publish.” [Benjamin Franklin, 1706-1790]

Communication of results, particularly via publication, is an essential part of a scientist’s life. I could describe the highly ritualized design of most modern publications: introduction, experimental techniques, observations, and conclusions. I am more intrigued, however, by the contrast between publications, which are dry and rational, and publication experiences, which can be heavily emotion-laden. Let us examine briefly the publication experiences of some of our greatest scientific forebears: Euclid, da Vinci, Newton, Darwin, Mendel, and Einstein.

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Pythagoras founded Greek mathematics and especially geometry in about 550 B.C.. He and his Pythagorean school made the geometry one of the greatest accomplishments of Greek science. For systematically expounding and expanding Pythagorean geometry, however, we owe thanks to Euclid. In Alexandria in about 300 B.C., he wrote Elements of Geometry, and until recent years it was the most translated and copied book in history except for the Bible [Bronowski, 1973]. Many famous scientists in these two millennia thanked Euclid's book for showing them the beauty of what Pythagoras called the 'language of nature'.

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Leonardo da Vinci (1452-1519) combined the eye of an artist with the curiosity and analytic ability of a scientist. He epitomized the breadth and depth of the Italian Renaissance, by the scope of subjects and the novelty of perspectives in his notes. He was self-taught, with no intellectual training and therefore minimal limiting dogma [Goldstein, 1988].

Unfortunately, Leonardo made absolutely no contribution to contemporary scientific knowledge. He did not interact with scientists and he did not publish anything. His now-famous notes were private, written backwards to prevent casual reading by others. If a researcher publishes nothing and thereby makes no contribution whatsoever to the field of science, can that person even be called a scientist? Such questions are as fruitless as the question of whether a scientist-administrator is a scientist. Certainly Leonardo was an inspiring example to later scientists. Certainly Leonardo's lack of scientific communications to his peers was a heartbreaking loss to science.

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The greatest scientific book of all time is Principia Mathematica, completed by Isaac Newton in 1687. Newton's paradigm of the physics of motion united terrestrial and planetary motions with simple mathematical laws. He elegantly demonstrated the ability of theoretical physics to derive precise predictions of empirically observable phenomena. Yet Newton was so insecure and so incapable of dealing with the criticisms of others that he nearly failed to make his findings public. He completed much of the work of Principia many years before publishing it. Only Edwin Hubble's constant encouragement and partial financing eventually compelled Newton to produce Principia.

Twenty years earlier, when Newton began his work on gravitation, he developed the calculus. Rather than publish calculus, he kept it secret, using it to make several discoveries but then couching the presentation of these discoveries in ordinary mathematics. In about 1676 Gottfried Leibniz developed calculus independently. Newton, convinced that Leibniz had somehow stolen the idea from him, started a bitter feud.

Although Newton was undoubtedly one of the most brilliant scientific minds in history, his insecurity fostered arrogance and prevented him from distinguishing scientific criticism from personal criticism. He was ridiculed, and he responded by trying to discredit and destroy other scientists. Personal weakness damped, at least temporarily, his scientific impact. Fortunately, he did publish.

* * *

Alfred Russel Wallace and Charles Darwin independently discovered the theory and mechanism of evolution. Both recognized the phenomenon of evolutionary divergence, based on extensive observations as a naturalist (particularly in South America). Both spent years seeking a mechanism for this divergence, and both credited their discovery of that mechanism to reading Malthus.

Wallace called the mechanism survival of the fittest and Darwin called it natural selection. But Darwin's insight was in 1838 and Wallace's was in 1858.

Darwin, like Newton, was reluctant to publish but even more reluctant to see someone else get the credit for 'his' discovery. Fortunately, other scientists arranged for Wallace and Darwin to present their results in talks at the same meeting. Darwin's Origin of Species, published in 1859, stunned the scientific world with the weight of its diverse data. Its conclusions were so radical that overwhelmingly compelling data were essential. Darwin left the task of arguing the case to others.

Wallace is largely forgotten today, but he had experienced far greater disappointment than seeing Darwin receive much of the credit for the theory of evolution: after spending four years collecting animal specimens in the Amazon, he lost everything when the ship home caught fire.

> "With what pleasure had I looked upon every rare and curious insect I had added to my collection! How many times, when almost overcome by the ague, had I crawled into the forest and been rewarded by some unknown and beautiful species! How many places, which no European foot but my own had trodden, would have been recalled to my memory by rare birds and insects they had furnished to my collection!
>
> "And now everything was gone, and I had not one specimen to illustrate the unknown lands I had trod or to call back the recollection of the wild scenes I had beheld! But such regrets I knew were vain, and I tried to think as little as possible about what might have been and to occupy myself with the state of things which actually existed." [Wallace, 1853].

