RETHINKING INTERDISCIPLINARITY

Description:
Social scientists, philosophers, historians, anthropologists and cognitive scientists will share their experience on the matter and will focus on the impact of new forms of communication on interdisciplinary research.

Starting from February 9th 2004, a general discussion is open by the moderators.

Previous texts, by Dan Sperber, Helga Nowotny, Pierre Jacob, Catherine Garbay, Steve Fuller, Dominique Pestre; Howard Gardner and Veronica Boix-Mansilla and Ian Hacking are archived at the botton of this page.

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Modérators:

Christophe Heintz (EHESS, Institut Nicod), **Gloria Origgi** (CNRS, Institut Nicod), **Dan Sperber** (CNRS, Institut Nicod)

Guest Panel:

Daniel Andler (Ecole Normale Supérieure), **Noga Arikha** (Institut Nicod), **Bruno Bachimont** (Université de Technologie de Compiègne), **Nicolas Balacheff** (Université de Technologie de Compiègne), **Sarah Bendaoud** (Institut Nicod), **Alban Bouvier** (Université Paris IV - Sorbonne), **Roberto Casati** (CNRS, Institut Nicod), **James Collier** (Virginia Polytechnic Institute and State University), **Jochen Glaser** (sociologist of science), **Davydd J. Greenwood** (Cornell University), **Stevan Harnad**, **Rainer Kamber** (Université de Bâle), **Julie T. Kl**ein (Wayne State University), **Grit Laudel** (Australian National University), **William Lynch** (Wayne State University), **Vanessa Nurock** (Institut Nicod), **Pietro Redondi** (Université de Bologna), **Claude Rosental** (C.N.R.S.), **Jean-Michel Salaün** (ENSSIB), **Hans Siggaard Jensen** (Learning Lab Denmark), **Marta Spranzi** (Université de Versailles), **Peter Weingart** (University of Bielefeld)

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Why Rethink Interdisciplinarity?

Dan Sperber (CNRS, Institut Nicod)

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Abstract: There is a conventional discourse in favor of interdisciplinary research. At the same time there is much indifference or even disregard for such research and there are important institutional obstacles to its development. This virtual seminar, and this first contribution in particular, aim at feeding reflexion on the conditions in which this research is either truly beneficial, even necessary, or is of little value. Favorable conditions for interdisciplinary research have a history, linked to that of scientific disciplines and their institutions. Is this history in the process of taking a new turn with the development of new forms of scientific communication through the Internet? Dan Sperber draws on his experience in the social and the cognitive sciences to reflect on the strength and weaknesses of interdisciplinary research and on its future.

This virtual seminar on "Rethinking Interdisciplinarity" is organised by members and associates of the Institut Jean Nicod (which describes itself as "an interdisciplinary lab at the interface between the humanities, the social sciences and the cognitive sciences"). We do not, normally, discuss among ourselves interdisciplinarity per se. What we do is work on issues that happen to fall across several disciplines, and, for this, we establish collaboration among philosophers, psychologists, neuropsychologists, linguists, anthropologists, and others. Still, we—and so many other scholars, students, and managers of scientific institutions—have good reasons to pause and reflect on interdisciplinarity itself. Research that falls across disciplines meets specific obstacles. It is easily construed as challenging the dominant disciplinary organisation of the sciences. This challenge is seen as positive by some, a distraction by others. Scholars involved in interdisciplinary research end up having to either articulate the challenge or downplay it. So it goes in the micro-politics of science. But surely, talk of interdisciplinarity should not just be opportunistic. It is, or should be, relevant to our understanding of the character and becoming of science. Hence the idea of this seminar.

I had initially intended, in this opening presentation, to outline a few ideas on the pros, the cons, and the future of interdisciplinarity, but in working on it, I felt more and more inclined to share reflections, concerns, and indeed emotions inspired by my experience, that of a social and cognitive scientist deeply involved in interdisciplinary research. I will do so by presenting a few vignettes and commenting them.

Cosmetic interdisciplinarity

I sit, once again, on a committee evaluating grant proposals that have to meet explicit criteria of interdisciplinarity. As usual, the committee is interdisciplinary in the sense that it is mostly made up of scholars from several disciplines, each recognised and powerful within his or her one discipline. Very few of us have been involved in intensive interdisciplinary work. Most of the grant proposals we have to evaluate have built in interdisciplinary rhetoric and describe future collaboration among people from different disciplines, but this is mostly done in order to meet the criteria for the grant. The actual scientific content generally consists in the juxtaposition of monodisciplinary projects with some effort to articulate their presentation. A few proposals are genuinely interdisciplinary, but often they are the less well thought through, the least likely to yield clear results. And now we have to rank two proposals: a really good proposal the interdisciplinary character of which is superficial and ad hoc, and a merely decent, but genuinely interdisciplinary and innovative proposal. Should we prefer the first one hoping

that, just as faith is said to come while praying, some true interdisciplinary interaction and thinking will occur in what was initially an opportunistic half-hearted effort, or should we favour the second proposal and see its more tentative and fuzzy character as the price paid for leaving the well-trodden paths? I have known similar dilemma before. This time, I vote for the better not-so-interdisciplinary proposal, which I see as more clearly deserving to be funded. At the same time, I wonder: What kind of a comedy is this, where we are pretending to fund novel, interdisciplinary research, while, at the same time, there is very little funding available for interdisciplinary teaching and training in the first place? How likely is it that outstanding interdisciplinary proposals emerge in such conditions? And aren't most of my colleagues on the committee quite content with this state of affairs, which allows disciplinary business to go on as usual at the cheap price of some interdisciplinary rhetoric?

Interdisciplinary disappointments

A team of eminent psychologists spends years providing experimental evidence in favour of the view that there are fundamental differences in the modes of thought of members of different cultures. While this view goes against the biases of most psychologists, it has long been defended by anthropologists, without however the benefit of experimental evidence. Our psychologists are invited to present their work at an anthropology conference. The disappointment is strong on both sides. The anthropologists fail to see the relevance of experimental evidence in favour of a thesis they feel confident has already been amply demonstrated with ethnographic data. They object to what they see as the artificiality of experiments collected outside of an ethnographic context. Moreover, they find the psychologists' view of culture, exemplified by the fact that they are talking about Western and Asian cultures in general, far too crude. The psychologists feel that the anthropologists are just blind to the importance of experimental evidence, that they criticise experimental methodology without understanding it, and that they fail to appreciate how much their work might contribute to a fruitful exchange between psychologists and anthropologists. In the end, the thesis itself is not given any discussion.

What is going wrong? The two communities, psychologists and anthropologists, have different vocabularies, presuppositions, priorities, criteria, references. In general different disciplines have different sub-cultures, and the difference is made worse, not attenuated, by the existence of superficial similarities, for instance identical words used with quite different meanings ("culture" and "mode of thought" in the present example). Because issues seem to be shared by two disciplines, scholars from each may seek, or at least welcome, interdisciplinary exchanges. More often than not, their expectation is not so much that they will learn much from the other discipline; it is that people in the other discipline can and should learn from them. It is much less challenging to think that one's message has relevance beyond its usual audience than to think that one has been missing a message of great relevance to oneself. In fact, in the story I just told, clearly, the psychologists made the greatest effort to go out of their way and produce novel work, but more with the expectation that they would have a message to share than one to accept. The anthropologists, on their part, were willing to welcome psychologists whom they expected to bow to the obvious superiority of anthropology over psychology in matter of cultural modes of thought. They were not at all ready to try and understand things from the point of view of psychologists (in spite of the fact that understanding other people's point of view is what anthropologists do, but then the people in question are far away and are not competing for academic recognition and resources). More generally, many researchers in many disciplines have participated in interdisciplinary encounters; public discourse on these occasions always underscores their positive side, but, in private, misgivings and frustrations are commonly expressed. Most participants return mildly intrigued but otherwise unmoved, the way business managers return to their routines after a self-awareness week-end retreat.

A slow learning curve

Some of the members of the psychological team I have just mentioned are involved in a graduate "Culture and Cognition" program at the University of Michigan. Every week all the participants in the project, graduate students and faculty, most from psychology or anthropology, meet and discuss their own work, papers by visitors, or general issues. It is fascinating, and somewhat disheartening, to watch how week after week, year after year, the same disagreements across and sometimes within disciplines are expressed in almost the same terms, as if disciplinary and theoretical affiliations could never be overcome. But this is only half of the story. Some people come a few time and leave for good, feeling that this is a waste of time, but others have been attending for years; they have developed a clear and detailed understanding of the work done in other disciplines, and, in their own work, they address truly interdisciplinary issues, drawing, even if sometimes defensively, from different disciplines. Some of the students in the program, even though they come from either the social sciences or psychology, think and work across disciplines. So all of us who participate in this program, as permanent members or regular visitors, feel both a sense of frustration—couldn't this work better, move ahead faster, leave once and for all behind the initial misunderstandings?—and a sense of achievement—though not as much or as well-developed as we would like, something novel and relevant is emerging that could not have been fostered in a disciplinary context.

More generally, it turns out that the only way to have interdisciplinary work paid attention to, and, even if often misunderstood, at least not right away dismissed is to produce different versions of it for each of the disciplines concerned. You submit, say, one article to a psychology journal, with streamlined introduction and general discussion, a standard detailed experimental section, thorough references to the psychological literature, and using all the disciplinary buzz words in the right way. You develop basically the same argument for an anthropology journal with, mutatis mutandis, the same strategy, which this time involves providing a mere summary of the experiments, what psychologists would call anecdotal evidence, and much longer theoretical sections anticipating the objections most anthropologists tend to have to any naturalistic approach. Same concerns when you adress to disciplinary audiences. Being an anthropologist, I have enjoyed going native in several disciplinary sub-cultures, and yes, there is much to learn from the experience. However, this makes serious involvement in interdisciplinary research a high investment endeavour. An easier way is to have enduring interdisciplinary collaborations among specialists of different disciplines. To be able to understand each other and conceive of common goals, they still need not just good will, but something like the kind of training provided by the "Culture and Cognition" program at Michigan.

A student's dilemma

D., a psychologist, and I are co-tutors of a particularly promising graduate student with degrees in philosophy, sociology, and biology, who is now at the end of his first year in a cognitive sciences doctoral program. He is participating in experiments in D.'s lab as part of his training. The student wants to choose, for his dissertation, an interdisciplinary research topic having to do with the cognitive basis and the cultural forms of morality. D., although he is currently involved in another interdisciplinary project on a related topic, tries energetically to convince the student to give up his idea and to choose—or accept—a strictly psychological research project closely related to work currently pursued in D.'s lab, and the results of which can be partly anticipated. Only if the student makes such a choice, does D. feel confident that he will be able to help him with his career. Interdisciplinary work is for when you already have a job! The student has been motivated throughout his studies by interdisciplinary goals and is very reluctant to accept. At the same time, he will need a

grant, and later a job, and I cannot but confirm that, from this important practical point of view, D. is essentially right. As I have told quite a few students who wanted to work within the kind of interdisciplinary approach I have been defending, choosing an interdisciplinary research topic at the doctoral stage involves serious career risks. Also, it is much harder to get a proper training without investing all of one's energy into one discipline, or rather sub-sub-discipline. Happily, in this particular case, after several exchanges between all the people involved, and helped by the manifest excellence of the student, we find what looks like a realistic compromise, which will involve downplaying the interdisciplinary character of the research the student will in fact pursue (just the opposite rhetoric of that of the typical interdisciplinary grant proposal!).

I see here a vicious circle: postponing interdisciplinary work to the time a researcher is well established means that such research is generally pursued as a side activity, with more goodwill than thorough competence, and that therefore, indeed, it will be much harder for a student to find proper supervision in an interdisciplinary than in a disciplinary area. Even more generally, this means that the inventiveness and creativity of younger scholars is discouraged from going into interdisciplinary work, slowing down this work, making it intellectually and practically less attractive, and so on.

The emergence of an interdisciplinary network

In the late 80s we were a few anthropologists trying to develop a different kind of cognitive anthropology, drawing on the work of Noam Chomsky and of some outstanding developmental psychologists, arguing that the mind involves a variety of domain specific mechanisms and that these mechanisms played an important role in permitting cultural transmission and in shaping cultural contents. In 1990, a conference on domain specificity in cognition and culture was organised at the University of Michigan (see Hirschfeld and Gelman 1994). It brought together these anthropologists, developmental and evolutionary psychologists, and others. The cross-disciplinary convergence of interests was striking to many participants and has influenced their work ever since. This conference was the starting point of a network of collaborations that took the form, over the years, of several other conferences, workshops, research project mixing experimental work and anthropological fieldwork (as for instance in the collaboration between Scott Atran and Doug Medin, or that between Rita Astuti, Susan Carey, and Gregg Solomon). All these meetings and projects were made easier by the fact that grant giving agencies favour interdisciplinary research, and we did not have to strain the rhetoric to meet their criteria. The scientific output of this loose and growing network of researchers has gained the recognition I believe it deserved. A number of younger researchers involved have had an interdisciplinary training and have done interdisciplinary work from the start.

More generally, in a number of fields, major advances have involved interdisciplinary interactions. The example I just gave is not untypical of what has been happening in the cognitive sciences. Howard Gardner, an early historian of what he dubbed the "Cognitive Revolution" wrote in 1985: "At present most cognitive scientists are drawn from the rank of specific disciplines—in particular, philosophy, psychology, artificial intelligence, linguistics, anthropology, and neuroscience. … The hope is that some day the boundaries between these disciplines may become attenuated or perhaps disappear altogether, yielding a single unified cognitive science." (Gardner 1985: 7). Almost twenty years later, what do we observe? The disciplines have not merged (and, in cases such as that of philosophy or anthropology, only sub-disciplines were involved in the cognitive science enterprise anyhow), but each discipline has borrowed concepts, issues, tools, and criteria from others. To give just a couple of illustrations, modelling, inspired by artificial intelligence, is more and more used as a tool in psychology and neuroscience, and, more generally, the existence of a clear possibility of modelling a

given hypothesis is recognised as a criterion for judging the acceptability of an hypothesis anywhere in the cognitive sciences. Issues about the character and role of representations, first raised in philosophy of mind, have become topics of controversy within and across all the cognitive sciences. It still is the case that most cognitive scientists squarely belong to a specific discipline, but it has become quite common for many of them to be routinely involved in intensive research programmes involving researchers from several disciplines. Some of us have gone one step beyond: we don't belong anymore to a given discipline, or we belong to several. I, for instance, have done research and published in anthropology, linguistics, philosophy, and experimental psychology: I am at ease in each of these fields but not exactly at home in any. There is however—or so I believe—as much unity to my work as there would have been had I followed a more traditional course: my goal has been from the start to explore and develop some of the common foundations of the social and cognitive sciences, and no single discipline offered an appropriate vantage point to do so. For some of us, interdisciplinarity (or transdisciplinarity, or call it the way you want) is a way of life. It is at least an ordinary aspect of their work for most researchers in the cognitive sciences (and also in other domains, for instance environmental studies). The cognitive sciences have become a new kind of (inter)disciplinary configuration, with less institutional unity than most established disciplines, but more dynamic interactions than recognised groups of disciplines such as the social sciences.

An interdisciplinary Web conference

Between October 2001 and March 2002, an interdisciplinary conference on the future of the text in the electronic age took place, appropriately, on the Web. (It was organised by the Library of the Centre Pompidou in Paris, the Institut Jean Nicod, the Association Euro-Edu, and the GiantChair Company, and led by Gloria Origgi and Noga Arikha on the web site: www.text-e.org). Every fortnight, a lecture was put on line for discussion. The lecturers were historians, cognitive scientists, philosophers, librarians, and a publisher and a journalist. The people who participated in the discussions had even more diverse background. We often heard the following objection to the Web conference format: you loose the voices, the bodily communication, the conversations in the lobby or at lunch. True, but these do not have only beneficial effects. They quickly stabilise a pecking order among the participants based on age, sex, fluency, aggressiveness, and academic status. Some intervene with ease in all the discussions and others feel inhibited by their real or perceived position in the pecking order. In the case of an interdisciplinary conference, the disciplinary divisions tend to be maintained by all these forms of direct interaction: lobby and lunch conversations tend to be among disciplinary colleagues, public interventions are in good part aimed, directly or indirectly, at members of the same discipline, and so forth. We found that a web seminar gives participants greater opportunity to contribute to a discussion across disciplines and languages, without worrying about their status, affiliation, or fluency. Thus, unlike what happens at an ordinary interdisciplinary conference, nobody felt compelled to hail the interdisciplinarity of the occasion: it was there as a matter of course. Only when it was directly relevant, did participants mention their own disciplinary affiliation. The whole debates had the character of a thoughtful conversation, with a common goal of enhanced understanding, rather than that of a series of short intervention aimed as much at asserting or reasserting the speaker's authority or the precedence of his or her discipline.

More generally, much of the difficulty of interdisciplinarity has to do with the fact that attention, recognition, and authority are channelled by disciplinary institutions. In fact, this can be viewed as one of their primary functions. Even in ordinary interdisciplinary events, disciplinary networking is still quite potent. Before the advent of the Internet and the Web, most scientific communication was channelled by disciplinary institutions, labs, conferences, specialised libraries, journals, and so on. With the advent of the internet it has become much easier for individual researchers to establish and maintain communication based on common intellectual interests rather than on institutional alliance. The ever

growing free availability of scientific papers on line renders researchers less dependent on the library of their home institution (including paid online subscriptions). Discussion lists (and now web conferences) recruit over time their own rapidly evolving communities. Thus interdisciplinary interaction becomes easier, and so does the recognition of interdisciplinary findings. The next step will come with the generalisation of teaching on the web: then, acquiring a scientific education à la carte may become a real possibility, boosting the development of interdisciplinary research in areas where it is genuinely fruitful, or so one may hope.

