Epilogue: Lessons for Code Writers, Theorists, and Researchers[1]

“History is philosophy from examples.”
—Dionysius of Halicarnassus, *Ars Rhetorica*, xi. 2

13.1 What we can learn from this history

It is often said that “those who do not learn from history are doomed to repeat it.” Why “doomed”? If history consists of noble achievements, of getting things right, we would be happy to repeat it. There would be no “doomed” about it. What ignorance of history dooms us to repeat is, it seems, the past’s mistakes. History (not the past itself but our rendition of it) consists largely of mistakes from which we are supposed to take the lesson, “Don’t do that!” Yet, according to one of the great students of history, “What experience and history teach is this—that people and governments never have learned anything from history, or acted on principles deduced from it.”[2] The lesson of history is that we do not learn its lessons; we are all doomed. Though sweeping enough to be widely quoted, this lesson seems too bleak to be true.

I do not deny that history, including this history, consists, in large part, of mistakes. How could it be otherwise? Humans make a great many mistakes and find those of others particularly interesting. That is why Hegel thought the history of a happy people (one for whom things generally go well) a “blank”.[3] Yet, it is success, not failure, that we want for our own projects; the successful, not the failed, that we admire and want to copy. The past, even when it is happy, seems to tell us something about the present, about how to shape the future, something we want to know because it could be useful, telling us what to do and what not to do. And, in some fields, such as engineering, we seem to have evidence that the past has been useful in just this way. Keeping good records is central to engineering. Engineers study their mistakes, develop routines designed to avoid making those mistakes again, keep to those routines as long as they seem to work, and in fact are much more likely to make new mistakes than to repeat old ones. Engineering certainly seems to testify to our ability to learn from history—or, at least, from the past.[4]

Even so, learning from history is not without paradox. Marx once observed that while great events may occur more than once, the first time is tragedy while the second is farce.[5] The difference between tragedy and farce is dramatic enough to suggest that Marx doubted that history could repeat itself. The second instance always differs in at least one way from the first. While the first is novel; the second is not. The existence of a predecessor is part of what converts the “tragedy” (the inevitability) of the first into the “farce” (the unnecessary and therefore laughable imitation) of the second. Yet, if history does not repeat itself, how is it possible to learn anything useful from it? Why is it that an engineer who makes an old (and important) mistake is generally out of a job (fired for “incompetence”) when a new mistake would have been excused as “just one of those things”? What can we take from one circumstance and apply in circumstances that always differ in many ways?

For practical people, the answer is obvious: we learn from history just as we learn both from today’s experience and even from the imagined experience of fiction. We simply “see” the similarities in different circumstances, adjusting our conduct in new circumstances to take account of what is old in them. What is not obvious is how we do that. Indeed, explaining how such “seeing” is possible is the domain of both traditional epistemology and contemporary philosophy of science—and questions belonging to any domain of philosophy are questions having at least two plausible but inconsistent answers (and, often, several more than two). I can, therefore, make only a modest claim for the “lessons” I will draw from this history.

The lessons of history seem to be no more than rules of thumb, mere presumptive approximations of what will stand up to experience we have not yet had. If following a certain rule of thumb seems unlikely to turn out too badly, we follow it. If following it ends up badly more than rarely, we develop “doubts” but stick to the rule until we find a better one (since even a rather unreliable rule is better than none). There is no rigor in either the presumption or the approximation. We cannot rerun events to determine what actually caused what; we simply guess and go on. While we can test our guesses against experience, our experience seldom, if ever, speaks decisively. Today’s apparent success (or failure) may turn into its opposite tomorrow. Though every history has a last page, the past does not—or, at least, by the time it does, it will not matter. We are always like the man who, jumping from a tall building and being asked part way down, “How goes it?” responds, “Very well—so far.” When should we declare the “experiment” over? The end of the world is too late—and too hypothetical.

The “lessons of history” are interpretations of events, interpretations that (as T.S. Elliot put it) “a moment may reverse.” These lessons cannot be about the future. The future will resemble the past only roughly. (Almost every day has its surprises, some nasty.) The lessons of history must be about something we can know—without knowing the future—possibilities, what *can* happen (rather than *will* happen). Whatever else history can show, it certainly can show what is possible, possible not simply in the abstract logical sense that unicorns are possible but in the practical sense in which (as we have learned) even an “unsinkable ship” can sink (and that therefore even an “unsinkable ship” should have enough lifeboats for all passengers and crew). Lessons about what is possible in practice are valuable as guides because they wake us to considerations that might doom us if we miss them. No substitute for judgment, these lessons merely aid it, much as a “check list” does. The lessons of history alert us to ways in which we might improve our deliberations. They do not prophesy. Even when stated as imperatives, the lessons of history should be treated as advice: “Think about this.” That, anyhow, is the spirit in which I offer this Epilogue’s “lessons”. They neatly divide into three categories: lessons for those writing a code of professional ethics (13.2); lessons for theory (13.3); and lessons for those interested in undertaking research much like this (13.4).

13.2 Some Lessons for Code Writers

The events recounted in the preceding chapters suggest at least the following lessons:

13.2.01. **Keep the writing of a code of ethics separate from any discussion of licensing (or other controversial projects).** Though some models of profession treat licensing as a defining feature, in practice licensing raises many administrative questions merely having a code of ethics does not. Hence, keeping the question of having a code of ethics separate from questions of licensing, even from the question of whether following the code should itself be a condition of keeping a license, should reduce the number of obstacles in the way of adopting the code. The chances of a would-be profession adopting a code of ethics is, at any time, sufficiently low that not adding to the obstacles seems prudent. (Had the Joint Steering Committee taken even the limited interest in licensing that SWECC did, it might have had as short a life as

SWECC, and the Code might have aborted.) There is also a theoretical reason to keep writing a code separate from licensing. The claim that licensing is a defining feature of profession is itself controversial.[6] That controversy may have practical implications. At least some practitioners take theories seriously enough to vote against a code that seems to them to violate their theory. A code destined for use in licensing may have to allow some conduct a code designed for individual guidance need not. Much of the difference between engineering's two major codes seems to have this origin. The Code of Ethics of the National Society of Professional Engineers was designed to be used by state boards of licensure; the ABET code was not.

13.2.02. **Keep the drafting committee small**. Preparing a first draft of a code is not an activity made lighter "by many hands". It is more like the soup that "too many cooks" spoil. That is why the IEEE's Standard writing procedure, followed in Melford's Guide 4.1.4, advises:

> The WG Chairperson should then identify an individual to author an initial draft. The author should be permitted to prepare the draft with or without additional input or assistance from any other WG members, at his or her discretion.[7]

Every code of ethics needs what Mechler called "a Thomas Jefferson" to do a first draft (5.2). The drafter has two problems that need to be solved before large numbers of people become involved in the drafting. One is content, determining what should be in the first draft. The other is criticism. Even a "first draft" should go through several stages before being declared a fit subject for public discussion. The entire committee concerned with drafting the code should not exceed ten; its core should be no more than three or four. The actual drafting should be one person's work (though there might, for example, be one drafter at an early stage and another later); the early criticism, the work of half dozen or so.

13.2.03. **Keep preparation for the first draft simple**. The problem of content can be solved in several ways. The Gotterbarn-Mechler method is one. Have a small working group read as many other codes of ethics are possible, looking for provisions its members like. Make a list of provisions anyone likes. Circulate the list, asking whether any more provisions should be added (and what those provisions should be). Continue expanding the list until it seems relatively complete. (Provisions to which anyone strongly objects should be put to one side to be revisited once there is a rough draft.) When the list seems complete, give it to the drafter.

Another way to generate such a list is to hold sessions at the appropriate professional meetings. A member of the drafting committee presents some "ethics cases" to resolve—and invites the audience to suggest others that are "different". The audience then seeks to resolve as many of the cases as time allows. Part of resolving each ethics case is stating a "value" or "rule" supporting the resolution. Any value ("candor") or rule ("Don't mislead") the audience finds at all persuasive goes on a list from which the draft will derive content. The drafting begins when the list seems to have become stable (or when the meetings no longer surprise those who lead them).[8]

There are other ways to develop the *initial* content of a code. The exact way does not much matter because the *exact* content put into the first draft does not much matter. Mistakes of content can be fixed later. All that is important about the initial content of the code is that it be, by and large, appropriate to the profession in question and that there be enough of it to get the drafter started.

The history of the Software Engineering Code nonetheless shows that not all procedures are equal. Indeed, that history definitely recommends against two strategies. One is "divide and conquer", breaking work on the code into parts; the other is choosing the architecture before drafting begins, the "Scope" statement.(4.3) These two strategies share a common mistake. They assume that we know more about the future code than we can know without knowing what it will say. Dividing the code into parts assumes we know what the parts of the code will be. A comparison of SEEPP's original division of working groups and the Code's final list of eight Principles provides clear evidence of how hard it is to know even the major points to cover. The main problem with choosing the architecture before the content is not that some structures (say, a short code) may rule out a good deal of content (although it certainly may). The problem is that the architecture of a code should serve its content. Debating the architecture without knowing the content is likely to be uninvitingly abstract, the domain of a few monomaniacs. Gotterbarn's working group was saved from that abstract debate only because no one cared enough about the abstract architecture to respond to his call for discussion of the Scope. Mechler's SEEPP/E was saved in exactly the same accidental way.

