

Ethics in the University:

Reflections on Responsible Scholarship

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CONTENTS

	Contributors	ii
	Preface	v
Chapter 1	Ethics in Research: Who Needs It? by Richard De George	1
Chapter 2	The Academic X-Games by Ann E. Cudd	7
Chapter 3	The Stories of Others: Art, Hypnosis and Visual Memoir by Tanya Hartman	12
Chapter 4	The Death of Kazahero: Engendering a Culture of Safety in Students by Michael Murray	17
Chapter 5	Reflections on the Kluge Called Peer Review by James F. Daugherty	20
Chapter 6	Socrates in Oread: A Dialogue on Responsible Scholarship by Charles Marsh	32
Chapter 7	Fieldwork in the Counterfeit Paradise: Questions of Interpretation and Responsibility by William I. Woods	39

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Joshua L. Rosenbloom

What are our professional responsibilities as scholars working in a university environment? Instances of scientific misconduct have become all too common news items in recent years, prompting calls for expanded training in responsible conduct of research (RCR). One of the major proponents of this training is the National Institutes of Health (NIH) Office of Research Integrity (ORI). The ORI defines responsible conduct of research as including nine broad subject areas central to the conduct of reliable and useful research.¹ The cause of RCR training has gained additional support from the Council of Graduate Schools (CGS), which has produced a number of publications intended to diffuse knowledge and understanding of RCR and its importance in graduate education.² The CGS has also distributed funds to promote RCR awareness to a number of universities. Indeed, the compilation of this volume was supported in part by funds provided by the CGS to the University of Kansas.



1 These areas are: (1) data acquisition, management, sharing and ownership, (2) conflict of interest and commitment, (3) human subjects, (4) animal welfare, (5) research misconduct, (6) publication practices and responsible authorship, (7) mentor/trainee responsibilities, (8) peer review, and (9) collaborative science. See <http://ori.dhhs.gov/education/>.

2 Paul D. Tate and Daniel D. Denecke, *Graduate Education for the Responsible Conduct of Research*. Washington, DC: CGS, 2006. See also the CGS website's description of its RCR programs: <http://www.cgsnet.org/Default.aspx?tabid=123>.

As the introductory essay of this volume, written by Richard DeGeorge, suggests, however, it is not only scientists who have professional responsibilities in the pursuit of their scholarship. Faculty in the humanities, arts and social sciences, as well as the University's diverse professional schools, all must confront ethical choices and regulatory constraints in pursuing their quest for knowledge. Moreover, the need for responsibility does not end when we shift our roles from that of scholar to teacher, administrator or citizen. Once we begin to think about it responsibility is everywhere.

The genesis of this volume was the notion of posing the relatively open-ended question of what "responsible scholarship" meant to a diverse sample of faculty at the University of Kansas. To help focus the discussion I asked each of the participants in that conversation to think about a particularly challenging issue that they had confronted in their own scholarship, broadly defined. The result of these provocations is the collection of essays that follows.

These essays are not a comprehensive catalog of the professional responsibilities of faculty at a modern comprehensive research university. Rather they are a more or less random sampling of perspectives from different parts of the academy. If they provoke the reader to think more deeply about her or his own responsibility, or to begin a discussion of these issues with colleagues or students, then they will have served their purpose.

I am deeply indebted to the authors of the essays included here. Each contributed a substantial amount of time and effort in the course of several collective discussions and the process of writing and rewriting their essays. In addition I wish to express my appreciation to the CGS, which provided funding to the University of Kansas to support a variety of programming by the Graduate School (now the Office of Research and Graduate Studies) to promote RCR awareness. Some of these funds were used to partially defray expenses related to production of this volume.

Ethics In Research: Who Needs It?

Richard De George

Last year NSF joined NIH in requiring some sort of instruction in research ethics for all students who would receive funding from a grant. You might think I ought to have been happy at the announcement, since I have a bias towards the value of teaching ethics. After all, I teach a course on moral issues in business and a course on moral issues in computing and information technology, and last year I co-taught a course on ethics in science and engineering.

My joy at the announcement was contained, however, because I knew that what government agencies are most interested in is not usually ethics but compliance with law and governmental and other regulations. The Office of Research Integrity is rightly interested in preventing unethical behavior in research, especially with respect to plagiarism, the fabrication and falsification of data, the unethical treatment of human and animal subjects of research, financial fraud, and conflict of interest. These are all important concerns, and students of course should be taught the rules and codes by which they must abide that have been drawn up by various committees, groups, and agencies. Following the rules is a good way to stay out of trouble, and since the rules and codes for the most part prescribe what morality or ethics demands, acting in accord with the rules is, in one sense, acting ethically. The rules are set down in black and white and are fairly easy to teach and to understand. They do not require training in ethical theory and they help foster the responsible conduct of research.

What more should one ask or expect of students working under such grants? After all, they are not philosophy students, and the purpose of the instruction is limited to the responsible conduct of research. That is true and instruction geared towards compliance with the rules is, as I have indicated, necessary and certainly better than not providing such instruction. Many scientists, as well as many people in ordinary life, get along fine by simply obeying the law and complying with whatever regulations govern their activities.

But let's suppose for a minute that NSF and NIH are really on to something. Let us suppose that they know what they are doing and that, although they can only demand compliance, in requiring something along the lines of ethics training in research they are helping push institutions in the direction of ethics. What would this mean?

We all know that knowledge develops and that what science, for instance, will look like in the future, and the ethical issues scientists will face, are impossible to predict. We teach our students in all fields not only what is known at present, but how to push knowledge forward and how to continue learning after leaving school.

Learning rules is learning what is presently known. If students are to be able to figure out how to act in their professions they should come to see how rules are formed, what their rationale is, how they are justified, how they can be evaluated, and how to think through issues that are not clearly covered by the rules. This is the broader task of training that ethics takes on. Following rules tends to be the submissive role adopted by those who need to be told what to do; autonomous agents are able to see how they should act because they understand the justification for the rules. Learning to think through ethical issues on one's own, given the background of understood rules and traditions, is the mark of a free person and a true professional in a field. In this sense responsible conduct of research refers not only to acting in accordance with rules, but being a responsible person and taking responsibility for how one carries on research.

Acting ethically is self-motivated. One acts as one does not because of external rules or fear of punishment but because one sees that people deserve respect and understands how that translates into how they should be treated, whether they are subjects of an experiment, or students, or colleagues. It is also a matter of one asking oneself what kind of a person one wants to be, what kind of a society one wants to live in. Compliance ends with the letter of the law. Ethics sees that, because not everything can or should be governed by rules, one has to think imaginatively and become sensitive to the results of one's actions. A compliance view tends to imply that whatever is not prohibited or required by the rules is allowed. An

ethics-driven view realizes that this is not the case. Compliance interpreted literally sometimes yields obviously wrong results. The requirement that all human subjects must sign a document stating that they understand the experiment and its possible dangers and that they give their consent to participate makes sense in most instances. It made little sense when the Office for Health Research Protections (OHRP) closed down a Johns Hopkins research project studying the effect of hospitals introducing a five-step checklist for doctors to follow (including such things as washing one's hands before inserting intravenous equipment into a patient). The study showed that following the checklist in Michigan intensive-care units saved 1,500 lives over an 18-month period. But the government claimed an ethics violation and refused to let the study continue unless all the doctors and all the patients signed an informed consent statement. Hospitals could, of course, introduce the checklist, but any study of its effectiveness had to pass an enormous hurdle, which would make it too expensive and difficult to pursue, even though the study endangered no one.³ Did the OHRP act ethically in enforcing a rule that in this case led to preventing research that would benefit patients? All laws and rules have to be interpreted and a judgment made about whether a particular action falls under the rule. An ethics approach suggests looking at the spirit of the law or rule; a compliance approach suggests looking at the letter.

The rules regarding the treatment of human subjects (such as the Belmont Report and the Nuremberg Code), for instance, were drawn up by people with experience with, and sensitivity to, ethical issues, usually in response to specific breaches of ethics. Ideally we would train our students to be those who take part in making up such rules; less loftily, we can teach them to appreciate why they are necessary to protect human rights and treat people with the respect they deserve.