Concluding remarks

As Peter Weingart observed, talk of interdisciplinarity is fraught with paradoxes—of a superficial kind, I would add. On the one hand interdisciplinarity is touted as a "good thing," contrasted with excessive specialisation, a "bad thing." Yet, rather than the one displacing the other, both have greatly developed in the past decades—and specialisation more than interdisciplinarity. "Interdisciplinary" is used to describe—and praise—courses, research projects, or grant proposals, as routinely as "full-bodied" is used to describe red wines. This month (March 2003), "interdisciplinary" has 1 700 000 entries in Google, as compared, for instance, to 255 000 for "experimental." Notwithstanding all this song and dance, the vast majority of scientific publications belongs squarely to an established discipline, as does the quasi-totality of academic and research jobs. Interdisciplinarity has not become a hot topic in philosophy of science. "Philosophy of science" combined with "interdisciplinarity" returns only 915 Google entries, as compared to, say, 4690 entries when combined with "reductionism." With a few notable exceptions (which will be well-represented in this seminar), most people who have written on interdisciplinarity have done so from the point of view of science policy rather than from the point of view of philosophy, history or sociology of science. It might look as if, somehow, interdisciplinarity is one of these grand notions handy in political discourse, but not to be taken too seriously. As I hope to have illustrated, this is not always the case. Interdisciplinarity is not always a good thing, nor specialisation a bad thing, for the advancement of science. In some areas, disciplines and specialised subdisciplines may well be producing optimal results. In many others areas, on the contrary, disciplinary boundaries are an obstacle to desirable developments and interdisciplinarity helps optimise research. Should we conclude then that interdisciplinarity emerges unproblematically in those areas where it is scientifically productive? This would ignore the force of inertia of established disciplines. The development of valuable interdisciplinary work in cognitive science, for instance, is slowed down and made harder in a variety of ways by the standard disciplinary organisation of research and teaching. This relative difficulty of doing effective interdisciplinary work might be viewed as a mild negative side-effect of the otherwise highly positive disciplinary organisation of the sciences, a side-effect appropriately compensated for by institutional policies of encouraging interdisciplinary work. However—and I have left this for other, more competent contributors to this seminar to develop—disciplinarity itself deserves some serious rethinking. After all, the disciplinary organisation of the sciences as we know it is not a mere reflection in scholarship of everlasting natural divisions among levels of reality. It is a historical product which, in its present form, goes back to the nineteenth century and to the development of modern universities and research institutions. This organisation of the sciences may rapidly evolve with new social and economic demands on science, with the Internet and its growing impact on scientific communication (both in teaching and in research), and with the advancement of science itself. The current disciplinary system may be becoming brittle, and the growth of interdisciplinary research may be a symptom of this brittleness. More positively, new forms of scientific networking may be emerging, helped by the growing role of the Internet. Describing these forms in terms of disciplines and interdisciplinarity may fail to capture their novelty. All this deserves some serious rethinking.

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Discussion

Because the Concept Is Flawed
Alexander Kravchenko
Apr 1, 2003 23:42 UT

Dan Sperber is absolutely right in drawing our attention to the 'comedy' of pretending to be interdisciplinary when almost everyone understands that this is an unattainable goal — at least, in the framework of the traditional scientific paradigm characterized by hyperspecialization and fragmentation of human knowledge about the world. This is a natural consequence of the preoccupation of modern science with analysis, when it is obvious that the time has come for synthesis. Dan's observation that talk of interdisciplinarity "should be relevant to our understanding of the character and becoming of science" calls for a revised understanding of science as knowledge applicable to the trivial routine of problem-solving in the life of an individual in the context of his/her social environment. Which means that all knowledge is, or should be, related.

Interdisciplinarity has long been a fad in the academe (Smith 2003), but can it go farther than that? Until the unhappy term 'interdisciplinarity' continues to persist, the whole thing will, in my opinion, remain a fad, because 'inter-' means 'between' or 'among' (the specialized sciences, in our case), and what can be found among specialists but another specialist? This is one of the reasons why it seems "as if disciplinary and theoretical affiliations could never be overcome". Yet this is a misleading impression.

Modern sciences (and respective disciplines as taught in educational institutions today) have all sprouted from philosophy. The process has taken a long time only to bring scientists to the realization that the more minute the specialization of each separate science, the less overall practical value it offers insofar as the understanding of man, life, and the world goes. This realization heralded the emergence of cognitive science as a new philosophy of life and man. As Brady (1997: 6) observes,

“the quality of our lives is largely determined by the quality of the political, economic, social, and religious organizations which structure them”. And this quality, in turn, depends on the quality of our essential knowledge of human society. To make any sense of our lives, we must have a good understanding of what it is to be human. From this point of view, we should not so much look for “common foundations of the social and cognitive sciences” (it is not at all surprising that Dan couldn’t find any), we should act on the assumption that the two cannot and should not be viewed independently of one another. Central to all sciences must be the understanding that all knowledge is the product of humans as a biological species, therefore, it serves a biological function. And if this function has not been identified, then the purpose of science has not been identified, either.

There is more and more talk of the necessity to work out a concept of unified science (on which Charles Morris insisted). Cognitive science is a very promising move in this direction, although the concept itself is far from being understood or applied more or less uniformly. However, the future of (unified) science lies with this new paradigm of human knowledge (Kravchenko 2002).

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Flawed? No, just superficial
Dan Sperber
Apr 2, 2003 12:52 UT

I thank Alexander Kravchenko for his remarks, which are distinctly more radical than mine. Generally speaking, I have a much more positive view of science than he seems to have. I would not say that “interdisciplinary …is an unattainable goal.” My argument rather, is that interdisciplinarity in general is not a goal at all. In specific areas, disciplinary boundaries and routines stand in the way of optimal research. There the goal is to go ahead with new research programmes, and, for this, to reshape the institutional landscape. So, the goal is, in a trivial sense, interdisciplinary, but it is not interdisciplinarity per se. More generally—and here I was wondering rather than affirming—it is conceivable that the advancement of science will involve so much reshaping of its institutional forms that the disciplines as we know them will have to go. So, the concept of interdisciplinarity is of use to point to a number of pressing issues in the theory and practice of scientific research, but it is too superficial to otherwise help with elucidating these issues.

The risks and challenges of interdisciplinarity
Patrice Ossona de Mendez
Apr 2, 2003 8:08 UT

In order to understand the problems related to interdisciplinarity, one has probably to first understand the concept of discipline.

This concept may be seen as a natural consequence of two historical issues: the fact that the whole knowledge is no more accessible to a single person (the last "universal scientist" might be Henri Poincaré) and the seek of each scientific field for some form of objectivity through a strong request for the adhesion of some specific accepted methodology (kind of positivism).

Hence, a discipline may be viewed as a scientific domain owning a specific methodology (as well as specific implicit hypotheses justifying it), as well as a specific vocabulary (support of the intuition within the specific conceptual framework).

Although it is obvious that pure mono-disciplinarity does not exist (human cognition is intrinsically based on associations and analogies), it is a common implicit prerequisite that scientific productions should not mention the genesis of ideas (because of its non objective form) but rather focus on the description of the "application" of the idea within a specific scientific context and methodology.

Attempts to build interdisciplinary bridges logically lead to the "intersection/union" problem: in order for a result to be accepted by two disciplines, one has to reduce implicit hypotheses to a set of common ones (intersection), and to extend the justifications to include a complete justification in both disciplines (union). Relaxing the implicit hypotheses, although increasing the generality of the result, will limit its "practical" consequences (the less you assume, the less you have), with the risk of reaching a feeling of too general empty statement. Within this approach, the vocabulary problem has to be solved in reducing the vocabulary to "generalize" concepts matching the reduction of the implicit hypotheses. To gain some interest in the audience, implicit hypothesis have to be made explicit, so that the generalization of the conceptual framework clearly appears, thus justifying a limitation of the results, while giving some hints on how they could be strengthened in each discipline.

Hence, it seems to me that one of the major challenges of interdisciplinarity is to explicit hypotheses that are implicitly made in specific disciplines and to show that some reductions of these may lead to the introduction of powerful tools matching the methodological requirements of several disciplines.

To the opposite, interdisciplinary culture allows a wider and diversified intuition of promising structures and concepts. As it is based on loose analogy, the consequent cross-fertilization mainly applies at the intuition level, thus needing a specific justification for each of its products.

The challenges are to the established disciplines
Dan Sperber
Apr 2, 2003 12:46 UT

Patrice Ossona de Mendez raises important issues and makes relevant suggestions. Still, as a practicing empirical scientist, I tend to view things in a somewhat more dynamic way. He writes: “one of the major challenges of interdisciplinarity is to explicit hypotheses that are implicitly made in specific disciplines and to show that some reductions of these may lead to the introduction of powerful tools matching the methodological requirements of several disciplines.” Interdisciplinary work may sometimes be a way to bridge two or more disciplines as they are, along the lines Ossona de Mendez suggests. It can also, and more often I believe, involve a more or less radical challenge to the current state of these disciplines, to their "methodological requirements" and to their theoretical presippositions. The interdisciplinary work I have been doing on the common foundations of the social and the cognitive sciences has rightly been seen by anthropologists as a challenge to dominant ideas and methods in the discipline—a challenge that most of them rejected, sometimes vehemently, and that others found useful.

disciplines, professions, and Taylorism
Davydd Greenwood
Apr 3, 2003 15:38 UT

I have spent all of my 33 years in the university navigating these interstial spaces as an academic and as an administrator. I share the sense of dilemma but would like to push the identification farther.

I find distingushing between the disciplines and the academic professions useful. We know from a number of good histories that the professions are an arbitrary and self-interested set of constructions that create mini-cartels and markets and that intervene heavily in peer review and promotion decisions. Inter-professional collaboration is rare because these organizations are set up to hold territories against each other.

The relation between academic professions and disciplines strikes me as exceptionally unclear, particularly when so called professions claim the same ancestors, e.g. Weber, Marx, Durkheim, etc.

We should recognize that the professions and their products, departments, are a form of work organization and management control. They are a classic expression of Taylorism as they are separate compartments of expertise and they require integration from above by omniscient managers (deans, provosts, presidents, rectors) because they are designed not to be collaboratively self-managing but to compete for resources.

The relationship of knowledge, discipline, methods, epistemology to this Tayloristic organizational matrix surely lies at the heart of issues about inter-disciplinarity.

Searching for clues in one's own discipline
Ira Noveck
Apr 2, 2003 11:22 UT

One only has to look into one's own discipline to see how interdisciplinarity (among SUBdisciplines) is easy to learn though difficult to master. Each discipline contains a microcosm of this interdisciplinary challenge. In my own initial discipline -- psychology -- it takes a little nerve and a lot of patience to sail between, say, developmental psychology, adult reasoning, psycholinguistics, and neuropsychology (and I think for the same sociological reasons Dan mentions). Within each of these subdisciplines, there are different codes, different priorities (e.g. one is the relative importance each gives to methodology), and different presuppositions (based usually on a reigning theory). Now, getting published in each of these subdisciplines is a minor achievement. But even if one can do that, I don't think it would add up to making one genuinely trans-(sub)disciplinary.

I think what is really needed is a re-emphasis on how work relates to a set of higher principles. In psychology, I suppose it would be knowing and showing how one's contemporary work (no matter the subdiscipline) is linked to ideas from older schools, i.e. in taking a long view of one's contributions. In doing interdisciplinary work in the cognitive sciences, I think it is similarly critical to try to place one's work in the light of others' discussions, but in this case -- among colleagues in philosophy. Doing interdisciplinary work is worthless (even if one publishes in the top journals of each of the disciplines) unless one knows and shows how one's work addresses a given philosophical approach or a given philosophical issue.

So here's a naive suggestion: With the idea that philosophers have the unique training and set of skills for defining classes and establishing principles, why not give them (or those so inclined) the task of defining the issues that ultimately require interdisciplinary cooperation. That is, we can handle the problem best by good management-of-science skills and not necessarily by us all becoming interdisciplinary in a pell mell fashion. I think that to some extent, this is going on already.

Don't ask too much of philosophers
Dan Sperber
Apr 2, 2003 16:46 UT

Ira is right that “inter**sub**disciplinarity” is,in many respects, similar to interdisciplinarity. Let me, however, point to some disanalogies. There are, within each discipline, major journals that welcome articles that combine two sub-disciplines. For instance an article combining adult reasoning and neuropsychology would be particularly welcome in several major psychology journals. Not so with interdisciplinary articles: either you publish in brave but minor journals, or, as I suggest, you tailor versions of your findings aimed at different disciplinary audiences. Regarding jobs too, combining two sub-disciplines is a plus in most disciplinary departments. By contrast, being, say, half a psychologist, half an anthropologist makes it harder to find a department that will see your double competence as particularly desirable and that will pay your full salary (and joint appointments are not that easy to find).

Regarding the role that Ira would like to see philosophers play (“defining the issues that ultimately require interdisciplinary cooperation”), I would like to say two things. First, I believe that Ira overestimates what philosophers are able and would be willing to do. Second, I am reluctant to see any authority, however enlightened and benign, define the issues on which scientists should work for the sake of the advancement of science. Science is a competitive game where your ideas win by convincing other scientists, especially younger ones, and not by fulfilling the wishes of whatever authority. At least, this is how science works best. (Of course, much of science is done in view of applications and responds to social/political/economic/military demands, but here it is as citizens that we should all – not just scientists or philosophers – reflect on what we expect - or fear - from scientific research).

Reply to a comment of Julie Klein
Dan Sperber
Apr 10, 2003 14:11 UT

Julie Klein (in her "Responses to Dan's initial responses") writes:

"Ira made the worthy suggestion that philosophers are in a key position to define issues requiring interdisciplinary cooperation. The kind of reflexivity they are trained to perform, though, is necessary in all disciplines and fields. If we pass the responsibility and capacity to philosophers alone (without diminishing their leadership) we limit the socio-epistemological reflection that must be part of any interdisciplinary endeavor, whether collaborating on a particular project or building a field. We need both."

Right, and this is why we wanted, in this seminar, to have input not just from philosophers and other scholars working on science, but also from people involved in interdisciplinary undertakings, and reflecting on their experience.

Maybe disciplines themselves are the real problem
Steve Fuller
Apr 2, 2003 17:46 UT

One of the disadvantages of e-mailing from UCLA is that one is always already a latecomer to the discussion. However, it looks as though people so far have been problematizing interdisciplinarity, when maybe the idea of 'discipline' is the real problem here.

Dan Sperber uses the relations between anthropologists and psychologists in the Culture and Cognition Program at the University of Michigan as his touchstone for a meditation on the problems of interdisciplinarity. However, I wonder whether the source of these problems is traceable to 'disciplines' as such or something more specific, namely, differences in method. After all, what Sperber treats as disciplinary differences between anthropology and psychology are, on closer inspection, the difference between an ethnographic and experimental approach to the study of human affairs. This difference is reproduced both within and between the disciplines of the social sciences. For this reason, I have always regarded the familiar idea that disciplines are incommensurable 'tribes' or 'cultures' as misdirected. Methods – with their strong sense of craft – are closer to tribes and cultures as sources of primitive feelings of epistemic affiliation.

A better socio-political analogue for the discipline is the nation-state, which is an explicitly constituted social entity containing a variety of cultures that sit often uneasily together, united by a commonly enforced language, which is itself spoken in many dialects. To be sure, incommensurabilities exist between nation-states, but they are of a different order from those that exist between cultures. One thing that helps to harmonize, or at least minimize, the different cultures within a nation-state is that citizens are taught a common national history in school. The disciplinary analogues are the airbrushed Whig histories in textbooks that Kuhn made such a big deal about. In both cases, they are largely inspirational and mythical.

Now all of this analogy-mongering starts to break down once we acknowledge that cross-disciplinary boundaries – such as they are – do not have the determinateness of geographical borders. Behind this point is the question of the grounds of disciplinary legitimacy, the ontological equivalent of 'territorial integrity'. For example, the policing ('back-tracking') capacities of the professional associations of academic disciplines pale by comparison with those of nation-states. There are some formal 'excommunications' from disciplines, but these generally have to do with the violation of more general, trans-disciplinary norms (i.e. the stuff of research ethics). Has anyone ever been expelled for more specific disciplinary malpractice? However, it may be argued, the educational ('front-loading') capacities of disciplines are stronger than those of nation-states. Here, I think, the natural sciences are better positioned to make this case than the social sciences. A successful physics major probably has a better sense of what it means to be a physicist than to be an American (especially these days!) – but I am not so sure about a successful sociology major!

One of the many real problems
Dan Sperber
Apr 3, 2003 12:04 UT

Steve Fuller raises important issues that, I am sure, will occupy us in this seminar in the coming months. Let me just comment on a couple of points, at the more experiential level at which I tried to make a contribution. As an anthropologist, I tend to think that the radicality of the differences across cultures and the autonomy of individual cultures have been exaggerated. So, I don't expect scientific sub-cultures to be well separated from one another, like islands, or to be incommensurable (whatever this means – of course, you may water down the notion of incommensurability to the point where it is quite easily instantiated). In my experience, prototypical anthropologists and prototypical cognitive psychologists have a hard time understanding each others, more because they quickly lose patience than because they lack the necessary conceptual resources (nothing anyhow that a bit of (self-)tutoring could not rapidly overcome). In the case of these two particular disciplines, each is pretty well wedded to its methods: participants observation for anthropology, experiments for cognitive psychology. Moreover prototypical representatives of these disciplines are, on the whole, doing work worth doing. I would not dream of trying to win them over to the kind of interdisciplinary research programme I have been advocating. The problem is rather the too exclusive control they exert on resources, publications, careers, a control that, in so many ways, impedes the development of novel nonprototypical research.

I hope that, in this seminar, we will have other concrete cases presented to us, possibly in a style less anecdotal than the one I adopted. I do not doubt that, in other areas of research, the situation is different in relevant ways.

