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The Ethical Scientist: An Old-Fashioned View

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INTRODUCTION

Don Crothers

This article is dedicated to Don Crothers, who was the epitome of an ethical person. He was always kind, considerate, honest, and of course an excellent scientist. In 1956 he was an undergraduate and bursary boy at Yale. Bursary boys received financial support; in return they were assigned to do work in the library, or in dormitories, etc. Don's work was in the laboratory of Professor John Kirkwood where I was a postdoc. I studied enzyme kinetics of unnatural substrates, including the hydrolysis of ethyl lactate by chymotrypsin. As the Michaelis constant is high (~ 0.2 M) for this substrate, we needed to use high concentrations of ethyl lactate to get results. A puzzled Don would sometimes ask me why the reaction mixture had suddenly become turbid. Apparently the enzyme had precipitated in the high-substrate solvent. I finally published some of this work¹ in 1958 at Berkeley; Don was acknowledged.

After receiving his B.S. in chemistry at Yale in 1958 and a B.A in biochemistry at Cambridge in 1960, Don went to UC San Diego for his Ph.D. with Bruno Zimm. In 1964 he returned to Yale as an assistant professor. His main research was on helix-coil transitions in biopolymers, particularly DNA, and on binding of small molecules to DNA. At Berkeley, I had been studying hypochromicity of DNA, absorption of nucleic acid bases, and optical rotatory dispersion of dinucleoside phosphates. Victor Bloomfield, who had shared an office with Don at UC San Diego, spent time at Berkeley in 1967. He had been an undergraduate in my lab and now worked on hydrodynamics and scattering properties of DNA. He thought the three of us made a good fit to write a monograph on physical chemistry of nucleic acids. We signed a contract with Harper and Row, a publisher not familiar with scientific or textbook publishing, and began writing. By the time we had a finished manuscript, it was six years later and we had been through four different editors at Harper and Row. The last editor said that he couldn't possibly publish the book unless we agreed to a reduction in royalties and significantly shortened the manuscript. We reluctantly accepted his terms, but wrote into the new contract that after a certain number of copies were sold, our royalties would go up. The publisher happily agreed, and only printed the small number of copies of the "The Physical Chemistry of Nucleic Acids"² that kept our royalties low. However, we learned a lot by writing the blue book, and we hope that others have benefited from it as well. Our goal, in the words of James Joyce, was to write a book that was read by one person a million times rather than one read by a million people. Eventually we decided it was time to upgrade the science, but none of us wanted to spend the time. The obvious way to do this was to find other people to do the work. We therefore convinced John Hearst, Peter Kollman, Doug

Turner, and David Wemmer to contribute sections or chapters to the new version. Don's chapters on DNA interactions with ions, drugs, and proteins were the longest and most up to date. Every time a new paper on protein-nucleic acid interaction appeared Don would delay his manuscript in order to include it. Meanwhile our co-authors were complaining that their contributions were becoming outdated. Twenty five years after the first edition we finally published "Nucleic Acids: Structures, Properties, and Functions"³ in 2000. We still receive royalties from this book.