Compliance limits itself to questions of how to act or not act. Ethics goes further and raises questions of what sort of person the researcher or budding researcher wants to be. Serious

3 For details and links to OHRP documents and comments, see *Wachter's World*, "Did I Violate Federal Regulations Today? (I Hope So)" at http://www.the-hospitalist.org/blogs/wachters_world/archive/2008/01/16/checklists-redux-did-i-violate-federal-regulations-today-i-hope-so.aspx (accessed 6-25-08).

research requires certain character traits or virtues that can be developed. Patience, accuracy, attention to detail, honesty in reporting results, self-discipline, intellectual courage in pursuing ideas where they lead—these are just a few that are broadly shared across fields. They are traits that are cultivated in pursuing research and that are necessary for it, although not part of any set of rules or regulations.

Given the richness of an ethics approach to research as opposed to a compliance approach, it is difficult to imagine how anyone could in principle be opposed to it. But implementing it in fact is a different matter. One way of teaching ethics is in a course; another, and not mutually exclusive way, is to integrate it into many courses, so that ethical issues are seen to permeate research and not to be an add-on that one considers from time to time when complaints are made or things go wrong. On the graduate level one-on-one mentoring is possible and possibly the best approach. But doing more than stating the rules is something many teachers feel uncomfortable pursuing, and they are reluctant to take time away from their subject matter to do so. If they are to do more the university would have to develop training sessions to help them, provide models of how to incorporate ethics into one's subject matter, and make available needed resources, such as cases and videos, stories, and background reading. That all sounds like more work added to an already overcrowded day.

Finding time to fit ethics into a course or curriculum is not an easy problem to solve. Yet other groups in recent years, such as the accrediting agencies for schools of business and some engineering programs, have similarly required some sort of ethics component for accreditation. Somehow, when externally mandated, programs manage to find room for the required ethics training. The requirements imposed by NSF and NIH will force us to start thinking of how best to fulfill that requirement, and whether to rest content with a compliance approach or aim for both an ethics and compliance approach.

While considering the possibility that perhaps the NSF and NIH are on to something, we might ask: why is ethics required only in research and only in science and engineering, and only for students? The answer is, of course, that these are where the agencies give money

and their authority extends only as far as the grants they give. Yet, if they are on to something, and if ethics training is important for research in science, why not for research in the social sciences and the humanities? Or, for that matter, in fine arts and the professional schools? Surely there are as many ethical pitfalls to be aware of in doing research in the social sciences as there are in the physical sciences. Respect for human subjects is central, but so is care in recording, interpreting and reporting data. The humanities typically do not engage in experiments, but still require intellectual honesty, accuracy, care with citations, respect for one's intellectual opponents, charity in the interpretation of texts by considering the strongest rather than the weakest reading, and so on. In all areas similar issues arise about plagiarism, fairness in peer reviews, and respect for the intellectual property of others. All areas of research also involve more than simply obeying rules. Are there areas of research in any discipline that should not be pursued? Are there topics in science, the social sciences, or the humanities in which results have been skewed by racist, sexist or other prejudices? These broader questions become part of thinking about ethics in research in any field.

Going a step further, if ethics is important for research, why not for other aspects of education? These questions illustrate the danger of teaching ethics instead of simply compliance. Unlike compliance, ethics and ethical thinking cannot be compartmentalized. Once one teaches students how to think ethically and analyze issues, one cannot set limits on what they are allowed to evaluate from an ethical point of view.

Ethics training turns out to be a two-edged sword. On the one hand it can be used to justify existing rules and regulations, where these are appropriate. On the other hand ethics provides one with a critical tool, opening up for ethical evaluation all rules, regulations, and practices. Many firms, for instance, teach their new employees the rules they are to abide by but do not encourage them to evaluate from an ethical point of view the practices of the company, which they are to respect and not upset.

Applied to the university, teaching students responsible conduct of research is salutary and commendable. But taking a larger perspective, does it make sense to expect students to be

ethical and observe the rules in their research but not in other aspects of their education? Surely we want them to be ethical when taking exams and doing assignments as much as we want them to be ethical in their research. When studies of the Center for Academic Research show that more than 70 per cent of college students admit to cheating at least once, singling out research as a separate and special area seems arbitrary. It may allay the concerns of funding agencies because they are focused on research. But the university has broader concerns. Might starting with ethics in research be the first step towards ethics in education generally, and beyond that towards ethics across the university?

Once attuned to ethical issues, one tends to see them where they were not before apparent. Faculty, staff, and administrators all face ethical issues, many of which are not discussed and which it is often implicitly deemed inappropriate to raise. Ethical issues in business, engineering, science, journalism, health care professions, the environment, law, politics, government, administration and, yes, education have become part of the contemporary landscape. The NSF and NIH mandates concerning the responsible conduct of research may be the nose of the camel pushing into the academic tent. The question is: do we want to let ethics in? After all, who needs it?

* * *

(Did I hear someone whisper, “We do” or was that wishful thinking on the part of an ethics teacher?)

The Academic X-Games

Ann E. Cudd

Academic research confronts one with a series of ethical decisions that lie behind apparently value-neutral epistemological questions. First, the researcher has to decide what to study, and the choice of questions is almost inevitably ethically tinged, even when there are many ethically acceptable choices to make. Second, social scientists and medical scientists have to ask: On whom or what to test the hypothesis, and how are the subjects to be treated? Third, researchers in the humanities, as well as the sciences, have to ask themselves what literature do they need to cite? This question is ethical on a couple of levels. Academic ethical standards require one to cite the literature that is quoted verbatim or indirectly, but it is also an ethical issue to decide whose work one will promote through the power of the citation. Fourth, the researcher has to decide what theory, model, or metaphors to use. Feminist critics, among others, have uncovered myriad ways in which models and metaphors can be sexist, racist, or otherwise unethically tinged. Fifth, what standards of theory choice should the researcher adopt? This is also an ethical issue particularly because of the tendency of non-epistemic values to play a role in these choices. Feminist critics have uncovered examples in which certain methods, models, or metaphors gained acceptance for reasons that go beyond the evidence, and worse, cases in which the gender and race of the scientist or researcher who propounds a theory helped or hindered its acceptance.

Researchers and philosophers respond to these challenges by arguing that the truth should guide us to the right answers to each of these questions. We should choose the topics, the models, the criteria that are most likely to lead us to more and more general facts about the world, so that the human store of knowledge expands and deepens. While this is a seductive response, there are two major problems with using truth as a guide to ethical research behavior. One is that there are too many truths. We inevitably have to make tradeoffs in investing time, money, and effort investigating some problems rather than others, so we must decide

which problems are most important. This is as true for the researcher, who wants to spend her time and effort on the most personally satisfying work, as it is for society, which must invest those lines of research that are most socially valuable. These choices cannot be guided only by truth; they must also be informed by values. The second major problem with truth as a guide is that we don't know the truth in advance of making these choices, so that we must use standards that do not require knowledge of the truth for deciding these issues.

So if not truth, then what is our guide for making these choices? If we think it is truth, and yet it is not, then there must be some other guide that is not readily recognized by researchers. I am not proposing an ethical theory here that should decide all the answers to these questions. That would be a long, exhausting, and tedious project. In this essay I simply want to shine some light on one such value that is unsavory, but seductive for many of us and for many of our fields, and that we, as researchers, should try our best to avoid. This is an aesthetic that values the surprising and the ironic over the obvious or the mundane, in at least the academic fields that I know best, which can amount to a form of careerism in the individual, and fashion in the field as a whole. I think of it as an analogue of the X-games that have proliferated in the age of cable television, where skiing fast is no longer surprising enough to captivate, now daredevils drive snowmobiles up vertical cliffs to see who can go highest before hurtling backwards to certain injury or death. There is something to win, but more to lose, and certainly nothing to inspire the spectator to imitate.

