TITLE: ON ACADEMIC AUTHORSHIP

ORIGINALLY ISSUED: SEPT, 1985  CURRENT VERSION: SEPT, 1985
CLASSIFICATION: GUIDELINE

SUMMARY:
Presents a systematic discussion of two related issues: first, the allocation of responsibility and credit for scholarly work; and second, those forces that are pushing toward a level of complexity in the conduct of research at which it becomes difficult to determine responsibility of authorship.

RELATED RESEARCH POLICY HANDBOOK DOCUMENTS:
2.2, Rights and Responsibilities in the Conduct of Research
2.7, Multi-Authored Research Papers

AUTHORITY:
Donald Kennedy, President
Endorsed by the Committee on Research

CONTACT PERSON:
Vice Provost and Dean of Research

The 1986-87 Committee on Research examined the issues raised in this paper and resolved:
1) that copies of the Kennedy paper be widely circulated to encourage discussion of authorship before research projects commence
2) that no attempt be made to establish University guidelines to define "significant intellectual contribution" or impose formal mechanisms for determining authorship
3) that faculty be reminded of their responsibilities to insure authorship rights and responsibilities of students and members of the AS-R
4) that periodic surveys be conducted to determine if academic authorship problems emerge in the future.

Dear Friends:

For some time, I have felt a need for systematic discussion within this faculty of two related issues: first, the allocation of responsibility and credit for scholarly work; and second, those forces that, in many disciplines, are pushing us toward a level of complexity in the conduct of research at which it becomes difficult to determine responsibility of authorship.

I am sure you are aware of the national attention given to cases involving misconduct and alleged misconduct in the production and publication of research results. You may not know that at Stanford we have been hearing increasing numbers of complaints and disagreements between students (postdoctoral, graduate, and undergraduate) and their faculty sponsors over credit for work to which both may have contributed.
The pattern of research in many fields is changing. Large laboratories may be under the general directorship of one or two individuals but often include relatively independent work being performed by groups of shifting composition. These changes often produce exciting developments, but at the same time they pose novel problems for the allocation of credit and responsibility.

Increased administrative burdens on principal investigators, arising in part out of more onerous government regulation, may also contribute — by decreasing the time available for active participation in research, just when its importance to academic careers is increasing.

For these and perhaps other reasons, matters of authorship, attribution, and acknowledgment have become more complex; responsibility for work in which we are less personally involved has become more common; and the customs prevailing in different fields have diverged. It is also likely that the incentive for claiming credit has increased.

In the past, appropriate standards in these matters have been determined by the individuals involved, based upon traditions that have grown up in the profession as a whole and in the separate fields. But the steady stream of problems now coming before me suggests that isolated individual judgments by faculty members may no longer be adequate. So I write this memorandum in the hope that it may initiate discussion among my colleagues.

Let me first describe how these problems tend to arise. Some actions, of course, remain relatively simple to label — and to condemn. One who has had no connection with a research project but presents its methods, data, or conclusions as his or her own has committed plagiarism; if the writings of another are taken essentially verbatim, there may be copyright infringement as well. I do not think our common understanding about the straightforward theft of intellectual product has been eroded.

But there are more difficult cases, and they generally involve the allocation of credit for work to which several individuals have contributed something. Often, one participant claims that the joint effort permits publication without the consent of the others, or publication as a sole author, or publication without acknowledgment of the contribution of others.

The following scenario is not atypical: Graduate student S is working in Professor P’s laboratory on an experimental problem within P’s general field of interest and competence. Perhaps S has a research assistantship funded by P’s grant. S conducts an experiment, gathers a set of interesting data, and writes up the results as part of what is expected to become a doctoral dissertation. S discusses the results with Professor P, who subsequently incorporates the data and new findings in a symposium article (or perhaps a grant application), with a cursory acknowledgment of S’s role, or none. S considers that original data from an independent experiment was wrongfully appropriated by P. P views the work as having been done with his support, in his laboratory, within the conceptual framework devised by him, and accordingly views the results as a joint intellectual product in which both have full rights of authorship.

Cases of this kind are especially resistant to easy solution because we are such a diverse and complex scholarly community. Each of us will think of factors not given in this bare sketch that would incline us toward one view or the other — but they will not always be the same factors.
Notions about how to handle joint student and faculty work in laboratories in organic chemistry at Stanford and at MIT will resemble one another much more closely than do those applying to organic chemistry and, say, economics within either institution.

In some disciplines it is quite customary for graduate students to publish their own research results by themselves, even when their work involves fairly close supervision by a faculty member — and in others, the professor’s name goes on virtually every paper produced in the laboratory.