In the years between the first edition of our work and the second, RNA became increasingly important. Tom Cech discovered RNA enzymes, ribozymes (1982); Andrew Fire and Craig Mellow discovered RNA interference (1998); many regulatory functions of RNA were established. The idea that RNA was the critical biological molecule in the origin of life had been suggested in the 1960's by Francis Crick and Leslie Orgel. The discovery of ribozymes gave this idea credence, and led to the hypothesis of "The RNA World": The ability of RNA to replicate and catalyze led to the production of DNA and protein, and to the evolution of life. It will be difficult to disprove the RNA World without reproducing the prebiotic environment of earth, but in the meantime RNA will continue to amaze us with its many abilities today. Before we, and everybody else knew any of this, Don and I, independently, started working on RNA. Frank Martin and Olke Uhlenbeck, in Paul Doty's lab at Harvard, had synthesized self-complementary oligonucleotides, A_nU_n , which we called 69 molecules. Paul gave some to Don, and Olke brought some with him when he came to Berkeley as a postdoc. In 1971 we both published on prediction of secondary structure in RNA. Don's contribution, "Prediction of RNA Secondary Structure", involved a sophisticated statistical mechanics analysis of loop formation in oligonucleotides⁴. In the paper from my lab, "Estimation of Secondary Structure in Ribonucleic Acids",⁵ we assigned a +1 stability number to each A•U base pair, a +2 to each G•C, and minus stabilities to loops. The sum of these stabilities predicted the RNA secondary structure. We both continued work on melting temperatures and thermodynamics of RNA oligonucleotides, and in 1973 our two labs published jointly "Improved Estimation of Secondary Structure in Ribonucleic Acids"⁶. The improvements were two-fold. We had quickly learned that assigning free energies to individual base pairs could not fit the data, but that the contributions of nearest-neighbor base pairs were consistent with experiment. Don's group learned that the contributions of single-stranded regions to free energy were different for hairpin loops, internal loops, and bulges. Combining these results gave us a good start in actually being able to predict RNA secondary structure from sequence. People, in particular biochemists and molecular biologists, have always asked why did you pick such a non-biological solvent (1 M NaCl) for your experiments. The answer is if we heated the oligonucleotides in Mg^{2+} to melt them, they would hydrolyze. Olke Uhlenbeck assured us that 1 M NaCl was equivalent to 10 mM Mg^{2+} , anyway. This assertion is approximately true for secondary structure, but is completely wrong for tertiary structure. Our 1973 paper, among its other benefits to science, apparently enthused Doug Turner, a postdoc at Berkeley from 1973–75, to acquire enough data to make RNA secondary structure prediction a reality (see his Chapter 8 in our second edition). His results form the basis of the many computer programs that predict structure from RNA sequence.

ETHICS

Don Crothers was a role model for many people. Thinking about how to remember him publicly, I decided to write an essay on ethics. We never discussed ethics, so I don't know his views, but I'm sure he would disagree with me, as you will, on some of my opinions. But I hope that this personal and biased essay will cause people to think and talk about ethics and ethical behavior.

Ethics is defined by Wikipedia as “... systematizing, defending and recommending concepts of right and wrong conduct ...”. NIH defines research misconduct as fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. I want to discuss ethics particularly in the context of science, with emphasis on situations where the ethical decision is not obvious. I hope that by analyzing and discussing specific situations, a general method of deciding between ethical and unethical behavior will become apparent. Obviously, we should all act ethically, whether at home or in the lab. I don't think most of us make a conscious decision to be unethical; we just do something without thinking clearly about the effect on others. Looking back, we may decide it was unethical. There are also some who deliberately cheat, steal, or lie, not caring whether their actions will damage others. I will not spend much time on deliberate unethical behavior, because it is rare I think, and because the people who do it probably won't read this article, and if they do, will not be deterred.

I taught a biophysical chemistry course at Berkeley for many years; it was mainly taken by pre-meds and a few bioscience graduate students. One year at the end of the semester, I was visited by a senior colleague in the department who told me that a foreign graduate student needed a B in the course to keep his fellowship. I checked and saw that the student was clearly in the middle of the C's. My colleague argued that a C meant that the student would lose his fellowship and probably would have to return to his country without finishing his graduate degree. I should not ruin a student's career because of one poor grade. I agreed to give him a B, not because I thought it was the ethical thing to do, but because I didn't have tenure and I didn't want to make an enemy in the department. Now I realize my action was unethical. It was unfair to the C-students who were not given B's, although most could surely present some personal problem that explained the C. The logical method to arrive at an ethical decision in your work is to separate the personal aspects from the professional ones. The student did C-work and deserved a C in my course. If a C would produce irrevocable harm, I could have assigned a grade of incomplete and had him take the final exam the next time the course was given. Instead, I felt sorry for him; I knew it would be better for me if I gave him a B, and I was unethical. The Dean or committee who made the decision about his fellowship was the proper agent to evaluate his entire situation and make a fair, ethical decision.