Philosophers of science have long recognized that researchers will make theory choices partly for aesthetic reasons. Aesthetic values include simplicity and elegance, values that I have no quarrel with on ethical grounds. But an aesthetic value I find less salutary but quite powerful as a motivator is valuing the surprising, the extreme, or the ironic for the sake of finding a career niche or being fashionable.

While I am sure that the reader will have their favorites in this regard, one of mine is the claim in feminist theory that, in a patriarchal culture, all heterosexual sex is rape. The author of this theory is highly intelligent and skilled in argument and rhetoric, and this claim does

seem to follow from some of her other ideas that sound much less extreme and surprising. The “all sex is rape” claim, though, has helped make her name very visible. But the claim has also brought unwarranted ridicule to feminist theory as a whole. It also cannot be true unless we are willing to say that there is no autonomy for any women under any male-dominated culture.

I don’t want to suggest by my example that only the academic left can be guilty of such silliness, however. Still another version of the problem comes from economics with the claim that the third world is “under-polluted,” meaning that it is a good place to send the first world trash and toxic residues. The economist who made this claim is notorious for many other surprising claims, some of which have had negative career impacts for him. Yet, he is considered a misunderstood genius by many of his economist colleagues, and his long string of leadership positions in government and academia testifies to the esteem in which he is held generally.

Nor are these extremist theories confined to theories that have practical application. The reader can no doubt fill in their own examples here, but a general formula for finding such theories is to ask oneself what the logical possibilities are for answering a question. If all the intuitively likely ones are taken, then choose the most unlikely, but still logically possible, option and run with it. Of course, this works particularly well when the empirical evidence is complicated and difficult to assess. But there is also some evidence that even in the empirical sciences there is a bias in publishing in favor of the more dramatic result, especially positive results, even though these results are quite often refuted within just a few years.⁴

Now there is no doubt that extreme theory is surprising, paradoxical, intriguing, and even fun. What I doubt is that it is conducive to finding truth or wisdom. Worse, I worry that pursuit of the extreme can lead us ethically astray and be less likely to approximate the truth in

4 “Science and Technology: Publish and Be Wrong; Scientific Journals.” (2008, October). *The Economist*, 389(8601), 109. Retrieved October 29, 2009, from ABI/INFORM Global. (Document ID: 1571254271)..

normal cases. It is easy to see how the incentive structure of academic work, which rewards publication and citation, can lead to extreme theory. Journals and publishers prize new things that will attract readers and new ideas tend to do that. There are only so many ideas that are centrist or intuitive, and once those are published the extremes will be pushed. Extreme theory also tends to be cited as other authors try to locate their theory in the literature.

10

I want to propose the principle that it is unethical to follow theory choice criteria that are likely to be false and harmful. Is there a general argument that what I am calling extreme theory is more likely than centrist theories to be false or harmful? Suppose that theory A and theory B make radically different predictions, and neither is true. Then, either the truth is farther from B than from A or farther from A than from B. To make a more extreme theory one has to go beyond A or beyond B. If the truth is somewhere in the middle between A and B, then the new theory will be farther away from the truth. But this argument is fallacious, since it depends on the assumption, for which we have no warrant, that truth and theory are along some continuous linear scale, and that truth tends to lie between extremes of false theories. I do not believe that there is a general argument for the claim that extreme theory is more likely to be false. Extremism in itself is not the problem we need to avoid. Rather, it is extremism for its own sake, or for the sake of aesthetic or rhetorical rather than epistemic value. To avoid this kind of extremism we need to examine our motives as researchers rather than only examining the theories themselves.

The motives that generate the academic x-games in individuals are careerism, desire for fame, and arrogance. While building one's career through hard work, creativity, and craft produces good research and generally advances the field of knowledge, advancing one's career without looking at the big picture and asking whether one's research is adding to that is not likely to do so. Of course, one might object that this is the function of peer review – to cull out the research that is without value. Yet I mentioned earlier the forces within the academic profession that tend to promote extreme research, such as aesthetic values and group thinking. The academy is notoriously cliquish, and some of the same forces that lead to the adolescent clique's adopting a ridiculous fashion or a target for bullying can play a role in

the academy, as well. Finally, publishing work that gathers notoriety or provides a target for one's own research has a value for reviewers, editors, and even readers. Thus as either a researcher or a reviewer we must be aware of these tendencies and work to fight them in ourselves and others.

However, all that said, it is important not to discourage real innovation and novelty that may produce advances in theory, policy, or technology. For this reason I think it would be wrong to try to build institutional checks on extreme theory. Those closest to the particular topics and methods of research in question are those best able to see the difference between innovative truth seeking and extremist careerism. Institutional checks are likely to be blunt instruments that eliminate the useful and productive innovation as quickly as the fashionable one. Since it is arguably just as harmful to eliminate or dull innovation as it is to allow unproductive, extremist, and false theories to hold our fancy for a time, the best route is probably to hope that individual scientists, researchers, reviewers and editors remain aware of the tendency of academia to promote its x-games, and simply apply their stringent, but virtuous, theory choice standards in order to promote truth seeking and wisdom.

Snowmobile jumping, anyone?



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The Stories of Others: Art, Hypnosis and Visual Memoir

Tanya Hartman

12

In September 2007, I made a strange decision. Having become increasingly disenchanted with what I termed at the time “academic art,” and remembering with nostalgia an adolescent longing to be a therapist or doctor, I enrolled at the Midwest Center for Hypnotherapy. My intention was to become a certified hypnotist, and to open a practice in Lawrence, Kansas. I “wanted to help people” because I felt increasing doubt about the power of art to alleviate suffering by testifying to collective experience. I had always made art because I believed that self-expression catalyzed self-awareness and allowed society to progress. Now, that belief was faltering. My experience in an academic department had disillusioned me, and the making of art seemed no more than a material product necessary to advance careers. I wanted to find a new way to engage.

However, my choice to study hypnosis was only somewhat arbitrary. The five years prior to my decision had exposed me to the loss of a father, a husband and an intimate friend. The best way that I can describe the desolation that I felt is to say that I never looked forward to anything, and that color seemed to have drained from the forms of the world. All was grey. Needing respite, I had contacted a local hypnotist, and had gone to visit her, hoping that she could help me to feel better.

The experience of hypnosis revealed itself to be tremendously healing. The “hypnotic trance” is merely a state of deep relaxation, familiar to anyone who has found him or herself floating pleasantly between sleep and wakefulness. What I discovered, while in this condition, was that peacefulness is always within me. Though life experience may conceal it, hypnosis teaches how inner peace can be accessed and possessed. This awareness of an

ever-present tranquility, just below the surface of the everyday, felt holy to me, and similar to the feeling of being completely, un-ambivalently engrossed in a work of art. I wanted to learn how to give this stillness to others. In some undefined way, I suppose, I wanted to exchange the healing I could offer as a certified hypnotist for exposure to the stories of persecution that had always obsessed me as an artist. My own grandparents were refugees from Germany to Mexico during the Second World War. The stark fact of their displacement was a central theme in my own self-perception. I sought now to open my art making up to encompass a broader range of survival narratives.

However, I didn't want to take anything without giving something tangible, and healing, in return. I was disturbed by what I saw as an unethical trend in contemporary art, in which artists hooked themselves to stories of victimization, aggrandizing their own work in the process, while offering the disadvantaged folks they represented the paltry reward of merely being connected to a work of art their pain had helped to engender. I didn't want to do that, and yet I did want to make visual art that testified to oppression and its aftermath of human suffering and isolation.

How then, as an artist, can I bear witness to human suffering in a way that ministers rather than exploits? How can I use the bleak stories of others to express the confusion that has been with me since childhood, when I wondered why the Jewish people were persecuted and why the human imagination is capable of both atrocity and miracle? How should I handle any career-advancement that results from the making of this art? Are these stories mine to use, even if my intentions are good? How can the making of this art be a giving, and not one more taking from people who have been decimated by misfortune?



Tayna Hartman. *Prayer Paddle # 4 (To Ask For Blessings)*, (front and back views), wood, wire, fabric, oil paint and collage, 11.5 inches x 6 inches x 1/2 inch, 2008-ongoing.

