PROFESSIONAL ETHICS, VOL. 4, NOS. 3 & 4

Case Studies for Teaching Research Ethics

Deni Elliott

Case studies have been proven to be an effective teaching tool for applied and professional ethics. They are provocative, allowing students the opportunity to vicariously experience moral problems as they arise in a practice or profession.

Case studies also allow students to try out different and more sophisticated ways of reasoning their way through moral issues. They require the student to confront the sort of controversy or conflict that arises in relationships among individuals in the lab, between individuals and the institution, or among individuals, institutions and the larger community. Controversy, or uncertainty as to which is the best alternative action, is the substance of the case discussion. According to one case teacher,

> One of the creative functions of conflict lies in its ability to arouse motivation to solve a problem that might otherwise go unattended. Major features of productive conflict resolution are, at the social level, similar to the processes involved in creative thinking. These include the development of conditions that permit the reformulation of the problem once an impasse has been reached; the concurrent availability of diverse ideas that can be flexibly combined into novel and varied patterns; and sufficient detachment from an original viewpoint to be able to see the conflict from new perspectives.[1]

A result of dealing with controversy in a public way is that students show preference for the more sophisticated modes of reasoning. According to Kohlberg,

> The Socratic view implies that, in a sense, knowledge of the good is always within but needs to be drawn out like geometric knowledge in Meno's slave. In a series of experimental studies, we have found that children and adolescents rank as "best" the highest level of moral reasoning that they can comprehend. Children comprehend all lower stages than their own, and often comprehend the stage one higher than their own and occasionally two stages higher, although they cannot actively express those higher stages of thought. If they comprehend the stage one higher than their own, they tend to prefer it to their own. This fact is basic to moral leadership in our society. Although the majority of adults are at conventional Stages 3 and 4, leadership in our society has usually been expressed at Stages 5 and 6, as the example of Martin Luther King suggests.[2]

An implication of preferring a more sophisticated mode of reasoning is that one will be motivated to employ the preferred mode.

The professor, along with controversy, is also an essential factor in the success of case discussions. The role of the professor is to focus the conflicts (Pederson, p. 151),[3] and to be an experienced guide in the discussion and proposed resolution. According to one master case teacher,

> student and teacher are in it together, exploring the place they have both resided all their lives. The teacher, perhaps, is more familiar with its general features, but not necessarily its details. So it would be a mistake for the teacher to take on the role of tourguide for visiting strangers. The enterprise is rather joint exploration of territory that is familiar to all parties, even if to varying degrees . . . . In case discussion, therefore, the role of the teacher is not that of expert or source of knowledge; it is that of facilitator and intellectual foil, assisting students in their collaborative deliberations and attempting to nurture in them the ability to handle ethical conflict effectively on their own, when the instructor is not around to monitor the conversation. The principal technique for accomplishing this aim is the reverse of the tourguide's; it is to ask questions. They must of course be the right questions, questions that introduce students to a certain path of inquiry, that keep the exploration moving in a productive

> direction, that press students to investigate areas not yet probed sufficiently, and so on. All with the outcome, one hopes, of mutual understanding, that is, understanding their shared ethical life together.[4]

Cases designed for discussion exist in a variety of styles. Three categories of cases for use in teaching research ethics are presented here: short vignettes, complex case narratives, and dialogues. Different styles of cases fulfill different pedagogical needs.

Short vignettes are designed to focus the students' attention on a particular ethical issue. While either based on published cases or on the observations and experiences of the case writer, vignettes provide at least one clearly identified ethical issue for discussion. Authors of the cases included in this article have refrained from using names such as Professor Prima Donna, or post-doc Donald Devious on the grounds that they tend to trivialize the case. While teaching and learning from cases is an enjoyable activity, students need to keep in mind that the intellectual exercise has the serious and important goal of making them more conscientious decision makers in the lab. Names that create a caricature of motivation reinforce the erroneous belief that ethical problems do not exist in real research laboratories.

Complex case narratives, while sometimes disguised to protect the privacy of participants,[5] are attempts to present actual cases in their contexts. Case writers examine primary documents and often interview principals in an attempt to give the reader a feel of how events unfolded. Students are challenged by these cases to separate the ethical issues from those of economics, law and prudence. They can see how seemingly insignificant decisions made early in a situation can lead to better or worse ultimate consequences.