UNETHICAL BEHAVIOR

Let us first consider clearly unethical behavior: research misconduct defined by NIH. Do not do them.

Fabrication

Falsification

Plagiarism

Do not make up data; do not sabotage others' experiments, do not steal their results, do not lie about what you did. Besides the moral burden you will carry, you are very likely to get caught. Other groups will try to reproduce your results. We always hear about the important discoveries that were fabricated, such as organic polymers that could reproduce many semiconductor properties of inorganic materials⁷, but we don't learn about fabricated trivial discoveries. Maybe it isn't worthwhile to make up results that nobody will care about.

Some universities require that you publish at least one peer-reviewed paper each year to qualify for a salary increase. That surely provides a strong incentive to publish and if necessary to fabricate results. Graduate students and postdocs who are caught cheating often say that it was pressure from their research advisor that forced them into this behavior. If you are working on what you conclude is an impossible project, talk to your advisor and discuss other projects. If necessary, switch advisors, or departments, or even schools. I know it is a serious waste of time, but it is better than getting caught making up data.

The environment of the lab can encourage or discourage ethical behavior, although a poor environment is no excuse for fabrication. The mentor sets the tone. If the postdocs and students are collaborative, not competitive, then even impossible projects get done or are made possible. There should be pressure to do good science. That means everybody questions the accuracy of the results, and suggests proper controls. The honesty of the researcher is not doubted, but the possibility of errors is recognized. Critical experiments should be repeated by a different person, not because the first student is suspected of cheating, but because it is important that the result be robust and reproducible. When Tom Cech discovered ribozymes, it was necessary to repeat the experiments many times and in many different ways before he convinced himself and others that the results were correct.

Stealing is definitely unethical; if it involves money or other tangibles it is also illegal. Stealing intellectual property is just as bad, but it is often not illegal. It is acceptable to use one or two sentences from a publication, but if you use a paragraph or more without citation, it is misconduct. I don't understand the excuse attributed to foreign students who complain that their English is not too good, so that when they see their idea expressed in an English publication, it is alright for them to use it. Nobody is hurt and the science in their paper is made more understandable and more useful. Of course the person whose words were stolen will feel differently. Anyone who understands what someone else has written, can present it in their own words.

GRADUATE STUDENTS

The first year of a graduate student's life is particularly difficult and stressful. You are taking courses, teaching courses, starting research, and maybe starting a new relationship. Any of these endeavors can be a full time job. You must learn to prioritize, and to learn to say no to some requests. I think courses should have the highest priority no matter what your research director says. You have to get at least a B in your classes, fulfill your duties as a

teaching assistant by learning the material before your students, attending office hours, grading exams, and explaining to the premeds why they don't deserve 8 points instead of 6 on the exam problem. When your course work and teaching are over, then research can be the highest priority. I also hope that your relationships and sanity can survive the stresses of the first year.

Before you can enjoy the joys of graduate school you have to be admitted, and you have to get in the best school, such as Berkeley. Obviously, good grades and good recommendations are important, but some students think further help is needed. An example was presented to me when I was chairman of the chemistry department. In 1980 we received a late application from Morehouse College, a school traditionally for black students. This was a time when the Department of Education was pushing hard for more minorities in elite graduate schools. Although we had already filled our quota of new students for that year, we offered him admission and flew him out to visit the campus. A full day of appointments with faculty and students had been arranged ending with a fancy dinner. I'm sure that I did not hide my surprise when I saw that he was white, red headed, and freckled instead of the black man I expected. As soon as he left my office I called the Graduate Division and asked if we had to honor our generous offer letter. I heard laughter in the background as my problem was explained, before they came back to me with the conclusion that yes, we were committed. I think that he accepted the offer from MIT or Harvard, instead of Berkeley. At the time I thought that he had acted unethically by letting us assume he was black, and that he would write a book about his experiences, but I never heard anything further. Now I see that he did nothing wrong; we were careless. If he would be admitted only because of his race, we should have telephoned one of his references and asked. He never said in his application what his race was. Maybe he went to Morehouse because it was near his home, not because it would give him an edge in his graduate school applications.