The dichotomy between good and evil has never made sense to me. It has driven the production of my artwork. I recognize the capacity for brutality and for love in my own nature, and often believe that had I been raised in harsh circumstances I, too, might become violent. For me, this duality is the central question of my own humanity, and so I present it in visual form for others to confront and contemplate.

14

These doubts are balanced by an intuition that the making of art can illuminate injustice and catalyze change. I believe that visual art, in its most potent state, functions simultaneously as public ministry and private meditation. Working as a volunteer hypnotist at the St. Louis Center for Survivors of Torture and War Trauma allows me direct and meaningful contact with refugees from conflicts in Somalia, Rwanda, Liberia, Sierra Leone, Congo, Bosnia and Kosovo. With them, I plan to create a project titled, *So That I Might Carry You with Me*. This work will take its form from Ethiopian processional crosses used in the Orthodox Christian Church. These beautiful, embellished sculptures are carried by hand and are expressive of loss and redemption. When touched, they confer blessing, When pointed in four directions they sanctify and renew the Four Corners of the earth. Often, they bear inscriptions. The processional objects that I plan to make will not take a cruciform but will be borne. They will be body-sized, embellished, wood, wire and painted paper sculptures embedded with text descriptive of a loved one lost to war or global violence.

I have developed some ground rules so that I might navigate the creation of this project in a manner that I hope proves to be ethical. The ground rules I have created have evolved organically throughout the last six months and represent, I suppose, the ideal that I usually try to live by, which is to treat others as I would want to be treated. Some of the rules are practical and some therapeutic, but all are aimed at protecting the clients at the Center for Survivors from any perception of having been taken from, intruded upon, or made vulnerable.

1. No story can be used in *So That I Might Carry You With Me* without the survivor's fully informed, written consent. If I use a survivor's story, I must be sure that telling the entire

story of their subjugation is something that they desire, and that they understand it will be used in a work of art that will be publicly exhibited.

2. No narrative heard in hypnosis can be used in the work of art. The two must remain discrete.
3. The provision of free hypnosis for the alleviation of chronic pain or the symptoms of Post Traumatic Stress Disorder is not the manner in which I plan to cull narratives for the art project, *So That I Might Carry You With Me*. The hypnosis is a way to give back to the Center for Survivors, while the creation of the work of art is a way to bring attention to the plight of displaced and persecuted peoples.
4. No money earned from this endeavor can benefit me personally. All profits from sales go directly to the Center for Survivors of Torture and War Trauma in St. Louis.
5. Text in each work must be in the client's native language and in English. The client must be able to read his or her story as both art and cultural artifact.
6. All clients are invited to openings, and are included in all facets of publicity and media attention.
7. When appropriate, I will seek guidance from Linda Gentry, my professor at the Midwest Center for Hypnotherapy and from members of the International Medical and Dental Hypnotherapy Association.
8. Finally, this is not "political" art, nor is it "public" art—two common, trendy art-world buzzwords. It is merely art that tells a universal human story of loss and survival. If such a story can affect some degree of awareness and change, then I will be satisfied and fulfilled.

In conclusion, I would like to say that it is my perception that language, and by extension imagery (because I see imagery as being an intimate part of language), are all that we have. For me, language and imagery are what allow us to be human. Without language, we would have no way to express the full range of love and longing that any human being experiences in a life, nor would we have a way to express the self, nor would we have production or a manner to express form. But most importantly, we would have no viable way to bring the inner self into the outer world, nor could we translate the outer world into terms that the

inner self could digest. So this proposed work is a way to discuss my amazement with the power of language, my awe of the power of unconscious processes such as hypnosis, and my profound faith in both these forces to promote healing in our lives and in our culture. It is my hope that when ethical questions arise, I can navigate them with precision and decency.



Tayna Hartman. Detail from Prayer Paddle # 2 (To Speak In Wonder And In Kindness), wood, wire, fabric, oil paint and collage, 16 inches x 3 inches x ¼ inch, 2008-ongoing.

The Death of Kazahero: Engendering a Culture of Safety in Students

Michael Murray

Kazahero was a graduate student at the University of Hiroshima who I worked with on the CERN experiment NA44. I was a post-doc at the time at Los Alamos. The Japanese students always had a reputation for working very hard and Kazahero was no exception. However, he also like to have fun and would enjoy playing touch football in the afternoon. He was very good at catching and once I remember the students shouting “Kazahero, you’re my hero.”

Kazahero worked until about 1 a.m. one night, calibrating some data, and came out of his office to find that he had left his carlights on. The car, a small Fiat Panda, wouldn’t start so Kazahero tried to push it up a small rise in order to get it going on the down slope. He lost his footing and the car rolled backwards, pinning him against a wall. He was stuck there bleeding until he was found by a security guard at 6 a.m. Kazahero died the next day.

I wondered at the time to what level we, his collaborators, were responsible for engendering this situation. We certainly benefited from the long hours that Kazahero put in. I myself became much more cautious about working alone after that.



The wall where Kazahero bled to death

At the time of Kazahero's death I worked for Los Alamos. Because of rather public environmental and security lapses the lab had adopted a very strong regime of compliance with safety and environmental regulations. This extended down to such things as rebuking secretaries who didn't have plastic jars that they used for watering plants given proper labels. There was also some pushback from the staff about this, partly from arrogance and partly because there was some feeling that the administration was more concerned about compliance than safety.

Becoming a project leader for a detector at CERN, or perhaps just growing older, has made me much more aware of the what we might call the "Culture of Safety" and my responsibilities to my students, post-docs and collaborators. I still have to be "compliant" but the meaning of that has changed and broadened.

When I was a student, working at CERN, I asked a technician about the safety of some operation and what the rules were. He said, "Michael, if there is an accident you and I will never work at CERN again." This made a certain amount of sense to me but is not adequate for complex projects. Safety planning is now an integral part of any scientific project. The implementation of this in Europe and the U.S. echoes the accounting practices in the U.S. and EU. In the states I am required to be able to document that I have followed the rules. In the EU there are rules but I am required to attest that the project is safe.

In the summer of 2008 I took a post-doc, two graduate students and two undergrads to CERN in order to test a detector we were building. My post-doc was against the trip since he said that we weren't ready, but I overruled him since it was critical to me that we get test data that year. When we arrived at CERN I showed my students where Kazahero had died and tried to impress upon them their responsibility for themselves and for each other. They then went through the official safety training at CERN. This they found very funny because of the British accents. We then started five weeks of frenetic work trying to get the calorimeter assembled while there was still time to test it in the particle beams supplied by the accelerator complex. I was amazed and very pleased at how hard my students worked

on “their calorimeter.” Despite many technical and some human problems they worked very long days in difficult conditions. Of course they gradually became exhausted. At this point I realized that I had an accident waiting to happen. I get on well with my students but in a strange way their dedication to me and to the project was becoming a problem. I am proud when students stop thinking that I know all the answers to physics and try to nurture the questioning spirit that a scientist needs. In the end I had to force the students to rest. We stopped coming back to work after dinner and stayed away from CERN completely on Sundays. My post-doc thought this was silly but I think it helped. We got along better and thought more clearly. If you can just be creative you can find another way. When we were finally taking data the one student who could understand the computers couldn’t stay up anymore. I took her back to the dorm and called a colleague in Chicago who helped us take data remotely.

The techniques of safety, analysis, work-planning, good housekeeping, etc. are relatively straightforward to teach. But the long-term perspective that it is better to stay safe, even if the project should “fail,” is harder for young people to grasp. I am very grateful for how that summer turned out but know that this summer I will have to guide a new group of students at CERN. I hope that my students will learn that my approval, or the success of any project, is not worth an injury or death.

Reflections on the Kluge Called Peer Review

James F. Daugherty

Formal peer review is one of academe's entrenched rituals. From journal manuscript and grant application appraisals to annual faculty evaluation and promotion and tenure decisions, this self-governing referee procedure entices its stakeholders with an initially appealing logic: How better to assess credibly and fairly the merits of specialized work than by reliance upon the judgment of other trained and experienced specialists?