The messy factors of reality are the essence of these narratives. According to Winston, "only the detail of a real case situates ethical conflict in such a way as to encourage the exercise of moral imagination in the search for innovative solutions. Without knowledge of the constraints and opportunities present in the environment, reasoning is either sterile (having no real application) or artificial (producing solutions all too easily). In many instances, the initial effort will be to figure out what the problem is, ambiguities in the narrative, reflecting obscurities in the circumstances described, may hide what is actually at stake."[6]

Dialogue provides another approach in which students are encouraged to try on the different perspectives of those engaged in a problematic situation through role-play. As this approach is likely to be the most unfamiliar of the three, dialogue case authors here provide discussion questions to help focus the conversation after role-play.

Regardless of the style of case presentation, cases can be analyzed in a systematic way so that students gain expertise in considering the major aspects of ethical concern. A well-facilitated ethics discussion can travel in a variety of ways, but the following structure provides a checklist to ensure that no aspect of analysis in ignored.

Case Study Analysis Procedure

I. Identify the ethically problematic action(s) or potential action(s).

II. Determine what makes each action or potential action ethically problematic.

III. Determine who are the responsible individuals and institutions in each case.

IV. Determine the role-related responsibilities of each individual and institution.

V. Consider if each problematic action is consistent with role-related responsibilities.

VI. If not, consider what changes need to be made to bring actions in line with role-related responsibilities.

VII. If the action is consistent with role-related responsibilities, and still ethically problematic, consider if there are alternative actions that meet role-related responsibilities without produce the same degree of harm.

Short Vignettes

Truth in Asking[7]

Drs. Town and Kirby are writing a grant proposal together to support work derived from a previous collection. In reading Town's section,

Kirby notices that Town has described a series of critical experiments that are said to be "in progress." But Kirby knows that the lab is only now ordering reagents for these experiments and cannot possibly start the work for a month or two. Kirby raises the issue with Town, a more senior PI, and is told that this is normal practice in grant writing.

The Responsibility of Shared Workspace[8]

Anil Gandhi is one of a large number of post-docs in Professor Richard Yee's lab and is known for his energy and enthusiasm. Though he is in the first year of his post-doctoral fellowship, he was also Yee's graduate student and has a long and positive history at the institution. Terry Miller is a new post-doc in the lab of Marilyn Theobald, a colleague of Professor Yee. Theobald and Yee collaborate on a wide range of projects and share some space and equipment. Gandhi is currently collaborating with Theobald and is in the habit of taking the tools used to prepare the specimens and leaving them at his work space. His workbench is notoriously cluttered and messy with spilled chemicals and solutions.

Miller is learning the techniques for which Theobald's lab is noted and uses the same equipment that Gandhi is using. When it is her turn to use the equipment, Miller finds that it takes her almost an hour to retrieve tools and clean up the equipment so that she can begin her work. She is irritated. She has mentioned her aggravation to Gandhi, who laughs and says that he will try to do better. Yee and Theobald seem oblivious to the situation. Miller is concerned that the reliability of her research might be compromised by contamination with chemicals or bacteria from Gandhi's project or work area. But she doesn't want to be thought of as a trouble maker.

The Value of Work Space[9]

Dr. Jones has offered to provide lab bench space to a student who is not working on one of Jones' own projects. The student makes an important discovery that she plans to write up for rapid publication. Jones insists that he be an author on this and on any subsequent publications. The student protests that the ideas and work were hers alone. Jones implies that if he is not included as co-author, he will withdraw the sponsorship of her Ph.D. research and that she will lose her current lab space. Out of concern for her future, the student complies.

Plagiarism and Self Plagiarism[10]

Dr. Gorchek has asked a senior graduate student to look over a grant proposal that Gorchek has submitted to the USDA. In reading the proposal, the student recognizes passages taken directly out of an invited review article that she and Gorchek just had published in the *Annual Reviews of Immunology*, but the passage is not referenced. Another long passage looks familiar and the student discovers that it was taken from last year's annual report that had been sent to a different funding agency in connection with a different grant. The student is concerned and asks the post-doc for his opinion. The post-doc assures the student that plagiarism occurs only when one fails to reference or acknowledge someone else's work.