Courses

You should not cheat on exams. To reduce cheating, I always gave open book exams, which meant that you could bring any material you wanted for help on the exam. You weren't allowed to bring another person, or to copy from your neighbor. I think that now including internet access would be fine. In science, memorizing facts is not important; learning how to put the facts together to solve a problem is.

When you are the teaching assistant, part of your ethical behavior is to be knowledgeable and well prepared for the course. You will try to make the subject as exciting for your students as it is for you, but you will transmit the basics even to the students who don't care. You are fair in dealing with the students, whether they are reasonable or you find them obnoxious. You don't favor the charming ones, or penalize the argumentative, annoying ones. You definitely do not date any of your students.

Research

When you start research you will need a lot of help, presumably your lab mates will provide it. You will repay the lab by helping the new students in following years. You will also help in general lab operations, but it is important not to be too willing. You don't want to be the

person who is always asked to remove old files from the hard drives, throw out samples from five years ago, order glassware, arrange parties, collect money for presents, and deal with visitors. Be fair and ethical to yourself, not only to others.

Your research project, at least at first, will probably be assigned by your mentor. You may be part of a team of students attacking a problem with different techniques and different points of view, or you may work by yourself. In either case you should have ideas about what experiments to do now, and what future directions the research should take. Write down your ideas, and discuss them with others; ideas are improved by being challenged. Don't be afraid that someone will 'steal' your idea; most of us are not Einstein with a truly original idea. Ideas about experiments are often obvious; the main question to decide is what their priority should be. If you have evidence that someone in the lab is taking your ideas and presenting them to the lab director as their own (definitely unethical behavior), then tell your ideas to the director directly, and tell everybody else in the lab also. However, ideas are difficult to label and to assign ownership, so do not be quick to assume stealing. In a discussion many people contribute, and we tend to forget the input from others.

As you do the research you will keep detailed notebooks of what you did, what you observed, and what you concluded. All primary data such as spectra, elemental analyses, and images must be saved, although most of the results will not be used further. You select the best data for publication; as long as it correctly represents your results. Spectra are usually filtered and smoothed to show the peaks most clearly. However it is unethical and poor science to delete peaks you don't like. Just because the software on an NMR instrument has a 'delete peak' function does not mean that you should use it. Impurity peaks in your spectrum must be left, but labeled as such.

It is acceptable to omit data from replicate experiments, if you have a definite criterion for omitting them and you state the criterion in the publication. Here is where comprehensive notebooks are helpful; they may remind you of what was different in the experiment where an outlying result was obtained.

Ethical behavior in doing research is just good science. You want your results to be correct, and you want to be sure other people can reproduce them. Readers of your paper may disagree with your conclusions, but they will know how you reached them.

Life

Scientists are also people. You will have responsibilities to: parents, spouses, partners, children, siblings, friends. Don't ignore them. Be honest and open about what you can and can not do. Do not make commitments that you can't possibly keep, but do put aside time for your family and friends. You may turn out to be a better scientist for it.

POSTDOCS

Postdocs' duties in research are identical to those of graduate students with a few added responsibilities. In a large laboratory you may be the leader of a subgroup. This means you must help train the graduate students in how to do research; of course the best way to do this

is by example. When the student makes a mistake in design or execution of an experiment; they are told their error and encouraged to quickly repeat the experiment. Wasting time and money on an experiment is like falling off a horse. It is important to get back on and regain your confidence. All students should be encouraged to do the best work they can. The research director of the lab should be told of the contribution of each student.

FACULTY

First, you need to obtain a job. The most important determinant in getting a job is recommendations. As a graduate student and postdoc, you had to convince your mentors that you were competent, dependable, and ethical. Your application should describe your accomplishments (presentations, awards, publications), but not embellish or exaggerate them. The quality and number of your publications are important, but, unfortunately, you can't assume that the recruiting committee will read any of them. They will count them, but you shouldn't pad the list by adding a number of titles *in preparation*. Also remember that submitting an article to a journal does not mean it is *in press*.