Despite its allure, scrutiny of this assumption raises some interesting and, at times, troubling questions for the conduct of responsible scholarship. The following reflections, informed by lenses of available research and personal experience, aim simply to stimulate discussion and to offer a few modest proposals relative to the design, function, and navigation of peer review as it occurs in the particular context of journal refereeing.

Research Lenses: A Brief Look

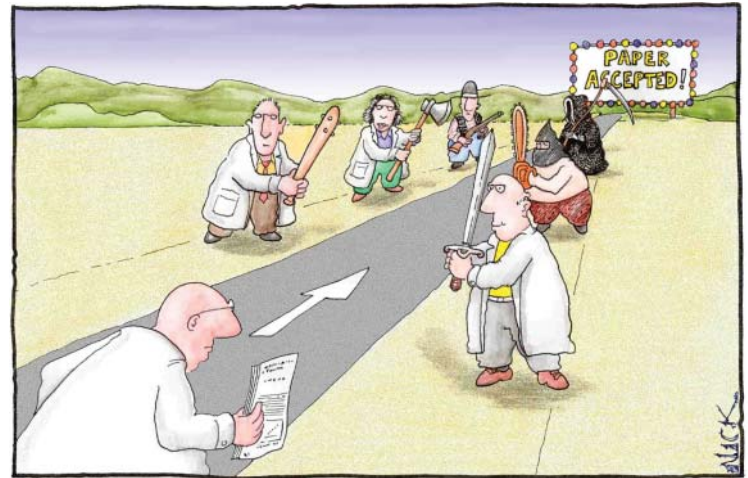
Scott (1974) famously described reviewer agreement in manuscripts submitted to the *Journal of Personality and Social Psychology* as only slightly above the level of chance. Subsequent analyses of manuscript peer reviews in a variety of disciplines likewise have underscored low reliability among reviewers (e.g., Cicchetti, 1991; Fletcher & Fletcher, 2003; Ingelfinger, 1974; Mayo, et al, 2006; Miller & Perucci, 2001; Rothwell & Martin, 2000; Von Blakanic, et al, 1987; Wood, et al, 2004).

Peer reviewers, moreover, may not always be adept at spotting errors, at recognizing studies previously admitted to the knowledge base of particular disciplines, or in detecting fraud. Goodlee, et al (1998) submitted a paper with eight deliberate mistakes in design and analysis to some 200 reviewers used by the *British Medical Journal*. On the whole, reviewers spotted only two of the errors. Similarly, reviewers in a study by Callaham, et al. (1998) spotted less than a third of major errors intentionally inserted in a manuscript.

Peters and Cici (1982) changed the author names and institutional affiliations on 12 previously published papers and resubmitted them for review to the same psychology journals in which the papers had been published. Reviewers detected the ruse with three of the papers. Of the remaining nine papers, referees rejected eight of them (89%) unanimously. An investigative report by the Bell Laboratories (2002) detailed instances of fraudulent data published by Jan Hendrick Schon in scores of articles in first-tier journals, deceptions previously undetected either by the Laboratories or journal referees. Cantekin, et al. (1990) surveyed some notable cases illustrating instances of bias and conflict of interest in peer reviewing. Opthof, et al. (2002) analyzed peer reviews of 3,444 manuscripts submitted to the journal *Cardiovascular Research*, finding reviewers exhibited significant geographical bias, with higher ratings awarded when reviewers and authors shared the same country of origin.

While such findings are disconcerting, the more disquieting fact is that studies of journal peer review constitute to date a relatively small, disparate, and by no means systematic or generalizable body of research. In other words, the academy, whose purpose is to seek, analyze, and promote knowledge based on credible data, appears to have done little homework on this subject either prior or subsequent to implementation of various peer review procedures by modern scholarly journals.

Among respondents to a recent international survey of 3,040 academics in science-related fields, 64% initially described themselves as satisfied overall with the peer review system used by scholarly journals, while 12% expressed dissatisfaction (Ware & Monkman, 2008). In a subsequent survey item, however, only 35% of respondents disagreed with the statement,



Most scientists regarded the new, streamlined, peer-review process as “quite an improvement.”

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“Peer review in journals needs a complete overhaul.” Such bipolar response might be an artifact of this survey’s design. On the other hand, it may point to some disconnection between faith in the propriety of peer review and the experienced realities of its implementation.

A View from Personal Experience

22

I have participated in journal peer review rituals for almost a decade now, long enough to encounter their foibles and short enough not to have abandoned all hope. Like many of my colleagues, I have done so as a suppliant (author and submitter of manuscripts), a priest (performing rites of mediation as a member of four editorial boards, including a six-year stint on my discipline’s flagship journal), and recently as a bishop (editor-in-chief) of a new journal.

While there can be no presumption such experiences are normative, I suspect they are not atypical of the broad scope of dilemmas encountered. As a reviewer, for instance, I struggle with what to do when, despite a double-blind procedure, an author’s identity becomes apparent. I sometimes wonder, when privy to comments of other reviewers during revisions of a manuscript, how we possibly could have read the same paper. I both feel for the author and empathize with a colleague at those times when one of the reviews is cursory, even perhaps superficial, because I, too, sometimes have found it difficult to balance review deadlines with the demands of my own research and teaching.

As an author, I have been on the receiving end of incompatible, often irreconcilable, reviewer comments. I have also learned that a manuscript rejected by one journal stands a good chance of being accepted, sometimes without revision, by another set of referees appointed by a journal of roughly equal stature.

I am aware of an editor’s potential to influence dispensation of a manuscript, unintentionally or not, by assigning to it particular referees. As an editor, I encounter instances where a manuscript has received positive reviews from all referees, none of whom spotted serious

flaws in design or data analyses. Increasingly of late, submissions reflecting attempts at two-for-the-price-of-one or piecemeal publication of the same data dumbfound me. I marvel at the number of such manuscripts eventually finding a published home.

Peer Review as Kluge

Although some discussions suggest scrapping journal refereeing entirely, I frankly see on the horizon no coherent, viable alternative to some form of manuscript peer review. In that stance, I find it helpful to think of our current peer review system as a kluge.

23

A kluge, as recently re-popularized by Marcus (2008), provides a haphazard, inelegant, inefficient, yet somehow functional solution to a problem. Granholm (1962) describes it as an “ill-assorted collection of poorly matching parts, forming a distressing whole” (p. 30). When engineers resorted to a concoction of duct tape and socks to keep astronauts alive on the 1970 Apollo 13 space mission (“Houston, we have a problem”), the result was a kluge.

Current rituals of journal peer review became widespread shortly after World War Two (Burnham, 1990), in concert with a marked rise in research activity and increasing academic specialization. Prior to the 1950s, manuscript review was largely the responsibility of journal editors and in-house editorial staffs. For example, James McKeen Cattell, whose editorship of *Science* spanned the 50-year period between 1894-1945, relied upon his son to review manuscripts (Sun, 1989). Although out-of-house peer review had occurred sporadically long before (Al Kawi, 1997; Kronick, 1990), the advent of xerographic and electrofax copiers in the early 1950s likely enhanced the feasibility and hence the expansion of this practice.

Arguably, the basic problem for which wide-scale peer review was deemed a solution is an epistemological one. What counts as knowledge is to varying extents negotiated and certified through critical discussion. In light of rapidly expanding scholarship, enlistment of working professors, experts in their fields, to participate in the negotiation of knowledge at the critical juncture of pre-publication afforded a greater sense of enfranchisement and face validity than a single editor or in-house editorial staff working alone.

Different journals and various disciplines, however, implemented outsourcing in diverse ways. This “ill-assorted collection of poorly matching parts” persists yet today. Some contexts, for instance, employ single-blind review (whereby reviewers know the author of a manuscript, but the author does not know the names of reviewers), while in other contexts reviews are double-blind (where both authors and reviewers presumably remain anonymous to one another). While some journals solicit at least three peer reviewers per manuscript, many more apparently use only one or two reviewers (Ware & Monkman, 2008). Some journals specify elaborated criteria for review, while others leave such matters largely in the hands of individual reviewers.