To Wallace, 1858 brought two joys: he solved a problem that had obsessed him for years, and he was personally responsible for the public awareness of the revolutionary concept of evolution. The most important thing that he brought back from South America was in his mind, not in flammable boxes.

* * *

Gregor Mendel undertook and published one experiment in his life. He used measurements of characteristics of sweet peas to lay out the basic pattern of genetic inheritance. The results overthrew the conventional theory that offspring inherit traits intermediate between their two parents; they demonstrated instead that offspring inherit each trait from only one parent, in predictable integer proportions.

"Mendel published his results in 1866 in the Journal of the Brno Natural History Society, and achieved instant oblivion. No one cared. No one understood his work" [Bronowski, 1973]. He picked an obscure journal, he failed to distribute copies of his paper to biologists, he was a monk rather than a professional scientist because he had flunked out of the university, and his research was before its time. Thirty years passed before biologists were ready to appreciate Mendel's paper.

* * *

Paradigm change can be explosively rapid on the time scale of evolution of a scientific field, yet ploddingly slow on the time scale of an individual scientist. While working full time at the patent office in 1905, Albert Einstein published five revolutionary papers: light quantized like particles rather than waves, diffusion-based estimates of the size of molecules and of Avogadro's number, Brownian motion (a final confirmation of the existence of atoms), the special theory of relativity, and conversion of mass into energy.

He later submitted the diffusion paper to the University of Zurich as a potential doctoral thesis. It was rejected as too short; Einstein added one sentence and resubmitted it, and it was accepted. But when in 1907 he wanted to leave the patent office and become a university lecturer, he needed to have an approved inaugural thesis. He submitted his 1905 paper on special relativity to Bern University; the paper was rejected as ‘incomprehensible’ [Hoffmann, 1972].

* * *

In considering the publication examples above, there are lessons that I accept intellectually but do not fully practice. I have not always published my research results. Nor have I always sold my results successfully. These examples increase my motivation to complete projects by publishing effectively. By revealing familiar personality traits, these examples increase my sense of community with past and present scientists.

* * *

A Scientist’s Life: Changing Motivations

Career motivations, within science or other professions, are not static. They evolve – sometimes radically. For a significant proportion of scientists, parts of the composite scientific life below may be familiar.

She chose science, or was chosen by it, while she was a child. Through childhood and undergraduate years, her fascination with science was a love of learning how the world works. Books, including textbooks, were the road. ‘Facts’ were collected uncritically and enthusiastically.

> Naturalist Edwin Way Teale was six years old when he first experienced, in a patch of forest, the fascination with nature that guided his life. In later years he tried unsuccessfully to refind that patch of forest, but the power of the initial experience remained with him:
>
> “For me, the Lost Woods became a starting point and a symbol. It was a symbol of all the veiled and fascinating secrets of the out-of-doors. It was the starting point of my absorption in the world of Nature. The image of that somber woods returned a thousand times in memory. It aroused in my mind an interest in the ways and the mysteries of the wild world that a lifetime is not too long to satisfy.” [Teale, 1959]

In graduate school, her motivations changed:

> “Enough of Science and of Art;
> Close up those barren leaves;
> Come forth, and bring with you a heart
> That watches and receives.”
> [Wordsworth, 1798]

After two decades of experiencing science through textbooks, she found a compelling alternative to texts: personal scientific discovery. Second-hand knowledge paled by comparison. The interpretations of others were subjective and required personal evaluation, mainly on scientific grounds. Observation and insight were an intoxicating combination. Competing and being first were part of the game.

In her thirties and forties, being first almost became the game. Recognition brought responsibilities that were essential to science. Time was short: it was more efficient to advance science through administration, management, and the training of students. Students took over the time-consuming data collection, but her scientific planning, data interpretation, hypothesis generation and insight continued unabated. Recognition and power brought their own rewards. The opinions of others, like dependent variables, could be modified to achieve her objectives.

Love of science seems to be universal among new scientists. Yet it fades in some scientists, particularly those who become managers and administrators. Perhaps individuals move from research into administration partly because of waning thrill of scientific research. Perhaps they move first, as reluctant draftees who are called on to serve a need, and they find later that love of science is being supplanted by fresher job satisfactions such as recognition and power. Failures we all can afford. The cost of success, for many, is loss of wonder.