Who's an Author? Who Decides?[11]

Helen Green is a junior faculty member in the chemistry department; she entered as a post-doc five years earlier. She works in an area closely allied with her mentor, Joe Johnson. They have collaborated on a number of papers including a well respected review article. Green is concerned that her contributions to the field are overshadowed by Johnson's reputation, so she is especially pleased to be invited to present recent work at an international meeting and to contribute a manuscript to the proceedings.

When she receives the galley proofs of the article she has submitted, she is surprised to find that Johnson's name has been added as second author. From the departmental secretary, Green learns that Johnson had seen the paper on the secretary's desk just before mailing and had his name added. When Green asks Johnson about this, he apologizes for forgetting to mention it to her earlier. Green is upset. While the work described in the paper and presented at the meeting uses a technique Green and Johnson had developed together and builds on previous work that they had done in collaboration, the new data presented, the figures, and the text were done by Green independently.

Data Ownership when Mentor Relationships Sour[12]

Professor Smith introduces his graduate student Jim Walsh to Professor Jones. Jones has a good research idea and some financial support. Walsh develops the computer algorithms to implement Jones' idea.

When Professor Jones' data are processed, it appears that the idea works and that it has potential commercial value.

Walsh begins developing his Ph.D. thesis on the research and prepares a manuscript for submission to a journal. Jones refuses to allow publication and insists that the research be kept secret. Jones then has the university file for patent on the idea and its computer implementation, without including Walsh as co-inventor.

When Walsh protests, he is dismissed from the lab and forced to leave his computer program along with Jones' data. Walsh decides to pursue the research anyway and persuades another student to let him secretly copy the disks containing his program and Jones' data. Walsh then continues with the writing of his thesis.

Complex Case Narratives

Learning the Conventions of Research[13]

Dr. Vipan Kumar was a post-doctoral fellow at Cal Tech working on the molecular biology of autoimmune disease, in the laboratory of Dr. Lee Hood. The Hood lab is exceptionally large, containing more than 65 scientists, technicians and students. Work on autoimmune disease constitutes only a part of the lab's focus, but the area is very competitive and a group at Stanford was working along lines very similar to those at Cal Tech.

In late May of 1990, a senior member of the Hood lab, Dr. Dennis Zaller, approached Hood expressing concern about his repeated inability to replicate one important experiment that Kumar and Hood had published in the previous year. When they re-examined the figures in the publication, it became evident that one important figure, showing DNA patterns from several different and presumably independent cell lines, had been falsified. The same peculiar DNA artifact was seen in several samples, although the samples were described as originating from different cell lines. It appeared that the same sample had been used repeatedly, but falsely labeled in the figure legend.

Dr. Hood immediately alerted the department chair and other university officials, who then began an official misconduct inquiry. At the same time several members of the Hood lab tried, again unsuccessfully, to repeat the questionable experiment.

When confronted with the allegation, Kumar admitted doctoring the figure but denied any attempt to deceive. He insisted that he was not aware that his attempt to create a more attractive figure was an unacceptable practice, and claimed that a more senior member of the Hood lab, who had served as Kumar's mentor early on, had not discouraged the duplication of lines when Kumar had queried him about the propriety of such an act. Indeed, in spite of Kumar's extensive lab experience, he had never actually prepared any of the text and figures of his thesis or publications, which were largely written by his various advisors. In his defense it was clear that no attempt had been made to doctor the figures in a way that would remove the telltale artifacts.

Following the inquiry, an investigation was begun. During the year-long investigation, Kumar was relieved of his duties but retained his official status. Having heard Kumar's admission during the inquiry, Hood retracted the published article and notified three other co-authors of the retraction. Hood also notified the funding agencies supporting Kumar's work, the NIH, and the universities to which Kumar had applied for a job. After learning of the investigation, Washington University officials immediately withdrew the job offer made to Kumar previously.

The outcome of the case was that Kumar's data fabrication constituted research fraud. The investigation identified other problems, including altered results and missing lab notebooks that Kumar claims were stolen.