Awards are more difficult to judge. We took a seminar speaker to dinner at the Hayes Street Grill in San Francisco, and presented him with the Hayes Medal made of cardboard and aluminum foil on a string. A few years later he sent us a clipping from the local paper reporting his Guggenheim award and listing the Hayes Medal from the University of California among his previous honors.

Once you get an offer, you must choose to accept it, to reject it, or to request more time for a decision. You may have an offer from Harvard, but you are still hoping that Berkeley will come through. You don't reject Harvard; you ask for more time while trying to learn what Berkeley will do. However, you don't accept Harvard's offer, and then when you finally get Berkeley's offer, tell Harvard you changed your mind. We heard once from a chemistry department chairman of a competing university (not Harvard) that our new hire had already accepted their offer. The chairman rightly questioned the ethics of this person, and asked if we really wanted to have him on our faculty. We were stuck with him at that time, but his science was also not great; he did not get tenure.

Teaching

Some people give the impression that they were born to be great teachers. They love to perform on stage, and they are happy to describe their research and other interests. They are enthusiastic about what they know, and they know their subject deeply enough to communicate it to the beginner as well as the expert. Richard Feynman could entertain and educate an audience including high school students and theoretical physicists. Most of us are not Feynman, but we can be competent teachers by knowing our subject and being conscientious about organizing our lectures to match the level of knowledge of the students. As always we are fair to the students, the teaching assistants, and any colleagues involved in the course. That means we don't place excessive burdens on any of them. If we must miss a lecture for any reason, we ask a colleague to substitute with the clear understanding that we will reciprocate. When a student asks for a special treatment, we should have a definite criterion for saying yes or no. It is too easy, and usually wrong, to decide based on how

generous you are feeling that day, or how good the student is at presenting their case. Remember that when you are generous to one student, you may be unfair to the rest of the class. There are no rules, but always think of the rest of the class as well as the pleading student.

Students who come to each office hour and who ask for help in understanding the material, rather than asking how to get a better grade, are rewarding. It is flattering to hear of their interest and their enthusiasm for your subject. However, you might also contact the students who don't ask for help, but obviously need it. If they don't respond, at least you tried.

Again remember that you don't date the students or the teaching assistants.

Research

As the head of a laboratory you have two responsibilities: to make new discoveries and to produce the next generation of scientists. The responsibilities are not conflicting. Some people think that at least in the short run (before tenure), they shouldn't worry about the students. It is important to generate data and publications, whether the students learn or are trained, is irrelevant. Some PI's continue this behavior throughout their life. This is clearly wrong, both ethically and scientifically. A motivated, well-trained student will do better science for you, will encourage new students to join the lab, and may even fund a professorship in your honor after their startup company is bought by Microsoft.

It is important to give each student equal opportunity. Some will be smart; some not so much. Their education and knowledge will be very different. It will be helpful if their first project is one where methods are learned; the long-term project can come later when the student and mentor know each other better. I have learned that the students who succeed the best are not the smartest or best educated. They are the ones who care the most about their research. They don't necessarily work 24 hours a day, seven days a week, but they always do think about their work and persevere to a satisfactory conclusion. They learn whatever they need to learn; they don't say that's molecular biology, or that's theoretical physics and I'm a chemist.

You should educate and train each student; you should also learn from the student. A theoretician told me that he met with each student once a week. At first when they met, the student would copy his notebook. Later he would copy their notebook; they learned together. When the student is ready to leave, you write an accurate letter of recommendation. One professor used to recommend a student every year for a Berkeley faculty position as the best student he had ever had. It is possible, but unlikely, that the claim was true. The impact of his recommendations however quickly fell to zero.