13.2.04. **Get a draft as soon as possible *but do not circulate it to any authoritative body or individual until the draft is "final"*.** Like all writing, code writing occurs largely on paper. Without a document, it is hard to do anything useful. To get a good draft may take years, but getting a "working draft" (that is, a draft good enough to work with) need not take more than a few months. Any committee (task force or working group) assigned to write a code of ethics should try to get a working draft as soon as possible. But, having got one, they should be slow to share it with the body that appointed them—or anyone else in a position of importance. We like to show off our accomplishments, and even a first draft feels like an accomplishment. We must therefore take care not to do "what comes naturally". Anderson's experience with the ACM Executive Council (3.3) and Mechler's experience with the Joint Steering Committee (5.6-5.7) tell the same story. Governing bodies are likely to have opinions about what a code of ethics should contain and to express them if asked. Having expressed those opinions, such a body is likely to find it hard to take them back. And having heard those opinions, the drafting committee is likely to want to write for the governing body rather than for the membership as a whole. That would not be such a bad thing if the drafting committee actually knew how the governing body would vote on particular provisions (or on the code as a whole). Generally, though, that knowledge is unavailable. In its place are the statements of a few outspoken members of the governing body or, at best, the body's first reaction, something not necessarily even close to how it would vote if it were voting on a final document.

13.2.05. **Have a well-defined procedure in place for turning the first draft into a final draft**. Those setting up a committee to draft a code should provide such a procedure. When Barbacci advised Gotterbarn and Melford to follow the IEEE Standards procedure, he did just that. However complex that process was, it turned out to be less complex than the one SEEPP fell into when it proceeded *ad hoc*. Because codes are never really finished, an "open ended process" must go on until arbitrarily cut off. Sometimes, especially in small groups (such as Mechler's), participants may understand the arbitrariness as necessary, especially if exercised late enough in the process that participants have begun to tire. In larger groups, however, there are likely to be some who resent the arbitrariness, becoming enemies of the code because they object to the process. So, if there is no set procedure in place when the drafting committee is appointed, one of the committee's first acts should be to define one, a procedure to follow from first draft to final adoption. The drafting committee should make the rules governing its procedure public as early as possible to avoid unnecessary misunderstandings. It should try to get official approval of the procedure but not treat silence as disapproval (or even failure to approve). And it should stick to the procedure (so long as not disapproved)—unless it discovers it has made some serious mistake. Most of the remaining "lessons" concern this public procedure.

13.2.06. **Make the procedure as open as possible once there is a first draft.** The openness of the procedure has two (related) functions. First, it protects the code from the influence of prominent but eccentric individuals who would otherwise "represent" those less prominent. We have seen how often prominent individuals may misjudge the opinions of those they try to speak for. Second, the openness protects the drafting committee both from that error and the error of forgetting about practical considerations altogether (always a tendency in so high-minded an enterprise as drafting a code of ethics). Recall how many "aspirational" provisions entered Version 2 or Version 3 of the Code only to be ejected by Version 4. The IEEE's system of voting with comments is one way to open procedure, especially in a large organization or collection of organizations. Anderson's presenting of drafts at sessions of professional meetings, taking comments, and trying to revise in response before the next professional meeting, is another (3.3). Gotterbarn's survey is a third (8.1-8.2). Even Mark Kanko's survey of his students provided much useful information of this sort (6.7). What all good open procedures share is the drawing in of many people who, until then, had no connection with the drafting but are a fair sample of much of the code's actual audience.

The IEEE Standards procedure has at least three advantages over the others (above) worth noting (even though it was designed for technical standards, not a code of ethics). First, it assures a mix of new people in a way the others cannot. Anyone opposed to the code is likely to object that the code ignores such-and-such a constituency. The IEEE procedure (more or less) automatically answers that charge before it is made (at least for any substantial constituency). The procedure incorporates that constituency into the voting. Second, the IEEE procedure, though selective, is likely to bring in more participants than ostensibly more open procedures. The number of people participating in any way in the writing of a code of ethics is likely to be small. Gotterbarn's original world-wide call for participants brought in less than sixty names. His survey, published in magazines with a combined subscription of perhaps a hundred thousand, brought in just over sixty ballots. The IEEE procedures brought in an initial vote of just under a hundred fifty. Apparently, people are more likely to participate in response to a personal invitation than to a general call. Third, the IEEE procedure sets a limit on how many people need to be brought into the process (enough to have a good representation of each important group). The limit avoids the charge that too few people were involved, an important charge to avoid because the process from drafting to final approval seems unlikely to involve numbers large enough to refute the charge directly.

13.2.07. **Email is no substitute for in-person meetings.** Email seems to have been an important new tool for helping to open the process of writing the Code to people who might not otherwise have participated. They seem to have participated by email when they might not have participated by phone, fax, or conventional mail—in part, it seems, because email is so much easier to use. Email is, however, much more like phone or fax than like a meeting in person. Much of the work of writing the Code was accomplished through ordinary meetings of two to six people. This is striking given the original commitment to do as much of the work by email as possible. Also striking is the importance software engineers themselves assigned to face-to-face meetings; even meeting by conference call was plainly “second best”. This is striking because software engineers are just the people we might expect to be unduly partial to email (and all the other new technologies).

13.2.08. **Plan on a slow process from first draft to final adoption.** There are at least four reasons for preferring a slow process to a fast one. First, rushing tends to increase the number of errors, not only small errors (such as “typos” or unnecessary variety in language) but even big errors like dropping provisions that would otherwise have been adopted or including provisions with relatively little support. Second, a slow process tends to build support. People have time to consider arguments, to learn how many others agree with them, and even to gather information. A slow process allows for more debate, more revision, and even more “politicking”. No one can (justifiably) complain that the code was “rushed through”. Third, a code of ethics adopted too quickly is, all else equal, more vulnerable to repeal than one that has had to win adoption more slowly. Fourth, planning for a slow process forecloses another sort of complaint. *So long as the work is “on schedule”*, there are likely to be few complaints about lack of progress even if the committee takes half a decade to write a code. There will, however, be complaints much sooner than that, perhaps even within a year, if there is no schedule, just as there would be if work were plainly “behind schedule”.

13.2.09. **Find ways to test the code by making people use it (“user testing”).** This lesson, though related to the two preceding ones, makes a different point. The problem that testing the code is to solve is not that of getting the right content or wide participation, but of getting a code that is “user friendly” (one that suits those who are to use it). Claims that such-and-such is the “right way” to write a code are many. Some people claim that codes of ethics should be short, or they will not be used; others, that they must be long or they will not say much of use. Some claim that a code should include principles of interpretation or it will be misinterpreted; others that the interpretive principles will not be used and are therefore a waste of space. And so on. As far as I can tell, none of these claims about how to make professional codes user friendly rests on much more than anecdote or personal preference. Holding sessions at meetings at which members of the audience are given a copy of the code, asked to use it to resolve some cases, and then asked to comment on the ease of use, is one way to test a code. Comparing those responses with responses for a code with the same content but a different format would provide comparative evidence. Another way to do this, less expensive in time, is to use students in the appropriate professional program as the research subjects. Mark Kanko’s students seem to have responded to the software engineering code much as did mature practitioners (6.7). If that is generally true, then professional students could be a convenient stand-in for practitioners.

What is actually needed is a systematic study of codes the results of which would be available in print to anyone about to write a professional code. Until we have such a study (or, better, several of them), the least anyone writing a code should do is some testing to identify serious impediments to using the code. Right now, the choice of code format seems to be one of those domains of expertise in which “nobody knows much” and “the louder they talk, the less they know”. Anyone who claims to know anything about code format should immediately be asked for the evidence.[9]

13.2.10. **Work for consensus** (rather than simple majority). Ideally, a code of ethics should consist of those standards everyone in the group, at her rational best, wants everyone else in the group to follow even if that means having to follow them to. Because people are seldom at their rational best, it is impractical to demand that everyone actually agree to the code. Yet, a bare majority, though practical in the sense of being relatively easy to get, is not a very strong indication of what members of the group at their rational best would agree to. Two-thirds, three-fours, or consensus (no strong objections) is a much better indicator. These higher levels of support, though harder to get than a bare majority, are practical in the sense of being useful (as well as possible). A code that survived by one vote today may die tomorrow with the change of a single one vote. A code that had three-fourths of the votes today is unlikely to face strong opposition tomorrow.

There are many ways to build consensus. The IEEE Standards process is just one. But it is a clever one, since it is designed to move from decisive support (two-thirds) to near unanimity in a way those participating are likely to respect. It commits people to reasons as well as votes; then responds to the reasons in ways likely to get the negatives to change their vote (not only as a technical requirement but with real conviction). Anyone designing a procedure for adopting a code should try for similar effects. The governing body should not find adopting the code a hard decision. The process by which the code reaches the governing body should vouch both for its workmanship and for its support among the general membership. As the IEEE's 1987 code showed, the life of a code of ethics lacking widespread support can be quite short. The process should build support.

13.2.11. **Get a writer to serve as the drafter or at least to work over the draft both at an early stage and again near the end.** By a "writer" I do not mean a grammarian, lawyer, or poet, but (merely) someone whose other work shows that she can write a clear sentence, order sentences so that one seems to follow from another, and produce large works that are a pleasure to read and easy to outline. Steve Unger is a good example of a writer in this sense; Keith Miller may be another. The writer need not have much to say about the substance of the code. Indeed, it may be an advantage to know no more than enough to ask, "What does this mean?" when a sentence is in fact unclear. Version 5.2, though improved in many ways since Version 1, clearly suffers from not having a writer work it over before adoption. A writer would, for example, probably have revised Principle 1 ("act consistently with the public interest") so that it is parallel to Principle 2 ("act in a manner that is…consistent with the public interest").