Reporting Results[14]

Between 1989 and 1992, Lt. Col. Robert Redfield of the Walter Reed Army Institute of Research (WRAIR) tested the therapeutic value of gp160, a vaccine made by the Connecticut biotech firm MicroGeneSys. The drug is intended to limit levels of HIV in the blood ("viral load"), and thus, it is hoped, retard the onset of full-blown AIDS. Measurement of "viral load" involves a process called the quantitative polymerase chain reaction (PCR). WRAIR's Maryanne Vahey conducted a new, experimental version of PCR for Redfield, who, speaking at a prestigious conference in Amsterdam on 21 July, 1992, compared PCR results from a group of untreated HIV-positive patients with those from 15 recipients of gp160, and called the differences in viral load "statistically significant."

However, Redfield was not telling the whole story; 26 people in all took gp160. It was also revealed that he had used questionable criteria for his statistical analysis. When William McCarthy, head of biostatistics

at the Jackson Foundation, reworked the data, he showed that gp160's effect was, if anything, minimal.[15] An informal WRAIR inquiry on 28 August, called by Redfield's superior, Col. Donald Burke, decided that the first analysis had been rushed, due to pressure of time, and should have been done like McCarthy's. Redfield accepted this conclusion, which both he and Vahey repeated in presentations at a gathering in Chantilly, Virginia, only days later.[16]

The explanation did not prevent two USAF AIDS researchers from lodging an official complaint that Redfield "overstated" his results. During the Army's investigation, undertaken by Col. Harry Dangerfield, Redfield said that full PCR data had not reached him until 24 July, 1992, and that he had consequently used what there already was on "the first 15 patients who had entered the study and who had been studied for a minimum of 18 months."[17] Vahey, for her part, contended that she had supplied full data by 19 May. She allowed that selection of results need not be suspect, but Redfield stated that he had selected nothing, and was backed up on this by Lt. Col. John Brudage, who had worked with him on the first analysis. Dangerfield accepted Redfield's account, without giving clear reasons for rejecting Vahey's.

Suspicions of data-selection persist. McCarthy resigned his post in disgust at what he sees as a whitewash, and unnamed WRAIR personnel have cast doubt on PCR's reliability, on the Army's impartiality, on how rushed Redfield's first analysis really was, and on the likelihood that reputable researchers ever used rushed analyses. Some think the Amsterdam report was part of a scheme to secure a large sum of funding. Redfield is on the advisory board of a group called Americans for a Sound AIDS/HIV Policy (ASAP). Vahey claims that W. Shepherd Smith Jr., ASAP's president and a gp160 therapy enthusiast, called her on 24 August, 1992, betraying familiarity with unreleased test data. Furthermore, Smith once conducted an investment seminar for MicroGeneSys, thus linking Redfield to the interests of a company whose product he tests.

Most controversially, MicroGeneSys conducted intense lobbying of several U.S. Senators to ensure that $20 million, earmarked for Army research into gp160, was added to the 1992 Defense appropriations bill, weeks after Redfield's Amsterdam report. Redfield at that time lobbied the NIH, FDA and the Centers for Disease Control to further the testing of AIDS vaccine in pregnant women.[18] Outraged, some say envious, researchers have accused Redfield of trying to make gp160 look better than

it is, using political influence to circumvent peer review. A blue-ribbon panel, convened by the director of the NIH, was sufficiently critical of the appropriation to have it reversed in January, 1994, the money going to more general research.[19]

Many Agents/Many Responsibilities[20]

A Wright State University graduate student consulted with Dr. Kathleen Beal, one of the consultants at the Statistical Consulting Center, about how to analyze her data. The design was straightforward: a three-factor repeated measures ANOVA, with repeated measures of one of the factors, and four subjects randomly assigned to each of the 20 factor level combinations involving the remaining two factors. The graduate student carried out the analyses herself and subsequently defended her thesis successfully.

After the student graduated, her advisor submitted a manuscript based on the thesis work to a peer-reviewed conference, including the student's name as one of the co-authors but did not give her the opportunity to review the manuscript before submission. The manuscript was conditionally accepted by the editor. The manuscript needed to be revised according to referees' comments and resubmitted for final approval, but the statistical part of the manuscript had not been questioned.