In 1494 shortly after Columbus discovered America, the Pope divided South America between Portugal and Spain. He wanted to be fair, but not knowing the extent of the continent, the meridian he chose gave the eastern tip of Brazil to Portugal and the rest of South America to Spain. No country, including Spain and Portugal, paid any attention. This papal action reminds me of the pronouncement some research directors make to a student who is leaving their lab for an independent position. Permission is given to work only on

some problems in a research area; the mentor reserves the rights to the others. Neither knows the size and scope of the research area, neither will avoid new interesting opportunities because of the agreement, and everybody else will not know or care about their agreement. When a student leaves your lab, it is better to state what you plan to do in the future and to ask that they inform you of what they are doing so as to avoid wasteful overlap in effort.

Obtaining Funding

Before you accept students and start research, you need to obtain funding. Most research support comes from the government, but private companies, or even individuals may be induced to support your work. If the money comes from the government, there is usually a time lag of about a year between the application and the funds. How ethical is it to do research not mentioned in your proposal? I think it is both ethical and scientifically valid to pursue more productive opportunities within the scope of your proposal. If you propose to study a protein enzyme, and find that the reaction seems to be catalyzed by an RNA, nobody will fault your ethics for studying the RNA. They may conclude that you wasted your time and their money because only proteins are enzymes, but their response will mainly be to not renew your grant. Government agencies are becoming more sensitive about overlap of support from two grants to the same project. They apparently worry that you may discover a cure for cancer, and NSF and NIH will end up fighting over credit for your discovery. I do not think it is unethical to obtain money from different sources to support similar projects, such as structure and function of DNA, and function and structure of RNA. Some people think structure determines function, other of us realize that in biology, function determines structure.

Choosing Projects

Choosing the research projects to work on and to suggest to your students is probably the most important determinant for the success and impact of your science. You get ideas for research by reading, attending meetings and seminars, talking to people, and just thinking or dreaming. You should then prioritize the possibilities depending on your interests, your assessment of the importance of each project, and your resources in funding, equipment, and students. Definitely write down your ideas. It is amusing and educational to read the ideas you wrote five years ago. You will judge some absurd and you will wish you had pursued some of them earlier.

If you read a proposal, or a visitor tells you their plans in detail, and you immediately start a project on that subject, it is clearly unethical. But what if you are told an idea you had five years ago that you did not pursue? After being reminded about it now, you may decide it deserves higher priority. If you are talking to the visitor, you should mention that you have considered this project earlier, and that maybe you should collaborate. Describe what experiments you have in mind, and see how they compare with those of the visitor. If you referee a paper that describes a project close to one you are doing, obviously you don't trash the work, so that you will have time to submit your own paper. You also don't tell your student to change directions or speed up one aspect of the work, without revealing that you learned this from a potential competitor. What you should do is contact the author—I know

this is not recommended by the editor—and state that you admire the work and want to use it in the following way. Similarly, if you learn of the project from a grant proposal, you should contact the writer. You may already have the required specialized equipment that the grant is asking to buy; an offer to collaborate could be mutually beneficial.

Before you write a grant proposal, you of course read the literature to learn what has been done before. If you see that some other group has the necessary facilities for your favorite project, you might contact them and ask to visit their lab and collaborate. Some people will advise you not to reveal your idea, because an unethical lab director will steal it, and do the experiments without you. I disagree. The best way to avoid theft of your ideas is to broadcast them widely. Tell everyone in the field, including the students in the lab you hope to visit. The way to get proper credit for an idea is to publish it. If it will take awhile before you can do this, do not try to hide it; disseminate it.

Doing the Research

You may learn from a grant proposal that another group has already done some experiments that your student is trying to do; you shouldn't let your student keep struggling. Instead, you contact the writer and ask permission to tell your student. You have acted ethically to the writer and your student, and the writer should act ethically by giving permission.

Some PI's tell their students never repeat to anyone outside the group what we are doing, because they will get an advantage from our creativity and hard work. This is a wrong conclusion. The whole point of science is to make new discoveries and to tell the world. Yes, publish your results, but don't keep them secret before publication. Sometimes someone will tell me I am planning to do a certain experiment, but don't tell anyone. I always say don't tell me, because I will forget the constraint and will pass the information on.