13.2.12. **Keep objectives modest.** I have already noted how small a group is likely to constitute the core of the writing process, and how few can be expected to take part even in a process of ratification as open as that Gotterbarn presided over. Hoping to involve a few hundred in the process of drafting, revising, and adopting a code of ethics may be reasonable; hoping to involve thousands probably is not (or Gotterbarn surely would have recruited many more participants than he did). Much the same is true about other aspects of writing the code. The objective of writing a code is to get a code that is "good enough", that is, a code (almost) everyone can live with, learn from, and have ideas about how to revise. Over all, it is reasonable to try to make small improvements on the procedures by which earlier codes were written, on the code's form, and on its content. To try for "perfection" in procedure, form, or content is not. Anyone who demands perfection will get nothing.

13.2.13. **Do not list "authors", "contributors", or the like in the code itself.** The Software Engineering Code followed the ACM's in listing names of individuals at its end, indicating that they were members of the body that developed the code.[10] In both cases, the list became a permanent part of the code. This was, I think, an innovation. I know of no other code that has such a list. Credit for helping to prepare a code is usually given in an accompanying report or in the minutes of the appropriate meeting. The names are soon forgotten. To give credit in the code itself may seem a good way to repay the contributions of volunteers who receive no other reward for their work (except the satisfaction of having done it). But there are at least two objections to the practice. One is that including a list of names in the document adds to the cost of reproducing the document, and to what one has to get through to use it, without adding anything to its utility. A code of ethics is not an academic publication but a tool to be used in making difficult decisions. It should be designed accordingly. The second objection is that the list is likely to be misleading. Those listed will not all be listed for the same reason; their contributions may be quite different—and, indeed, some may have made no contribution at all, except signing up for the task force. When I first asked Gotterbarn about his list, I described it as the list of “signers”. He objected:

> At each go round of the Code, different people had differences of opinion. We all worked on a document and I don’t think anyone, including myself[,] is completely satisfied[,] so I guess it is wrong to say the people on the list “signed off” on the Code.[11]

Gotterbarn also did not want me to describe the list as consisting of those who “contributed” to the Code because there “are at least two names on the list that never contributed anything.” Why were those names still on the list? Gotterbarn could not, he thought, simply remove them once he had put them on—even if he put the name on only because the person named had once signed up for the task force:

> There is no way to remove a name from a volunteer task force, so I subtly sent out a message asking people to say how the[y] wanted [their] name to appear on the task force list. Yes[, they] did send a preferred form for their names. So the [name] appeared on all of the publications as members of the task force. Notice the word “active” or “participating” does not appear in front of the word “member” on the task force list.[12]

Gotterbarn may not have been able to avoid the pettifogging distinction between “participating” and “non-participating” members of the task force. But he could have avoided having that problem become a permanent attribute of the Code. He need only have followed standard practice, omitting all names of individuals from the Code itself.

13.2.14. **Never suppose that there are experts on what a code should say**. Over the six years during which the Software Engineering Code developed, a fair number of people, important and not so important, made claims about what a code of ethics should or (more often) should not say. Many of these claims were, at one time or another, significant impediments to writing the Code. In retrospect, most seem pompous, theory-driven prejudices, with no basis in the common sense of software engineers or in any statistical study of what codes of professional ethics actually contain. There are, or at least may be, experts on what codes *in fact* say; and such experts could be quite useful in developing the initial list of provisions and later thinking about how that list might be augmented.[13] But any code of professional ethics is simply a set of (morally-permissible) standards to guide members of the profession in ways they (at their rational best) want each other to conduct themselves. We have no procedure by which to learn what members of a group will want at their rational best except by asking them what they do want, challenging those answers with reasons, and seeing whether their preferences change. Their decisions, after due deliberation, are the best indicator of what the standards should be (though even that indicator is fallible). An expert may be able to spot contradictions, infelicities in expression, or provisions that have a history of causing trouble. What experts cannot identify is provisions that must be included in any code of ethics or provisions that (though morally permissible and competently written) do not belong in a code of ethics.[14]

13.2.15. **Don't expect "blue ribbon" committees to do much work**. A "blue ribbon" committee, that is, a committee of those highly respected in a profession, may be useful for some purposes, such as protecting a proposed code of ethics from criticism. What blue-ribbon committees seem unable to do is write a code of ethics (or other document). There are several reasons for this. Three seem to be particularly important. First, most members of a blue-ribbon panel are likely to be too busy to write much. The code will be just one of many projects, one without (much) institutional support. Second, the skills of the highly respected in a profession generally are elsewhere, no profession having much opportunity to earn a reputation writing codes of ethics. Third, a blue-ribbon committee is likely to consist almost entirely of people successful enough to have hardened in their views and far from the work most members of the profession now do. Codes seem to be written instead by the "middling sort" of professional, long enough in the field to know what it is like but not so successful to be insulated from its pedestrian concerns; they are also likely to be disproportionate academics, even in fields where academics are not otherwise prominent. Many of these academics may (like the ACM's Anderson, 3.3, or me) come from outside the profession itself.

13.2.16. **Start planning early for dissemination, education, and administration.** A code of ethics is simply a piece of paper unless it enters practice. The first step to helping it enter practice is publication. "Publication" used to mean printing—in books, journals, or stand-alone pamphlets. It still does—in part. But web publication has also become an important means of dissemination. An organization that lacks its own web page may still place its code on other web sites. Among these others is IIT's Codes of Ethics Online (ethics.iit.edu/codes).

Codes are, however, not necessarily used just because they are published. They can be daunting documents and, in any case, they are not self-interpreting. Practitioners need help with interpretation. Classroom instruction is one way to provide that help, whether in an undergraduate class or in a session at a professional meeting. Supporting documents, guides to the use of the code, are another way to help members of the profession use the code. Professional societies can also establish "ethics committees" to receive and resolve inquiries from members concerning the interpretation of the code. These resolutions ("opinions") may be formalized and published in the society's journal, in a collection (such as those of the National Society of Professional Engineers, the American Medical Association, or the American Bar Association), or in some other way.

Providing authoritative interpretations of the code is part of its "administration" (as well as part of its dissemination). A professional society may also arrange for adjudication of disputes among members concerning "unethical conduct", for investigation of complaints against members concerning violations of the code, and for other quasi-legal procedures. Like licensing, however, quasi-legal enforcement is likely to generate opposition to the code, even if the process of enforcement is largely or entirely educational. Few people like to be told something they have done is unethical—especially if they are told so by an organization the judgments of which their colleagues or employers are likely to hear of and take seriously.

13.2.17. **Establish a procedure for review and revision.** Like people, codes age. What was up-to-date in 1970 may sound ancient even thirty years later. Part of keeping a code a part of practice is providing for regular reexamination. And even if codes did not age, they would be imperfect, with experience revealing unexpected imperfections or confirming the existence of imperfections already suspected. While it is never possible to have a perfect code, it is always possible not only to improve what age has damaged but to improve what has aged well. The opinions of an "ethics committee" will, from time to time, identify provisions in need of rewriting and even, now and then, a provision that everyone can see should be there but for some reason is not. Where there is no ethics committee, the work of looking after the code can be given to a body with wider responsibilities, such as SWECC was, or to a temporary committee established, say, every five or ten years (for example, in every year ending with a zero). Such a body could carefully review the entire code, survey the users, or compare it with other codes, propose revisions based on what it learned, and then dissolve. Those who write a code of ethics should, if possible, send their governing body, along with the final code, a recommendation for a permanent or regularly reoccurring revision committee. That recommendation should include a procedure for moving from proposals for code's revision to final approval. The procedure should be, more or less, the same as the original procedure for adoption of the code.[15]

13.2.18. **Be wary of official translations of code.** Translating an international code of ethics into the native language of those likely to use it may seem like one of those ideas to which no one should object—and perhaps it is. But it also has consequences worth taking into account before proceeding. A translation is, of course, never quite equivalent to the original. The same problems of choosing just the right formulation of a certain rule reappears all over again each time the code is translated into another language. Even software engineers fluent in both languages may disagree about whether the translation is close enough to the original. Each translation is a major commitment for those who undertake it. As the translations accumulate, revising the original code becomes a much more complicated process than it would be if the code were in only its original language. The translations should be tracked. Any amendment of the original should mean that within a few weeks or months all translations or, at least, all those posted on the internet, should be revised as well. Translation creates a problem of coordination, one that grows as the number of translations grows.

13.3 Lessons for theory

The story told here should teach us something about professions. But what it can teach us depends in part on what it is. I began supposing it to be a work of sociology. I was to describe, analyze, and (ultimately) understand a certain social process, the writing of a code of professional ethics. I have, however, come to think of the book more as history (because, as I reported in 1.5, I have come to trust the documents much more than the interviews). I can only offer a plausible narrative founded on the documents that events left behind (and interviews conducted well after those events). Even when "I was there" (as minor participant), I (the narrator) was not. My own memories granted no privilege even when I was trying to understand my own acts. However dirty the past, historians write with clean hands.

