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Research Funding and the Value-Dependence of Science[1]

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> The traditional search for objectivity exemplifies science's pursuit of one of its most precious ideals. But for the scientist to close his eyes to the fact that scientific method *intrinsically* requires the making of value decisions, for him to push out of his consciousness the fact that he does make them, can in no way bring him closer to the ideal of objectivity. To refuse to pay attention to the value decisions which *must* be made, to make them intuitively, unconsciously, haphazardly, is to leave an essential aspect of scientific method scientifically out of control.[2]
>
> Richard Rudner

An understanding of the ethical problems that have arisen in the funding of scientific research at universities requires some attention to doctrines that have traditionally been held about science itself. Such doctrines, we hope to show, are themselves central to many of these ethical problems.

It is often thought that the questions examined by scientists, and the theories that guide scientific research, are chosen for uniquely *scientific* reasons, independently of extra-scientific questions of value or merit. We shall argue that this is an illusion. It is an illusion to think, especially in the present era, that science can even *have* a coherent direction apart from extra-scientific considerations.

In setting the stage for our argument, it is useful to try to picture what science might be like if it were guided by scientific considerations alone. For one thing, it is plain that the choice of problems to examine, whether guided by considerations external to science or not, must at least be guided by *something*. No one--not even Francis Bacon--has supposed that science can get anywhere by randomly jotting down data. One chooses problems that are *interesting* or important, and one is guided in one's quest for answers to such problems by hunches (or "hypotheses," if you like) about what is relevant.

Just collecting data, with no guiding hypothesis, no problem that the data is supposed to be relevant to, makes no sense. A picture of physics or biology or any other science that imagined the day-to-day work of its practitioners to be anything like this aimless data-gathering would be even stranger. Science, on such a portrayal, would be a paradigmatic example of incoherent activity. It is thus understood on all sides that scientific activity must be directed activity. It is anything but aimless.

Yet it is widely held that science, *qua* science, is guided, in its choice of problems to examine and hypotheses to explore, exclusively by considerations that are internal to science itself. While it might readily be conceded that there are countless examples of external motivations of scientific research to be found in the historical record, such extra-scientific influences on the activity of scientists are often taken to be indications of human failing.

Science is thought to be best pursued, on the view we are considering, when it is objective and disinterested, and when scientific activity takes its direction from pure scientific concerns. This might be held to be a matter of working out the details of some theory, resolving some puzzling anomaly provoked by disputes among scientists, adding to the list of anomalies a new puzzle, deemed important only because it is a puzzle, and so forth. This, it might be argued, is the proper--and, possibly, the typical--work of scientists and of science departments in universities.

The question of external support for scientific research becomes relevant at this point. Scientists have scientific motivation to pursue their work, the argument has gone. Where research gets expensive, outside funding may very well be required. But the choice of what research to do has already been made, guided by scientific considerations alone. If scientists can find a willing source of funds, then the work proceeds. If funding is not available, scientists do what they can on a shoestring, and search with even greater vigor for funding sources. But they do not tailor their main lines of work to the demands or interests of funders; they look for funders with common interests.

Thus, on this view, it is simply incorrect to imagine that the external interests of funders direct or determine what scientists do. What might appear to the critic to be the sale of university research facilities to the highest bidder, and the consequent corruption of the university, is a simple misinterpretation. Scientists pursue their work independently of the interests of funders, although they are happy to find funders when they can. The funding is interested, but the funders are not in control.

We think this view is an illusion:[3] scientific research never proceeds without extra-scientific values entering in.

I.

The remarks quoted at the beginning of this essay--made by Richard Rudner in 1953--were originally directed at what Rudner perceived to be value judgments at the heart of scientific procedure itself. Rudner was emphatically not addressing the question, at issue here, concerning how decisions get made to pursue scientific research in this direction or that. Indeed, Rudner's own inclination appeared to be to conclude that such questions are of course subject to extra-scientific motivation.

But Rudner, like most philosophers of science at the time, took this to be quite irrelevant to the questions that he took to be central to an understanding of science. It was scientific method that made the difference between science and non-science, that accounted for the apparent success of science, and that therefore needed careful philosophical explication.