Some Items for Discussion

When a kluge imperfectly cobbled in response to one set of circumstances encounters confounding variables that potentially alter those circumstances in salient ways, adaptations must be considered. At least three interacting developments, certainly unanticipated by those implementing peer review in mid-20th century, suggest the problem current peer review practice was crudely designed to address has morphed in some interesting ways (“Houston, we have another problem”).

(a) A rabid “publish or perish” culture now employs journal publication as a dependent measure of quality and consistency for such extrinsic rewards as tenure, merit pay, and laboratory funding. No longer is manuscript acceptance or rejection largely a matter of knowledge negotiation, for all participants (authors, reviewers, editors, and the academy as a whole) are willy-nilly mindful of the professional consequences and rewards associated with the results of journal peer review.

(b) The advent of copy machine technology enhanced the feasibility of peer review in journal contexts. Subsequent inventions of the personal computer, Internet, and World Wide Web, however, appear to fashion a double-edged sword. On the one side, digital technology increases the efficiency of some peer review procedures (e.g., electronic

submission of manuscripts and written reviews) and opens up innovative avenues of knowledge dissemination (e.g., online journals, prepublication access to in-press articles, sophisticated data bases). On the other side, digital technology challenges at least one assumption cherished in some quarters, i.e., the presumed efficacy of double-blinded review.

(c) Exponential growth in knowledge production and dissemination continues, but on a scale unimaginable a half century ago. This part of Toffler's (1980) Third Wave prophecy appears to have been right on the money. Yet even as such growth threatens to overwhelm current resources for manuscript peer review it may underscore anew the need for such adjudication.

25

A self-study on refereeing of grant applications by the National Institutes of Health (NIH, 2008) notes challenges to traditional peer review procedures posed in that context by the "increasing breadth, complexity, and interdisciplinary nature of modern research." The report also offers this appropriate reminder: "it is critical that the processes used...are fair, efficient, and effective" (p. 3). The following proposals, offered simply to spur discussion, address such expectation in light of the interactive developments sketched above.

Reliability. We must seek ways to increase the number of referees assigned to review a manuscript, despite growth in manuscript submissions. Though many aspects of peer review remain under-investigated, a solid preponderance of panel adjudication and psychometric studies in various fields of endeavor indicates unacceptably low reliability ensues from using one, two, or even three judges.

A study by Bergee (2007), for example, employed Generalizable Theory to investigate variability in adjudicators' ratings of music performance. Estimated reliability by generalizability coefficient for a one judge scenario was .47. Addition of a second hypothetical judge increased reliability to .69. It took five such judges, however, to meet a basic standard of .80 reliability, and 17 judges to approach .90 reliability.

Recent experiments with modifications of peer review procedures perhaps begin to address this matter creatively. The prestigious journal *Nature*, for instance, attracted attention in 2006 by allowing authors an option to post their manuscript submissions on a web site. There, other qualified scientists could read and post signed comments. These “open review” remarks, as well as the anonymous comments of traditionally appointed referees, were considered in decisions to accept or reject papers for publication. Similar procedures have become policy in a few other journals, e.g., *Atmospheric Chemistry and Physics*.

Whether or not a self-selected cadre of open reviewers can enhance reliability without sacrificing the face validity of current practices, and whether or not each manuscript would attract sufficient numbers of volunteer reviewers, are matters worthy of discussion and continued experimentation. As will be addressed below, the academy at large needs to examine the extent to which its practices may impede securing sufficient numbers of appointed referees.

Meanwhile, every journal that does not now do so should include somewhere in its front matter journal policy with respect to the number of reviewers assigned to manuscripts, and whether its reviews are open, single-blinded, or double-blinded. Obtuse language, such as “review by a panel of editorial referees” or “blind referee process,” can no longer suffice.

Challenges to the Credibility of Double-Blind Review.

Double-blinded studies are the gold standard in science. Perhaps for this reason, double-blinded manuscript review has been touted as a means to minimize reviewer bias and to reassure authors of the objectivity of the review process.

Several factors dispute the soundness of that assumption. The advent of powerful electronic search engines, in concert with the proclivity of authors both to reference themselves and to present papers at conferences, severely compromises author anonymity. Editors’ efforts to disguise identity by removing an author’s name from the title page and replacing real names

with the generic term “author” in references within the body of a manuscript may even compound the problem, by confirming that a particular body of previously published or presented work is indeed that of the author.

Reviewers need not intentionally seek author identity in order for that identity nonetheless to be revealed. To be conscientious in reviewing a manuscript, for instance, I have at times sought to learn more about an unfamiliar methodology or a particular assertion through use of a search engine. That endeavor sometimes reveals an author’s name beyond a reasonable doubt. On several occasions, I have encountered at conference presentations or poster sessions studies I was reviewing at the time. In all such circumstances, I inform the editor of my discovery; yet, I suspect the author remains uninformed.

27

Electronic manuscript submission and distribution can reveal identities when editors have not been careful to sanitize the document properties feature of newer word-processing programs. Author anonymity, moreover, is likely a lost cause in those areas where relatively few investigators conduct sustained research. If an editor assigns a referee because of his or her expertise in that area, chances are the reviewer can readily identify a particular author.

Blank (1991), in a randomized experiment at *The American Economic Review*, found that 45.6% of reviewers correctly identified the authors of manuscripts under double-blind review. In four trials on the effect of double-blinding. Godlee (2002) reported medical journal reviewers correctly identified authors in 23%-42% of the cases.

Reasonable people of goodwill may be found on either side of this issue. For example, it is also true the studies just cited found that 54.4% and 58%-77% of reviewers could not correctly identify authors of double-blinded manuscripts. No study to date, however, indicates double-blinded review protects anonymity in 95%-99% of the cases examined. Absent that level of assurance, I fail to discern how double-blinded review adequately controls for potential bias or conflict of interest. As Fine (1998) succinctly puts it, “Bias and the impersonal are quite happy companions” (p. 14).

Time Allocated and Time Valued.

Peer reviewers play a necessary role in advancing the knowledge base of a discipline by inspecting carefully the architecture of submitted manuscripts (e.g., premises, validity, methodology, analysis procedures) and by assessing a manuscript's potential contribution to knowledge. Their task differs from that of editorial proofreaders, whose role is to spot errors in grammar, spelling, syntax, and organization.

Most reviewers are university faculty members. Professors presumably have the requisite expertise to review, as opposed to proofread, a manuscript. They also come from a ready pool of cheap labor, in the sense that service traditionally has been among the responsibilities of the professorate.

Professors, however, understand they will be evaluated most for their own research and teaching, and least of all for their service. Professors who referee journal manuscripts also know it takes time to review a manuscript thoroughly.

In my experience, a conscientious review, from initial reading of the manuscript to writing one's two-to-four, single-spaced pages of remarks to the author, consumes anywhere from three to 10 clock hours depending on the manuscript. That timeframe, of course, does not include the extra two hours to review revised versions of some of those manuscripts. Ware and Monkman (2008) found that active referees reviewed annually an average of 14.3 manuscripts. This past year I reviewed 18 manuscripts, in addition to serving on various university committees and fulfilling sundry responsibilities in professional organizations.

That the academy views journal manuscript review to some extent as a form of service to the profession is appropriate, even necessary. However, some rethinking of time allocated in relation to time valued may be in order, particularly in light of increasing numbers of journal submissions and the need to use more referees per manuscript.

Quantitatively, perhaps there should be a reasonable expectation set for manuscript review as service. During those years when faculty members serve regular terms on journal editorial boards, all subsequent reviews beyond the established expectation for service would then be evaluated in annual faculty assessments on a par with published book reviews, or short, invited papers. That tack might also begin to address the quality of reviews by requiring, after appropriate measures to protect confidentiality, copies of the reviews themselves as part of a professor's submitted portfolio of materials. Conversely, faculty might be formally excused from any other form of service, including committee work, in those years when they are active reviewers. In any event, so long as the academy persists in using refereed journal publication as an evidentiary yardstick for advancement in its ranks, it must seek better ways of allocating and valuing the time required both to produce that yardstick and to improve its quality.