> "If I would be a young man again and had to decide how to make my living, I would not try to become a scientist or scholar or teacher. I would rather choose to be a plumber or a peddler in the hope to find that modest degree of independence still available under present circumstance." [Einstein, 1954]

Few of these motivational changes were based on systematic strategic planning of her career. More often she simply reacted to the many victories, frustrations, and emotional fireworks of day-to-day life. Yet she perceived the true importance of these when suddenly she faced her own mortality.

> "Sometimes one finds in fossil stones the imprint of a leaf, long since disintegrated, whose outlines remind us how detailed, vibrant, and alive are the things of this earth that perish." [Ackerman, 1990]

During the Cuban missile crisis, we faced the world's mortality. After the crisis, we reassured each other, saying "I'm glad that's over." We returned to our old lives but found that we had somehow changed. Facing mortality changes one ineffably: the critical becomes trivial, and new priorities emerge. In the blazing light of awareness of death, the inessential and peripheral are burned away. Few things remain: love and living science are two.

Between now and my death is an opportunity. How shall I use it?

> After the albatross was killed, and before it was avenged:
> "The fair breeze blew, the white foam flew,
> The furrow followed free;
> We were the first that ever burst
> Into that silent sea."
> [Coleridge, 1798]

* * *

Process and Product

They say that Tantalus was punished by the gods, doomed to see a branch of fruit tree waving in the wind just beyond his reach, doomed to see the waters retreat from him each time he dipped his palm to drink, and thus consigned to be forever hungry and thirsty. Millennia later, the Buddha sat beneath a bo tree, determined to remain there until he gained knowledge. Both were tantalized by their objective; only one eventually embraced the path itself, learning the archer's skill of knowing when to pull and when to let go. Today, eager to quench our appetites, we scientists grasp for the same fruit of knowledge.

> “I dreamed that I floated at will in the great Ether, and I saw this world floating also not far off, but diminished to the size of an apple. Then an angel took it in his hand and brought it to me and said, ‘This must thou eat.’ And I ate the world.” [Emerson, 1840]

“Knowledge is our destiny,” said Bronowski [1973], and sometimes I am similarly goal-oriented in expressing my motivation toward science: I accumulate facts in hopes of finding understanding; I accumulate understandings in hopes of finding wisdom. Certainly these are aspects of my drive for living science, but perhaps the ends are merely a justification for the means. I think that Joseph Campbell [1988a] perceived a deeper obsession in his parable of the ‘motivation’ of the grass in a lawn:

> The grass grows, and yet every week or so a human comes along and ruthlessly mows it, annihilating all of the week’s progress. Does the grass think, “Oh, for Pete’s sake, I give up!” Of course not. For the mower, as for the mown, it goes on, toward ends unknown.

> “It bothers some people that no matter how passionately they may delve, the universe remains inscrutable. ‘For my part,’ Robert Louis Stevenson once wrote, ‘I travel not to go anywhere, but to go. . . The great affair is to move.’ . . . It began in mystery, and it will end in mystery, but what a savage and beautiful country lies in between.” [Ackerman, 1990]

[Watterson, 1993]

Those who are living science love the *process* of science -- the unique synergy of control and freedom, of skepticism and innovation. They love to use all of the scientific methods and try to dodge their pitfalls. Only rarely does the lightning flash of insight course through them, more often they feel the satisfaction of a successfully diagnostic experiment, and daily they overcome minor hurdles.

> At times, when I lived in Alaska, the brightness of the night sky kept me awake. Last night, its darkness did the same. How can the night sky be dark? If the universe is infinite, then shouldn’t it be uniformly bright, lit by an infinite number of stars in every direction? This ‘dark-sky paradox’ has puzzled astronomers for more than a century, and it has been ‘solved’ more than a dozen times [Gleick, 1992a]. The modern solution begins by reminding us that what we see in the night sky is the photons currently reaching our eyes, recording events that happened on different stars light-years ago. And how far back in time can we see? No farther than the 12 billion-year-ago big-bang origin of the universe. Stars more than 12 billion light-years away are invisible to us today, because the light hasn’t reached us yet. It goes on, toward ends unknown.

I'm no astronomer, but still I wonder. Didn't Einstein show that two stars cannot move apart faster than the speed of light? How, then, can the unseen stars be so far away? Who can relax and sleep, if the universe is breaking its speed limit? Is the universe infinite, and are those unseen stars there in the black portions of sky? Walt Whitman [1892], as usual, had the keenest vision: "The bright suns I see and the dark suns I cannot see are in their place."