The student had the opportunity to review the manuscript prior to resubmission. She discovered that her adviser had rerun the analyses without including the SUBJECT term in the model, which led to a declaration of significance for more of the model terms, and hence more interesting conclusions, than appeared in the thesis. The student insisted that her advisor replace the analysis in the manuscript with the analysis and results from the thesis. The advisor refused and threatened to remove the student's name from the co-author list. The student notified the chair of her department, a senior faculty member and the American Statistical Association.

Blowing the Whistle[21]

Dr. Carolyn Phinney was a research psychologist at the University of Michigan in 1989, working under the supervision of Dr. Marion Perlmutter in the Gerontology Institute. In a discussion with a colleague, Phinney expressed concern that Perlmutter had incorporated Phinney's research into an NSF grant application without crediting Phinney. The

colleague, in turn, reported Phinney's concern to the Institute Director, Dr. Richard Adelman.

Adelman initiated an investigation immediately and compelled Phinney to take an active role as whistleblower. By the end of the investigation four separate panels had been convened; in each case Perlmutter was found not guilty of plagiarism and theft of research material. It became known, however, that each panel appointed by Adelman contained at least one member who had been a participant in one or more of Perlmutter's grants.

In 1990, Phinney filed suit charging that Adelman had sought to discredit her and damage her reputation during and after the investigation. A jury eventually ruled in favor of Phinney deciding that Adelman had violated the Michigan State Whistleblower Protection Act, and "that Perlmutter had committed fraud by making false promises regarding grants, authorship and employment to Phinney in order to obtain access to Phinney's research."

Nationalism and Foreign Funders[22]

A foreign investment agreement that raises concerns is the arrangement between Otsuka Pharmaceutical Co. and Senitiroh Hakomori, a professor of pathobiology and microbiology at the University of Washington who is well known for his research with membrane glycolipids. He also is the scientific director of the Bimembrane Institute, a non-profit research center affiliated with the university, which was established in 1987 with $5 million in funding from Otsuka. In return for the first right to license any promising developments in the institute, the company also provides about $4 million a year to fund the institute's faculty of 10 Ph.D.s and M.D.s with the understanding that those faculty members would also find supplementary funding from outside sources.

While most faculty members have not found supplementary funding, Hakomori does receive about $600,000 a year in a long-term grant from NIH for his research, which has led to the discovery of many new types of glycolipids, some of which modify transmembrane signaling and have potential as tumor suppressors. While he conducts the NIH-funded research at his lab in the Biomembrane Institute, he insists that he can keep the results separate from those of the other work he is doing, which can be licensed by Otsuka. And so far at least, Hakomori says, Otsuka has actually encouraged institute researchers to explore alternative

arrangements. He points out, for example, that the company recently waived its right to license several ideas coming out of research at the institute, in favor of encouraging the researchers to set up collaborations with interested U.S. firms. "I feel that is a generous arrangement," says Hakomori. Like many U.S. academics who accept funds from the Japanese, Hakomori feels that "large Japanese companies are in general less aggressive and concerned with their own bottom line in this type of funding relationship than U.S. or European companies."

The Limits of Funder Control[23]

Scripps Research Institute is the nation's largest nonprofit biomedical laboratory. Recently the Institute developed a collaborative agreement with Sandoz Corporation that would give Scripps $300 million in return for licensing rights to their research. The agreement would not give Sandoz veto power over what Scripps does, but it would allow Sandoz to delay the publication of scientific papers for up to 75 days to allow patent applications. Scripps also accepts approximately $100 million from NIH. The Scripps-Sandoz agreement thus appears to allow Sandoz rights to discoveries made under that federal funding as well. Moreover, Sandoz' control of Scripps could be perceived as interfering with its research activities with NIH. Eventually, yielding to pressure from NIH, Scripps renegotiated the contract so that Sandoz would have rights only to research it funded.

Dialogues for Role Play

The Mentor[24]

Participants: Alice Williams, internationally recognized researcher in neuropharmacology; Mike Finch, a post-doc in Dr. Williams's lab; Harvey Miller, a junior faculty member in the same department; Betsy Olsen, another post-doc in Williams's lab who has one year seniority over Finch.