To understand scientific method is to understand science, and understanding method appeared to be a matter of understanding characteristic styles of reasoning within science. Questions about method, on this view, are essential to understanding science; questions about how interest or attention gets directed are accidental. The crucial problems for science concern questions about the relationships between evidence and conclusion, about hypothesis and experimentation, and so on.

Rudner's argument about the role of value judgments in science was notable, in fact, just because it was not based on the observation that what a scientist deemed interesting was a question of personal taste, or that the hopes and other interests of scientists may affect their ability to evaluate the results of experiments dispassionately. Rudner argued that the very decision that a particular scientific conclusion was supported to some particular degree or another by the evidence was itself irrevocably a value judgment. This was especially striking in an intellectual environment in which understanding science was regarded as almost exclusively a matter of understanding the logic of its method.

Subsequent developments in our understanding of science have led, fairly uncontroversially, to a broader understanding of "scientific method." Against the background of earlier views that had portrayed such method in strict logical terms, "scientific method" may even appear to be an awkward or otherwise inappropriate name for the widely based set of factors that appear to account for the family resemblances among sciences and scientists. What is scientific is broadly seen to be a function of characteristic modes of activity identifiable in the practice of entire communities of scientific practitioners.

Such a move may already have been hinted at by Isaac Levi, in his response to Rudner, when he argued that

> . . . the tenability of the value-neutrality thesis depends upon whether the canons of scientific inference dictate assignments of minimum probabilities in such a way as to permit no differences in the assignments made by different investigators to the same set of alternative hypotheses. An answer to this question can only be obtained by a closer examination of the manner in which minimum probabilities are assigned in the sciences.[4]

The discussions that dominated the attention of philosophers of science in the 1960s and 1970s--concerning the growth of knowledge, the criterion of demarcation between science and non-science, and the rationality of science--led to more or less grudging agreement among the contesting parties on at least one central idea: decisions about which is best, among competing scientific theories, are inevitably underdetermined by the data and require the exercise of judgment.

Part of what has to be decided, in such situations, is the question of which way of approaching a given problem area is most apt, or most likely to be fruitful. Thus discussion shifted, to a large extent, from focussing on the logical relation between abstract theories and concrete observations to a focus on ways in which entire communities of practitioners progress--or at least change allegiance--from one way of looking at their domain of inquiry (from one theory, or paradigm, or "research programme," or whatever) to another. Again, the judgment of how to look at a domain is a judgment that allows room for disagreement among reasonable scientists; it is underdetermined by evidence and underdetermined by logic. This much seems by now to be as uncontroversial as such things get. Whether such judgment is "rational" or not, and the extent to which logic and evidence have real force in the judgments that get made, continue to be contested questions.

Scientific judgment about which of two alternative approaches to a problem area is best, it is now widely agreed, is not, in particular, a function of whether any of the competitors can survive the critique of opponents (since any theory can survive, come what may), but rather of which approach serves which ends best. That judgment is a compound one. First, the successful approach is the one that will serve the most important ends best, and this question of importance is a value judgment. Second, given a judgment about which ends are most *important*, the successful approach is the one that serves the most important end *best*, all things considered. This is also a value judgment.

To Rudner's conclusion that value judgments must enter into the decision that a particular conclusion is supported by the evidence must thus be added the conclusion that entire approaches to given problem domains must be chosen on the basis of value judgments. Indeed, these value judgments will often involve decisions concerning which problem areas are most important to explain or control.

Now, these valuations of relative importance may well still be regarded as internal to science. For while it may be true that a given theory may come to "replace" another because of its greater success in a particular problem area that is deemed important, this very importance may be no more than a reflection of factors that are truly internal to science. For example, problems explainable on the new approach may have been especially annoying anomalies on the old; problems explainable on the new approach may appear to be fundamental, in the sense of offering clues to other matters; and so forth.