This heterogeneity of custom sometimes makes it very difficult to decide, in disagreements like the one between S and P, whether we are dealing with professional misconduct (the wrongful appropriation of another’s intellectual product) or whether we are in a domain of ethical judgments about the proper allocation of credit between joint researchers — judgments so close that they should be resolved by personal values, etiquette, and generosity, rather than by a faculty disciplinary process. It is especially difficult for the disputing parties to see a matter in the same way.

The resulting level of bitterness can be very high, and the University should bend every effort toward its reduction. Although we probably cannot hope for a set of specific rules that could set criteria for the shared ownership of intellectual product in all scholarly disciplines, some clarification ought to be possible. Here are some propositions that I think are generally true:

**First:** In settings like ours, research and training are closely related, deeply interpenetrating functions. The research apprenticeship undertaken by any student—undergraduate, graduate, or postdoctoral—in the academic domain of a faculty sponsor has as one of its purposes the training and development of the student. The other purpose, of course, is the advancement of the scholarly program of which the faculty member is the leader. The relationship between the two is complementary; but the functions of the student participation, and its value to the student on the one hand and the faculty member on the other, will vary.

In particular, the functions served and the kinds of benefits to each party change with the student’s academic level. In some fields, for instance, undergraduates and beginning graduate students rarely contribute significantly to the intellectual design and content of advanced research; they can be useful as “hands.” The benefits they realize may chiefly be that of becoming familiar with fairly routine procedures, learning the methodology of research, the operation of any relevant equipment, and absorbing the “culture” of the discipline. These are substantial benefits, and the contributions they make are helpful to the enterprise without being essentially scholarly. However, we also should be cognizant of our primary responsibility to our students and alert to opportunities to involve them as early as possible in the genuinely intellectual aspects of the discipline. If we keep using them for routine tasks long after they have gained command over them — or if we fail to recognize by appropriate credit in publications that they have progressed and are making intellectual contributions — the relationship becomes exploitative. And even in the case of undergraduates or beginning graduate students, there will be situations in which they are in fact contributing to the intellectual
content of the project, and thus functioning as colleagues. To fail to maximize all these opportunities for our students is to default on one of our central missions as educators.

**Second:** There is a cluster of questions about authorship and intellectual “ownership” that includes these, among others: What level of contribution by the various parties to a research enterprise qualifies for (co)authorship of the product? What circumstances entitle one to independent or first publication or to the use of data in another publication or project without attribution? In considering these, I am struck by the seamlessness that often characterizes collaborative research, and I wonder if we would do better if we gave thought — and voice — to our assumptions at the outset, even as we understand that those initial assumptions may well change as the course of research progresses.

Even at its beginning, it is often difficult to trace the source of the ideas and insights from which a research project originates; it is hard to say what was identifiably generated by one individual and what was “in the air” in the intellectual domain. Although my personal experience is with the natural sciences, I know that ideas in the social sciences and the humanities are likely to be of similarly elusive, and mixed, parentage. In those sciences where the gathering of experimental data is a regular part of the work, there are matters of experimental concept, design, instrumentation, hands-on execution, and data interpretation; in other fields, an idea is shaped and developed as it is committed to writing, so that the original inspiration may defy reconstruction by the time the project is completed. One member of an experimental team that includes a professor, graduate students, postdoctoral fellows, and technicians may provide an absolutely critical skill, without which the entire venture could not proceed; one contributor to a scholarly project may have had an idea that was essential to the working out of the problem — even though by the time the project is completed it may have so changed from the original conception that the germinal idea is no longer central. The more interactive the process, the less we can retrospectively divide the work into parts corresponding to particular roles or contributions.

Furthermore, faculty members often rely on their own familiarity with the conventions of the discipline regarding coauthorship and other forms of credit, forgetting that students and other participants in a project may be unacquainted with them. Again, I wonder whether departments or laboratories could ameliorate the bitterness of disappointed expectations by a general discussion, in advance, of the ground rules. The understanding in my laboratory was this: If I had contributed to the idea of the project and had also contributed significantly to the hands-on work, coauthorship was justified; but any coauthor had to have a complete enough grasp of the whole effort to defend it effectively in a scientific meeting. This test, or course, is tailored to an experimental science and surely is not the only one applicable even there. Whatever the agreement, it is necessary also that there be a prior understanding of the scope of the particular project or sub-project — that is, both should know the anticipated product to which the agreement applies.

**Third:** Another aspect of the same cluster of issues—who may publish first, who must consent, what connections with the work need be acknowledged and how — is associated particularly with review articles, books (or chapters of books), or symposium
contributions, especially “state of the discipline” pieces. Opportunities to produce more comprehensive works of this kind come mainly to senior scholars. In describing the significant developments in one’s field, there is a natural tendency to include work done by oneself and one’s students and junior associates. In the usual case, the scope of the topic is broad enough so that including all associates as coauthors is impractical and/or silly (although in a few cases it may be managed). Where the piece deals with data or results of others that are already published as a paper or dissertation, or have been accepted for publication, then employing them with appropriate citation is obviously proper.