What can history teach us about a social process when all we get out of a narrative is what we put in? That is one of those "philosophical questions" that give philosophy a bad name. Of course we will get out of a narrative only what we put in, but what-we-put-in is not the same as what we *consciously* put it. So, for example, many of the lessons of 13.2 (concerning how to write a code of ethics) were new to me. I had not thought of them before I reached the end of my story and could see it (more or less) whole for the first time. I put those lessons into the narrative without realizing it. The lessons were as much a surprise to me as some of Euclid's theorems were when I first encountered them in high school. Simple axioms (and postulates), *when put together in the right way*, justify conclusions we do not anticipate when accepting the axioms. Even Euclid would doubtless have found some of the theorems his successors derived from his axioms surprising. We seldom know all that our acts imply.

There is nonetheless a question about what my narrative can tell us of "the social process" of writing a code (or organizing a profession). The problem now is "social process" (not, as in 13.1, "history"). "Social process" is an abstraction, a product of social *theory*, something to be stated in general terms. What I have narrated are particular events. Though open to generalization, one narrative does not itself justify any (interesting) generalization. The justification of generalizations must await other similar studies. The lessons about social process we are to learn from this research are (in large part at least) a function of how research not yet imagined turns out. All one narrative can do is raise questions. Many of the questions take the form of a presumptive counter-example to a theory: such-and-such a theory says that all X's are Y's, but here is an X that is not a Y. Whether this presumptive counter-example actually is a counter-example will depend on whether the generalization it threatens can be revised to turn the example into no more than an interesting exception (or whether, upon re-examination of the evidence, the presumption simply disappears). A single narrative is, at best, an invitation to further research and theory.

Does my narrative offer sociology any such invitations? I believe it does. It certainly suggests that we know less about how, why, and even when professions write codes of ethics than commonly supposed. Consider, for example, Larson's classic characterization of (nineteenth century) professions:

> The professional sector of the middle classes aspired to gain at least as much prestige as the most acceptable commoners (or, in America, the most successful businessmen) within this social stratum. This social ambition colored the professional project; it is likely to have inspired the ambition of most of the professionals who responded to the calls of the early organizers, for it is obvious that efforts to secure a relatively new market would have resulted in some benefit for the self-seeking individual.[16]

If we treat this passage as a claim about all professions, including software engineering, we have reason to doubt that gaining the "prestige" of the "most successful businessmen" has much to do with the "professional project" (even "in America"). There is, first, the problem that none of the participants in writing the Software Engineering Code of Ethics ever expressed any interest in gaining such prestige, much less the expectation of gaining it through organizing software engineering as a profession. Indeed, the disparaging comments regularly directed at Bill Gates (and Microsoft) are evidence that whatever "prestige" software engineers want has little to do with success in business. The "prestige" software engineers explicitly said they were seeking—if "prestige" rather than "deserved reputation" is the right word here—is the "prestige" that comes from belonging to a group known to do software engineering well. Those organizing software engineering as a profession (apparently) wanted to avoid the embarrassment of working in an occupation that produced poorly constructed software behind schedule and over budget. They did not want to be associated with what they considered incompetence.

Our research would not raise doubts about what the ambitions of those organizing the first professions were *if Larson's claims about them rested on the sort of historical evidence that our does.* But Larson has no such evidence. Behind her claims for what is "likely" or "obvious" is nothing more than the theory of economic rationality (that people act only, or at least largely, to serve their own private "self-serving" interests). Economic rationality explains much market behavior well. It does not, however, explain all of it. It does not, for example, explain why self-interested individuals would buy life insurance, (anonymously) contribute to charity, support their aging parents, or engage in any number of other acts important in most economies but at least apparently "altruistic". Most thoughtful people, including most economists, understand that a (purely) "economic man" would be a monster to whom the term "rationality" would apply only in a sense (the economic sense). The professions, or at least the writing of professional codes, *may* exist (in large part at least) outside the domain of economic explanation (just as most other forms of altruism do). That, at least, is a reasonable inference from the story told here.

But (it might be said) I have misinterpreted Larson. She did not claim that the "project" of self-interest was conscious; she merely spoke of it "coloring" the (conscious) project of establishing a profession. Indeed, she explicitly noted that sociological theory does not understand "project" in the ordinary sense (as a "planned undertaking"):

> it does not mean that the goals and strategies pursued by a given group are entirely clear or deliberate for all the members, nor even for the most determined and articulate among them. Applied to the historical results of a given course of action, the term "project" emphasizes the coherence and consistence that can be discovered ex post facto in a variety of apparently unconnected acts.[17]

In other words, "project" is probably the wrong word for what Larson has in mind. "Tendency" would be much better for two reasons. First, "tendency" does not suggest planning or even consciousness of the outcome in question. Second, "tendency" allows for degrees (just as "color" does). Tendencies can be strong or weak (while projects just are or are not).

Because a tendency can exist in any number of degrees, disproving its existence is much harder than disproving the existence of a project. For example, suppose (as I have claimed) that this book shows that writing the software engineering code of ethics was primarily (at least at the conscious level) an altruistic undertaking. Larson might agree and yet claim that she can show (using apparently unconnected acts) that it is "likely" that self-interest "colored" the project to *some* degree. Who can doubt that? The interesting question is: To what degree? And the story told here suggests that self-interest can offer little insight into the acts recorded here (or at least, little beyond what altruism already provides). Now, there may be another way to tell this story, one respecting the evidence at least as much as this one does but leading to (something like) Larson's conclusion. That cannot be ruled out *a priori*. But neither can it be assumed *a priori*. We are certainly not entitled to draw conclusions from such a possibility until it exists in a form that can be read, checked, and appraised.

I have picked on Larson only to make a point I could have made against many sociologists writing on professions, including many of the most important. The general tendency of all the social sciences is to look for explanations beyond subjective purposes. Sometimes this search ends in an important insight (such as “the invisible hand” of economics). But sometimes it does not. Subjective purposes explain some events as well as they can be explained. The social sciences then have nothing (interesting) to add; there is only history.

I believe the best explanation of the events narrated here are the conscious motives of the participants. But I might be wrong. From the beginning, I knew that the events I—as participant—knew of were not the whole story. One reason for interviewing participants was to fill in parts of the story that the documents did not reveal. I was concerned, for example, about the possibility of secret meetings at which “the real decisions” were made, rendering the process I had observed mere epiphenomena. There may have been such meetings, especially when the ACM withdrew from the Software Engineering Coordinating Committee (SWECC). But I have found no evidence of “midnight meetings in smoke-filled rooms” at which decisions were made for reasons that could not bear the light of day. No one seems to have thought, or at least suggested, that the motives of participants even at the meeting at which the ACM voted to withdraw from SWECC differed in important ways from those that appeared in official reports or at the meetings for which we have the minutes, much less that the decisions taken in closed meetings require explanation in terms of processes not wholly conscious. When I asked about “the politics” of this or that event, I heard about bad decisions, hasty decisions, ill-informed decisions, but nothing that required me to look for social dynamics bypassing what people knew, intended, and consciously did. The reasons given in public seem to have been much like the reasons given in private and seem to explain the acts of the individuals in question. There does not seem to be any reason to “go deeper”.

Of course, my focus has been on the writing of a code of ethics. Perhaps writing a code of ethics is not important enough to be subject to the social dynamics of concern to serious social theory. I must admit that possibility. While I have argued that codes of ethics are central to professions (1.4), that centrality remains controversial. For most sociological theories, the code of ethics is of little or no significance. The body of knowledge, the curriculum, and licensing are much more important.[18] Because most sociologists may reject my claim for the importance of codes, it is worth recalling that I have not ignored those features of profession most sociologists consider central. My story includes much about the body of knowledge, the curriculum, and licensing. Yet, there was nothing in my story to suggest that the code of ethics was unimportant or that development of the body of knowledge or the curriculum proceeded in a fundamentally different way. The one surprise in my story is licensing, the primary means of achieving the market monopoly at which many sociologists assume professions aim. Licensing was so controversial *among* software engineers (or, at least, among computer scientists) that the ACM withdrew from SWECC just because SWECC *seemed* to provide support for licensing. (12.4) Licensing, though important, was important in exactly the way social theories like Larson’s say it should *not* be.[19]

Whether history presents a good counter-example or not, it may raise new questions for research. For me (as a philosopher), the most interesting question that my narrative raises is why so many philosophers appear in this story of software engineering—and why they appear so prominently. Though lawyers outnumber philosophers ten to one in the population as a whole, philosophers outnumber lawyers in the writing of the Software Engineering Code of Ethics. This was true even though Gotterbarn sought out lawyers (and actually recruited one, Barber). Of the philosophers, two (Weil and I) were (more or less) a simple byproduct of this study. We would not have been there but for the research. Gotterbarn recruited two others (Fodor and Sullivan), though not because they were philosophers. Gotterbarn also recruited one other, Fairweather, not because he was a philosopher, but as a member of the Centre for Computers and Social Responsibility (CCSR). Weckert volunteered, but as a computer scientist, not as a (former) philosopher. Philosophers were not part of Gotterbarn's plan (even though Gotterbarn himself had once been a philosopher). The philosophers just accumulated.[20] They also seem to have been present in disproportionate numbers for the writing of the ACM Code of Ethics.[21] Why? What do philosophers contribute that "mere" computer professionals cannot? Or is the explanation of their unexpected numbers not what they contribute but what they find attractive? Does the pattern reappear in other professions?