Such factors in decisions concerning which problem areas are most important, and thus concerning which theoretical approaches are, in the long run, most fruitful, must not be ignored. But to think that they are the only factors, or even the most important, would be to commit the same kind of error identified by Rudner. For scientists to close their eyes to the fact that even theoretical progress in science tends ever more glaringly to move in directions that meet extra-scientific need would be to take a peculiarly un-objective view of science itself; it would involve leaving an essential aspect of scientific practice scientifically out of control.

Putting the matter like this shows, we hope, that this involvement of extra-scientific values in the practice, and thus in the growth, of science is not necessarily a bad thing. Indeed, it tends to highlight the value of science within human communities. The need for better medical techniques in particular problem areas, the need for improved methods of cleaning the environment, and the need for better ways of generating clean energy thus stand side by side with desires for wealth and more deadly weapons as among the extra-scientific demands that move science in one direction or another.

But extra-scientific considerations enter the scientific enterprise in other ways as well, and the effects of these introductions of value judgments are often deliciously more subtle. Once a university department has filled its faculty with competent professionals, it may hope that profit-seeking, for example, will play a secondary role to pure science in activities undertaken by faculty. Indeed, such hopes may very well be warranted; a professional science faculty will, more often than not, behave professionally because it wants to. But what, for instance, has determined which scientists were hired?

Universities must have professionals trained in the discipline, equipment they can use in their inquiries, buildings to house the equipment, students to work in the labs, funding to attend meetings, draw up notes, keep track of experiments, and so on. A scientist is struck with a problem, finds amenable graduate students, works on obtaining funding, gets equipment that is needed, finds the time to engage in the project, and so on. Teaching faculty have to be chosen and allotted among the several courses considered vital to the undergraduate and graduate curriculum. The curriculum itself must be evaluated on a fairly regular basis. And within each course in the curriculum, decisions must be made about texts and about relative weighting of different topics.

At each juncture in this enterprise judgments must be made, usually among competing claims. One could illustrate this point for any university or college. The sorts of extra-scientific considerations we think enter into the development of programs, the selection of faculty, and so on are common to every institution of higher education. We have chosen for our illustration the Rochester Institute of Technology, which has recently been in the news, both in the United States and abroad, for its connections with the CIA.[5]

II.

For many years, the Rochester Institute of Technology had a relatively strong program in what was called "photographic science."[6] With such a program, the school had a faculty, with relevant training and skill, interested in certain lines of research, and it had students who enrolled in the program to learn a trade and, in many cases, to pursue that research with that faculty. Over the course of many years, as is understandable, given its interest in new possibilities for gathering intelligence, the CIA became interested in RIT's program.[7]

RIT has a flourishing "co-op" arrangement in many of its undergraduate degree programs, and this is one feature that seems to attract many students to RIT in the first place. Students have the opportunity, when the program works well, to spend time during their undergraduate years working in the field they are training for. This allows them to see the "real world" side of the field, as opposed to being limited to the academic side available on campus. It has the added benefit of giving them valuable contacts with prospective employers well before they are actually ready to look for permanent jobs.

In the case of the photo-science students, one of the earliest arrangements between RIT and the CIA involved co-op work opportunities with the agency. Upon graduation, RIT students occasionally found jobs with the CIA.[8]

As time went by, the coalescence of interests between the CIA and the photo-science program at RIT broadened, and in 1985 RIT found CIA support for a proposed PhD program in imaging science. Such support eventually came in many forms, direct and indirect, and the PhD program was inaugurated a few years ago at RIT. Notably, this is RIT's very first (and, to date, only) doctoral program.

One source of RIT's interest in pursuing this line of development appears to have been that the development of a PhD program would be only the first among other future doctoral degrees. RIT felt that, in any area where "Documented external demand, an internal base of academic strength, and significant financial support" were present, it was appropriate to plan RIT's development to include PhD programs.[9] The associated increase in prestige would attract more funding, better students, and perhaps even more students, with all the attendant advantages to the institution.

At the same time that RIT was developing its doctoral program, RIT researchers began to become engaged in some research, funded by the CIA, that the CIA felt it necessary to classify. This work was conducted at RIT's Research Corporation, an entity that was deemed by those who established it to be sufficiently independent of RIT to meet the demands of both the CIA and the Institute.