We assume that your research has been done carefully, competently, and honestly. Proper controls were done and described. The results are interpreted reasonably and without excess claims of their importance. Unfortunately, some scientists think that unless a paper is published in the highest impact journal, it is clearly not important. Some think that in order to have a chance of publication in an elite high-impact journal they should make outrageous claims in the abstract. This will get the submission sent to referees, who they hope will praise the results and recommend publication after the offending sentences in the abstract are deleted. Everyone should recognize that fighting with reviewers first with *Nature* (2014 impact factor = 42.351), then with *Science* (impact factor = 31.477), before finally publishing in *J. Am. Chem. Soc.* (impact factor = 11.444) can waste a year of valuable research time better spent doing new experiments. Clearly, it is unethical to delay publication of presumably valuable science, and to waste tax-payer money while doing so.

One of the most contentious aspect of publishing a paper seems to be the number and order of authors. The customs can be very different in different fields and different labs. Whose name should be on the paper? I think the minimum requirement should be that every author know the paper well enough to defend any part of it against criticism. In some Institutes the director may require that his name be on all the papers published. I disapprove, even though

the director may read the paper and understand the science. Sometimes the senior author may add a name because the student has worked hard on another project without results; a publication will help the student get a job. It is kind to the student, but unfair to the people who actually deserve to be co-authors. When it comes to deciding the order of names, arguments and hard feelings increase. Usually the PI's name is last, but who is first is not obvious. If you ask three or more authors what fraction of the research each contributed, you will find that more than 100% work was done. We all know how hard we worked and how many hours we spent; we don't know what the others did.

LIFE

Presumably you will have a life outside of the laboratory. You will probably face some of the usual problems of buying a house and paying the mortgage, of sending your children to MIT, supporting and caring for your aging parents, and so forth. You will need money. The common method to get promotions and raises in academia is to get job offers from more prestigious universities—maybe that is why you only want high-impact journals. You should only allow yourself to be recruited if you would actually be willing to move. At least you should tell them that your chances of moving are less than 1%, but if they want you to visit anyway, you are willing. If you allow the process to go to completion and do get an offer, don't delay your response for months while you await the counter offer from your school. One senior professor kept telling Berkeley that he had his acceptance letter in his hand, but somehow it was never sent.

It is obvious that you should not date students in your research group. But also in general you shouldn't date ex-students. Once a student has been in your group, he or she belongs to you for life. If they do well you get credit, if they are poor scientists you get blamed. You will be asked to write recommendations for their tenure, promotions, and prizes. This asymmetry of power can be detrimental to a relationship. An exception to no dating can be made if you plan a more long-term relation, even marriage. I make this exception because after the death of my wife of fifty years, I started dating a postdoc in the lab. We have been happily married for eight years so far; nobody expects unbiased recommendations from me.

CONCLUSIONS

My goal here has been to recognize and choose ethical behavior when the choice is not obvious. The title of this essay refers to an ethical scientist; it might better refer to a competent scientist, or a good person. A good, competent, ethical person will treat everyone and everything with respect and consideration. I chose science, as did many of my colleagues, because we didn't want to deal with people. I naively thought I could work alone in a laboratory and after a few years emerge with a great discovery; only salesmen, politicians, and MDs had to deal with people. I now know, as we all do, that research like any other endeavor requires other people. There are students, colleagues, administrators, and custodians to nurture, inspire, and placate. Ethical behavior means treating people fairly, but can be extended to inanimate objects: data, instruments, funding. We won't misuse or distort the data by only choosing the results we want. We won't damage instruments by careless actions or failure to provide maintenance. We won't waste money and time by designing

poor experiments, or ordering unnecessary supplies. These traits are characteristic of good scientists, and of good people. So being an ethical scientist just means being the type of person you would like everyone else to be, and the person you think you are.

This essay began by describing Don Crothers as an ethical, outstanding scientist. The conclusion is best recognized by talking to his ex-students. They are successful scientists who love him. It is clear that Don excelled in producing the next generation of scientists while discovering fundamental, new science.

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