(Alice Williams calls out to Mike Finch as Finch passes by her office on his way to the lab early one Friday.)

Alice: Good morning Mike. Can you come in for a minute?

Mike: Sure. Have you had a chance to look at our paper yet? Your suggestion that I collaborate with Harvey worked out extremely well.

Alice: You two have done a great job sorting out the effects of the new non-tricyclic antidepressants on sleep. I can see that his background in anatomy really made a difference. I've just made a few minor changes.

(Alice takes the paper from a stack on her desk and hands it to Mike.)

If I've made any comments that aren't clear, just ask and I'll clarify.

Mike: Thank you. We'll make the changes and give you a final draft next week.

(As he begins to walk out the door, Mike looks down at the cover sheet. Slowly he turns back into the room with a puzzled expression on his face.)

Mike: Alice, why did you write Betsy Olsen's name on the cover sheet with a question mark? Is there something that you want me to check with her?

Alice: Oh, I almost forgot. I'd like to suggest that you add Betsy's name to the paper. She's done a lot of work on those new compounds.

Mike: That's true. But she didn't have any input on this project.

Alice: I disagree. I think her work laid the foundation for the project. Also, didn't she make some important suggestions at group meeting that led to new experiments?

Mike: As I recall, she was out of town when we gave our first report at group meeting. Janice and Steve had some good ideas and you were terrific, but I don't remember anything from Betsy.

Alice: Well, I really think her early work in the area justifies putting her on the paper. Why don't you talk to Harvey and then get back to me. I've got to finish these lecture notes before class.

(The next afternoon, Mike sits in Harvey Miller's office. Mike looks tired and distressed.)

Harvey: I'm surprised that Alice asked you to put Betsy's name on the paper. Did Betsy make some contribution that I don't know about?

Mike: No, of course not. I just don't know what to do. I didn't sleep at all last night. If we put Betsy's name on the paper as well as Alice's, everyone will think it's an extension of Betsy's project and it's not. On the other hand, I don't want to say "no" to Alice. My career depends on her recommendation.

Harvey: I still don't understand why Alice is doing this. Do you have any idea?

Mike: No, I don't. Alice and Betsy are close friends. They play tennis together at least once a week during the summer . . . . You know, Janice made a suggestion early on that really did change the course of the project. Remember, we discussed the possibility of putting her name on the paper, but both she and Alice said we should just thank her in the notes. I'm really at a loss.

Harvey: There is one other possible explanation. I probably shouldn't tell you this, but I have a friend at the University of Texas. He told me that Betsy gave a great job talk there, but for some reason they didn't offer her the position. My friend thinks Betsy may have lost out because she didn't have as many publications as the other top candidate.

Mike: That may be, but it still doesn't justify putting Betsy's name on our paper. What do you think I should do?

Harvey: I think you should talk to Alice again.

Mike: Maybe the three of us should sit down together.

Harvey: I don't think that would be such a good idea. I don't want Alice to think we're ganging up on her. I'll mention it to her separately. It's a little awkward for me because you and Betsy are both in Alice's lab. Also, I didn't come into the project until you had it up and running.

Mike: I'm really disappointed. I know your tenure decision is at the end of the year and that Alice is very influential with the other senior faculty, but I really hoped you'd support me on this.

Harvey: I'm sorry you feel that way. My tenure decision has nothing to do with it. I just know if I were Alice, I wouldn't want another faculty member telling me how to run my lab. I'll try to catch her after the seminar this afternoon.

Mike: (Shakes his head wearily and gets up to leave.) Thanks. I guess I'm just tired. I'll make an appointment to see Alice this week. I'll let you know how it comes out.

Discussion Questions:

1. If you were Mike, how would you deal with Alice's suggestion?
2. Alice understands that Mike is reluctant to add Betsy's name. What would you do if you were Alice?

3. If you were Harvey, would you take a more active role in the discussion?
4. If you were Betsy, how would you feel?

At Sea![25]

Participants: Jim Farber, Assistant Scientist in physical oceanography; Tom Donato, Assistant Scientist in marine chemistry; Dan Stern, Research Associate in Tom Donato's group; Dick Werner, Senior Scientist in marine chemistry; Anna Wong, graduate student in marine chemistry.