That is the short version of what has become a very long and complicated story. But it will suffice for present purposes. The story is offered here as an illustration of what many people have in mind when they say that the interest of the researchers comes first. External funding may be found to support that interest, but the relationship is merely contingent–a congenial pairing of scientists with outside parties. It is not that the research is directed by these outside interests; it is merely supported by them. The direction the research takes is guided by scientific considerations, not the CIA's interests, it is claimed, and the fact that the CIA profits from scientific progress should neither come as a surprise (who does not profit from scientific progress?) nor be overblown by critics into a fear that the CIA is in the driver's seat. Individual researchers' concerns to further scientific interests led them to seek outside funding, and nothing about that implies that the research undertaken was controlled by anything but the concerns of science.

Such a story is not unique, of course. One could tell a similar story for almost any major university. When the University of Chicago was chosen as a primary site for research into nuclear fission during World War II, it was a boon to the university. Surely the University of Chicago could, and no doubt did, have motives for taking on the research that were directly related to the well-being of the university and its faculty, and the work would not have been done but for government funding. One could tell similar stories of convergence of interest for universities engaged today in work--funded by for-profit corporations as well as by governmental agencies--in biotechnology or in microelectronics.[10]

The stories all go like the RIT story: initial expertise and scientific interest among university faculty finds congenial partner in government or industry. Neither the researchers nor the funders see a potential partnership as anything other than precisely what they say it is--a convergence of interests. And so, at one level, it is.

But science is a dynamic undertaking, and what it is can no more be assessed by taking a snapshot at the moment of such an initial agreement than a river can be understood as fully described by the particular water that is in it on a particular date.

At RIT, early interactions between the CIA and the Department of Photographic Science led to cooperation in some projects but not in others. How could it be otherwise? As the idea of improving and expanding the program began to be considered, this had to be done in the way that simple wisdom dictates. RIT considered the strengths of its faculty, the marketability of various program alternatives, and the potential for financial support from outside the Institute. How could any reasonable academic institution do otherwise? Decisions made to focus on these potential programs rather than those, upon those particular research issues rather than these, could not, in reason, be made without considering potential funders, and potential employers of the students who would graduate from whatever programs were chosen. And once decisions upon programs were made, faculty and administrators would need to be hired to improve the university's ability to deliver the full program promised.

The program was thus embarked upon, a new building was constructed, expensive equipment bought, faculty and administrators hired, and students recruited. The university, in short, was changed considerably.

The crucial issue, however, is not so much a matter of the new things that were now being done where they were not done before. Rather, one must remember that all these things were choices. That is, there were alternatives not chosen. Thus there are many things that might have been done at RIT by now, but which are not done, because attention and resources were not devoted to those things. In particular, there are directions that might have been taken within the imaging science program that have not been taken because resources have not been directed in those ways.

That, in turn, means that what understands itself to be the only PhD program in imaging science in the country--perhaps in the world--will be training people in a particular way, with particular potential employers in mind, working on research projects funded with particular potential end uses in mind. The future development of photographic science itself may thus have been changed.

There is nothing wrong with this, of course; it is the only way things can be. The point is not to assail universities over programmatic decisions that are influenced by interests external to science, but rather to point out the importance of such interests.

Such examples begin to illustrate the deep penetration of consideration of values into the scientific enterprise. The list of factors that inform decisions about the relative importance of various problems that might be investigated, and thus the relative value of different approaches, is no doubt long. But whatever else that list includes, it must include such extra-scientific considerations as need or demand. Not to include such things would be impractical and, probably, irresponsible. The enormous amount of basic research in the last ten years into the structure of viruses and how they penetrate and replicate is driven in no small part by concerns about AIDS and the need for some control over its spread.

Consider what settles the question of whether a particular theoretical solution is adequate. Again, the list is long, but it must include the promising solution of important problems. And the relative importance of problems vis-a-vis one another is often a matter of satisfaction of important external needs or demands. Which ones those are is surely not a scientific question.