At a more general level though, I agree with Steve Fuller that the idea of 'discipline' is, if not "**the** real problem" at least a particularly important and interesting problem, among so many problems, most of them local, that people involved in interdisciplinary work encounter.

Reply to Dan Sperber
Steve Fuller
Apr 3, 2003 19:30 UT

OK. However, not all anthropologists are participant observers and not all psychologists are experimentalists – though perhaps they are the norm or the mode or the stereotype in their respective disciplines. And this point is quite important to keep in mind when discussing interdisciplinarity. The disciplines as institutionalized entities – most noticeable from university department structures and professional associations – place constraints on inquirers that are somewhat different from one's personal ties to particular modes of inquiry. Sometimes these constraints are enabling but more often (I believe) they are inhibiting. It would be interesting to imagine what the configuration of human sciences would look like if all of those who value face-to-face 'in situ' encounters with their subjects joined together in one discipline, while all of those who prefer laboratory settings got together in another discipline. At the moment, most have elements of both to varying degrees (not to mention some purely text-based folks as well).

However, I do not want to sound too negative about the prospects of disciplines as enablers. Here we should always keep in mind one very important 20th century case in which disciplinary constraints may have turned out to be enabling – namely, biology after the Neo-Darwinian synthesis in the 1930s and 1940s. I raise this example because biology has traditionally had exactly the same range of methodological variation as the human sciences: paleontologists, natural historians, ecologists, experimental and behavioural geneticists, evolutionary theorists, molecular biologists, etc. Biology managed a fruitful disciplinary unification of diverse methods under a more-or-less common conceptual framework in a way that neither psychology nor any of the other social sciences has ever done – or are likely to do in the foreseeable future. (By the way, this had nothing to do with logical positivism, which suggested quite different strategies of unification for biology.)

However, I am somewhat cautious about the biology example because I think market forces are in the process of disintegrating this unity, as the field is becoming increasingly instrumentalized – i.e. via biotechnology. At the same, the ongoing disintegration of the social sciences has proven fertile ground for more unification-minded biologists (E.O. Wilson is probably the paterfamilias of them at this point) who want to keep the synthetic ideal alive. But more on this at another point.

Methods and objects
Rainer Kamber
Apr 5, 2003 16:51 UT

I agree with Steve Fuller where he suggests that the interdisciplinary gap may be constituted mainly through methodical gaps and that they are an important source of "incommensurability" (like Dan Sperber, I believe that this highly technical term should mostly be used metaphorically in our context). Apart from methods, I regard disciplinary ontologies as another important source of this kind of gap. For one thing, disciplinary methods are shaped by assumptions about the properties of research objects and, of course, vice versa (the underdetermination thesis plays, among other things, on the suggestion that methods can shape the objects of research). To my mind, methods as well as ontologies will strongly determine disciplinary "language games" (another metaphor). But if this were true, both methodical and ontological structures would necessarily need to be considered in cognitive cooperation between disciplines. How could this be done effectively? Since disciplinary ontologies are usually mapped in the semantical content of axioms, theorems, experimental hypotheses etc. they are accessible to analysis. If it were possible to model ontological (i.e. semantical) and methodical structures in specific research projects then this could constitute a generic and applicable means to be deployed in interdisciplinary cooperation processes. In his reply to Fuller, Sperber hints at this by referring to "necessary conceptual resources" that would be at the disposition of most scientists, were they to commit themselves to interdisciplinary work. Although I don't doubt that scientists are usually able to acknowledge and understand many salient aspects of conceptual boundaries between disciplines I am not as optimistic as Sperber whether this fact (plus "a bit of self-tutoring") would suffice to lower the conceptual thresholds at play in interdisciplinary work. I tend to think that a robust sense of the specific methodical and ontological boundaries between disciplines presupposes a rather specialized repertoire of cognitive skills that would need to be developed in academic training to become operative in cross-disciplinary research. The analogy between disciplines and nation-states sounds intriguing. As Fuller has already pointed out, it goes only so far since the concept of boundary is much more indeterminate in science. This is a point that has also been raised by Julie Klein in her contribution where she suggests that disciplinary boundaries are "constantly remade". To my mind, the boundaries of a discipline are more or less continually reshaped with each research project that generates new knowledge. If this were true it would imply that the term discipline is usually stronger associated with institutional boundaries than with cognitive boundaries. (I briefly remark on the concept of a discipline in my reply to Bill Lynch)

brittle disciplines
Tim Moore
Apr 2, 2003 19:23 UT

No doubt, Dan is right to say that "the current disciplinary system may be becoming brittle". In fact, all such systems have been brittle over time. But as Dan also indicates, institutional factors favouring particular disciplinary divisions have become very powerful. The question then is, how to achieve a revolution.

Revolution?
Dan Sperber
Apr 3, 2003 11:53 UT

Dear Comrade Tim,

Yes, “institutional factors favouring particular disciplinary divisions have become very powerful.” But is the role played by disciplinary institutions altogether evil? Do we have a general,

workable, alternative way of organising scientific research that would work better? My answer is "**no**" to both questions. So, I am in favour of bringing about local improvements – including, on occasion, by removing local powers – when we have a good idea of how to do this – in fact, an idea good enough to convince enough people, so that, in most cases, it can be peacefully achieved.

Still, I do expect changes in the organisation of science and academe to be so important in the coming half-century that it will amount to a revolution. But having anarchist sympathies, I am against planning this revolution. Let it happen, let us participate, and let us enjoy it!

An alternative model for organising scientific research
Gloria Origgi
Apr 3, 2003 20:35 UT

Dan asks: "Do we have a general, workable, alternative way of organising scientific research that would work better"?

I think that there are alternative ways for organising advanced research, and we should look at them to get inspiration for organising research training and education.

Take the case of the Santa Fe Institute. No tenure faculty, no departments, a general commitment to interdisciplinary projects and an ongoing re-description of its goals. Here's the way in which the Institute is presented on its web site: "Santa Fe Institute seeks to catalyze new collaborative, multidisciplinary projects that break down the barriers between the traditional disciplines, to spread its ideas and methodologies to other individuals and encourage the practical applications of its results. The Institute's research is integrative and there are no formal programs or departments. The two dominant characteristics of the SFI research style are commitment to an interdisciplinary approach and an emphasis on the study of problems that involve complex interactions among their constituent parts."

The physical and temporal organisation of this Institute - no departments, no permanent faculty - is already a revolution in the mode of thinking interdisciplinary work. Researchers are selected on the basis of their quality but also of the relevance of their work for a particular ongoing project, not just through a disembodied criterion of "excellence" in one's own field, based on a consultation of the Science Citation Index.

Content-driven research groups that pursue a goal within a limited time span seems to me a promising way of organising research institutions in the future. This should be coupled with a different policy of employment, of course, detached from disciplinary affiliation.

On Julie Klein on revolution and evolution
Dan Sperber
Apr 10, 2003 14:19 UT

Julie Klein (in her "Responses to Dan's initial responses") writes:

"Responding to Tim Moore, Dan commented that he is in favor of bringing about local improvements. Yes, indeed. The cumulative force of local developments can be powerful. "Revolution," though, may not be the most appropriate metaphor. If we factor in both major (opportunistic) events and the quiet daily flow of influence across disciplinary boundaries, we're talking about "evolution" (though I concede it too is a loaded metaphor)."

DS: Instead of asking whether we are dealing with evolution or revolution, we might just ask: How radical the changes in the organisation and in the teaching of the sciences might be in the coming decades? Will academic curricula still be largely determined by disciplinary departments? Can the changes in supply and demand for higher education, linked in particular to novel uses of the Internet, put an end to the quasi-monopoly of these departments, or even render them obsolete? Will new forms of publication, of evaluation, and of recognition of

research move, at least in part, institutional power from very steady disciplinary structures to more dynamic active-research-programme structures? I wonder.

Julie adds an interesting example, and here I quote without further comment:

"Relatedly, Gloria Origgi invites us all to think about more alternative models for organizing research, from Santa Fe to lesser-known organizations and networks. In this vein, I also welcome Rainer Kamber's insertion of sustainability into the conversation. The Man-Society-Environment (MGU) at Basel was a striking exemplar. Students gained transdisciplinary skills in project with stakeholders in thematic areas such as land use, biodiversity, and conservation by focusing on "real-world" problems. After basic courses on the interface of ecological, economic, and social topics, they selected modular courses that might complement disciplinary interests while remaining within the general framework of MGU."

A transdisciplinary academic program
Rainer Kamber
Apr 14, 2003 20:23 UT

I want to thank Julie Klein for her remarks (05-Apr-03) about our program MGU (Mensch Gesellschaft Umwelt) at the university of Basel. Since Dan Sperber briefly made note of our program too (10-Apr-03), let me simply supplement Julie's acute overview by pointing out first that MGU is both an educational academic program as well as an important sponsor of transdisciplinary research at the university of Basel and associated academic institutions in northwestern Switzerland. Over the last ten years MGU has wholly or partly financed 40 "transdisciplinary" research projects in the area of sustainability research. Four new projects have just started in 2003. Most of the projects have at least featured cooperative efforts between the natural and the social sciences, many also included the humanities. Most projects have been constructed around a partnership with stakeholders from without the academic context. Regarding the educational program, since 1993 around 120 students have completed a minor (master-level) or the MGU postgraduate program.

The first ten years of MGU were in a way exceptional in the context of inter- or transdisciplinary academic programs in at least two regards. For one, although MGU was fully integrated into the university operatively it has been independent institutionally, being financed through a public foundation. The educational program has been and still is supervised by an academic, transdepartmental scientific committee with representatives of all departements that allow MGU as a minor (i.e., all but the medical sciences department). The research program is being supervised by an external board of academic reviewers from Swiss and German universities. The second point is that both the MGU research as well as its educational program have been designed from the beginning to focus on scientific cooperation between the natural and the social sciences and the humanities plus problem-oriented cooperation with non-academic stakeholders. Furthermore, research and teaching have been quite tightly integrated, with researchers of most projects also giving courses in the academic program. Since 2003 MGU is now fully integrated as an academic institution in the university of Basel, meaning that our budget is no longer independent.

There is, of course, much to be said about experiences, successes, and failures of this program and I will not attempt this in the next 100 words. I would like to return to some aspects, though, in the course of further discussions (and I will certainly answer specific questions if I can). Let me just note that, in most respects, MGU was and still is struggling with all the challenges that such a program might face, many of which have already been mentioned in this online conference. According to my personal experience in academic workgroups, panels, commissions etc. it seems that quite a few people within the university perceive the institutional integration as a welcome opportunity to finally check what has, in their eyes, represented a strange chimera of an academic program whose scientific value they tend to judge as questionable.

Santa Fe Institute
Jochen Glaser
Apr 16, 2003 4:47 UT

Gloria's description of the Santa Fe Institute is very interesting. I do believe that 'content-driven' interdisciplinary research has the highest likelihood of success.

What makes me feel a bit uncomfortable is the idea of a 'projects only mode' without permanent faculty and without departments. The growing practice of funding research by funding research projects apparently has created the belief that all scientific research can be done that way. I don't think this is true. For example, German freshwater ecologists who designed a long-term observation of a lake said in an interview that they were told by Canadian colleagues “You are lucky that you can do this. We must report results after every two-year project.” The project mode did not make the colleagues’ work impossible, but it made some types of observations impossible and thus changed the content of their work.

The non-permanent staff has shortcomings, too. Since tenure is an important asset for scientists I am not too sure that the Santa Fe Institute manages to hire the best scientists all the time. Competent scientists, yes (given the labor market situation), but not the best.

That is why I think that the Santa Fe model represents a specific way to organize interdisciplinary research that is not applicable to all types of research. In order to advance the management of interdisciplinary research, one would have to look at types of interdisciplinary projects and to relate them to management types. I am sure the Santa Fe model would turn out to be an effective solution for a specific type of interdisciplinary research.

Reply to Jochen Glaser
Gloria Origgi
Apr 20, 2003 0:02 UT

I wonder whether the Canadian ecologists’ research project is interdisciplinary in the same sense that seems to underlie the Santa Fe program. Ecology is an “interdisciplinary discipline”, that is, a permanent alliance of a number of subjects. Santa Fe seems to encourage a more “creative” way of merging fields of knowledge to produce a new insight.

Both are aspects of interdisciplinary work, but they may represent different “stages”: ecology represents here a mature stage in which a content-driven research project has evolved in a more complex practice that is able to take care or its own organisation.

But Jochen is right in pointing out that we should look at different types of interdisciplinary work and match them with different types of management of research.

Problem-solving with adequate means
Rainer Kamber
Apr 3, 2003 18:22 UT

I thank Dan Sperber for his well-informed and inspiring introductory sketch. It is obvious that he speaks from experience. I am a philosopher (philosophy of science, epistemology and metaphysics) and my working environment is an academic program in the sustainability sciences that organizes and administrates a "transdisciplinary" education program as well as a research program that funds appropriate projects at the University of Basel, Switzerland. We are currently doing empirical and theoretical research on the conditions for succesful cognitive integration processes and I hope to share some of our current insights and assumptions. But to legitimize a new discussion let me just remark on a point raised in Sperber's contribution that stood at the beginning of our approach a few years ago.

Regarding the two concurring grant proposals in "Cosmetic Interdisciplinarity" one more point could be made. If a grant proposal is approved because it is seen "as more clearly deserving to be funded" then this usually means that it conforms better to some set of canonical scientific standards. I gather that

this is just what Sperber is saying. But what other criteria could actually be deployed? "Specific problem-solving capacity" could be one. To my mind, a basic assumptions in much of the discourses about "inter-", or "multi-", or "transdisciplinarity" etc. is that these specific modes of knowledge production can deliver something that disciplinary science cannot. What is it? And what is the lack that needs tending? There seem to be certain kinds of problems that are best solved not in disciplinary but in interdisciplinary mode. If it is not unreasonable to assume that "disciplinary problems" are best solved in disciplinary mode there would have to be "non-disciplinary problems" (NDP) for non-disciplinary modes of knowledge production. I see two subclasses of problems belonging to NDP: (i) Scientific but non-disciplinary problems and (ii) non-scientific problems, i.e problems in the "Lebenswelt". While (i) scientific problems (the kind Sperber himself has been involved in) could be seen as representing epistemic desiderata determined mainly by internal parameters (history of a discipline or the co-development of several disciplines, publication and grant opportunities etc.) "Lebenswelt"-problems will very likely not be reducible to epistemic desiderata. I tentatively describe "Lebenswelt"-problems as perceived divergencies between actual (social, economical, cultural, ecological etc.) and desired states of affairs. It is easy to see that "Lebenswelt"-problems will in general not simply correspond to scientific problems but will have to be taken apart somehow to become scientifically solveable. Furthermore, solutions to non-scientific problems will presumingly differ somewhat from solutions to scientific problems. In short: Cooperative and boundary-crossing modes of knowledge production will be deployed if the problems at hand demand it and certainly not just for the sake of interdisciplinarity. Apart from the many theoretical issues raised with these general remarks this amounts at least to the challenges of (1) further specifying what happens if science attempts to solve non-scientific problems and (2) what exactly the nature of the means for cognitive integrative processes in knowledge production could be.

An insightful contribution
Dan Sperber
Apr 3, 2003 20:52 UT

Thank you for a very insightful contribution, several themes of which are sure to resurface in this seminar.

Terminology Matters
Julie Klein
Apr 3, 2003 22:14 UT

Dan Sperber's contribution contains compelling reminders of how difficult interdisciplinary work can be. It also underscores the need to exercise caution when using the terms "disciplinarity" and "interdisciplinarity." Both terms are still used, too often, with a presumed singularity of meaning -- as in "the disciplines" impede interdisciplinary work (they do and they don't, in varying degrees and contexts) and "interdisciplinarity is" (a monolithic assertion of definition that falls apart in the face of what Ludwig Huber called a "jungle of phenomena").

Sperber offers a range of negative and positive experiences in an equally full range of formations, from ephemeral conversations to respected networks and programs that become the site of graduate training. All the while, disciplines continue to exert power in the political economy of the academy, but there is ample evidence to suggest that both disciplinarity and interdisicplinarity are now entangled in new webs of relation. The older contest of disciplinary identity and interdisciplinary unity has been replaced by a more complex array of borrowings and crossfertilizations, new subdisicplinary and interdisciplinary formations.

I took particular note of Sperber's account of serving on a grants committee, forced to choose between a "good proposal" with a superficial and ad hoc interdisciplinary character and a "merely decent but genuinely interdisciplinary and innovative" proposal. "How likely," he asks, "is it that outstanding interdisciplinary proposals emerge in such conditions?" I accept his answer but add another. A review committee made up of scholars from several disciplines is not "interdisciplinary." It is an assembly of disciplinary experts: at best learning from their own multidisciplinary conversation, at worst acting upon their ignorance of the accumulated wisdom of practice and theory of interdisciplinary research.

Enter the internet. Recent studies of projects funded by the European Commission in the Fifth Programme's quality of life initiative reveal more multidisciplinary than interdisciplinary outcomes. At

the same time they reveal a tremendous amount of learning that is now being assembled and disseminated. See the forthcoming November issue of FUTURES and the ongoing efforts of SAGUFNET in the realm of sustainability (http://www.transdisciplinarity.ch). To Sperber's examples, I would add the growth of multidisciplinary databases that facilitate communication in interdisciplinary networks. At the same time, since I teach interdisciplinary research and problem solving online, I would caution that the internet is not a panacea. Unless we bring a new complexity of understanding to the most basic terms in the discussion we will recycle old meanings that are eclipsed by the current plurality of activities, institutional formations, and epistemological implications.