(Jim Farber and Tom Donato meet in the lunchroom of the Neptune Marine Institute, where they both hold tenure-track positions.)

Jim: Hi Tom. I'm glad I ran into you—I just finished looking over the manuscript you sent me last week. That's very exciting work—attaching in situ measurement of dissolved organic carbon [DOC] to a CTD instrument is a very exciting technical development. It could revolutionize the way we look at the global carbon budget.

Tom: Thanks very much. I'm pleased by the results. Dan Stern is a genius with instrumentation. His innovations really laid the groundwork for this one.

Jim: He is very good. In fact, I was surprised that you didn't make him a co-author on the paper.

Tim: Oh, I think it's counterproductive to have too many authors on a paper. Anna is included because the project is part of her thesis work. And Dick had to be a co-author—he's provided most of the financial support for the project. Besides, I'm encouraging Dan to do a separate paper on the instrumentation. Our paper deals more with interpretation and computer modeling of the DOC profile.

Jim: That's true. But my understanding is that you have to rely heavily on technical details of the instrument to evaluate the validity of the data. Didn't Dan contribute to that part of the project?

Tom: Well, yes . . . but you know Dan. He doesn't pay any attention to publications. Besides, he's at sea so much that it's hard to pin him down long enough to read a manuscript!

Jim: I know how hard it is to catch him on dry land, but he's extremely good—and his next promotion depends on authorship just like ours do.

Tom: I put him first in the acknowledgments, and I plan to recommend him for promotion to Research Specialist at his next review. I don't think he'll need more than that.

(Tom looks at his watch.)

Tom: I've got to run now. I have a meeting with Dick. Thanks a lot for reading the paper. I'll pass your comments along to the others.

Jim: Well, congratulations. I'm sure you'll have no trouble getting the paper accepted.

Discussion Questions:

1. Do you agree with Tom that too many authors is "counter-productive"? How many is too many?
2. Do you agree that Dick had to be a co-author?
3. Is Tom's perception that Dan "doesn't pay any attention to publications" relevant?
4. If you were Jim, would you urge Tom to reconsider his decision? Would you take your concerns to Dick or to the Director of the Institute?
5. What is the appropriate role of the "acknowledgments" section in a scientific paper?

Crucial Antibodies?[26]

Participants: David Smith, Associate Professor at University of the Atlantic; Paula Jones, Associate Professor at Lake Erie University; Karen Alexander, Associate Professor at University of the Atlantic.

(David Smith is looking at a poster at a national professional meeting in Tucson, Arizona when someone taps him on the shoulder.)

Paula: Hello. You're David Smith, aren't you?

David: Yes I am. *(He looks at her name tag.)* Oh, and you're Paula Jones. I'm so pleased to finally meet you. The antibodies you supplied for the TZF-z paper were tremendously helpful. It was such a stroke of luck that you had them in your freezer. And you were great when we needed to recheck the affinity constant. Your turn-around-time was terrific. I don't know how we can ever thank you.

Paula: Being third author on your paper will be sufficient.

David: What do you mean?

Paula: I called Karen last week. Didn't she tell you?

David: No. I was visiting my family in Colorado before I came down here. Why?

Paula: Well, when Karen talked to me originally about the antibodies, I was glad to help out. Then you needed reconfirmation of the affinity constant, and I went back and rechecked it. I naturally assumed that you two would include me as an author on the TZF-z paper. When I didn't hear from you, I began to think that I'd misunderstood. So, I called Karen last week to tell her that I had some time and would be pleased to read and comment on the manuscript. I asked her about authorship then.

David: Well, what did she say?

Paula: She said she didn't think there would be a problem, but she would have to ask you. What do you think?

David: I guess I'm a bit stunned. It never occurred to me that you would expect to be included. In fact, I guess I have some pretty strong objections.

Paula: Why? I dropped everything to check that affinity constant.

David: That's true. But you haven't done any work with the TZF-z system. It's really outside of your area of expertise. No offense, but it just doesn't make any sense.

Paula: Now wait a minute. I was a co-author on the TZF-z paper out of Fresno University last year. They didn't seem to have a problem with my participation.