Whereas it may once have been true that the development of theory was almost solely motivated by theoretical problems set by theoreticians, and technology could be understood as a "spin-off" of pure theoretical development, one measure of the success of science has been that this is no longer the case. Science is now beleaguered with questions and demands for solutions to problems that are technological. The questions that are of the most stunning theoretical importance, in fact, often come precisely from developments outside science as such. Theory is thus driven by such technological demand, and often finds itself behind technological capability, struggling to come up with theoretical answers to questions that have arisen in practice.

Actual and potential shortages of energy have thus driven scientific investigations into new energy sources--cold fusion being the most notorious example of a failure, so far--and into new investigations of old sources. And the success of these inquiries will be in part determined by new practical sources of energy. To the extent that external value drives the research, and to the extent research drives theoretical development, the development of theory depends on these value judgments.

These concerns about what informs a line of research and what settles the issue of whether a particular theory is adequate is a concern about the practice of science. To ask such questions is to look beyond the relation between a theory and its data, for instance, to the ways in which scientists engage in research. But once one makes that switch in the way one looks at the enterprise, one can see a multitude of ways in which values enter.

One extremely interesting feature of this way of looking at things must not, however, be neglected. There is no denying that, from the perspective of the well-supported researcher at a research institution, the funding for the work may very well look like a fortunate convergence of interest between researcher and funder. Researchers are aware, of course, of the necessity to write up research proposals just so to maximize the chances of support. Indeed, universities have entire staffs devoted to the cultivation of just these skills. But this is regarded only as a matter of facilitating the convergence. It simply is not the case that, in any significant way, successful researchers have been forced to sell out to their funders.

Yet university scientists who understand themselves to be asking only for funding for research that they are independently interested in have neglected the factors that got them into their faculty positions in the first place. They have been selected by a process that is anything but "value-free." Among the many candidates for the jobs they occupy, they have been chosen, in part, for their ability to bring funding to their institutions. And such factors are of course not neglected in the course of promotion and tenure decisions. It is actually likely that the faculty members of prominent research institutions will have a robust independent interest in just those issues that will find convergent interests among funders.

Consider another variable in the enterprise, the increasingly high cost of scientific equipment. Measuring time to the nano-second is important for some experiments in physics, but the equipment to do that is inordinately expensive. Universities must compete with corporations for the best scientific minds, and one effect of that competition is that universities must be able to provide the same kind of equipment that the most well-endowed corporation can provide. How can you keep 'em down on the quad after they've seen IBM?

So universities must seek external funding to provide the equipment that attracts the scientists that make the university the best place to do research and so the best place to learn about doing research. They may seek it from alumni, or as gifts from foundations, but the most likely sources are the corporations which need the results of research done with that equipment, by those professors in that university. Having the right faculty asking the right corporation for the right equipment at the right time may make the difference to being able to sustain the well-being of a department, or even the university. Researchers may imagine themselves to be practicing "pure research," and asking of their universities only that financial support be found for what they are independently inclined to do, but this is nowhere near as simple a matter as it may seem.

Funding by the National Science Foundation and other supposedly disinterested organizations is no salvation unless the NSF were to revise its policies to reflect a purely random selection of beneficiaries for the public monies it dispenses. Specifically because it takes itself to represent a public interest in pure research, NSF is demonstrably interested that the work it sponsors be valuable, that it pass muster in a court of peers, and so on. This is not a bad thing; it is just that, plainly, it is a question of making value judgments.

One may fortify this observation in at least two ways. On the one hand, one may argue, as we did in Section 1, that judging among research proposals is judging which research project best serves the ends deemed most important–a compound value judgment made within science. Or, on the other hand, one can argue that even in the best of cases–the projects most clearly about basic science–requests for funding are accompanied by arguments about how such research directly benefits society and so has social value of some sort. It is easy to find examples by looking casually over the past twenty to forty years of funding–from how research into fusion in the sun will benefit energy research in the United States to how a space station will allow the production of commercially valuable crystals grown in a gravity-free environment.[11]

That a funding agency like the NSF justifies its requests for money by appealing to public benefits does not preclude a scientist from pointing out the benefits to science itself, independently of any presumed benefits to society. But either argument appeals to values, we are claiming.