There is a similar question about the role of academics. They seem to have a much larger role in the writing of the Software Engineering Code (and, indeed, in the writing of the ACM Code) than their absolute number would lead us to expect. Even in Mechler's little group, they accounted for more than half. (Of the seven, only Mechler, Norman, and Sullivan were not academics at the time they worked on the Code—and Sullivan was a former academic while Norman worked on the staff of a university and even taught a course most semesters.) Gotterbarn's informal drafting committee at CCSR (and his executive committee as well) consisted entirely of academics (though each had considerable experience of practice). Even the original list of SEEPP volunteers (1993) seems to have had a disproportionate number of academics (17 out of 29). Nothing in the documents I have examined suggests a tension between academics and non-academics such as exists in some parts of engineering—perhaps because (as we saw) the boundary between practitioners and academics in software engineering is quite porous.[22] The academics do not seem at all interested in seizing power or using the code to feather their own nest. They seem to be there because they have more time to devote to the work—or, at least, better control over the time they have. But perhaps they are present in professional organizations in unexpectedly high numbers for other reasons too. What might those be?

13.4 Some lessons for researchers

Are there any practical lessons for those who might be considering similar research, that is, research into the writing of codes of professional ethics? I believe there are—though, as with the lessons for 13.2, these are best treated as "things to consider".

13.4.01. **For some purposes, a telephone interview is no substitute for an in-person (on-site) interview.** One review of our original NSF proposal suggested that we interview by telephone rather than in-person. The reviewer cited his (or her) experience for dismissing in-person interviews as an unnecessary expense. After reviewing the (quite small) literature on the question and consulting social scientists we knew who did open-ended interviews, we concluded that the reviewer must have been relying on experience with survey questionnaires rather than open-ended interviews. I now have four more reasons for that conclusion.

First, open-ended interviews tend to last much longer than survey interviews done over the phone. The typical phone survey seldom lasts half an hour. Our shortest interview—with Weckert—was just over an hour. I think there is a practical reason for the shorter duration of phone interviews. Twice during my research, I tried (something like) a phone "interview" (once with Douglas and once with Chikofsky). Each interview was at the suggestion of the interviewee after I contacted her or him about an in-person interview. In each case, the interviewee promised to fill me in on "background politics" (as a supplement to the formal interview to occur later) and, in each case, the phone call lasted less than a half hour. The phone interview seemed to run out of energy in a way in-person interviews (generally) do not. Both times I was surprised at how tired I felt after the interview, especially how sore my arm and ear were. The interviewee likely felt the same. This, then, is anecdotal evidence that phone interviews may have to be shorter than in-person interviews in part at least because holding a phone to the ear is more tiring than ordinary listening. Perhaps too, there is another factor, the special attention required to listen to a speaker without the facial cues, gestures, and so on that one has when conducting an in-person interview.

Second, the in-person interview gives the interviewee a better opportunity than a phone interview to assess the interviewer. The interviewer has the same opportunity. The in-person interview can build a relationship in a way a phone interview cannot. A disembodied voice is not as easy to trust as a full human being. Several of my interviews ended with the interviewee and me together going to lunch or dinner. The interview was over, but we had discovered enough in common to continue the conversation. Most of the interviewees who later sent me documents, or answered questions I emailed, even several years after the interview, were the ones I shared a meal with. The in-person interview paid off in ways I would not have predicted.

Third, the relationship between interviewer and interviewee is different when they meet in person "on-site" (that is, at, or at least near, the home or office of the interviewee). There is, of course, the honor implicit in the time and money that the interviewer has spent to do the interview in person. A phone interview cannot do the same honor. The honor is much increased when the interview is convenient for the interviewee but far from the interviewer's home.[23] (Is the honor doubled if there are two interviewers rather than one?)

Fourth, I conducted about half the interviews in the interviewee's office (academic office, law office, laboratory office, or the like). I learned something about the environment in which the interviewee worked that I could not from a phone interview or even from an interview in my hotel room or at some neutral location. For example, when I entered the Software Engineering Institute (SEI) to interview Barbacci, the first thing I noticed (apart from the impressive scale of SEI's modernist building) was its security. Though located on a university campus, it was plainly not an (ordinary) academic building. There was just one unlocked entrance, with a guard just inside the door, a large desk before him, and a book on top of it into which visitors had to enter their name, "company", time of arrival, and host. After I entered that information, the guard phoned Barbacci, issued me and my graduate student each a "guest badge", and asked us to wait until Barbacci came to escort us upstairs. When Barbacci arrived, he too was wearing a badge, but his had his name and photo on it. The security procedures at SEI were even stricter than at IBM's Watson Research Center (perhaps reflecting SEI's relation to the Department of Defense or just its location on a big-city street). That introduction to SEI helped me appreciate Barbacci's informality (and candor) when we finally reached his office and began the interview.

13.4.02. **Think twice before doing in-person on-site interviews.** I have nonetheless come to sympathize with that anonymous reviewer who thought the in-person on-site interviews might not be worth the cost. Scheduling an interview took about as much time as the interview itself. Travel consumed much more time, especially after the changes in airport security that followed September 11, 2001. Anyone thinking about doing research involving interviews should think about whether in-person interviews are worth the time—as well as the expense—of travel to distant locations. While I think my interviews were worth the time and expense, some interviews may not be, especially if the questionnaire is relatively short, the questions multiple choice or at least short answer, and the information to be solicited not very personal. Interviews at conferences also have attractions that interviews on-site do not, so long as there are several people to be interviewed there, or the site is close to the interviewer's home, or the interviewer would be there even if no interviews were scheduled.

13.4.03. **Take notes no matter what.** When we prepared our NSF proposal, we worried a good deal about whether to tape interviews or just take notes. Our social scientists advised us that taping would not significantly reduce the spontaneity of the interview. We (the philosophers) nonetheless had doubts. Now, having done (much the same) interview with and without taping, I agree with the social scientists. Our shortest interview, our first (Weckert), was not taped (because we did not think to bring a tape recorder); some interviews were not taped because the recorder failed to work. But no interview went untaped because the interviewee refused. Our interviewees, like most of those our social scientists had interviewed during long careers, did not seem to find the tape recorder intimidating. We did, however, have a few occasions when, in the middle of the interview, the interviewee asked to have the tape recorder turned off. In each case, the interviewee wanted to speak candidly about a particular individual, providing background for what was on the tape. In each case what was revealed was not a scandal but the interviewee's personal judgments, something (he or she thought) I—but not the world—should know. After the revelation, with perhaps some follow-up questions (and answers), I switched the tape recorder on again and continued the formal interview.

The problem with taping interviews was not that taping intimidated our interviewees but that it was never entirely convenient. There were always the awkward few minutes at the beginning of the interview when we tried to make conversation while seting up and testing the machine to make sure it would pick up the interview. That over, we could begin in earnest. These preliminaries were, however, not the only inconveniences of taping the interview. Tape for the portable recorders we used could not be made to last over 90 minutes while our interviews tended to last about 120 minutes. The tape would, then, have to be changed at least once (twice if we used a higher fidelity setting). If we heard the recorder click off, we would have to stop the interview to change the tape, breaking the interview's flow. Sometimes, however, the interview was so engrossing, or the click so quiet, that we missed it. Sometimes we soon noticed that the recorder had gone off, changed the tape, and switched the recorder back on. Sometimes we did not realize the tape had run out until the interview was over. (Once we even discovered that we had somehow failed to turn the recorder on.) We soon learned to treat the tape as auxiliary to our notes. Even when I interviewed alone, I took notes (though making notes slowed the interview process and broke the flow almost as much as, and much more often than, stopping to change tapes did).[24]

Another advantage of notes over tape is that turning notes into an "interview report" is much easier than turning tape into one. Transcribing a tape is slow. Few people can type as fast as people talk. Transcribing usually requires stopping, reversing, and replaying the tape once, twice, or even several times, every few sentences. Few academics have the patience for transcription. Giving the job to someone not present at the interview, even a commercial transcriber, has risks of its own, however. Transcribing is not necessarily reliable. Sometimes crucial words are not clear on the tape (even though they were clear to the interviewers). The transcriber should, of course, note any doubts in the transcription, allowing the interviewer to correct the text. But sometimes what the transcriber takes to be clearly one thing is in fact something else. That will not be indicated in the transcript. The only way for an interviewer to catch it is for him to read the transcript while listening to the tape—and even then he may miss the change. If he does not catch it, the interviewee may—but only if the error is inconsistent with what she meant to say.

13.4.04. **What counts as "the interview" is not a question of fact.** Most of the interviews we conducted exist (or, at least, existed) in at least four versions (apart from the event itself). We have discussed two versions already. First, there are the notes one of the interviewers took and second (with a few exceptions) the tape of the interview. The notes (sometimes with assistance from the tape) were then turned into a formal document (the third thing that might count as "the interview"), our reconstruction of what happened. When I worked with a graduate student, the formal document was sometimes close to a transcription. When I worked alone, the interview document was really a summary (with a few good quotations), much shorter than a transcription of the interview would have been.[25] Once there was a formal document the interviewers agreed on, I sent it ("the edited interview") to the interviewee, who was to check it. Interviewees occasionally found that we misreported what they had said, but more often corrected their own grammatical errors or errors of fact. Occasionally, they toned down a phrase, substituted a formal expression for slang, or otherwise treated the document as a rough draft of their final remarks. What they sent back, we called "the approved interview".