If the material is yet unpublished but will be issued as a joint work, I think it is generally accepted that any of its prospective coauthors may refer to it, even at length, in a separate work of sole authorship—provided that its joint origin is prominently acknowledged and provided that the opportunity for regular scholarly publication is not preempted. Common courtesy as well as an appropriate concern for the welfare of coauthors of the yet-unpublished work —especially, of course, if they are one’s own students — requires that they be consulted and that reasonable requests be accommodated. If the material is as yet unpublished and if it will not be a joint work, permission must be obtained for any extended discussion and should be obtained as a matter of courtesy even for a passing notice. (Acknowledgment of the source is always essential, of course; if one permits it to be understood that the work is one’s own, I think we are back to plagiarism of the simple kind.)

Fourth: There is a tight coupling between authorship and responsibility. Let us suppose that the name of a faculty member has been included on a paper resulting from the relatively independent experiments done by a student or fellow. If the data are then shown to be faulty, or worse, invented, it seems clear to me that the faculty member is responsible. Indeed, as Provost Albert Hastorf’s 1984 memorandum on academic fraud pointed out, faculty members are generally responsible for the scholarly conduct of staff and students involved in their research enterprises. When one assumes coauthorship, a still higher duty of certainty prevails. The defense of minimal participation in work done in one’s laboratory is generally questionable; surely it is entirely inapplicable when one is coauthor of the disputed work.

Once again, however, clarity is clouded by differences in disciplinary culture. An individual’s place in the list of authors of a work may or may not be a meaningful signal about the degree of contribution: in some fields the authorship sequence is rich in meaning, elsewhere it may be entirely empty. Even within a discipline, customs vary: In most biological papers, the sequence of authors is in approximate order of extent of contribution; but there is also a tradition that places the chief of the laboratory last regardless of the relative weight of contribution, and at least one leading journal extracts all meaning from sequence by requiring alphabetical listing of authors.

But those complexities only underscore the importance of establishing sound principles for determining coauthorship and provide further reason for extreme care by faculty members. While I understand the need to respect the nuances that readers (including prospective employers) will derive from author sequence in a journal article, it does seem to me that as a prima facie matter, each coauthor of a work is accountable for its
authenticity and quality. Shared credit should entail shared responsibility. Where, by custom or agreement, that is not the case—in fields where 10 or more names regularly appear on a single paper as coauthors, for example, or where the work is a collaboration between scholars from different fields who lack intimate command of one another’s areas—some explicit disclosure of that fact would seem desirable.

Finally, let me offer an observation on a different topic, one that has an oblique relationship to the issues considered above. I note that the pressures have increased—unacceptably, in some disciplines—to produce immense lists of publications. Those who have served on the Advisory Board, or who have seen promotion and tenure files from throughout the University for other reasons, will know the extraordinary range in numbers of publications. In the humanities, most social sciences, and some natural sciences, only a handful of works will be listed. In some Medical School departments especially, there may be more than 100 publications in a period of five or six years.

Again, the functions and traditions of publication vary by field: In some, it is (or seems) important that each small step be documented and circulated as soon as possible. In other disciplines, even primary research work is published only after extensive collection of data and the attainment of some theoretical outcome. Works of the same size can, of course, be more or less significant, as scholars in the field will immediately discern.

Nonetheless, I think the exaggerated growth of publications in some fields has become pathological, and I would like to see the problem addressed here at Stanford and, perhaps, in conference with our colleagues elsewhere. It seems likely that the pressure to have a list of publications several pages long to achieve tenure, or to produce a dozen or more papers a year to maintain one’s status as a productive scholar, may lead to the kinds of behavior mentioned above: to the seizing of authorial credit where it is not due and, perhaps, to ungenerous attempts to exclude others from authorship. Indeed, there are suggestions that it contributes to the likelihood of outright academic fraud. It is very well to say that such behavior is improper even in the face of great pressure. We do say it, and mean it. But should we not question the propriety of letting the pressure continue to increase, and address ourselves to means of relieving it while maintaining (indeed, enhancing) the quality of scholarly output?

These reflections do not cover the full range of problems in connection with authorship, acknowledgment, and use of intellectual product. As I said, I hope they will provoke discussion and, if not resolution, at least steps toward the expression of commonly held principles. Surely many problems can be ameliorated by a constant awareness of the special duty of faculty to foster the intellectual growth and independence of their students, by a habit of generosity in giving as much credit to their contributions as is consistent with realistic appraisal, and by the meticulous observation of strict standards of citation and acknowledgment. Others are harder; they may ultimately yield, if at all, only to the careful development of consensus about the ethics of our profession.

To begin our consideration of these issues, I am asking the Committee on Research to examine them during the coming academic year and to forward their views to the Provost and to me. We shall then discuss their further disposition with the Academic Senate.