Other terminology, I would add, must be part of our discussion. The recent heightened rhetoric of transdisciplinarity in Europe documents a new phase in the rethinking of interdisciplinarity, challenging both older notions of interdisciplinarity and the relationship between science and society. In the process, we should also be rethinking assumptions about boundaries. The older premise that disciplinary boundaries would disappear (and disciplines merge) ignores the fact that boundaries do not disappear. They are constantly being remade.

Reply to Klein
Dan Sperber
Apr 5, 2003 14:11 UT

Let me first say how glad I am to see Julie Klein participate in this seminar. Nobody has written more extensively and insightfully or is more authoritative on interdisciplinarity than she is.

There is nothing I disagree with in her very useful comments. In particular I did mean to imply that, as she puts it, "A review committee made up of scholars from several disciplines is not 'interdisciplinary.'" I agree that "it is an assembly of disciplinary experts: at best learning from their own multidisciplinary conversation, at worst acting upon their ignorance of the accumulated wisdom of practice and theory of interdisciplinary research." In France at least, it is very hard to convince the relevant academic and political authorities that such a committee is not ideal to evaluate interdisciplinary projects and appointments. But of course, there are not enough genuinely interdisciplinary senior scholars to have true interdisciplinary committee. One way to improve things here is to convince the relevant authorities to have as many genuine interdisciplinary researchers as possible in interdisciplinary committee, and for this, to relax seniority criteria (however, in the best of cases, it would still be useful to have some monodisciplinary specialists of the disciplines involved).

Terminology issues will come up again and again, in this seminar, starting with next month's presentation by Helga Nowotny. I recognise their importance. However, from the practitioner's point of view which is mine, I wonder whether issues of interdisciplinarity, as they arise, for different reasons, in different areas of basic and applied research, all fall neatly under any general concept, or whether they have just enough of a family resemblance to make it worth sharing the experiences, but not enough unity to call for a general and specific theory.

questions de terminologie
Dan Stoica
Apr 7, 2003 8:37 UT

Bonjour!

Je pensais deja intervenir sur la terminologie et je me sens beaucoup plus a l'aise depuis que Julie Klein a fait des précisions. Il ne me resterait à ajouter à la distinction interdisciplinarité/multidisciplinarité une autre, qui me semble échapper à Dan Sperber (ou, du moins, c'est ce que sa conférence laisse voir): interdisciplinarité/transdisciplinarité. Le deuxième terme de cette opposition, dans l'acception de Stéphane Lupasco et de Basarab Nicolesco, renverrait au besoin de transgresser les frontières des disciplines, de se placer au-delà de toute discipline. Un autre sens que propose B. Nicolesco pour "transdisciplinarité" serait "ce qui traverse toutes les disciplines possibles" (dans l'Introduction a "L'homme, la science et la nature", Le Mail, 1994). Toujours dans cet ouvrage, Solomon Marcus parle de trois interprétations du terme: 1)au-delà des discipline; 2) à travers les disciplines; 3)la

métamorphose des disciplines par leur évolution même. Cette metamorphose se produit même en l'absence d'interdisciplinarité, pouvant etre orientée vers la proliferation des disciplines ("Vers une approche transdisciplinaire du temps", op. cit., pp. 54-55). J'ai fait ces remarques justement parce que je trouve que toute opposition terminologique est enrichissante.

Réponse à Stoica: et "postdisciplinaire"?
Dan Sperber
Apr 7, 2003 9:20 UT

Je m'en suis tenu au terme le plus général (non pas étymologiquement, mais en pratique) "interdisciplinarité", sachant que de débat terminologique arriverait très vite dans ce séminaire. Les recherches qui m'intéressent sont sans doute aussi bien, ou mieux, décrites comme transdisciplinaires que comme interdisciplinaires. Cela dit, j'ajouterais volontiers le terme de "postdisciplinaire", non pas pour prophétiser mais pour poser la question de savoir si le futur des sciences est forcément disciplinaire (avec un peu de pluri-, d'inter- et de trans-disciplinarité en accompagnement.

Interdisciplinary networks
William Lynch
Apr 5, 2003 4:05 UT

I could empathize with Dan Sperber's account of the disciplinary obstacles faced in developing his program of research. I have experienced similar obstacles in the course of my graduate training and subsequent work in Science and Technology Studies, an explicitly interdisciplinary field bringing together different fields that study scientific development. In the first graduate program that I attended, the faculty had a largely multidisciplinary approach. Philosophers, historians, and sociologists of science agreed they had much to learn from each other, but this did not transform their individual work much. I argued that the development of their individual fields had produced problems that they could not solve with their own methods. Thus, I disagree with Rainer Kamber that "'disciplinary problems' are best solved in disciplinary mode." Sometimes it is the limitations of host disciplines that lead more adventurous scholars to adopt interdisciplinary methods. I take it that cognitive science is a good model for this kind of interdisciplinarity via disciplinary exhaustion.

I decided to transfer to a (seemingly) more aggressively interdisciplinary program that embraced newer approaches, such as the social constructivist examination of scientific controversies (anthropological relativism applied to science, basically). Historians could apply traditional historical methods with a narrative overlay of relativism, or something like it. Sociologists could apply ethnographic methods to contemporary cases. In this sense, a shared commitment to descriptive methods and relativist narratives forged an "interdiscipline," a new node with its own dogmas and limitations. Philosophical methods and quantitative sociology were largely marginalized. And those exploring alternative narratives (realist, empiricist, critical) were marginalized. It worked pretty well in developing a new field with journals, graduate programs, and funding, though graduates usually had to establish traditional disciplinary credibility to get a job. In terms of solving the disciplinary problems that spurred these forays, however, STS is largely a failed revolution, in my opinion.

On the other hand, I think that Dan Sperber's emphasis on the importance of removing disciplinary obstacles that individuals face in trying to pursue specific research questions would shift the focus away from some over-arching narrative of the state of the field. The revolutionary content of interdisciplinarity, then, would not be in the slogan of interdisciplinarity but in the changed patterns of training, research, communication, and dissemination that it facilitates.

And as a couple of people have mentioned, the internet is the key technology here. It may be useful here to draw a connection with Manuel Castells' work on how the internet and other technologies is leading to a "networked" society. Talk of shared cultures and subcultures has to give way to an understanding of how each individual user finds their own path through the web (something portals try to control). In other words, we can no longer presume that others around us share some common culture, since we all piece together our frames of reference from our own set of linked resources. As it plays out in research, this would imply that there are as many interdisciplinary nodes as there are individual researchers

Let me be clear, however. I am not advocating that we should let a thousand flowers bloom. This creates a serious problem in scientific organization, just as Castells suggests it does for citizenship more generally. No shared culture means no shared knowledge.

Social structures and cognitive structures
Rainer Kamber
Apr 5, 2003 16:31 UT

Bill Lynch rightly points out that, in his experience, "individual fields had produced problems that they could not solve with their own methods", and that this fact contributed to interdisciplinary efforts. In my earlier contribution ('Problem-solving with adequate means', 03-Apr-03) I did not want to generalize my statement that disciplinary problems are best solved in disciplinary mode. I only meant that this will usually be the case (maybe mostly so in "normal sciene", to use Kuhn's term; but interdisciplinary research likely does not to represent normal science in this sense). But what are "disciplinary problems"? I believe that an answer will bear on Lynch's interesting remarks. Obviously, the term "discipline" can have several sensible (and certainly many metaphorical) meanings, e.g. as a sociological concept referring to an organization with an approximately definite number of members, a normative structure of some kind, specific internal institutions regarding the division of labor etc., where all these social properties are meant to mark the boundary of a "discipline". To my mind, this kind of social concept of a discipline is well-formed for the needs of empirical social research about science but it seems to lack a conceptual grip on the cognitive structure of a "discipline" (Kitcher's concept of a discipline is maybe better equipped for this). What this could amount to if one assumes, like Julie Klein in her contribution, that disciplines "are constantly being remade" is that social organizational structures as well as cognitive structures of a discipline are constantly being remade, but not necessarily in a convergent sense where, e.g. the social structure of a discipline determines the development of its cognitive structure or vice versa. Plausibly, certain aspects of its social organization will restrict its cognitive development. On the other hand, there will be aspects of the cognitive structure that will bear on the differentiation of its social structure. I believe that many of the concerns about interdisciplinary cooperation in science adressed in this conference so far stem exactly from the fact that the cognitive and the social structures of a discipline are only loosely coupled and that, e.g., curricular frameworks that strongly determine cooperative skills (or their absence) in research are more determined by the social structure of a discipline than by its cognitive structure. Bill Lynch's point about the origin and the impact of certain disciplinary or cross-disciplinary problems seems to me to illustrate this. Having said the above, I believe that genuinely interdisciplinary enterprises are most often fuelled by the fact that there are cognitive developments in a certain area of research - encompassing, maybe, several disciplines in an area where some of the subject matter overlaps the cognitive disciplinary boundaries as in the case of the Cognitive Sciences - that go beyond the scope of the social disciplinary structures. It thus seems that there is a limit to the possible divergence between the social and the cognitive structure of a discipline.

Kitcher, Philip (1993); The Advancement of Science. Science Without Legend, Objectivity Without Illusions. New York etc.: Oxford University Press.

Global and local issues
Dan Sperber
Apr 6, 2003 16:22 UT

When Bill Lynch writes that my “emphasis on the importance of removing disciplinary obstacles that individuals face in trying to pursue specific research questions would shift the focus away from some over-arching narrative of the state of the field. The revolutionary content of interdisciplinarity, then, would not be in the slogan of interdisciplinarity but in the changed patterns of training, research, communication, and dissemination that it facilitates,” he captures quite well something I am trying to say. I see little point in chanting “Interdisciplinarity! Interdisciplinarity!” More concretely, a policy in favour of interdisciplinarity *in general* would mean what? That a small but growing percentage of research funds should be earmarked for interdisciplinary research? Something like this is the case in France. One of the effects of this policy is indeed to favour the real thing, but my hunch is that disciplinary bosses learn how to dress their grant proposals in interdisciplinary garb and get hold of most of the moneys. To do

better means focusing on more local issues where there is a mismatch between, to use Rainer Kamber's notions, cognitive and social structures. For individual researchers, this means highlighting such mismatches when they occur (at some risk to themselves, so there should be some institutional incentive and protection for these researchers). For scientific policy-makers, this means auditing, so-to-speak, specific research areas whenever there is a suspicion of cognitive/social mismatch.

A genuine question
Jose Luis Guijarro
Apr 5, 2003 16:33 UT

I am definitely not conversant in the issue of this seminar. This is probably one of the reasons why I find the ongoing discussion so informative for me. From what I have read until now, however, a genuine question, which has been already raised, albeit laterally, comes up to my mind. Is it not possible that if we were to achieve interdisciplinarity with some success in the future, the natural human trend would be to become specialised in one field or another thereby creating a new map of disciplines getting more and more apart? Maybe it's a silly simile, but in the European endeavour to create a new super-nation out of the existing ones, tiny nationalities (the Basque, the Catalan, the Galician in my neck of the woods are a good example, but there are others in the Balkans and even in France and Italy I gather) try to emerge and new collocations (i.e., "OId Europe" vs "New -or, as I call it, "Americanised"- Europe) seem to be appearing as well. If this is indeed a natural human trend, what sort of actions must be taken to overcome it? It seems to me that, important as it is, the real deep issue is not the present social arrangement of disciplines, but rather the human condition to become specialised when living our lives (be it the life of peasant, fisherman, bus driver, researcher or whathaveyou). The problem, then, is twofold: (1) is it a good idea (and if so, why) to try and change this human trend for specialisation? And (2): are there any cognitive or psychological means which might warrant some sort of success in that pursuit?

Reply to Guijarro and Luchian
Dan Sperber
Apr 6, 2003 23:50 UT

I reply here to both José Luis Guijarro and Radu Luchian, and post the same reply in the discussions of their two messages. Both argue that the problems I tried to raise are grounded in very general aspects of human nature. For José Luis there is a "natural human trend … to become specialised." Well, human are much less cognitively specialised than any other species. Moreover, when acting at a social-cultural level over historical time, they are remarkably good at overcoming whatever specialisations they may have (I have written quite a bit on these issues in my work on modularity and culture, by the way). For Radu Luchian, there is an "animal fear of the unknown, of the different, of the 'other' which still plagues us." Well, whatever fear of the unknown humans may have, it has not been strong enough to prevent the development of science, which seems to be guided, rather, by a taste for the unknown. If you must invoke such vague and general human tendencies, why not mention also a taste for analogies and generalisations, and plain curiosity? In any case, we are discussing here the recent disciplinary organisation of the sciences and the fact that this organisation is now being challenged in a variety of ways. It seems to me implausible that some general human cognitive tendencies imposed this disciplinary organisation, or that it would render impossible its replacement by a different organisation in the future. While I would be the last one to deny that cognitive factors – including species-specific dispositions – are relevant to the study of historically situated social-cultural phenomena, I would argue that the cognitive factors involved are subtler, and that their role is never so simple.

Responses to Dan's Initial Responses
Julie Klein
Apr 5, 2003 20:31 UT

I'd like to respond to several of Dan's responses. In answering my "Terminology" posting, Dan remarked that changes must made in the system of grant evaluations. Public agencies and private foundations have made advances in insuring interdisciplinary evaluations. The models remain too few

in number, but Dan introduces a crucial generational dynamic that is also evident in new approaches in the disciplines and a general increase in interdisciplinary approaches.

Responding to Alexander Kravchenko, Dan indicated he is more positive. I agree that interdisicplinarity is attainable and that interdisicplinarity per se is rarely, if ever, the goal. I think the concept of interdisciplinarity, though, is not too superficial to aid in elucidating issues of theory and practice. Yet, I'll admit, my optimism is checked by widespread superficial understandings of the concept.

Responding to Patrice Ossona de Mendez, Dan remarked that he sees interdisciplinarity in a more dynamic way. I agree, while affirming Dan continuing argument that there is a plurality of activities of activities, formations, and attitudes.

Responding to Ira Noveck, Dan argued that he overestimated what philosophers are capable of. Ira made the worthy suggestion that philosophers are in a key position to define issues requiring interdisciplinary cooperation. The kind of reflexivity they are trained to perform, though, is necessary in all disciplines and fields. If we pass the responsibility and capacity to philosophers alone (without diminishing their leadership) we limit the socio-epistemological reflection that must be part of any interdisciplinary endeavor, whether collaborating on a particular project or building a field. We need both.

> Responding to Tim Moore, Dan commented that he is in favor of bringing about local improvements. Yes, indeed. The cumulative force of local developments can be powerful. "Revolution," though, may not be the most appropriate metaphor. If we factor in both major (opportunistic) events and the quiet daily flow of influence across disciplinary boundaries, we're talking about "evolution" (though I concede it too is a loaded metaphor). Relatedly, Gloria Origgi invites us all to think about more alternative models for organizing research, from Santa Fe to lesser-known organizations and networks. In this vein, I also welcome Rainer Kamber's insertion of sustainability into the conversation. The Man-Society-Environment (MGU) at Basel was a striking exemplar. Students gained transdisciplinary skills in project with stakeholders in thematic areas such as land use, biodiversity, and conservation by focusing on "real-world" problems. After basic courses on the interface of ecological, economic, and social topics, they selected modular courses that might complement disciplinary interests while remaining within the general framework of MGU. I also agree with Rainer about the urgency of problems in the Lebenswelt, and his contention that an inter-disciplinary or transdisciplinary approach is not always the most appropriate. At the same time, Bill Lynch made a good point in cautioning against thinking that "disciplinary problems" are best solved in a disciplinary mode. One of the striking developments in knowledge over the latter half of the twentieth century was the reconceptualization of some disciplinary problems as multi- and interdisciplinary problems.
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> **Interdisciplinarity: a theoretical or an historical concept?**
> Dan Sperber
> Apr 10, 2003 14:08 UT
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> (I answer two other points raised by Julie's posting in the threads where the issues started, namely "Searching for clues in one's own discipline" and "brittle disciplines")
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> Julie writes:
>
> "I agree that interdisicplinarity is attainable and that interdisciplinarity per se is rarely, if ever, the goal. I think the concept of interdisciplinarity, though, is not too superficial to aid in elucidating issues of theory and practice. Yet, I'll admit, my optimism is checked by widespread superficial understandings of the concept."
>
> Let me just suggest that the notion of discipline in its current sense (referring not just to relatively autonomous and relatively integrated areas of research, but also to institutions) may be of greater historical than theoretical relevance. That is, it may denote an historical phenomenon in the development of the sciences rather than a basic form of organisation truly constitutive of the sciences. If so, then the same should be true or interdisciplinarity: the notion

may be useful to describe specific interactions in this disciplinary age of the sciences. Of course, this would make the notion useful enough.

The Inter/Disciplinary Relation
Julie Klein
Apr 10, 2003 21:17 UT

I want to echo Dan's important distinction between "discipline" in an historical sense and in an organizational sense. Following suit, the same distinction is legitimate to transfer to the meanings of interdisciplinarity, which have shifted in meaning over time and, more evident of late, in sync with the shifting character of how people actually perform "disciplinary" work in a multitude of settings.

Nothing to rethink.
Radu Luchian
Apr 6, 2003 3:56 UT

Mr. Sperber offered us a few examples, most because of the dissapointment they provided, the last two with a welcome air of optimism. As a student in a field interdisciplinary by definition (Cognitive Science), I sadly identify with the student example he gave us.

The important distinction I did not find in Mr. Sperber's paper, but later on appeared in Ms. Klein's discussion, is the one between multidisciplinarity (impossible due to terminological and methodological barriers) and interdisciplinarity (possible only in communities of people who keep an open mind and can see meaning beyond the literal reading). There's nothing to rethink about interdisciplinarity. What we have to fight is the animal fear of the unknown, of the different, of the 'other' which still plagues us.