According to the "protocol" (the explanation of the research that we gave to the interviewee along with the consent form), we were supposed to mark corrections interviewees made with "Interviewee's Comment". We interpreted the protocol to mean we should mark the corrections when, but only when, we disagreed with the interviewee about what was said at the interview *and* the correction was more than editorial. We never had such a disagreement. We interpreted the protocol this way because it allowed "additions" to be treated in exactly this way.

I naively supposed that the "approved interview" was "the interview" (and the earlier versions were merely stages in achieving that final document). My advisory board, or at least the two sociologists on it, disagreed. The approved interview was one thing, they noted, something careful and contrived, while the transcription (or tape) was something else, the (relatively) spontaneous expression of the moment. The factual and grammatical errors, the slang, the heated expressions, all told something about the interview itself (and so about the interviewee) lost in the revisions. While I conceded their points, I did not have the same concern about preserving the moment of the interview. The point of the interview, as I saw it, was to get the interviewee's help in reconstructing what happened. I preferred an interviewee's considered judgment to spontaneous expression.[26] I thought that the interviewee had a right to shape the interview to his or her satisfaction. For me, *the* interview was not the original oral event, but the final (approved) document.[27]

I was so sure of that that—for at time—I threw out my notes as soon as the interview was approved. Once I had the approved interview, the notes seemed to have no further use. Indeed, they might, in the wrong hands, threaten the confidentiality I had promised. I would have erased the interview tapes as well if I had thought of it. But I had initially bought such a large package that I threw the tapes into a box under my desk once I was done with them, thinking I might reuse them later (never supposing that within a few years digital recording would make tapes obsolete). The tapes still exist only because the advisory board, shocked when I told them what I considered "the interview" and what I intended to do with the tapes when I got around to it, emphatically advised me to save them.[28]

Having saved the tapes, our research group faced a problem not anticipated. Along with the consent form, each interviewee had received the research protocol explaining the research in greater detail. For our purposes (though perhaps not for the IRB's), the protocol was part of the consent form and as morally binding. What the protocol said about the tapes was:

> For our own research purposes, and for the benefit of future historians, we will tape record or take notes of this interview. We will transcribe or write up the notes and send you a copy for comment. You may suggest corrections or additions. Your corrections will be inserted at the appropriate point, but marked as "Interviewee's Comment". We will do our best to keep the text of your interview confidential—unless you notify us, in writing, that the interview may become part of our Public Archive.

There is nothing in the protocol about what will be done with the tapes of the interview (except for the reference to "the benefit of future historians"); and there is nothing at all about what will be done with the notes.[29] There is, however, a distinction drawn between "your whole interview" and a "summary":

> In addition, we plan to prepare a summary of your interview which—subject to your written permission (and also subject to the written permission of any living others named in the summary)—may be placed in our Public Archive as part of a database accessible on line. Should you give us written permission to place your whole interview in the Public Archive, this summary would also be placed in the on-line database. Nothing placed in our Public Archive is confidential. You may withdraw your material from it at any time up to the moment it is placed on line. Simply send a notice in writing to: [CSEP]

The advisory board interpreted the "whole interview" to be the tape—and the approved document, especially when short, to be the "summary".

While there is something in the advisory board's interpretation of the protocol worthy of Solomon, there is also the sort of problem that arises when someone uses Solomon's means (for example, threatening to cut a baby in half). The protocol says that "the portions of interviews for which you have requested confidentiality (including your subsequent comments) will be placed in a Research Archive available only to registered and approved researchers who will be bound by the same rules of confidentiality as the original project researchers." None of our interviewees has requested confidentiality for the tapes—in part no doubt because we gave them the impression that we were only going to use the tapes as aids to memory ("[for] our research purposes"). Should we keep the tapes confidential until January 1, 2020 (when "this Research Archive will have the original material removed and personal references restored to become a Historical Archive available for use by the general public and future historians")? Or should we keep the tapes confidential until the interviewee gives permission to reveal them (or until the interviewee dies)? If the interviewee signed a consent agreement allowing the "interview" to be put online, may we put the tape online? (When we wrote the protocol, putting tapes online was not technically feasible; now it is.)

For now, we are even asking consent to put "the summary" online. While no one has refused to give consent, about a third of our interviewees have still not responded one way or another to our request. Should we put these summaries in level 1, 2, or 3? What about the approved (or merely edited) interviews? The Advisory Board is still considering these questions.

13.4.05. **Not all interviewers are equal; an interview teams including a philosopher may be better than one consisting entirely of social scientists** (anthropologists, sociologists, political scientists, historians, or the like)**.** I conducted about a third of the interviews with a graduate student in sociology, discussing the interviews with him afterward. I soon noticed that we did not pay attention to the same aspects of the interview. I recalled arguments, descriptions of career, and names of organizations; he recalled tone of voice, gestures, odd expressions, and the names of individuals mentioned during the interview (evidence of, he said, "networks"). During the interviews I sometimes departed from the questionnaire to learn more about how this or that job was done, the way one organization was related to another, or other details I thought my interviewee particularly qualified to help me understand. My graduate student would ask such questions as, "How do the people that are supposed to adhere to it see the code?" or "How much of this was driven by concern for the public and software engineer's relationship with the public and their safety?" He did not ask those questions because he believed the interviewee would know the answer. He was simply interested in seeing what sort of answer the interviewee would give. He was trying to understand the interviewee.[30]

The difference between our two interviewing styles strikes me as emblematic of the difference between philosophy and sociology (and so, between most philosophers and most sociologists). I do not claim that I was a better interviewer than he was—or even (what is more likely) that he was a better interviewer than I was. What I do claim is that we were somewhat different interviewers likely to uncover somewhat different information. Insofar as I am right about the difference and its relation to our different disciplines, having a philosopher interviewing along with a social scientist seems likely to improve the interview. Perhaps much social-science research would be better if philosophers were part of the research team not only helping to shape questions or interpret data but helping to collect the data too. Philosophers tend to remind social scientists of the (substantial) contribution of reason to human conduct; social scientists, of that part of human conduct reason cannot entirely explain.

13.4.06. **Think of research like this more as journalism than as ordinary social science.** The protocols, consent forms, and rules concerning confidentiality now governing social science research began in medical research, a response to a series of scandals. Medical researchers undertook to avoid repeating not only the crimes of Nazi medical research but also the deception of the Tuskegee experiments, quasi-voluntary research on prisoners, experimentation on patients and wards of the state without informing them, and other once-common but now clearly objectionable practices. Much social-science research resembles this sort of medical research insofar as the research subjects are young, unduly subject to pressure (students in a class, for example), poor (and so easily won over with small incentives), uneducated (and so easily misled), or otherwise especially vulnerable. Such vulnerable subjects of research in the social sciences need the protection of an IRB for much the same reason that they need that protection when subjects of medical research.

Our interviewees do not much resemble such research subjects. Our interviewees were much more like the "newsmakers" journalists interview. They were (and remain) relatively sophisticated adults not likely to be in awe of their interviewers. They were, for example, quite capable of asking to have the tape recorder turned off for a time when they wanted some part of the interview to be "off the record". For some of the interviewees, such a Bill Wulf, then

President of the National Academy of Engineering, it was the interviewers who were the ones likely to be in awe. Most of our interviewees were far enough along in their careers, and important enough in their discipline, to risk little should they happen to give offense. None was a graduate student or even an untenured assistant professor. They were not (in any significant way) dependent on the interviewers.

The First Amendment legally protects from IRB regulations the work of journalists based in a university in a way it does not protect other university-based researchers. Behind that legal protection is a moral argument balancing the need to protect the subjects of journalistic research against the importance of what may be gained by exempting the research from IRB regulations. I think the same moral argument applies to the research we did. What we could learn from the interviews was important; the vulnerability of our interviewees was low.

That is not to say that the sort of research we did should be entirely exempt from (formal) IRB review. Though our research group was (and remains) relatively conversant with research ethics (a majority having served on an IRB and several of us having as well a scholarly interest in the subject), we benefited from having the IRB look at our research plan. Researchers, even those with a strong commitment to research ethics, have a tendency to overlook or underestimate ethical difficulties in their own research. Having the IRB look over our research plan and make suggestions certainly helped to compensate for our (natural) partiality. The IRB identified questions we needed to think through (for example, how much confidentiality to provide documents). We might have benefited even more from IRB review had its regulations distinguished between our sort of research and the standard social-science research involving human subjects.

13.4.07. **Ordinary standards of confidentiality for medical documents should not (automatically) apply to "newsworthy" documents generated for another purpose.** We may divide the documents collected, placed in the archive, and used (or at least useable) during the writing of this book into at least six categories. For each category, the weight of reasons favoring confidentiality is different.

Part of the archive consists of articles published in a newspaper, magazine (including some sent out to subscribers by email), or online (for example, an IEEE-CS website). These, of course, need not be considered confidential at all. They may be cited in the way any public document may. Closely related to these are official reports widely distributed but not published (for example, some of Gotterbarn's reports to the Joint Steering Committee, distributed to a large mailing list beyond the Committee without any indication that they were to be kept confidential). I see no reason why these should not be treated as if they were public documents (even though, strictly speaking, they are not).