Since researchers and universities are in a position where they need support from potential users, this support must necessarily be interested, until such time as there are no competing uses for the funds (until such time that is, as money grows on trees). Universities and researchers are simply not likely ever again to be in a position where they can ignore the need for external funding.

One can consider each of the other variables that matter in the practice of scientific research–the choice of which students to accept into a program, for instance, or which buildings on a campus ought to be refurbished or rebuilt--or reallocated from other uses--or which students apply to which programs, or which journal articles get published, thus giving prestige and bargaining power to their authors and their institutions, and so on. One will discover that extra-scientific considerations are crucial determinants in making such decisions.

It is a rare small liberal arts college, for instance, that can boast of a new humanities building before a new science building. The electrical current to an old science building must be upgraded to handle new equipment. Laboratories must be quieter and cleaner to make sure that ever more subtle experiments are not contaminated. Humanists can make do with old blackboards and are still attracted to such places while a working scientist, the only kind such an institution wants, needs the new equipment. Without it, new funds for new research cannot be generated, the interest in staying diminishes, and the science program decays.

In short, extra-scientific value considerations infect the enterprise of science to its very core–not just the determination of the most apt way of considering a scientific domain and the choice of which theory best serves the ends chosen, but also each variable affecting the scientific enterprise, from the choice of the researcher to the determination of what kind of building the researcher ought to work in.

III.

It is sometimes thought that the need for funding to mount the hugely expensive experiments that basic research in science increasingly demands is a detriment to the purity of science. The concern is that the need for such funding will harm the enterprise, injecting into what should be objective judgments of what is scientifically necessary a value judgment that some research ought to be done because it pays to do it.

What we have suggested is that this picture of how funding may distort science itself rests upon a distorted view of the purity of science. Science is infected with value judgments to its core, and though the need for external funding can certainly distort someone's judgment about what properly ought to be done in pursuit of a solution to some scientific problem, it is not the need for and difficulty of obtaining external funding that, in itself, is a necessary source of distortion.

As we have argued, the question of which theory will come to be adopted in a given area of science, when theories come into conflict, is largely a function of which does the best job on the most important problems of the moment. Which problems are most important, in turn, while apparently a question that calls for the informed judgment of scientists, will often, in the end, be determined extra-scientifically.

But it is one thing to accept the reality that much of future scientific research will be driven by the interests of the funders. It is quite another to conclude that no other goals will be relevant or that universities must accept all contracts, whatever the interests of the funders may be. Universities ought not to draw the conclusion from what we have argued that there is no point in trying to control and monitor the external factors we have noted.[12]

Indeed, if universities do manage to keep on attracting the best minds to their faculties, and if they are able to maintain research facilities that are at the cutting edge of scientific and technological capability, they will have a great deal of bargaining strength in working out the details of research contracts with both corporate and governmental partners. This is one of the most important reasons for making every effort to attract those minds and maintain those facilities.

Institutions like MIT, Caltech, Johns Hopkins, and Harvard are in a good position to insist on strong guidelines in regard to funding, for example. When a researcher at Stanford submitted a proposal to the National Institutes of Health regarding a "five-year research project on an artificial heart device," NIH made it a condition of the contract that "researchers . . . obtain government approval before publishing or otherwise discussing preliminary research results."[13] Stanford sued, and its victory in *Greenberg v. Stanford* is an example of how a financially strong institution concerned about such values as academic freedom can effectively support its concern by taking the offending funder to court–the National Institutes of Health, in this case, not the CIA.

Institutions like RIT must struggle and be very selective about which areas, if any, they will pursue funding in, and from what sources. The appropriate path may lead such institutions to attract local funding by offering services to specifically targeted local industries that would not normally turn to the MITs and Caltechs for partnerships. Only after such institutions have developed the internal strength naturally to attract world-class funding should they seek it, and then, presumably, they will have the strength to bargain to maintain their integrity.