The basic problem is not limited to science. Generation after generation of philosophers, theologians, artists have struggled with it throughout history and across civilizations. What happens is that people are limited by many factors, the harshest of which being time. Social constraints are also very powerful. So we tend to work in 'established disciplines'. And whenever someone comes in with an idea we don't understand, the first reaction is to say "it's wrong". Until someone pig-headedly works on the idea/methodology and shows an open-minded community that it is worth something.

Here's an example.

Coming from a neuroscience background, David Marr spent most of his tragically short carreer writing on the ideas that the cognitive processes can and should be described at different levels; the three levels currently codified under the term Marr's tri-level hypothesis (especially in MIT circles), are the computational, algorythmic and implementational. Between 1977 (with Tomaso Poggio) and 1980 he was working on a book on Vision (which got published two years after his death). If interdisciplinarity would have been as popular then as it is now, Marr would have benefitted from learning about Anderson's Model. Philip W. Anderson, Nobel Prize winner for Physics challenged the reductionist paradigm in 1972: "Each (physical) level has its own 'fundamental' laws and its own ontology." It's interesting that there are papers on color vision and other human sensory apparata which quote Anderson's 1972 paper, but do not even mention Marr's work.

This view is consistent with everything I have experienced to date in any field I studied. There are NO absolutes, no final law, the closest we can get to Truth is by creating theories consistent within themselves and attempting to relate them. But what interdisciplinarity can do is to provide fora like this one, where people can share opinions and evidence. Out of such discussions, research advances.

Individual disciplines are just as necessary, however. That is where the actual research is done. Nobody can reliably follow two methodologies at the exact same time. Comparisons involving different methodologies are highly prone to divergent interpretations. But exposure to the pros and cons of different methodologies can give raise to new ones, better suited to the study of specific phenomena.

Reply to José Luis Guijarro and Radu Luchian
Dan Sperber
Apr 6, 2003 23:51 UT

I reply here to both José Luis Guijarro and Radu Luchian, and post the same reply in the discussions of their two messages. Both argue that the problems I tried to raise are grounded in very general aspects of human nature. For José Luis there is a “natural human trend … to become specialised.” Well, human are much less cognitively specialised than any other species. Moreover, when acting at a social-cultural level over historical time, they are remarkably good at overcoming whatever specialisations they may have (I have written quite a bit on these issues in my work on modularity and culture, by the way). For Radu Luchian, there is an “animal fear of the unknown, of the different, of the 'other' which still plagues us.” Well, whatever fear of the unknown humans may have, it has not been strong enough to prevent the development of science, which seems to be guided, rather, by a taste for the unknown. If you must invoke such vague and general human tendencies, why not mention also a taste for analogies and generalisations, and plain curiosity? In any case, we are discussing here the recent disciplinary organisation of the sciences and the fact that this organisation is now being challenged in a variety of ways. It seems to me implausible that some general human cognitive tendencies imposed this disciplinary organisation, or that it would render impossible its replacement by a different organisation in the future. While I would be the last one to deny that cognitive factors – including species-specific dispositions – are relevant to the study of historically situated social-cultural phenomena, I would argue that the cognitive factors involved are subtler, and that their role is never so simple.

Simple or over-simplified?
Radu Luchian
Apr 7, 2003 0:17 UT

I did not say the cognitive factors involved in socio-cultural phenomena such as the ones we discuss in this seminar are simple. I said that (among other phenomena), we observe a continuous interplay between the innovating spirit and the conservative one. And that both are equally useful and different people choose different points of balance between them. Fear of the unknown is always a brake for unchecked curiosity. Depending on the goal one has in mind, the brake is useful- or it isn't.

When the body of knowledge was smaller, it was easier to be 'interdisciplinary' Physicians were physicists and chemists and biologists. Architects were sculptors and painters and mathematicians. And so on. There's nothing to rethink. The term may be new, but the concept behind it is old and as necessary for the advance of the models we build, as specialization is for the consistency of those models.

Precision matters, too
Jochen Glaser
Apr 7, 2003 0:56 UT

The discourse on “interdisciplinarity” seems largely artificial to me. It can be kept going only because nobody is too precise about what is referred to by the word (I don’t think it is a concept). Just three examples:

- Since the seventies we know that “disciplines form the teaching domain of science, while smaller intellectual units (nestled within and between disciplines) comprise the research domain” (Chubin 1976: 448). Steve Fuller introduced this idea by emphasizing differences in methodologies, but objects or problems can constitute specialties, too (Whitley 1974). If we accept the idea of specialties, then “interdisciplinary research” refers to the degree of heterogeneity of knowledge combined in research. Moreover, the difference between research domain and teaching domain makes it possible to understand some of the problems of scientific careers mentioned by Dan Sperber.

- At the level of single research processes, there is a vast area of almost unproblematic interdisciplinary collaboration. Scientists attempt to solve very concrete problems, and they specialise and subsequently collaborate because they couldn’t solve the problems otherwise. Yes, there are

problems of language, conceptual differences, etc., but they are overcome in most cases because the collaboration would fail otherwise (Laudel 2001). It is primarily at higher levels of aggregation where things get messy. Apparently one could distinguish between bottom-level interdisciplinary research that is conducted opportunistically according to scientists needs, and a 'top-down interdisciplinarity' which is imposed on scientists for 'political' reasons without too much consideration for practical problems.

- The politically induced "interdisciplinarity" is rooted in the observation that the new combination of knowledge that is part of interdisciplinary research is often a source of important scientific innovations. Indeed, the studies on the emergence of scientific specialties have demonstrated that combining heterogeneous knowledge is one of the main ways in which new specialties emerge. The demand for "interdisciplinarity" tries to promote innovations in science by turning the above observation into a generalised expectation. Empirical studies have shown that funding of interdisciplinary collaboration can trigger sustainable interdisciplinary research programs, i.e. programs that continue after the initial funding was ended. However, most collaborations are more short-lived. We can observe a typical process here: Institutionalization of funding criteria is also an over-generalization of these criteria, and science responds partly with window-dressing.

I tried to show with these remarks that the more interesting problems arise when we leave the general "interdisciplinarity" discourse and specify levels of aggregation, content of interdisciplinary relationships, and relations to science policy. Please understand this as a plea for a more concrete debate.

Chubin, Daryl E., 1976. The Conceptualization of Scientific Specialties. Sociological Quarterly 17: 448-476.

Laudel, Grit, 2001. Collaboration, creativity and rewards: why and how scientists collaborate. International Journal of Technology Management 22: 762-781.

Whitley, Richard D., 1974. Cognitive and social institutionalization of scientific specialties and research areas. Richard Whitley (ed.), Social Processes of Scientific Development. London: Routledge & Kegan Paul, 69-95.

I agree
Dan Sperber
Apr 7, 2003 9:41 UT

I agree with Jochen Glaser "that the more interesting problems arise when we leave the general 'interdisciplinarity' discourse and specify levels of aggregation, content of interdisciplinary relationships, and relations to science policy." This is why I tried to introduce the debate at a quite concrete level. On the other hand, given that I participate more as a practitioner of interdisciplinary work reflecting on his experience than as a student of science itself, I welcome and greatly enjoy the contributions of people who have worked on the issues from a philosophy/history/sociology of science point of view. I hope the seminar, in the coming months, keeps going back and forth between these two perspectives.

Not Artificial but Essential
Julie Klein
Apr 10, 2003 21:09 UT

Replying to Jochen Glaser, and Dan's response, I would not call the discourse on "largely artificial," but I do agree that concrete examples are crucial for testing theory in the forge of practice.

I also agree that there is a "vast area of almost unproblematic interdisciplinary collaboration" when researchers are focused on concrete problems – the "bottom-level." However, I would ask you for more examples of "top-down interdisciplinarity" imposed on scientists for political reason, "without too much consideration for practical problems." A good deal of interdisciplinary research being targeted by industrialized nations at present is favoring selected problems (and

concrete ones at that). So, the discussion turns to the theme Rainer Kamber introduced. Where do the problems originate? We must talk about problem choice. Finally, I want to echo Dan's response to Jochen. We need a dialogue of both the general and the concrete. Going back and forth is crucial to understanding, especially in a seminar that targets the question of "Why Rethink Interdisciplinarity?" Each will enrich the other.

'top down interdisciplinarity'
Jochen Glaser
Apr 14, 2003 4:56 UT

Julie is quite right in demanding more precision from me. With 'top down interdisciplinarity' I refer to anything where the demand for interdisciplinarity is stated prior to any assessment of the need for interdisciplinarity for solving research problems (see Grit's comments). Thus, "practical problems" referred to practical problems of the conduct of research rather than practical problems of society. Some examples of 'top-down interdisciplinarity':

- I observed the early history of research institutes that were newly founded after German unification. Part of the institutes' mission is interdisciplinary research. This is due to the fact that in Germany the institutionalisation of research outside universities has to be justified, and for at least one type of institute the justification is that the institutes can conduct interdisciplinary research easier than universities. However, the fields combined in the institute originally didn't come up with problems suitable for interdisciplinary research (except for one which was very applied in nature and therefore confronted with problems that demanded interdisciplinarity). Two developments could be distinguished: With weak organisational leaders, fields simply did not become integrated and proceeded without much interdisciplinary research (i.e. with as much interdisciplinary research as was necessary to pursue their research program). Strong organisational leaders enforced interdisciplinary research by (a) having an interdisciplinary research program of their own and crowding out all fields that were not needed for this program; or (b) giving resources only to projects that were applied for by more than one department (=fields). In both cases, the departments which could not integrate themselves in the interdisciplinary work suffered: In the first institute they got shut down, in the second institute they had to change their research programs until they could be integrated.

- A similar case can be made with regard to funding programs (as in Dan's example). In Germany, this seems to work well as long as collaborative projects are submitted to a (very specific and extended) interdisciplinary peer review (Grit knows more about this than I do). However, there are counterexamples where interdisciplinary research is promised because of the funding but doesn't happen. As far as I remember, some environmental research programs had this problem: Natural and social scientists could not collaborate successfully. They rather dealt with their own problems by applying their own methods. The results could still be combined, but this was a multidisciplinary rather than an interdisciplinary approach (at least in my understanding of the terminology).

With these examples, I would like to reinforce the point that interdisciplinary research is possible only if certain cognitive preconditions are met. 'Top-down interdisciplinarity' demands interdisciplinarity without being sure that these preconditions are given.

Finally, I agree with Julie's demand for a "dialogue of both the general and the concrete". Unfortunately, generalizing from empirical studies of interdisciplinary research plays only a minor role in this dialogue. I think that this is to a great extent due to a weakness of science studies which currently appear to lack a common frame of reference for empirical descriptions of research processes. Idiosyncrasies abound.

Interdisciplinarity works when it is actually needed
Grit Laudel
Apr 7, 2003 6:35 UT

Taking Dan's example of "Interdisciplinary disappointments": It is an example of failed interdisciplinary collaboration between anthropologists and psychologists. This is not surprising because it seemed to lack the basic pre-requisites of any kind of collaboration. Why should the anthropologists be interested in the research results provided by the psychologists? They have a shared object ("culture") but it is not clear if they have a shared subject matter ("culture" and "mode of thought" have different meanings). Have the psychologists formulated a research question that is interesting for the anthropologists? Have they formulated this research question in the language of the anthropologists? Obviously not, if the anthropologists think that the thesis "has already been amply demonstrated with ethnographic data". This is different to the many successful interdisciplinary collaborations I have observed in the natural sciences (Laudel 2001). The usual situation was that a scientist had a problem that he or she could only solve by borrowing methods from other specialties. To give one example: a group of cell biologists was interested in studying the movement of cells. They couldn't solve the problem with their own conventional methods (light microscopy). The cell biologists interested a group of biophysicists in the problem. With their help the biologists adapted another microscopical method and hence solved their problem. The main difference seems to be that there is a general interest in the other specialty's methods and not the attempt in the first line to reproduce the methodical and methodological differences between the disciplines, as Steve Fuller described it. The interest of many natural scientists is produced by a cognitive need to combine knowledge from different specialties, a need that is much weaker in the social sciences and humanities.

Concerning "A student's dilemma": It is a pity that Dan didn't describe more clearly what was the original topic of the student and in the way in which it had to be adapted. Indeed, the institutions that influence a scientists' career path do not keep step with the development of new research areas. A PhD degree is usually awarded in the older disciplinary structures. In my empirical studies I had several examples of PhD students successfully working in interdisciplinary projects. In these cases, the supervisors of the PhD student stemming from two different specialties, agreed about a research question that should be answered by the PhD student. The PhD student collected methods from both specialties and solved the problem. There were no cases where there was a problem of getting the degree from the faculty because the research problem to be solved was recognised as important for the degree rewarding discipline. Borrowing methods from other specialties is unproblematic because it is part of the scientific culture in many natural science specialties.

Laudel, Grit, 2001. Collaboration, creativity and rewards: why and how scientists collaborate. International Journal of Technology Management 22: 762-781.

> **When it is needed, or when the need is perceived?**
> Dan Sperber
> Apr 7, 2003 10:05 UT
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> Grit's contribution makes me envious of natural scientists. Among cognitive and social scientists, as I tried to describe, things are not so smooth. Is it because interdisciplinarity is not actually needed, or is it because the need is not well-understood? Often the latter, I would argue. It is true that needs are not as easily perceived in these fuzzier disciplines, which lack generally shared goals and criteria. In a good part of my work, I have been arguing that anthropological theory is at a dead end because of its inability to interact seriously with the cognitive and biological sciences. Some agree, some disagree. The right policy would be then, it seems to me, a pluralistic one: Let any given avenue be explored once there are enough serious scientists who have argued the case and want to go ahead. In practice, the disciplinary organisation of the social sciences (a real kludge, by the way), makes it much harder to explore an interdisciplinary than a disciplinary avenue, especially at a theoretical level, and especially when this would involve joining forces with cognitive or biological scientists.

research problems in the social sciences
Grit Laudel
Apr 8, 2003 6:14 UT

I agree with Dan's more precise formulation: It is not only the need for knowledge from other specialties but scientists' perception of such a need. In the social sciences it often appears to be not clear what the research problem is and hence when it is actually solved. Consequently, there is also a much weaker pressure to get adequate methods for solving the problem.

Two Requests for Grit
Julie Klein
Apr 10, 2003 21:06 UT

Grit, like Jochen, is such a welcome addition to our group. I'd like to ask, in fact, whether we might construct a library on this site where individuals could post writings that intrigue each other. Grit's piece on collaboration would be a welcome entry (especially since I'm finalizing now a book chapter on the nature of interdisciplinary collaboration and am eager to have a copy).

Apart from that request, I'm wondering Grit if you could please elaborate on your comment that the cognitive need to combine knowledge from different specialties is much weaker in the social sciences and humanities. I'm not sure I entirely agree but, before responding, I want to understand more what you are thinking here.

Online library for interdisciplinarity
Christophe Heintz
Apr 11, 2003 9:05 UT

To answer Julie's wish to have a library for interdisciplinarity on this site, I would like to point out that the site 'interdisciplines' does include a bibliography. You can sort it by conference so as to see directly which entries deal with interdisciplinarity. Grit Laudel's article has been promptly added to the bibliography and we hope we will be able to enrich it along with the discussion and articles. So you are encouraged to cite the relevant literature during discussions. ALSO, the bibliography can somewhat play the role of an online archive (library) insofar as the entries have the corresponding text online. When it is the case, please mention it (or write directly to us) and we will enrich the entry with the hyperlink to the online text.

Weak cognitive needs for interdisciplinary collaboration in the social sciences
Grit Laudel
Apr 14, 2003 3:48 UT

To answer Julies' question: Why is the cognitive need to combine knowledge from different specialties much weaker in the social sciences and humanities?

The vast majority of interdisciplinary collaborations I observed in the natural sciences had been driven by the use of methods from other specialties. The growing complexity of objects, also led to a growing need for methods from other fields. Scientists I observed were eager to use as many methods as possible in order to produce substances or to get complementary data; these methods often came from different fields. If the complexity of the research object is a driving force of interdisciplinary collaboration, then the weakness of the latter in the social sciences is surprising: The social sciences have to deal with the most complex object of all: human beings. But this object creates trouble for research: human objects can't be investigated in the same way as objects in the natural sciences. Consequently, the kind and number of methods applicable for observing this object is very limited. In their 1979 laboratory study, Latour and Woolgar jokingly commented: "Occasionally, when members of the laboratory derided the relative weakness and fragility of the observer's data, the observer pointed out the extent of the imbalance between the resources which the two parties enjoyed. 'In order to redress this imbalance, we would require about a hundred observers of this one setting, each with the same power over their subjects as you have over your animals. In other words, we should have TV monitoring in each office; we should be able to bug the phones and the desks; we should have

complete freedom to take EEGs; and we would reserve the right to chop off participants' heads when internal examination was necessary. With this kind of freedom, we could produce hard data.'"(Latour and Woolgar [1979] 1986: 256-257) The limited and fairly constant spectre of social science methods reduces the demand and opportunity for interdisciplinary collaboration.

Latour, Bruno, and Steve Woolgar, [1979] 1986. Laboratory Life: The Construction of Scientific Facts. Princeton: Princeton University Press.

Two Comments: Risking the Future
Bill Benzon
Apr 7, 2003 20:13 UT

I would like to approach the subject obliquely. 1)It seems to me that for the human sciences, broadly considered, control over most of the descriptive, analytic, and explanatory territory is asserted by several competing bodies of intellectual practice. Thus, linguistics, for example, is practiced by several schools, of which the Chomsky school (in its varieties) is only the most prominent, at least from the outside. There is no approach to syntactic analysis which all linguists share even to the troubled extent that biologists, for example, share a commitment of Darwinian evolution. Linguistics is in what Thomas Kuhn called a pre-paradigmatic state.