A third category contains the broadcast emails (without any indication that they were in any way confidential). These differ from official reports only in lacking a certain formality. Insofar as they lack formality, they are likely to be less careful in expression. They are more likely to include intemperate remarks, grammatical errors, and other signs of spontaneity. Gotterbarn plainly began thinking of the working group's list as "closed" (much as one might think of email circulated among employees in a firm). That was why, initially, he took care to prevent spamming (by not allowing automatic retransmission of any message sent to the list). While spamming turned out not to be a problem, getting people to use the list—and later the listserv—was. As he sought to make using the list (and listserv) easier, he incidentally opened it to personal messages not intended for broadcast and therefore had to remind users of the distinction between replying to individuals and to "all" or to the listserv. Gotterbarn has given consent to use *his* broadcast emails freely. Mechler, Miller, and most of the others who contributed important broadcast emails have done the same. One important contributor to the list, Prinzivalli, is dead; we felt justified in presuming his consent to be quoted (especially since Gotterbarn considered his contribution to the Code important). We have therefore not had to decide the hard question whether the newsworthiness of this category of email justified quotation where consent was deliberately withheld or just negligently not granted.

I have not, please note, justified quoting these emails by appeal to Gotterbarn's initial notice (now apparently lost) that a research group at IIT would be monitoring SEEPP's communications as part of NSF-funded research. I have not appealed to that notice because I have come to think that the time between it and most of the emails was too great—and that, in any case, the notice seems not to have been given in a way likely to be noticed. While no one we interviewed objected to the use of his or her emails in writing this book, no one recalled the notice, even those who had been put on a SEEPP list in late 1993. Consent should not be supposed to last longer than the memory of it. I think it reasonable to suppose that the memory would last a few months, perhaps even a year, but no longer than that without reminders. Most of the private emails quoted in this book date from a later period. Hence, we have abandoned any appeal to "original consent".

Two other categories of email in our archive have forced us to face the question of using email where we do not have consent (but where it was not refused). One of these categories consists of emails to Gotterbarn (or Mechler) from individuals on one of Gotterbarn's lists who chose to write Gotterbarn (or Mechler), not the list. This is a disparate category. Often, the reason for not using the list seems to have been doubt that it was working, not a desire to keep the communication confidential. Sometimes the reason for not using the list was that Gotterbarn had not used it, for example, because he was some place where he could not use it. His respondent simply pressed "reply"—without thought of confidentiality. Gotterbarn occasionally forwarded emails of this sort to the list when he was again in position to do it, suggesting that he too did not suppose private communication necessarily implied confidentiality. None of these correspondents seems ever to have complained of the re-transmission.

Sometimes the reason for writing privately was that the subject discussed was not appropriate for the list, for example, an editor's response to Mechler journal submission. Sometimes it is not clear why the writer chose a private message rather than the list. For all we know, he may have used Gotterbarn's address, rather than the list's, because he (or his email program) recalled the one and not the other. Where we have quoted emails in this category and not had consent to do so, we have avoided giving the writer's name. We thought this sufficient to protect the author's rights (especially when the author in question was identified in Gotterbarn's email only by a common name and an expired—and opaque—email address).

Student contributions to the Code, whether going to the list or to Gotterbarn alone, posed a special problem. Some of the students (Kanko's) had no choice about contributing. They had an assignment and their instructor passed the results to Gotterbarn. These statudents certainly have a right not to have their names connected with what they wrote. But what they wrote was, I thought, too important not to quote. I have therefore quoted (or paraphrased) them—without giving a name (or any other information likely to reveal an individual's identity. Other students (such as those at Eau Claire) wrote at the suggestion of their instructor but (apparently) without any pressure to write or expectation of confidentiality. They would nonetheless probably be surprised, and not necessarily pleasantly surprised, to find themselves quoted by name in a book. I have therefore also quoted them anonymously. But anyone who checks the endnote and knows the student from class should be able to figure out who he is. This seems harmless since what they wrote was intelligent, not something to be ashamed of even if they have changed their minds since—and they wrote it to a stranger without any promise of confidentiality.

The sixth category of email consists of "quotations". Some of these are true quotations, a few lines taken from a larger document, but most are whole documents the email program automatically placed below the response. Whether the original writer intended the email to be confidential, it is not confidential once quoted in a document not itself confidential. Yet, I did feel a residual duty to say as little about the source of the quotation as consistent with making an appropriate use of it. Where the author writes in an official capacity, especially where the source of the email is important, I have generally named the author. Otherwise, I have not.

In discussing quotation of email, it is easy to confuse two issues, copyright and confidentiality. I believe my quotations are all either "fair use" or covered by consent actually obtained. Though copyright has not been a problem, I could have avoided copyright issues altogether by paraphrasing rather than quoting directly. I have often preferred to quote directly to preserve the immediacy, pungency, or individuality of the original. I have paraphrased only when I saw no advantage to direct quotation. But, even if I had paraphrased every email, I would still have had much the same problem of confidentiality. Confidentiality concerns content as much as expression. People have a (limited) right to control to whom they reveal what they think—whether their exact words or merely the sense expressed.

13.4.08. **One way to compensate for treating this sort of research as journalism rather than "human subjects research" is to have an advisory board that includes representatives of those under study.** In anthropology, it is today common to create a committee of "the community" (a rural village, say) to oversee the study of that community. Such a committee is a way to maintain good relations with the community (by, for example, providing insight into the culture before the researchers commit some gross indiscretion). It is also a way to protect the community from the researchers (by, for example, working out in advance the rules for conducting the research). The community committee considers questions not likely to arise in any ordinary IRB. For example, the community committee might make it a condition of research that no lodge songs be taped, transcribed, or otherwise carried beyond the lodge. Because the community committee provides this sort of control over research, it can vouch for the researchers, easing their way into a community that might otherwise be much less cooperative.[31]

Our advisory board functioned in much the same way even though its membership was only in part drawn from the community under study. Having advisors helped in obtaining documents. Gotterbarn's contribution was, though enormous and irreplaceable, no surprise. We had built it into our original plan. Engel's contribution, though much smaller, was both important and a surprise (though, in retrospect, it seems we should have anticipated much of it). Engel understood the IEEE-CS in a way no one else on the committee did, especially the relations among various committees, certain IEEE terminology (for example, the difference within the IEEE between "IEEE standard" and "IEEE Standard"), and ways in which we might unintentionally give offense by some seemingly unimportant choice of words (for example, by referring to associate members as "mere associate members"). He also loaned me one hard-to-find report from his own files, reminded the advisory board of the larger institutional context in which the process we were studying took place, and provided the name and phone number of sources from whom certain documents might be obtained.

Most important here, Engel—as well as Gotterbarn and Bernstein—spoke for the community under study concerning what emails (and other communications) were appropriate to quote in this book, what should only be paraphrased, and what should only be mined for information. They also helped us decide when it was appropriate to give the source's name. Their concern with confidentiality was not primarily with privacy (as it was for the IRB) but with not (unnecessarily) hurting the feelings of people or entire organizations with whom they might later have to work. Often, the way to satisfy their concerns was not, as at first I supposed, to suppress the quotation in question, but to enlarge it or otherwise provide more context. They did not seem to mind the light of day falling on what they or their colleagues had done; what they minded was having what they had done put in a false light. Only when the question of confidentiality concerned a private individual who wrote directly to another private individual (or to an office holder in some private capacity) did they express concern about confidentiality similar to that the IRB typically expresses.

13.4.09. **There is always another interpretation.** On January 6, 2004, Mechler sent me a dramatic reminder of how different interpretations of the same event can be. The story told here is of well-meaning people devoted to a good cause and working in ways more or less meeting standards of good conduct. Yet, after reading all but this chapter, Mechler posed six "ethical questions" about the conduct I described—and asking what I thought. What I thought was, "Here are six questions my readers should think about":

> 1. If one volunteers, whether recruited or not, shouldn't one give time to the project or resign?
> 2. If one is working on a project with a clear objective[,] should one's ideas rule how the work is done or what work is done or should the objective [rule the work]?
> 3. Is it ethical for Don to change the code without the original developers being involved?
> 4. Is it ethical to develop the code using a political model instead of an ethical model?
> 5. From the beginning to the end of your chapters it appears like the leaders want certain people, stack the membership, instead of volunteers. I know of at least two calls [in IEEE-CS publications] for volunteers and ACM probably had one also. How many volunteers were from these calls and how many hand picked? Or maybe the practitioners of SE didn't volunteer for the task. How many volunteers were there? You never give a count.[32] This point is related to question four [4] above, political or ethical?
> 6. Is it ethical to force a method on reality, method fit to reality vs reality fit to method? I have been trying for years, with papers and examples, to depict another method to use, especially in social issues but have had little success.

There is a popular saying: "Hindsight is 20/20". It asserts, in effect, that once we know that a decision turned out badly, we know not only what we should not have done but also what we should have done instead. The saying is wrong. For hindsight to be 20/20, we would have to know not only what we do know, how things turned out, but also how things would have turned out had people conducted themselves differently (for example, whether the Code would have passed if Gotterbarn had followed an “ethical model” instead of a “political model”). We cannot know the outcome of choices not made without going back into the “history lab”, reprogramming the world, and watching the alternative history reveal itself. Unfortunately, we have no history lab. We are stuck with one set of events and our opinions about what-would-have-happened-if. Some of those opinions may, upon closer examination, prove untenable. Others, perhaps both Mechler’s opinion (that the process could have been more “ethical” and still succeeded) and Gotterbarn’s (that it was a near-run thing as it was and any less political document would have failed), might, upon close examination, turn out to be equally consistent with what we actually know (though at odds with each other). Seldom does history tell us who is right. Hindsight is not much better than foresight.[33]

NOTES

[1] Much of sections 13.1 and 13.2 have been published as “Eighteen Rules for Writing a Code of Professional Ethics”, *Science and Engineering Ethics* 13 (July 2007): 171-189.