Concluding Reflections

Scholarly journals assist the academy's mission by refereeing and disseminating emergent knowledge. Yet the journal referee process itself remains largely under-investigated, a fact that leaves room for legitimate questions about both its efficacy and its fairness. With peer review as the focus of four world congresses to date, the biomedical and health care disciplines appear to have taken the lead most recently in encouraging sorely needed research in this area. May others follow suit.

Results of journal refereeing procedures in many disciplines may not potentially contribute to life or death decisions. Still, they do impact the knowledge base of these disciplines and, obviously, the professional careers of the scholars and researchers who submit manuscripts for adjudication. The kluge called peer review, although more or less serviceable at the time of its modern incarnation, has never been an ideal solution to the problem of knowledge negotiation. But surely in this day and time we must do better than continuing to rely upon duct tape and socks.

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Socrates in Oread: A Dialogue on Responsible Scholarship

Charles Marsh

32

SOCRATES: Ah, Chuck, I see that you too like the free coffee in Oread Bookstore. Come, let us sit here a moment among the few undergraduate readers. But, my friend, you have that vacant look that means you're trying to think.

CHUCK: Yes, Socrates. I'm organizing my thoughts for an essay about responsible scholarship.

SOCRATES: A truly noble topic, Chuck. And what have you concluded about responsible scholarship?

CHUCK: Well, so far I'm for it.

SOCRATES: I'm delighted to hear that. And as you organize your thoughts, perhaps you've begun by asking yourself what is this thing called "responsible scholarship"?

CHUCK (humming two bars of "What Is This Thing Called Love?"): Socrates, responsible scholarship surely means not embarrassing the university with my research.

SOCRATES: It's too late for that, I fear. I see now we have much work to do. Let us retreat a bit. You just said "research." Is scholarship, then, research? Or is it something more – or less?

CHUCK: Scholarship isn't less than research, Socrates. Either it's the same thing – or it's something more.

SOCRATES: Chuck, we cannot organize our thoughts on something until we know its true meaning. We cannot consider or conduct responsible scholarship until we know what it is. Let us turn then to etymology.

CHUCK: To bugs?

SOCRATES: That's entomology, my learned friend. Etymology is the study of word origins. Scholar traces its origins to the Latin *schola*, for school – thus one definition of scholar is one who is deeply involved in a school, such as this university. And -ship, from Old English *-scipe*, simply means “the condition of being.” So I ask you, as an alleged scholar, Chuck, what are the conditions of your being deeply engaged in the mission of this university?

CHUCK: Socrates, the sages have written in the Blue Form that a professor's duties to the university are three in number: research, teaching, research, service, and research.

SOCRATES: I see you have been speaking with those sophists Promotius and Tenuro. But let that pass. Are these three duties equal?

CHUCK: No, Socrates. The University of Kansas is a research university. In terms of scholarship, research is primary – but even I can see that all three functions of scholarship are essential and important.

SOCRATES: Are these three functions of scholarship – research, teaching, and service – mutually exclusive?



Above the Philosophy statue in the Library of Congress is this quote from Francis Bacon's essay "Of Truth": "The inquiry, knowledge, and belief of truth is the sovereign good of human nature." Photograph by Carol M. Highsmith. Courtesy of U.S. Library of Congress.

CHUCK: Yes – well, that is, no, Socrates. I do like to share my research when I’m teaching. I think my students get more for their money; I can add to what they learn from their textbooks and their own research projects. And, now that you ask, I realize that I certainly do my best service in the areas in which I can apply and develop some of my research and teaching interests.

SOCRATES: Is it truly possible that research can influence service?

CHUCK: Socrates, my own research touches on the link between reading and critical thinking. I think the printed – as opposed to the heard – word is essential to intellectual development. So I’m on the University Libraries Committee. In fact, I’m sure I must be late for a meeting....

SOCRATES: Then let us see where we now stand. Scholarship means more than research; rather, it is deep engagement in the mission of the university. But scholarship is research-driven. And for professors, scholarship necessarily involves different fusions of research, teaching, and service.

CHUCK: Well, that sums it up, Socrates. Hey, I think I see Larivierus over there trying to catch your attention.

SOCRATES: And as for the meaning of responsible....

CHUCK: Oh, hell.

SOCRATES: Shall we try etymology again?

CHUCK: Would that be Latin *re* for *re*, and *sponsible* for – well, for *sponsible*?

SOCRATES: I see you know little Greek and less Latin, to bend the words of Shakespeare’s friend. In a literal translation from its Latin origins, responsible means “capable and worthy of promising in return.”

CHUCK: In return? What was I promised first?

SOCRATES: More than you deserve, I suspect. But surely the university promised you job security, especially when you somehow earned tenure, plus academic freedom and some resources for research, teaching, and service.

CHUCK: That's true, Socrates. But I'm a little fuzzy on what I promised in return. I know I promised to conduct scholarship that doesn't break the rules – no plagiarism, fabrication, or falsification. Don't cheat students or miss mandatory meetings. Don't hurt research subjects.

35

SOCRATES: So does responsible simply mean promising to be "legal"? Or does it mean more, or less?

CHUCK: I guess responsible means more than "legal," Socrates. I can think of relationships that involve important promises beyond just following the law. For example, I need to consider the university's values as well as its rules. But I'm still lost: As a responsible scholar, what do I promise "in return"?

SOCRATES: Not only what, Chuck, but also to whom: Responsibility, as you say, implies relationships. In general, you promised scholarship – deep engagement in the mission of the university. But more precisely, what did you promise, and to whom did you promise it?

CHUCK: Socrates, I feel uncharacteristically serious in trying to answer such an important question. I've promised myself to do legal, values-driven research that enriches and satisfies my own curiosity.

SOCRATES: Chuck, we agreed that scholarship is more than research.

CHUCK: But you're interrupting me. My promise is that I will use that research to inform and enrich my teaching, my service, and my area of study.

SOCRATES: This begins to sound like responsible scholarship. And to whom do you promise?

CHUCK: Myself. My colleagues, here and elsewhere. My students. Kansas taxpayers. Hey – in *A Man for All Seasons*, Sir Thomas More adds “God” to a similar list and says, “That’s not a bad group!”

36



SOCRATES: “I underwent peer review in Athens, and I must say that Oread Bookstore’s coffee tastes better than hemlock.”

Photograph reprinted with permission. Greek Picture Gallery Historylink101.com.

SOCRATES: Thomas More? Perhaps, Chuck, we should question the sanity of anyone willing to be a chancellor. But, having defined responsible scholarship, let us see if such a thing exists. You now enjoy the privileges of the university’s promises to you: job security, academic freedom, and support for research, teaching, and service. Can you prove that you are “capable and worthy of promising in return” to conduct research that gratifies your curiosity, enriches your teaching and service, and serves the needs of your discipline?

CHUCK: As a matter of fact, Socrates, last Spring Break I was in Watson Library doing research for a journal article, and one of the librarians said it was too bad I had to work during vacation. And without really thinking – strange as that may sound – I said that I wasn’t working; I was having fun. Great minds like Maslow or Dr. Phil would say that I was into self-actualization.

SOCRATES: Chuck, at Watson Library you were honoring your promise to yourself – and, perhaps, to others. Do you recall what Parker Palmer said about finding your true vocation?

CHUCK: Parker Palmer? You mean Spider-Man?

SOCRATES: That's Peter Parker, Chuck. Parker Palmer is an award-winning professor. He said that professors have found their true vocation when their "deep gladness" meets the "world's deep hunger" – I believe he's quoting author Frederick Buechner. In other words, as a responsible scholar you're honoring a personal promise to develop yourself and a social promise to enrich the lives of others.

37

CHUCK: And if I'm using the resources promised to me to keep my promises in return, then I'm conducting responsible scholarship?