Thus, a psychologist, anthropologist, or literary critic seeking an interdisciplinary alliance with linguistics cannot expect to make cause with a consensus linguistics representing the views of more or less all linguists. Rather, she must seek an alliance with a partisan on one school or another and so must undertake to discover just which school is most compatible with her aims. The same, of course, holds for a linguist looking for a literary critic – which brand of critic do I choose?

Taking the long view, one might wonder whether or not linguistics will always be thus fractured. I see no change in the foreseeable future, but I would hope that, in the long run, linguists would arrive at some substantial consensus. But how would that come about? I do not know, but I can't help be thinking that compatibility with other disciplines will be a factor. In particular, I think that neural evidence will play a critical role. That is to say, matters internal to linguistics are going to be partially adjudicated through relations with other disciplines.

But I think that is true of the neurosciences as well, not to mention, anthropology, rhetoric, musicology, and so forth. It seems to me that we are seeing a whole-scale revision of the human sciences and that interstitial and bridging work between and among disciplines is part of this process.

2) In one way or another the institutional problem is conservativism: how do you encourage institutions to take more risks? The question I would pose here is whether or not the distinction between "deep" and "superficial" interdisciplinarity could be put to use. If one is going to commit scarce resources to a risky intellectual venture, it is better to risk those resources on "deep" rather than "superficial" interdisciplinary work.

But how do you make the distinction? We may all agree that some such distinction is useful, but, when it comes to actual cases, we might have very different judgments. Can this distinction be articulated in a way that provides some useful constraint on picking longshots?

Reply to Benzon
Dan Sperber
Apr 7, 2003 23:40 UT

1) Yes, one of the good reasons for lowering disciplinary boundaries is to make evidence from one field relevant to assessing hypothesis in another one. Sheer anathema to most social scientists.

2) “A psychologist, anthropologist, or literary critic seeking an interdisciplinary alliance with linguistics … must seek an alliance with a partisan on one school or another and so must undertake to discover just which school is most compatible with her aims.” Well, this can mean seeking an alliance with an approach that will cause as little revisions as possible in her views (this is indeed what generally happens: sociologists prefer linguists who argue that language must be approached as a social phenomenon from the start, and so on), or an alliance with an approach that will best contribute to the best overall understanding, even if at the price of serious revisions in her initial view (and this is very, very rare)

3) Incidentally, yes, linguistics is divided, but, at the same time, and with a dwindling number of exceptions, even starch opponents of Chomsky have been deeply influenced by the “Chomskyan revolution.” There is more agreement among schools than the rhetoric might lead one to assume.

4) The "superficial interdisciplinarity" Bill Benzon is talking about is, more than anything else, a way of getting hold of moneys earmarked for "deep" interdisciplinary work by authorities who have no reliable way of telling apart the two.

Comments on Human Sciences and Risk
Julie Klein
Apr 10, 2003 21:03 UT

I enjoyed Bill Benzon’s contribution enormously, because I am both a humanities professor working the faultlines of “human sciences” and a consultant to colleges and universities on how to promote and enhance interdisciplinary approaches in research and education.

The first activity puts me much in agreement with your speculation that “we are seeing a whole-scale revision of the human sciences and that interstitial and bridging work between and among disciplines is part of this process.” I glimpse that in all of my courses and research, whether the focus on music, visual culture, etc.

The second activity plunges me into the heart of institutional politics. Bureaucracies are inherently conserving organizations. I start the process of encouraging people to take more risks by throwing out their usual means of answering the question of what people in their institution are doing. The typical first place to look is the organizational chart. If you change the question – What are people actually DOING? – the answer changes dramatically in many places. As for “deep” and “superficial,” I would be more inclined to say “full” and “partial” and to ask what degrees of interaction, collaboration, and integration are most appropriate at any one site. I don’t argue that your distinction is not useful. It is, because it reminds us of the role of innovation and risk in the growth of knowledge. I’d like to put another distinction on the table, as well.

La philosophie, et autres intrus
Christophe Heintz
Apr 8, 2003 11:35 UT

Quel est le rôle qu’ont – ou devraient avoir- la philosophie des sciences, l’épistémologie et la sociologie des sciences dans les processus qui mènent à une recherche interdisciplinaire? Dans son texte, Dan Sperber souligne les difficultés liées aux institutions scientifiques et les difficultés de penser de manière authentiquement interdisciplinaire. Pourtant, quand Ira Noveck suggère que la philosophie pourrait avoir un rôle prépondérant pour l’interdisciplinarité, Dan répond qu’il ne tient pas à voir une autorité, même éclairée et bienveillante, définir les enjeux sur lesquelles les scientifiques devraient travailler. Je suis d’accord avec cette réaction : la philosophie n’est plus – si elle l’a jamais été- la reine des sciences, dictant les directions de recherches. De plus, ajouterais-je, les philosophes ne sont pas de par leur formation nécessairement aptes à traiter des questions d’interdisciplinarité. L’affaire Sokal en est une triste illustration (Le titre de l’article canular de Sokal est ‘Transgresssing the Boundaries …’!).

D’un autre coté, la **philosophie naturaliste** a renoncé au statut d’autorité suprême gouvernant les sciences. La philosophie naturaliste prétend se situer en **continuité** avec les sciences. Ainsi, les arguments philosophiques ne sont plus des prescriptions, mais juste des arguments à considérer de la même manière que les arguments proprement scientifiques. A vrai dire, il n’y a pas de frontière stricte et claire entre arguments philosophiques et arguments scientifiques.

Par ailleurs, les arguments pour le développement d’étude interdisciplinaires tendent à être plutôt de type philosophique. Ces arguments désignent des programmes de recherches qui **devraient**aboutir à des résultats empiriques. Mais ces résultats ne sont pas encore disponibles et l’argumentation est forcée de rester plutôt spéculative. (Exemple : les spéculations philosophiques de Turing sur ce que peuvent faire les ordinateurs et le programme interdisciplinaire - psychologie, informatique- de l’A.I.).

Mon questionnement, jusqu’ici, à porté sur la notion de ‘discipline intruse’ dans l’élaboration d’un programme de recherche interdisciplinaire. Finalement, c’est encore une question sur la nature et la justification des frontières entre les disciplines (ici, la philosophie et les sciences proprement dites).

Pour ouvrir, je voudrais noter qu’une grande partie des participants à ce colloque appartiennent aux Science Studies ou à une de ses branches. D’autres sont des administrateurs de la recherche. Chacun a plus ou moins l’espoir ou la prétention de pouvoir intervenir favorablement dans l’élaboration et l’implémentation de programmes interdisciplinaires. J’espère que nous pourrons, tout au long de ce colloque, voir plus précisément quelles sont les actions positives que peuvent mener ces disciplines ‘intruses’.

Vers un savoir postdisciplinaire?
Dan Sperber
Apr 9, 2003 23:41 UT

Le cas de la philosophie naturaliste qu’évoque Christophe est intéressant ici. En se voulant en continuité avec les sciences, il semblerait que la philosophie ainsi conçue renonce à son statut de discipline pleinement autonome. A certains égards ceci peut être vu comme un retour à une conception prédisciplinaire classique de la philosophie et des sciences illustrée aussi bien chez Aristote que chez Descartes. Plus intéressant ici est la possibilité de voir dans cette conception naturaliste un pas vers une organisation "postdisciplinaire" du savoir. Il ne peut s’agir de revenir de la spécialisation actuelle à une omnicompétence qui n’est plus possible depuis longtemps. En revanche, on peut imaginer que la formation et la spécialisation se fassent bien plus « à la carte » et que les scientifiques soient organisés non pas en disciplines autonomes, mais en en un réseau continu avec des zones plus lâches et d'autres plus denses évoluant assez rapidement. Dans une telle organisation, il n'y aurait plus, bien sûr, de "disciplines intruses"

web et interdisciplinarité
Jean-Michel Salaün
Apr 14, 2003 8:08 UT

Web et interdisciplinarité

Les exemples donnés par D. Sperber sont éclairants sur le fonctionnement actuel de l'interdisciplinarité, mais en restent, me semble-t-il, à une défense et illustration classique. N'y a-t-il pas une façon radicalement nouvelle d'analyser la montée du thème de l'interdisciplinarité avec le web d'aujourd'hui, et plus encore avec celui qu'on nous prépare pour demain ? L'accès quasi-immédiat à un nombre considérable de connaissances, souvent de très haute tenue pour qui sait un peu naviguer, transforme notre rapport au savoir en nous faisant sauter les étapes classiques de son assimilation. Cette relation inédite aux informations savantes nécessite de renouveler notre façon de construire nos connaissances et, à mon avis, devrait conduire à un enseignement général des bases d'une interdisciplinarité (dont il reste à préciser les contours..) pour qu'elle ne conduise pas à des raccourcis trompeurs, mais, au contraire, permette un enrichissement lucide de chacun.

D'accord
Dan Sperber
Apr 21, 2003 12:18 UT

Jean-Michel Salaün a raison. En effet, les exemples que je donnais visaient à éclairer le (dis-)fonctionnement actuel de l'interdisciplinarité. Cependant, mon propos n'était pas de déboucher sur « une défense et illustration classique » de l'interdisciplinarité. Je crois, comme Salaün, que le Web change la donne, et pour la recherche, et pour l'enseignement, au point que l'organisation disciplinaire des sciences pourrait bien, à moyen terme (30-50 ans ?) être remplacée par une organisation « postdiciplinaire », fondée sur un réseau continu et sans frontières avec des zones de densité plus grandes, zones elles-mêmes changeantes avec le mouvement des connaissances. Entre la description de l'état actuel, et les spéculations sur les futurs possibles, il faudrait aussi s'interroger sur la transition entre ce présent et ce futur, transition dont on peu penser qu'elle sera pour une bonne part chaotique plutôt que doucement progressive ou dramatiquement révolutionnaire. J'espère bien que nous aurons l'occasion de revenir sur tout cela, dans les mois à venir, au cours de ce séminaire.

What counts as good interdisciplinary work? An empirical view
Veronica Boix Mansilla
Apr 18, 2003 20:15 UT

Dan Sperber's committee experience resonates clearly with that of many: Journal peer reviewers, funding committees, and interdisciplinary researchers alike puzzle over what counts as high quality interdisciplinary work. Over the last two years, my colleague Howard Gardner and I, together with a team of researchers, have been studying the criteria by which experienced interdisciplinarians assess their work. Our interviewees were researchers in centers like the Santa Fe Institute, the MIT Media Lab, and the Bioethics Center at U Penn-- What we have discovered resonates with several of the claims made in the discussion so far and adds a few new criteria:

1. Proposed interdisciplinary research approaches (or the results obtained) are assessed against the background of what is known and "trusted" in the disciplines involved. Many would agree, this is in part a necessary yet a rather conservative "default" approach employed by multidisciplinary review committees.

2. Interdisciplinary work is also assessed vis a vis its "leverage" to provide insights that would have been unattainable through canonical disciplinary means. This echoes Rainer Kamber's and Julie Klein's targeted reference to the "problem solving capacity" of a piece of work. In our analysis this criterion applies to "Lebenswelt" problems (How can we create a just society in a globalized world?) as well as to scientific ones (How can computer modeling allow us to identify market behavior patters in Rainassance Florence?).

3. The most experienced subjects in our study also value work that stands in what we are coming to call "reflective equilibrium". In it, the relative presence of specific disciplinary views is weighted in light of the aims of the work; the methods proposed are selected against the background of a variety or fit contenders, and a fruitful level of tension among disciplinary views is delicately maintained.

4. Our interviewees seemed to value healthy skepticism-an awareness of the specific imitations of even their best integrative efforts.

5. Finally, because interdisciplinary work is communicated in the form of specific "genres of performances"-a research paper, a computer enhanced musical instrument, a new media art exhibit--each genre imposes particular standards to the work. Our subjects referred to this criterion with ease.

Interestingly, like Dan and many others in the discussion, most of our subjects highlighted the absence of clear criteria to assess interdisciplinary work as problematic and no individual subject provided us with a full picture of the criteria described above. It is my hope that, as we gain more clarity about how to carry out quality interdisciplinary work, we will find fewer reasons to be disappointed with the research and the educational practice that we see taking place in the name of "interdiscplinarity."

Let us hope you are right
Dan Sperber
Apr 21, 2003 13:45 UT

Veronica Boix Mansilla suggest that the sense of frustration felt by many people involved in interdisciplinary work – and which I expressed and illustrated in my presentation – might be excessive: in fact, interdisciplinary work may be progressing more smoothly, and with more reasonable criteria than we realize. Good news! Being highly aware that one's personal experience and point of view may be misleading in a variety of ways, I am quite willing to revise my views and accepts a more positive assessment when, later this year in this seminar, we read and discuss the paper by Veronica Boix Mansilla and Howard Gardner. In the meantime, I still wonder to what extent their findings reflect the experience of particularly successful interdisciplinary endeavours such as the Santa Fe Institute or the MIT Media Lab -- as opposed to the general situation (which of course would not make these findings any less interesting, but would affect their interpretation).

Interdisciplinarity in practice
Rich Gazan
Apr 22, 2003 1:56 UT

What counts as good interdisciplinary work? Veronica Boix Mansilla's comments and the other interesting discussions here have touched on many of the issues I've encountered in my dissertation research.

I'm looking at what could be viewed as a case study of interdisciplinarity in practice, in the creation of an oceanographic information system. This project has brought together physical oceanographers, ichthyologists, meteorologists, archivists, librarians, programmers, educators, and managers from several institutions to create a unique combination of content to serve researchers and the general public. It combined data sets on fish catch statistics and marine conditions that had been unavailable in digital form with mission logs of research vessels, oral histories of research scientists and archival photographs to provide an inclusive, multifaceted view of oceanography. Having researchers and professionals come together to help design this system and combining these disparate collections was supposed to create new knowledge, in the sort of "integrative synthesis" Julie Klein ("Interdisciplinarity: History, Theory and Practice" 1990, p. 118) says typifies true interdisciplinarity.

Though the grant proposal that funded this project and several of the constituent institutions have interdisciplinarity as a stated value, I certainly haven't assumed that this synthesis has taken place. This project is an example of a multidisciplinary environment, where researchers and professionals from different fields have been brought together to work on a common problem. But this doesn't automatically bring about meaningful integration.

What I'm trying to do is locate and identify evidence of interdisciplinarity in this project. I'm looking at project documents, the roles and interactions of the participants, the collections and the metadata used to describe them, and how these interactions of different disciplinary perspectives manifest themselves in the finished system. I'm asking questions like how did people share knowledge on this project, how did they negotiate meaning and find common frames of reference, how did they reconcile vastly different conceptions of what oceanography is?

What I've found so far underscores the importance of a translation role. This was typically taken up not by the content experts (the oceanographers and allied scientists), nor by the system experts (the programmers, designers and builders), but by the librarians and information scientists, people who commonly provide access to information without regard to the discipline that produced it.

In other projects I've worked on that have called themselves interdisciplinary, I can echo Dan Sperber's lament as to the lack of a reliable metric. Sometimes the presence of differently-degreed folk on the grant proposal is evidence enough. Since bibliometric data is readily

available and comfortingly quantitative, publications co-authored by members of different departments seem to satisfy some funding agencies. Interestingly, in this project a heuristic (albeit not a strong one) was embedded in the formal usability analysis of the system: if a non-scientist considered scientific data useful, or if a scientist found the less technical aspects of the site useful, that was considered evidence enough of the desired cross-fertilization of ideas. Surely we can articulate more clearly what we want out of interdisciplinarity.

interdisciplinary work, collaboration, or research?
Jochen Glaser
Apr 28, 2003 3:39 UT

Rich Gazan's contribution "Interdisciplinarity in practice" illustrates nicely why I am always uncomfortable with the word "interdisciplinarity". Creating this information system is obviously interdisciplinary work that is important for the progress of science. It is also an interdisciplinary collaboration. It may even produce new knowledge (though I am not sure about this). But I don't think it is interdisciplinary research.

In my opinion, the distinction between research and other types of activities is an important one, which is unfortunately too often obscured by the "ity" word. In the case of a project described by Rich one would expect the relations of the project to each of the collaborators prior and ongoing lines of research collaborators to be different from what usually occurs in collaborative research projects.

Comparing research with other types of interdisciplinary work could lead to a better understanding of all these activities. It would also show that a general "interdisciplinarity discourse" is of limited value because it tends to hide important differences.

The Geography of Thought
Bill Benzon
Apr 22, 2003 11:36 UT

The New York Times Book Review has recently reviewed a book reporting the kind of cross-cultural psychological results that Dan Sperber mentioned in his initial article -- I'd even hazard the guess that the book reports those same results. The book is *The Geography of Thought: How Asians and Westerners Think Differently . . . and Why*, by Richard E. Nisbett and the review is written by Sherry Ortner, who identifies herself as an anthropologist. Here are some critical passages from the review:

On the methodology: for an anthropologist like me, what counts as meaningful research is what is called "participant observation," joining as deeply as possible in local social and cultural worlds to try to figure out what is going on for those who live within those worlds. The idea that by taking individuals and putting them in rooms to do strange tasks one will learn something significant about their cultures seems to me quite dubious.

But there is more here than methodological difference between an experimental social psychologist and an ethnographic anthropologist. Even within Nisbett's "scientific" framework, his arguments are not convincing. It is common knowledge, for example, that the vast majority of subjects in psychology experiments are college students; in fact, they are the subjects of most of the studies discussed in this book. Yet college students are a very specific subset of any population, and one cannot help wondering about the generalizability of findings derived from testing such not-very-typical individuals.

There was also the question of interpreting the numbers. How much difference does there have to be between the Asians and the Westerners in a particular experiment to demonstrate a cultural divide? This question is never answered, even though some experiments seem to show relatively small differences. Moreover, in a few experiments in which the groups were broken down further by specific nationalities, the differences between Asians and Westerners became very fuzzy indeed. In one, 75 percent of Americans and Canadians gave "Western" answers, and only 20 percent of Koreans and Singaporeans agreed with them. The Japanese were close to the Koreans and Singaporeans at 30 percent. This would seem to lend credibility to the hypothesis -- except that the French, Italians and Germans also weighed in at 30 percent.