[2] G. W. F. Hegel, *Philosophy of History*, trans. by F. Sibree (Colonial Press: New York, 1900), p. 6.

[3] Hegel, *Philosophy of History*, p. 26: “The history of the world is not a theater of happiness. Periods of happiness are blank pages in it…” George Eliot said it better (but a bit later): “The happiest women, like the happiest nations, have no history.” *The Mill on the Floss*, bk. V, ch. 4.

[4] For details on the importance of record keeping even in the early history of engineering, see my *Thinking like an Engineer*, esp. pp. 8-12. The past that engineering uses may well not count as “history” (strictly speaking) because it often does not take the form of a narrative. It is “history” only in the lose sense of “the past”.

[5] Karl Marx, *The Eighteenth Brumaire of Louis Napoleon*, in *On Revolution*, edited and translated by Saul K. Padover (McGraw-Hill: New York, 1971), p. 245: “Hegel remarks somewhere that all great world-historic facts and personages appear, so to speak, twice. He forgot to add: the first time as tragedy, the second time as farce.”

[6] See, for example, Eliot Freidson, *Professional Powers* (University of Chicago Press: Chicago, 1986), especially, Chapter 6 (“The Question of Professional Decline”). While I do not accept Freidson’s definition of profession (for reasons given in 1.3, I think his debunking in this chapter of the independent-consultant model of professions (and, in general, of the single line of development picture of professions) quite useful for freeing up thinking about professions and their codes.

[7] Gotterbarn\94-96 MISC\OPGUIDE.

[8] Kenneth Kipnis, "Toward a Code of Ethics for Pre-school Teachers: The Role of the Ethics Consultant", *International Journal of Applied Philosophy* 4 (Spring 1988): 1-10.

[9] There is some research on “heuristics” that might be relevant. Some of this research concerns the layout of texts generally, but not applied to codes; some, to corporate codes of ethics (that is, the codes of ethics to be used by employees most of whom lack the education and commitment of the typical professional). Such research, though suggestive for design of professional codes, is far from definitive. Yet, any claim to know “what works” would, presumably, have to rely on such research if it is to have any empirical basis at all.

[10] The exact wording for the ACM is, “This Code and the supplemental Guidelines were developed by the Task Force for the Revision of the ACM Code of Ethics and Professional Conduct [with the names following].” The exact wording for the software engineering code is

much the same: "This Code was developed by the IEEE-CS/ACM joint task force on Software Engineering Ethics and Professional Practices (SEEPP) [with the names following]."

[11] Email (Gotterbarn to Davis), October 27, 1999.

[12] Email (Gotterbarn to Davis), October 27, 1999.

[13] For a good example of what experts can do, see Wallace C. Koehler and J. Michael Pemberton, "A Search for Core Values: Toward a Model Code of Ethics for Information Professionals", *Journal of Information Ethics* 9 (Spring 2000): 26-54.

[14] For examples of poor advice on writing codes of ethics, see Karim Jamal and Norman E. Bowie, "Theoretical Considerations for a Meaningful Code of Professional Ethics", *Journal of Business Ethics* 14 (1995): 703-714. This article is but one in a whole issue on codes of ethics. Among these, I found only one that seems to offer good advice (for managers at least): Muel Kapein and Johan Wempe, "Twelve Gordian Knots When Developing an Organization Code of Ethics", *Journal of Business Ethics* 14 (1995): 853-69.

[15] Gotterbarn has doubts about this recommendation: "Do we really want to repeat the nightmare of 4 different approval processes?" GotterbarnChapter12cmt (September 30, 2004). That question answers itself. But the conclusion I draw from that answer is not to use a simpler process for amendment than for original adoption. A simpler method threatens the legitimacy of the amendments. The conclusion I draw is that the process of adoption of the original code should also be relatively simple. Adopting a code of ethics should not be a nightmare.

[16] Margali Sarfatti Larson, *The Rise of Professionalism: A Sociological Analysis* (University of California Press: Berkeley, 1977), pp. 57-58.

[17] Larson, *Rise*, p. 6. This appears as a footnote—with an asterisk at her first use of "professional project". The footnote is unique in the book; all citations and all other side remarks of this sort are given in endnotes. My guess is that the footnote was added at the last minute in response to an editor's concern for what the ordinary reader might suppose.

[18] Consider Larson, *Rise*, again, p. 49: "a successful project of professionalization, one that comes close to attaining the goals of market monopoly, social status, and work autonomy, must be able to combine certain structural elements." A "code of ethics" is not on her list of "structural elements".

[19] That there is in this story not much evidence of self-interest at work in the debate over licensing does not *prove* that self-interest has no significant part. What the story I tell should do is *shift the burden of proof*. Absence of evidence is, after a reasonably diligent search, evidence of absence.

[20] Even in SEEPP's original (1993) flurry of 29 volunteers, philosophers were as numerous as lawyers (with one each, Fodor and Phillips).

[21] The ACM committee had two philosophers (Gotterbarn and Johnson) but no lawyer.

[22] See, for example, Stuart Shapiro, “Degrees of Freedom: The Interaction of Standards of Practice and Engineering Judgment”, *Science, Technology, and Human Values* 22 (Summer 1997): 286-316.

[23] While this argument seems sound, I should point out that the one perfect stranger we interviewed in our backyard, Sigut (when he was in Chicago for a convention), responded much as interviewees we traveled long distances to interview. We had, of course, met him at *his* hotel—and otherwise worked to serve his convenience. Perhaps that explains the similarity between that interview and those we did “on site”.

[24] The new digital recorders, with memory in gigabytes, may finally have solved this problem. I say “may” only because I have not yet used one in an interview. Experience with other new technologies has taught me to wonder what the hidden disadvantages are. (How, for example, will I save what I have on my digital recorder in a form recoverable in five or ten years—after several changes of format and technology?)

[25] There are two exceptions. Steve Barber wrote out his answers in advance. We merely discussed them during an interview of over two hours (in a coffee shop on Wall Street), with me writing in clarifications as seemed appropriate. I then sent him the revised interview, which he corrected and approved. Dennis Frailey also wrote out his answers in advance—both to the original questionnaire and to supplementary questions I sent him later. Our interview (in a restaurant) still lasted well over two hours, with my notes adding many details to what he had written out in advance.

[26] While I cannot deny that spontaneous expressions might give insight into the individual interviewed and (through him or her) into the large context, I have doubts. How does one decide when carelessness is only carelessness and when it is something more? Psychology is far from an exact science even when a psychologist has weeks or months to evaluate a patient. How reliable could our judgments be when we had only two hours mostly busy with other matters—and only a few words to study later? A spontaneous expression might suggest a hypothesis, but the hypothesis would be no more than that without a way to test it. How do we test it—except by trying to include the insight in the narrative and seeing whether the narrative seems more informative in consequence? My experience here is that, generally, the narrative is no more informative in consequence.

[27] Interestingly, there is a disagreement something like this between the common law and civil law concerning “testimony”. For the common law, testimony (ordinarily) consists of the oral statement made in court (with documents having to be “read into the record” before they can be considered evidence). Even the court transcript is simply a representation of the oral event (minus certain improprieties “stricken from the record”). For the civil law, on the other hand, testimony is (ordinarily) a written document submitted into evidence. Oral testimony in open court is more the exception than the rule.

[28] Minutes of SEA meeting of February 7, 2003, p. 4.

[29] Even if there are academic customs settling how historians or social scientists are to share such "raw material", the interviewee would have no idea of them—and it is the interviewee whose consent is supposed to be "fully informed". What the protocol does not say can only be supposed implicit if the ordinary interviewee can reasonably be expected to infer it.

[30] The advisory board suggested a very similar interview strategy, for example, questions such as: "To what extent was the development of a code profession-oriented? To what extent government-imposed? And to what extent oriented toward technical education?" (Minutes of SEA meeting of February 7, 2003, p. 2.)

[31] Alison Wylie, "Science, Conservation, and Stewardship: Evolving Codes of Conduct in Archaeology", *Science and Engineering Ethics* 5 (July 1999): 319-336.

[32] Sec. 4.1 does in fact have a "count" (from 1993-94 for SEEPP) and I did count additional names from succeeding years (for Gotterbarn's working group). These total fewer than sixty. The problem is that I do not have a *complete* count for SEEPP or even for the one working group that seems to have worked. The reason I do not have either is that no complete list of volunteers survives. Even combining all the lists that do survive would not yield a complete list. While Gotterbarn does not seem to have been trying to "stack the committee", he does seem to have continually tried to add to its membership, both in order to have the "right names" associated with it and to have help with its work. His record keeping seems not to have been equal to the efforts he made to recruit.

[33] Compare, for example, this question put by J. Barrie Thompson, "Any Real Progress, or Is It Just Politics and Turf Wars?" *Forum for Advancing Software Engineering Education* 11 (September 15, 2001) http://www.cs.ttu.edu/fase/v11n09.txt: "Why has the project, led by Don Gotterbarn et al, concerned with defining a Software Engineering Code of Ethics and Professional Practice, received such approval and been accepted by bodies outside the US while SWEBOK has led to such controversy?"