SOCRATES: Exactly. And have you kept your promises to others? Have you shared what you learned at Watson Library?

CHUCK: Yes, Socrates. It was so interesting that I've discussed it in the classroom. And I've submitted an article to a peer-reviewed journal.

SOCRATES: Ah, the infallible peer-review process. I underwent peer review in Athens, and I must say that Oread Bookstore's coffee tastes better than hemlock. I wish you well with that Godlike process. Much to my surprise, Chuck, your Watson Library story suggests that you might be a responsible scholar.

CHUCK: Well, Socrates, my colleagues do say that I'm responsible for a lot of interesting things that happen in our unit.

SOCRATES: I've no doubt you're very responsible in that sense as well. But let us now summarize what we've learned about responsible scholarship.

CHUCK: Perhaps, Socrates, you might do so for me – in a 1,500-word essay.

SOCRATES: But then you would be the minor author – which does seem appropriate.

CHUCK: Socrates, here's what we've learned. For professors, at least, responsible scholarship means using the resources we've been given to become deeply engaged in the research, teaching, and service mission of the university. We undertake research – legal, values-driven research – that interests us as well as enriches our teaching and service, and we share our results with colleagues in hopes of advancing our disciplines. Within the mission of the university, we accept, make, and keep important promises.

SOCRATES: Chuck, you should write that down before this magical caffeine wears off and you return to normal. But who now approaches so angrily from the Philosophy shelves?

FOUCAULT: I'm delighted that you two privileged, middle-aged, white males have developed such a comprehensive, self-serving definition of responsible scholarship. However, if I might speak for the disenfranchised . . .

- End-

Fieldwork in the Counterfeit Paradise: Questions of Interpretation and Responsibility

William I. Woods

The future of lowland Amazonia is a much discussed question and, as with almost all such questions, the answers to the future are largely based on interpretations of the past. Is this huge area a pristine wilderness largely unaffected by humans and, indeed, a counterfeit paradise that cannot sustain all but minimal populations? Or, is it a feral garden capable of becoming a utopia of sustainable development? These diametrically opposed positions have become the cornerstones for stances in an intense debate the implications of which are enormous for the future of this critical region.

Until recently the former was the predominant viewpoint, and with reason. Most large-scale attempts in the modern era to develop agricultural and agroforestry colonization had failed. Huge commercial plantations developed first by Ford and then Ludwig for rubber and pulpwood production, respectively, proved to be colossal failures. In the 1970s the government of Brazil developed a series of roadways termed the TransAmazonian Highway linking the densely populated and desperately poor northeastern part of the country with the Amazonian interior. The stated aim was “to bring a man without land to a land without man.” Although many thousands of colonists rapidly settled along the roadways, most of these smallholders did not succeed and this effort too was largely ineffectual.



**Maize growing on the terra preta, Belterra, Lower Amazon.
Note ceramic shards on the surface.**

The failure of all of these ventures was commonly judged to be the result of the implacable environment; furthermore, it was felt that the same environmental limitations applied to the Amazonian Amerindian groups during the pre-Columbian period. Such peoples have been seen as dependent on extensive, rather than intensive horticulture, with both soil infertility and rare, dispersed protein contributing to the inhospitable quality of their environment. This model pertains to virtually all modern Amerindian groups in lowland Amazonia. However, while the pre-Columbian situation is really just becoming known, evidence is accumu-



The author in newly sprouted maize field on *terra preta* site, Belterra, Pará, Brazil

lating for the prior existence of enormous populations living in large permanent settlements within complex integrated systems, extensive raised field networks and other earthworks, and enormous cumulative areas of anthropogenically enriched soils, termed *terra preta do índio*, or Indian black earth. Of particular significance for the future development of Amazonia are the implications of the anthropogenic soils. If Amazonia once held prosperous and numerous populations supported by intensive agriculture and other productive pursuits it is reasonable to assume that it can again.

I first encountered the Brazilian Amazon as a graduate student in 1974 on my way across Brazil, moving from a project in Argentina to another in Colombia.

The vast expanse of forest below the plane from Brasília to Manaus and later from Manaus to Bogotá seemed to continue unchanging to the horizon interrupted only by an occasional glimpse of a huge river. Although I continued fieldwork in various parts of Latin America in the intervening years it was not until 1993 that I was able to return to Amazonia. Since that time my work there has concentrated on developing information about the anomalously fertile soils, and this essay is a reflection on some of the practical and ethical considerations involved in this endeavor.

In 1974 Brazil was under military rule. The military, being fearful of intrusions by neighboring countries and peoples into the huge, sparsely settled region of lowland Amazonia, had embarked on a program of colonization largely by immigrants from the densely populated northeast of the country. Roadways of a sort were constructed and title to vast areas was transferred to both small and large holders. Incentives were provided that encouraged expansion of large cattle ranching and farming operations. The Brazilian Amazon was to become productive and this meant removing the forest. Largely in response to these measures environmental concerns were voiced worldwide and a period of intensive study of the threatened rain forest and its peoples commenced. My work is an outgrowth of that concern and has been dedicated to developing a firm basis for considerations of the past and their implications to decisions about the present and future. Preconceptions are often not valid and mine were no exception. Where I had expected simplicity and uniformity I found complexity and diversity in physical, biotic, and cultural manifestations and thousands of years of enormous human impact on the environment that is clearly still in evidence today. AND, significantly, most of these impacts were positive ones. The trick would be identifying the specific nature of these successes and translating them into meaningful application in the present Amazonian situation without repeating the disasters of past endeavors.



The Terra Preta Nova Group, Manaus, Brazil, 2002.

The articulations that the pre-Columbian people had with their environment were exceedingly complex ones that had developed over millennia, and they would not be readily duplicated. Missteps could lead to disaster for the still poorly understood ecosystems of the region, as well as its human inhabitants. In addition, related political, social, and economic consequences could also be severe if implementation of this ancient technology was not

properly conducted. Consequently, my colleagues and I had extended conversations over releasing this startling information about the past and potential productivity of the terra preta and associated land use systems. The implications for the future development of Amazonia were quite serious ones not to be taken lightly. We simply could not allow oversimplification or ill-considered implementation to occur; the stakes were just too high.

However, the sense of scholarly responsibility mandated that we report our findings in their proper context and the field and laboratory data were initially published in 1999. This was definitely the proper stance to have taken. While our fears for undisciplined exploitation of this technology were justifiable ones, the reality has not been dire; indeed, quite the reverse. Why? Primarily this was because the researchers involved recognized both their individual and collective responsibilities and decided to that if they worked together closely they could greatly influence the direction of the development and implementation and avoid the chaos of the unplanned.

In 2001 a multidisciplinary, international collection of researchers termed the Terra Preta Nova Group was formed. The unifying factor for this group is a common goal to understand these soils and assure their proper use. Based primarily in Brazil and associated with the governmental organizations Embrapa and INPA; the Museu Goeldi; and the universities of São Paulo, Pará, and Amazonas, members of this group have been conducting collaborative archaeological and ethnographic field investigations, pedogenic chemical and physical compositional studies of ancient dark earths, and agronomic studies through field trials of various forms of organic soil enhancement and crop response. Replication studies have also been conducted with the express goal of providing methods of sustainable agricultural production, particularly by smallholders. These joint efforts have led to an exponential increase in both scholarly and popular publications, documentaries, articles, and websites concerning the distinctive soils, including four edited volumes focusing on this topic and two more in press. Significantly, almost all of the scientific publications are co-authored. Related investigations related to biochar concern soil fertility enhancement, carbon sequestration and emissions abatement, and hydrogen energy generation and are now under way in the Americas,

Europe, Africa, Asia, and Australia. And there is now a Biochar Group that has had meetings on three continents.

So, the finding that at the time of European contact there were extensive areas of fertile, anthropogenic soils throughout lowland Amazonia and that these were associated with large, nucleated populations has not unleashed devastation upon this region. Rather, it has spurred the pursuit of cooperative efforts to provide a firm basis for improving the lives of the inhabitants, not only in Amazonia, but in the world as a whole.