The second set of problems follows closely from this point. It concerns the question of framing the whole argument as a contrast between Asians and Westerners in the first place. The book is set up as a relentless attempt to cram everything into the Asian/Western dichotomy. The question of differences within the categories is occasionally acknowledged, but generally set aside.

Nisbett seems to think this is a minor issue. At the beginning of the book he "apologizes" to those readers who might be "upset" to see "billions of people labeled with the single term 'East Asian' and treated as if they are identical." But it is not a matter of being upset. It is a matter of wondering whether the differences within these absurdly large categories aren't at least as large and important as the differences between them. It is in fact a question about the scientific validity of the enterprise.

You may find the full review here:
http://www.nytimes.com/2003/04/20/books/review/20ORTNERT.html

> The first chapter of the book is online here:
> http://www.nytimes.com/2003/04/20/books/chapters/0420-1st-nisbe.html

Interdisciplinarity? if we need you, we'll call you
Dan Sperber
Apr 26, 2003 15:03 UT

> Bill is right; my “interdisciplinary disappointment” vignette was based on a conference given by Dick Nisbett at an anthropology meeting. I have endorsed Nisbett’s book, which is a major and novel attempt from the side of psychology to come to grips with the cognitive consequences of cultural diversity. Still, I have some serious disagreements with Nisbett regarding his general thesis and the interpretation of his evidence, and I share some of Sherry Ortner’s reservations, expressed in her NYT review quoted by Bill. However – and this is where issues of interdisciplinarity arise –, Ortner ends up dismissing the whole work, on scientific and even, at the end of her review, on political/moral grounds. So here is an attempt by a psychologist, based on years of hard team work, to start a conversation with anthropologists, and the anthropologist's answer is, in substance: you shouldn’t even have opened your mouth (and forget interdisciplinarity: if we need you, we'll call you).
>
> The alternative would have been to discuss Nisbett’s thesis, to offer a different interpretation of his data, to think about the kind of evidence, experimental and/or observational that would help decide among these interpretations, to accept that, in the process not just psychologists but also anthropologists might end up revising their views, and so on.

From epistemology to faith
Bill Benzon
Apr 26, 2003 15:38 UT

> I agree with your take on Ortner's review, Dan, though I must admit that I've not read Nisbett's book. The defensive and dismissive nature of the review is pretty clear. The alternative approach you indicate clearly is not in the cards for Ortner.
>
> The unfortunate effect of this dismissal would seem to be that cultural anthopology becomes the study of abstract and disembodied culture, visible in behavior and artifacts, but not in brains or minds. It would almost seem as if cultural relativism has been transformed from an epistemological starting point into a profession of faith. Within this faith one may describe this or that culture, but thou shall not compare cultures one to the other in any way.
>
> And, yes, this is the kind of conceptual blockage that stands in the way of intellectual progress.

To try hard is not enough
Grit Laudel
Apr 28, 2003 2:58 UT

Dan wrote about his disappointment that the attempt of a psychologist to start a conversation with anthropologists was completely rejected by the latter. Judging from the passages of the review of Sherry Ortner, cited by Bill Benzon, it seems to me that Ortner's dismissal is justified. In Ortner's opinion the work of the psychologist Richard E. Nisbett has very serious shortcomings: over-generalization of the findings from experiments with college students, too global theoretical concepts ("The Asian", "The Western") etc. Why on earth should anthropologists start a communication with this psychologist whose work they perceive as being of low quality and who is not even meeting the standards of his own discipline? What I intend to point out is that the abilities of the collaborator are a crucial precondition for any research collaboration and especially for interdisciplinary research collaboration. The scientists I observed were never concerned about interdisciplinary collaboration as such, but were always very careful about selecting partners whom they perceived as highly skilled.

Reply to Grit Laudel
Dan Sperber
Apr 28, 2003 11:04 UT

Grit writes: "Judging from the passages of the review of Sherry Ortner, cited by Bill Benzon, it seems to me that Ortner's dismissal is justified." This amount to saying that if her premises are right, then her conclusion is right. But are her premises right? Why should an anthropologist, not particularly competent in psychology, decide whether a given psychologist meets "the standard of his own discipline"? Would an anthropologist accept to be so judged by a psychologist? As a matter of common knowledge among psychologists, the scientific credentials of Dick Nisbett in his own field are impeccable. Ortner is not dismissing one psychologist because he is not good enough for her, she is dismissing psychology as a whole because it is not good enough for mainstream anthropologists.

A nice interdisciplinary disagreement
Jochen Glaser
Apr 29, 2003 1:36 UT

Finally, we have a concrete case of interdisciplinary disagreement, which is much more fun than the abstract discussion. In the discussion about the review of Nisbet's book by Ortner we can find many arguments common to an interdisciplinary argument. Three possibilities occur immediately:

- The outsiders might have prejudices against the other field,

- The outsiders might impose standards and cultural perspectives of their own field on the other field,

- The outsiders see a piece of work in the other field not meeting the standards of this field.

The most likely case is of course that we encounter a mix of two or three of the possibilities. I have become really curious now and would like to ask Dan a question: Sherry Ortner and Grit Laudel voiced two very concrete criticisms:

1) The author generalized from College students to 'Westerners' and 'Asians'.

2) The author ignored counter-evidence in his results (Europeans acting like Asians as opposed to US-Americans) and, while occasionally admitting the problematic character of his categorisations, disregarded this problematic character when making his overall argument.

Dan, is this already a prejudiced perception of Ortner or are these points valid? And if they are valid, do both practices meet the standards of the field of experimental psychology? This is very

important to me because, given that colleagues' attention is the scarce resource nowadays, reviews play an important role for the audience. I would never read a book if a colleague I trust writes or tells me: "Well this book sounds interesting, but it rests on an over-generalisation, and some counter-evidence has been neglected." But I would still be curious about it if the colleague damns it from a general perspective (social scientists don't share perspectives anyway). So the main point is not if Ortner has prejudices (that is very obvious), but if her concrete objections are correct.

The psychological subject
Bill Benzon
Apr 29, 2003 16:21 UT

Let us consider one of Ortner's objections as indicated by Grit Laudel and Jochen Glaser. Here is what she says:
It is common knowledge, for example, that the vast majority of subjects in psychology experiments are college students; in fact, they are the subjects of most of the studies discussed in this book. Yet college students are a very specific subset of any population, and one cannot help wondering about the generalizability of findings derived from testing such not-very-typical individuals.

This has the form of an objection to *all* psychological experimention using college students as subjects, not just to the studies reported in this book. One must thus wonder whether or not Ortner believes that psychological experimention has taken place for decades without this issue being seriously considered. Unless she has *specific* reasons for believing that college students are likely to perform differently on Nisbett's experimental tasks from other adult subjects, this sounds more like a blanket and pro forma objection to psychological experimentation in general than a well-considered objection to this particular research program.

Even granting the objection, we still have a comparisons between European, North American, and East Asian *college students*. If there are significant differences between those groups -- and it seems there are -- then those differences must be accounted for. But Ortner ignores this.

Unless one considers her previous paragraph, where she asserts: "The idea that by taking individuals and putting them in rooms to do strange tasks one will learn something significant about their cultures seems to me quite dubious." Again, this sounds like a blanket and pro forma objection. One might well turn the tables and ask: "What tasks do you propose that we use?"

The burden of proof
Jochen Glaser
Apr 30, 2003 2:21 UT

I would like to put aside Ortner's prejudices as an established fact and focus on the interdisciplinary disagreement, because it is interesting beyond the prejudices. As I read her review, Ortner did not object to using college students as subjects, but to the subsequent generalization. Ortners objection is void if the book is about styles of thinking of Asian and Western college students. But the book isn't just about that, is it?

But even if the generalization has been made, it is not necessarily invalid. The interesting point for our interdisciplinarity discussion is Bill's following statement:

"Unless she [Ortner] has specific reasons for believing that college students are likely to perform differently on Nisbett's experimental tasks from other adult subjects, this sounds more like a blanket and pro forma objection to psychological experimentation in general than a well-considered objection to this particular research program."

Well, this is a type of objection that is very difficult to raise for somebody outside the discipline. But if using college students as subjects is a general practice in experimental psychology, I am sure the generalizability of results has been extensively investigated. One could then (and would have to!) refer to previous investigations about generalizability from college students to

justify the current one. If Nisbet has done this, he has justified his generalization, and not discussing his justification was a very bad thing to do by Ortner. If he has not done it, Ortner's objection is still valid.

Could it be that we have one of the characteristic culture clashes between disciplines here? For psychology using college students as subjects appears to be a unproblematic thing to do, either because all believe it is ok for some reasons that are obvious to psychologists but to nobody else, or because it has been extensively justified in the past. Outsiders looking from social science disciplines from a 'qualitative' perspective (emphasizing the differences between individuals and situations) or a 'quantitative' perspective (being very concerned about sampling) spontaneously see a problem in generalizing from college students to larger samples but naturally cannot argue the point in the framework of psychology. That means that the burden of proof is with the discipline that has presented the results.

Reply to Jochen Glaser
Dan Sperber
Apr 30, 2003 10:27 UT

To answer Jochen's initial questions:

Nisbett and his colleagues found, in a series of original experiments, systematic differences in perception, interpretation and reasoning among participants, who were all students at American universities. Moreover, these differences clustered into two cognitive styles, one more "holistic," the other more "analytic." These differences strongly correlated with the cultural background of the students, students from East Asia being more holistic in their performance, American students of European origin more analytic and European students somewhere in between, more on the American side. The fact that the population tested was homogeneous apart from cultural origin, far from being a defect in design, gives strong evidence that the cause of the difference has to do with the cultural background. The fact that Europeans are somewhat less "analytic" than the Americans is interesting, but in what sense is it counter-evidence to Nisbett's thesis?

Nisbett's work definitely meets "the standards of the field of experimental psychology," and these standards are more explicit, demanding, argued for, and generally accepted than any standard used in anthropology. The idea that East Asian (Chinese, Japanese, and Korea) on the one hand, and Europeans and American of European origin on the other hand share a lot in terms of culture is one anthropologists have no difficulty with, when it is expressed by one of them, or by an historian of ideas such as Geoffrey Lloyd. Similarly, Nisbett's general thesis, that people of different culture (or of different cultural zones), have different modes of thought is, if anything, commonplace in anthropology. Is it that it is so commonplace that anthropologists are now only interested in much subtler differences among local cultures? To some extent yes, but anthropologists don't shun, and even sometime produce the kind of generalization Nisbett is offering, based, it is true, on their ethnographic knowledge, rather than on experimental evidence. Well, is it that ethnographic knowledge is so secure and experimental evidence so flimsy that the latter is not worth any attention? Isn't the convergence of this evidence of some interest? Aren't any of Nisbett's experiments worth adapting and testing in fieldwork conditions? You really have to be a wholly parochial anthropologist to believe this.

As I said, I have serious disagreements with Nisbett. I believe that his experiments show not two discontinuous modes of thought, but two cognitive styles that are available across culture, with cultural preferences that can easily be overturned in specific situation. This disagreement could be submitted to further experimental research.

Regarding the issue of using college students as experimental subjects, as Bill and Grit mention in their last postings, these are standard practices the rationale for which has been extensively discussed. Still, I believe that there are important limits to using such subjects and that, in many areas, including the study of cultural aspect of cognition, it is crucial to use more diversified populations (this has been nicely demonstrated in work Atran and Medin, which, by testing a variety of types of subjects, reversed previous conclusions in the area of category-based

inference). I am sure Nisbett would agree that his work needs to be extended, in particular by using other types of subjects. Since his findings go in the same general direction as that suggested by work in anthropology and history of ideas, there is no a priori reason that I can think of to expect that doing so might reverse or cancel his finding, although it would certainly lead to a more complex picture. This however suggests, if anything, that bringing ethnographic and experimental evidence to bear on issues of cultural cognition is the way to go. Nisbett is among the few scholars who, from different theoretical perspectives, are paving the way for this.

Inter/post-disciplinary strategy versus disciplinary-strategy; and 'vicious circle' in learning
Maria Rossi
Apr 22, 2003 14:17 UT

As many other graduate students of interdisciplinary-research centers, I welcome Dan Sperber's initiative and contribution. Being involved in several interdisciplinary programs since the beginning of my doctoral research, I can recognize in Dan's article a particularly lucid and helpful analysis of the paradoxes of interdisciplinary practices.

Among the set of grounding epistemological arguments for interdisciplinary research, one of the most general is related to critical thinking. It is well illustrated by Dan's criticism of the field he was originally trained in. We can flesh out this point by contrasting a disciplinary-strategy with an interdisciplinary-strategy (any particular research could incorporate both, but to a different extent). Each can be specified, at least, along these 8 variables: (1) researcher type; (2) methodological type; (3) ontological type; (4) historical type; (5) conceptual type; (6) epistemic-flow type; (7) collaborative type; and (8) institutional type.

A disciplinary-strategy in field F would frequently be based on : (1) highly specialized experts, with a high social power over the F-world ; (2) traditional or routine-based methods of F ; (3) possibly non-realist or relativist (incommensurability of disciplinary fields) ; (4) the long-standing history of F ; (5) a low critical activity on F conceptual grounds ; (6) a lot of highly internalized epistemic flows, structured according to F-rules ; (7) not necessary collaborative activities ; (8) institutionalized background. For instance, according to a disciplinary-strategy, research has to conducted under the supervision of one leading Disciplinary Expert (henceforth DE). DE has the mastery of F-methods. DE has the current benefit of both social power and scientific recognition due to his career in F. He also has developed long-lasting routines, or 'script-based' schemes of thought, in order for him to publish a lot in F-Journals, and attain tenure.

An interdisciplinary-strategy would frequently be based on: (1) experts having knowledge of more than one disciplinary field ; (2) methodological pragmatism and cumulative use of methods ; (3) realism (several methods can study the same real/natural phenomenon) and commensurability of knowledge fields; (4) few historical background, but project of building new histories ; (5) critical and foundational conceptual type ; (6) highly externalized epistemic flows ; (7) inherently collaborative networks ; (8) weakly institutionalized, but with the hope of building new interdisciplinary institutions. For instance, according to the critical interdisciplinary-strategy, there is a primacy of the phenomenon being studied over the historical contingencies/boundaries of human institutions. If several disciplinary fields can cooperate for studying a given phenomenon P from different levels of generality, there is no reason to decide a priori to restrict the number of methodologies available for studying P. On the contrary, it seems to be reasonable to constrain any particular methodology/result/analysis by most of the available critical tools.

Dan is worrying about how to increase the role of a post/inter-disciplinary strategy in general and in the doctoral formation in particular – in order to defeat the 'vicious circle' he referred to. This is a valuable goal, in reason, among others arguments, of the critical power of the interdisciplinary-strategy.

Playing the monodisciplinary devil's advocate
Dan Sperber
Apr 26, 2003 17:12 UT

I agree with Nicolas that a major advantage of an interdisciplinary approach is that it greatly favors critical thinking. However -- this is a comment, not an objection --, what drives or should drive an interdisciplinary research project, just as a monodisciplinary project, is some interesting, plausible, non-trivial, hypothesis. It is or should be, then, one's hypothesis that determines one's strategy, and in particular its mono- or inter-disciplinary character.

Yes, but what of the process of hypothesis formation itself? This process is not well-understood. There is no methodology for generating worthwhile hypotheses. If you are a realist, as most scientist are, then there are at a given time specific domains and areas, not just "constructed" by scientists, but with some basis in reality, where worthwhile hypothesis are more likely to be developed. What if someone were to say that the disciplines, on the whole, are better placed and better geared to generate such hypotheses and to foster discovery, not because "the system" favors them, but because they are themselves the outcome of an ongoing historical process of relative optimization of inquiry and of progressive adjustment to the way the world happens to be? That, yes, there might be occasional nuggets to be found in between the disciplines, but the real ores are in their middle? To this one might answer that, even if it were true, some "affirmative action" in favor of interdisciplinarity might still be productive. But how much? Grounds are here a bit shaky. I find it much easier to argue for specific interdisciplinary programs than for interdisciplinarity in general.

There might be another way to look at the issue. In my paper, and, as a result, in the discussion, the focus has been on research. But we want research to contribute to an understanding of the world that, even though it obviously remains very fragmentary, should be as coherent and integrated as possible. There is, therefore, good ground to object to a simple reproduction of the disciplinary organization of the sciences in teaching (and in the diffusion of scientific knowledge). A more interdisciplinary approach to university studies might respond to our general intellectual interest, and indeed foster more critical thinking. My guess is that this would help rather than hinder the minority of students who end up doing scientific research. Such a view, I know, is not original. It has been implemented in a number of institutions across the world, and I look forward to seeing it presented and discussed by people more competent than I am, later in this seminar.

Research goes where the problems are
Jochen Glaser
Apr 28, 2003 5:57 UT

I was surprised by the two strategies that were described by Nicolas. I would love to see the empirical data from which the two strategies were derived. In my own empirical investigations, I have never found strategies or research processes that would fit the dichotomy between a disciplinary and an interdisciplinary strategy.

Furthermore, I completely agree with Dan's statement about hypothesis formation. Don't let us forget that fields represent cases of successful sustainable knowledge production. By 'sustainable' I mean the fields' ability to generate new research problems out of solved ones. This sustainability provides the basis of researchers' "research trails" (a concept proposed by Chubin and Conolly 1982). While a combination of knowledge from different fields can lead to interesting research problems, the sustainability of this combination has yet to be proven. Interdisciplinary programs often initiate new combinations of knowledge and thus create situations in which sustainability may emerge. If it actually emerges, we will get a new field.