David: I noticed that. In fact, I called Joe Saunders and asked him about it. He told me that with eight authors, one more didn't really make any difference. But we only have two authors, and Karen and I have worked very hard on this project for three years.

Paula: I don't know why you should object so strongly. Karen says it's fine and she's first author.

David: I guess we just have a difference of opinion. I think it matters for my tenure case.

Paula: I don't think it's going to help your case to make a big fuss over this.

David: Karen will be flying in this afternoon; I'll talk to her then. Now, I've got to go find my wife. I'll see you later.

(Three hours later, David knocks on the door of Karen's hotel room.)

Karen: *(She opens the door.)* Hi, David. Are you enjoying the sun? What's wrong?

David: I met Paula Jones this morning.

Karen: Oh good, she's terrific, isn't she?

David: No. She told me that she expects to be an author on the TZF-z paper. I couldn't believe it.

Karen: I was planning to talk to you about that. I was hoping you wouldn't mind. She's in a small department and doesn't have many opportunities for collaboration. She was so willing to help us out.

David: But there was no intellectual contribution on her part.

Karen: I suppose that's true. But it would have taken us days to do that work ourselves; she'd already done the procedure so it went very quickly. What harm does it do to include her as third author? It's not alphabetical, so everyone will know that she had the smallest contribution.

David: Come on Karen. You know as well as I do that dual authorship is very important to us; after all, we're both coming up for tenure. I wish you'd told me in the beginning that she was expecting to be an author.

Karen: I really didn't know. In fact, it didn't occur to me either until she called. Besides, I don't think there's much difference between two and three authors.

David: Well, I do. And now Paula seems to think you gave her the green light, and I come out as the heavy.

Karen: Listen, I tried to be noncommittal with her on the telephone. If you feel that strongly, I'll just tell her it won't work.

Discussion Questions:

1. If you were David, would you agree to adding Paula as an author?
2. Is Paula's expectation reasonable?
3. Do you agree with David that adding a third author would alter the impact of his contribution?
4. How would the situation change if Paula were paid for the time she spent recalibrating the antibody? If she were mentioned in the acknowledgments?

Notes

1. Pederson, p. 151.
2. Kohlberg, p. 46.

3. Complete references for works cited can be found in the comprehensive bibliography at the end of this issue.

4. Winston, pp. 6-7.

5. See for example, case studies produced by the Kennedy School of Government and Harvard Business School.

6. Winston, p. 3.

7. Ed Berger and Bernard Gert are the authors of this case.

8. Vivian Weil and Robert Arzbaecher are the case authors of this vignette. This vignette is based on a case from the Whitehead Institute of Massachusetts Institute of Technology.

9. Vivian Weil and Robert Arzbaecher are the case authors of this vignette.

10. Vivian Weil and Robert Arzbaecher are the case authors of this vignette.

11. Vivian Weil and Robert Arzbaecher are the case authors of this vignette. This vignette was adapted from a case in an unpublished collection compiled by C. K. Gunsalus, Vice Chancellor for Research, University of Illinois at Champaign-Urbana.

12. Vivian Weil and Robert Arzbaecher are the case authors of this vignette.

13. Ed Berger and Bernard Gert are the case authors of this vignette. Information from the case came from Roberts (1991).

14. Patricia Werhane and Jeffrey Doering are the case authors of this vignette. The source of the material comes from Cohen (1992b), (1993a), (1992a), (1994).

15. Cohen (1992b), p. 825.

16. Cohen (1993a), p. 825.

17. *Ibid.*, p. 824.

18. Cohen (1992a), p. 539.

19. Cohen (1994), p. 463.

20. Vivian Weil and Robert Arzbaecher are the case authors of this vignette.

21. Ed Berger and Bernard Gert are the case authors. The source of the material is Anderson (1993c, d and f).

22. Patricia Werhane and Jeffrey Doering are the case authors for this vignette. The source of the material is Gibbons (1992a).

23. See Gibbons (1992b), Anderson (1993f, 1993d).

24. Prepared by Eve K. Nichols and Stephanie J. Bird.

25. Prepared by Jan Whelan in collaboration with Stephanie J. Bird and Eve K. Nichols.

26. Prepared by Eve K. Nichols and Stephanie J. Bird.