A great deal is, therefore, at stake in the present struggle being undertaken by universities to preserve their faculties and the quality of research undertaken on campus. Only preservation of a strong research base can put universities in a position where what they have to offer is worth bargaining for. And only if they are in that position can universities have the means of entering into research arrangements on acceptable terms.

Ridding universities of the myth that science is "value free" is the first step in coming to understand exactly how difficult a position they are in as they try to balance the need for external funding with the demand for internal integrity.

Notes

1. We are grateful to Victoria Varga for comments and suggestions on an earlier draft of this paper.

2. Richard Rudner, "The Scientist *Qua* Scientist Makes Value Judgments," *Philosophy of Science*, Vol. 20, 1953, pp. 1-6; p. 6.

3. While it will soon become clear that we regard this protrayal of the relation between researchers and funders as myopic, we must insist that what is described here as the critic's view is, from our standpoint, just as short-sighted. Things are considerably more complicated than would appear to be the case on *either* of these two accounts. For a fair and reasonable portrayal of the relationship between researchers and (especially) *corporate* funders, see Michael Davis, "University Research and the Wages of Commerce," *Journal of College and University Law*, Vol. 18, No. 1, 1991, pp. 29-38.

4. Isaac Levi, "Must the Scientist Make Value Judgments?", *The Journal of Philosophy*, Vol. LVII, 1960, pp. 345-57; p. 357.

5. This example is of special interest to us because we are both members of the Department of Philosophy at RIT. The natural interest that philosophers have in the array of issues addressed in this

essay has been complemented in our own case by the natural interest faculty members have in the operations of their own institution. This confluence of factors led to our organizing a conference on "Ethical and Procedural Issues Concerning University Research" in the fall of 1991 and subsequently to this paper and "The Myths of Academia: Open Inquiry and Funded Research," forthcoming in the *Journal of College and University Law*.

6. Why *this* was true is itself an interesting–and relevant–story, but let us begin here.

7. Early RIT/CIA relationships seem to have been limited to ". . . 1) individual faculty associations with the CIA in the context of individual faculty research contracts–chiefly if not exclusively in the photographic arts and sciences; 2) $15,000-20,000 annual grants provided by the CIA–for the first time in 1966 or 1967–to support, on a voluntary basis, individual unclassified senior student research projects in photographic science and instrumentation, in return for copies of the students' senior theses; and 3) the steady CIA employment of RIT graduates, particularly those with degrees in the graphic arts, such that RIT became the largest source of image analysts coming into the National Photographic Interpretation Center (NPIC), a national center administered during peacetime by the CIA" (from *The Past and the Future: Rochester Institute of Technology and the Central Intelligence Agency*, Report of the Review Panel and Senior Fact-Finder to the Board of Trustees and to the Administration, Faculty, Staff, Students and Alumni/ae of Rochester Institute of Technology, 15 November 1991, p. 12).

8. See Davis, *op. cit.*, for a nice summary of the general benefits of collaborations between universities and outside funding agencies from the point of view of both sides.

9. Report from the Institute Study Group on Graduate Education, quoted in *The Past and The Future*, p. 55.

10. For discussion of other such stories, see both Davis, *op. cit.* and Martin Kenney's excellent article, "The Ethical Dilemmas of University-Industry Collaborations," *Journal of Business Ethics*, Vol. 6, 1987, pp. 127-35. These two articles–especially Kenney's–are also valuable for their citations of other sources.

11. For a good discussion of the ways in which scientific projects are justified by appeal to social benefits, of how the reasons used by research scientists and those used by such agencies as the NSF may diverge and yet be consistent, and for the source of the example about fusion, see Robert J. Baum, "Can Governmental Support of Philosophy of Science Research Be Justified?", *PSA 1976*, Vol. I, pp. 289-312; pp. 297-99.

12. Kenney, *op. cit.*, offers a persuasive portrayal of the dangers of *not* monitoring or controlling these things.

13. Harold Greene, *The Board of Trustees of the Leland Stanford Junior University v. Louis Sullivan*, Filed September 26, 1991 before the Clerk, U.S. District Court, District of Columbia, p. 2.