With regard to teaching, the main tradeoff appears to be the one between depth and breadth: Disciplinary teaching is important because students have to acquire much knowledge and many skills in a relatively short time. You can't be interdisciplinary without a lot of disciplinary knowledge. While I agree that a more synthetic view of the world and the sciences would be good, there are limits to what a student can take in, and there are priorities. Chubin, Daryl E.,

and Terence Connolly, 1982. Research Trails and Science Policies. Norbert Elias, Herminio Martins and Richard Whitley (eds.), Scientific Establishments and Hierarchies. Dordrecht: Reidel, 293-311.

La science à la chaîne ?
Vanessa Nurock
Apr 28, 2003 13:23 UT

L'une des questions soulevées de manière commune par le texte de Dan Sperber et la réponse de Nicolas Bullot est la possibilité d'impliquer réellement les doctorants dans des démarches interdisciplinaires collaboratives. Il y a bien des façons de conduire un projet collaboratif interdisciplinaire. L'une d'entre elles repose sur ce que l'on pourrait appeler un 'travail à la chaîne'. De quoi s'agit-il ? Par exemple, de manière très schématique, lors d'une collaboration entre philosophes et psychologues : (i) le philosophe est chargé de 'fournir' la théorie d'ensemble et de cerner le(s) problème(s), (ii) le psychologue est chargé de ramener ce problème à une hypothèse expérimentale et de réaliser un design expérimental, de passer les expériences, et (iii) de coder et d'analyser les données obtenues afin d'en tirer des conclusions et de les confronter aux hypothèses de départ puis (iv) le philosophe discute avec le psychologue des données obtenues et en tirera les implications théoriques. Evidemment, un tel fonctionnement se justifie parce qu'il permet à chacun de travailler au mieux de ses compétences. Cependant, ce genre de fonctionnement n'est pertinent que si le partage des tâches intègre un véritable dialogue, tant au niveau théorique qu'expérimental. On ne peut pas nier par exemple que l'interprétation des données suppose également la compréhension de la méthode utilisée pour les obtenir ; inversement, l'interprétation théorique des résultats doit respecter la teneur des données et ne pas les distordre pour leur faire dire ce que l'on veut. Le 'travail scientifique à la chaîne' n'est satisfaisant que si chacun met à la fois le mieux en oeuvre ses compétences particulières au niveau où il intervient, et s'il comprend suffisamment ce que les autres font. Cela suppose également que chacun des ouvriers soit suffisamment spécialisé pour mener à bien sa tâche en étant plus qu'un bon 'touche à tout', ce qui pose le problème des formations interdisciplinaires très précoces. Pour cette raison, il apparaît nécessaire de former les futurs chercheurs en les habituant d'emblée à 'ouvrir le capot', ce qui suppose notamment de former les étudiants en leur permettant, à partir de leur formation disciplinaire, d'aller chercher ou de constituer des données expérimentales et des analyses théoriques en rapport avec leur objet. A ce sujet, voici une proposition concrète : dans la plupart des pays et des disciplines, il est prévu que les étudiants fassent des 'stages en laboratoire' durant plusieurs années. Pourquoi ne pas structurer précocement -dès la 3e année d'étude- cette pratique de stages sur le moyen terme (2 ou 3 ans) autour d'un projet de recherche choisit par l'étudiant, en permettant à l'étudiant de faire ses stages dans des laboratoires centrés sur différentes disciplines, mais travaillant sur une même thématique en rapport avec ce projet ? Ceci ne suppose pas une révolution dans les esprit, ni l'existence de financements autonomes, et ne met pas en jeu la reconnaissance 'disciplinaire' de l'étudiant. Ceci permettrait en outre aux étudiants d'acquérir une bonne formation disciplinaire tout en s'ouvrant réellement à la démarche interdisciplinaire.

Scope and use of this distinction, challenging the deflationist view about interdisciplinarity
Maria Rossi
Apr 30, 2003 3:39 UT

(a) The distinction between the two strategies was intended to draw a rough (and idealized) schema in the available logical space in our thinking about interdisciplinary research. This was neither a normative thesis nor an empirical claim, but a conceptual tool that remains to be refined. Even though it remains crude, the distinction may capture some of the background intuitions that continuously drive, in these discussions, the interpretation/understanding of the contrast between disciplinary and interdisciplinary activities. The variables I was referring to were not actual experimental variable (reply to Jochen Glaser), but dimensions in a conceptual space that can be needed for conceptual specification. The variety of these variables/dimensions show at least that the debate about 'interdisciplinarity' can be at stake at many level of analysis, which may be an indication of its interest (instead of its vacuity). This schema can have two very different types of use, either descriptive or normative -- cf. Roberto

Casati's three claims. The primary point I was willing to stress was only related to critical thinking, which seems to fit more naturally to the interdisciplinary strategy or phase.

(b) Thus, I agree with Dan and Jochen about hypothesis formation and sustainable knowledge production. Once again, any particular research could (maybe, have to) incorporate (parts of) the two strategies. Moreover, these two strategies – or, more likely, subsets of them – may be generally instantiated as distinct phases of a particular research.

(c) Astonishingly enough, there seems to be an emerging agreement on the 'Interdisciplines' website for dismissing the *general* discourse about interdisciplinary research (e.g., Casati 28 Apr.; Glaser 7 Apr; Sperber 6 Apr, 7 Apr.). We could perhaps call this view the 'deflationist epistemology' (either descriptive or normative) of interdisciplinarity. Why not try to challenge this deflationist assumption? Successful interdisciplinary research may tend to satisfy a set of general *epistemic constraints* that could be (or have been) analyzed in an 'epistemology of interdisciplinarity'. Moreover, how could we '(re-)think interdisciplinarity' if do not assume as being required some (weak or strong) epistemological generalizations about interdisciplinarity per se ?

History of disciplines and disciplines in history
Noga Arikha
Apr 22, 2003 21:52 UT

It is perhaps significant that the discussion so far has focused primarily on intra-scientific instances of interdisciplinarity, as well as on the theoretical issues involved in the awareness that previously fixed boundaries are becoming gates - some easier to open than others. It seems to me, however, that the institutional difficulties encountered by scholars with interdisciplinary tendencies, which Dan recounts in his piece, could also themselves be described from the point of view of a 'general audience' whose familiarity with the culture of specialisms is not a given. When specialists enter the public arena, they often have to soften their expertise, adjust their style; and at times, they can be reviled by their colleagues for doing so.

At the same time, journals such as the *New York Review of Books* have been practising an interdisciplinarity of sorts for years, not because it covers a large variety of topics (this, indeed, amounts to multidisciplinarity) but precisely because it presents sometimes ground-breaking essays in the guise of research summaries that can be of 'internal' intellectual use by readers who might ordinarily never read anything specialized on the same topic. Divulgation need not be vulgarization; it can reveal problems previously hidden within the specificity called for by the practice of a discipline. Style can do a lot for broadening horizons; rhetoric, by definition, has an impact on information processing. Outside the properly academic world,'interdisciplinarity' might in this way connote not the alliance of, say, anthropology and psychology, so much as a broader movement, a 'humanism' if you will, from whose vantage point any specialism denatures the problem under scrutiny.

For this reason, it is perhaps quite significant that the continued rhetoric one hears both in academic institutions and in public debates concerning the need to bridge the 'Two Cultures', as they were pinpointed by C. P. Snow some fifty years ago now, has not yet been mentioned within the discussions here. In my own work, I have tried to confront head-on the relative dearth of scholarship which marries the insights internal to the *practise* of the philosophy of mind with the history of science which informs empirical research on the mind. That is just one instance, however, of a gap between a humanistic discipline and a scientific one. Given that the history of each discipline can to some extent breed the problems that then become those *of* that discipline, and which can in turn give rise to a need for interdisciplinarity, there might be a profound need to inject history into the practise of disciplines that do not otherwise partake of what is generally lumped under the 'humanities'.

It is perhaps important to remind ourselves, again and again - as some participants here have done already - that the age of specialisms is new. Early modern natural philosophers were more often than not dilettantes in their experiments and humanists by education. It is unlikely that a new Leibniz should emerge today. But it is possible that, if he were alive now, he would still try to open gates.

The flow of scientific information
Dan Sperber
Apr 28, 2003 10:46 UT

Noga makes some excellent points. The flow of scientific information takes place not only within the community of researchers, and, through applied research, towards technicians, engineers, doctors, and so forth. It is also directed at students, school children and the general public. The standard view is that there is no relevant feedback from these wider audiences. Relevant feedback, however, comes in many forms. Direct input from non-scientists in the scientific inquiry process is rare (with some interesting exceptions, particularly in the social sciences). But, for instance, some disciplinary barriers are effectively attacked through an initial success with the wider public. This has been the case for instance with Richard Dawkins' "memetics". This is an attempt by a biologist to redefine issues in the study of culture. It has been extremely successful in the wider public and on the web, and through this success it has forced itself on the attention of social scientists, being mostly criticized in mainstream anthropology and sociology, but gaining some level of acceptance in evolutionary approaches to culture and in some areas of applied social science. More generally, through their influence on students and public and private source of funding, scientific ideas successfully addressed at the wider public contribute to the definition of the issues on which specialists end up working. These are only some examples, added to those given by Noga, of the ways in which the standard view of the flow scientific information can be challenged, sociologically, historically, and, I would suggest, also from a normative point of view.

If interdisciplinarity is the answer, what is the question?
Hugo Alrøe
Apr 24, 2003 15:15 UT

First of all, the question is: what are disciplines and is "discipline" a unitary concept in this discussion? I think not. There are different kinds of differences between disciplines (though not as distinct as I will present them here) and interdisciplinarity across these different borders is not at all the same thing. One important distinction is between "ontologically determined disciplines" and "self-organizing disciplines". The first is based on differences in the subject area of science. Physics, biology and psychology can exemplify three major ontological levels. This kind of differentiation is determined by the emergent properties (or whatever one prefers to call it) that are characteristic of new levels. The second, self-organizing disciplines, result from the continuous differentiation into different, relatively independent disciplines that is so characteristic of modern science. It is driven by various forces such as competition for new funds, status and recognition and the need for effective communicative communities, which lead to the generation of new organizational structures that provide those things. With respect to ontologically determined disciplines, interdisciplinarity involves the avoidance of reductionism while still recognizing the powers of reduction. For example, using physics and biology in a study of human behaviour in a way that acknowledges, that such a study involves aspects that are beyond the scope of physics and biology. With respect to self-organizing disciplines, interdisciplinarity is a question of forced cooperation of relevant disciplines in order to provide satisfying answers to real problems. This may spur new "cross-disciplinary" disciplines in the ongoing dynamics between self-organizing differentiation and forced cooperation. Apart from habits and organizational structures, there are no 'real' borders that prevent interdisciplinary action between such disciplines. If interdisciplinarity is the answer, this answer will also depend on what the problem is taken to be and, hence, on what the purpose of science is taken to be. Is science to satisfy our curiosity and gather knowledge for the sake of knowledge, is it to boost economical and technological development of nations or companies, is it to help and assist those in need, or is it to fulfil aspirations for the future of our civilisation? I suspect science has all of those purposes, in varying degrees. But the answer, interdisciplinarity, will be quite different dependent on what the purpose of science is taken to be.

disciplines and beyond
Dan Sperber
Apr 30, 2003 12:28 UT

Hugo Alrøe's distinction between "ontologically determined disciplines" and "self-organizing disciplines" is an interesting one. I wonder however whether "self-organizing disciplines" (which, incidentally, must have some ontological niche) need still be "disciplines" in the ordinary sense,

i.e. fairly large, permanent, richly institutionalized structures, or whether we have a continuum between these and smaller, more provisional and evolving research programs. If these self-organizing "disciplines"/programs are going to play an ever greater role, we may be moving toward a post-disciplinary stage in the organization of the sciences, which would not mean that the large ontologically determined disciplines would altogether vanish, but that their role would be greatly diminished in favor of structures more readily adaptable to the advancement of science and to the variety of need Hugo is talking about.

Matter-of-factness and normativity
Roberto Casati
Apr 28, 2003 12:25 UT

After having read most of the comments to Sperber's opening contribution, I wonder whether we are working with two quite different projects in mind. The first project is that of adequately understanding the role of interdisciplinarity in intellectual life at large, and in scientific work in particular. The second project is that of evaluating the good and bad sides of interdisciplinaritiy. And indeed, the title of the discussion ("Rethinking...") may suggest an ambiguity between normativity and matter-of-factness.

I have three claims here.

First, although the two projects are interrelated, they are distinct. The factual project takes for granted disciplines and interdisciplinaritiy and tries to explain them, but it need not encourage any particular approach, suggest any policies, give any advice to decision-makers or students. The normative project, is a tad more ambitious. It looks as if the normative project is what most contributors have in mind.

Second, we should pursue the two projects in relative independence, short of being distracted by the normative project in trying to understand the facts.

Third, and more dialectically: the normative project, although very exciting, is quite too ambitious. Who knows where science is going, and how it will get there? Openness to interdisciplinarity may be the only sensible (if unexciting) recommendation we could ever be able to issue; anything more than that could turn out to be useless (if interesting) instances of wishful thinking.

Interdisciplinarity wishful per se?
Grit Laudel
Apr 30, 2003 7:42 UT

Roberto's observation that the "Why Rethink Interdisciplinarity?" discussion has two projects in mind is interesting. I agree with him, and I was wondering about the dangers of the normative project he sketched. One danger could be that 'interdisciplinarity' is regarded as wishful per se in today's knowledge production. To have an interdisciplinary approach in a research project is good if it helps to solve a problem better than with a disciplinary approach. Therefore a general criterion for project proposals to be interdisciplinary doesn't make much sense. It is also true that researchers do not always look for help from other disciplines. On a very general level, a demand for 'openness' to other disciplines is surely important. In the discussions, an interdisciplinary education was often demanded. Jochen Glaser pointed at the limits for interdisciplinary teaching. I'd like to add an empirical observation. The PhD students I interviewed all got a disciplinary education. Sometimes they had to face a big change, for example, from conducting a biological project in their master thesis to conducting a physics project in their PhD thesis. The general pattern was that the students got a solid disciplinary education but learned during their PhD career phase special concepts and methods from other fields. They did this because it was necessary for solving the research problem. Job advertisements for these projects, seeking PhD students often had this form "We are looking for a biochemist or a biophysicist or a molecular biologist …". Thus, it was institutionalised for PhD students to have a second learning phase in another discipline during their research.

Merci! Thank you!
Dan Sperber
Apr 30, 2003 15:17 UT

Je voudrais remercier tous les participants à cette discussion. J'ai beaucoup appris de leurs critiques et de leurs commentaires. Je me réjouis à l'idée de participer, à partir de demain, à la discussion des conférences à venir dans ce séminaire.

I would like to thank all the participants in this discussion. I have learnt a great deal from their criticisms and comments. I look forward to participating, starting tomorrow, in the discussion of the forthcoming papers in this seminar.

The Potential of Transdisciplinarity

Helga Nowotny (ETH Zurich)

'Date of publication: 1 May 2003)

Abstract: Transdisciplinarity has a semantic appeal which differs from what one often calls inter- or multi- , or pluri- disciplinarity. And, note that the prefix - trans- is shared with another word, namely transgressiveness. If it is true that knowledge is transgressive, then it means transdisciplinarity does not respect disciplinary boundaries. It goes beyond the disciplinary boundaries, but it does not respect institutional boundaries, either. In addition, there is a kind of similarity, a kind of convergence or co-evolution, between what is happening in the sphere of knowledge production and what we can see going on in the way that societal institutions are developing.

Introduction

Transdisciplinarity is a theme which resurfaces time and again. It responds to an underlying need and an inherent belief. The former is the loss to what is felt to have been a former unity of knowledge. The latter is the expectation that transdisciplinarity contributes to a joint problem solving that it is more than juxtaposition; more than laying one discipline along side another. As Francois Taddai puts it: “No discipline knows more than all disciplines”. If joint problem solving is the aim, then the means must provide for an integration of perspectives in the identification, formulation and resolution of what has to become a shared problem.

One virtue you need when working in transdisciplinary research: patience. You must be very patient indeed. The evidence clearly shows that developing transdisciplinary teaching takes time and commitment from both academics and institutions. To understand the language of other disciplines takes time. But you also need to understand where the pressure for joint problem solving comes from. Recent discussion has revealed that science and society are still being treated as unproblematic categories. In the two books which form the background for this presentation [The New Production of Knowledge. The Dynamics of Science and Research in Contemporary Societies (with M. Gibbons, C. Limoges, S. Schwartzman, P. Scott, M. Trow) (1994). London: Sage, and Re-Thinking Science. Knowledge and the Public in an Age of Uncertainty (with Peter Scott und Michael Gibbons) (2001). Cambridge: Polity Press], we show that, in fact, they have both become problematic categories. Here I want to assert that knowledge, as well as expertise, is inherently transgressive. Nobody has anywhere succeeded for very long in containing knowledge. Knowledge seeps through institutions and structures like water through the pores of a membrane. Knowledge seeps in both directions, from science to society as well as from society to science. It seeps through institutions and from academia to and from the outside world. Transdisciplinarity is therefore about transgressing boundaries. Institutions still exist and have a function. Disciplines still exist and new ones arise continuously from interdisciplinary work. Therefore: beware!

Some characteristics of Mode-2 knowledge production

In previous research we put forward the proposition that a new form of knowledge production has emerged. We called it Mode-2. We introduced the idea of Mode-2 in order to bring in a new way of thinking about science, which is often described in strictly disciplinary terms. We need another language to describe what is happening in research. We identified some attributes of the new mode of

knowledge production, which we think are empirically evident, and argued that, all together, they are integral or coherent enough to constitute something of a new form of production of knowledge.

The first attribute of Mode-2 is the fact that contemporary research is increasingly carried out in the context of application, that is, problems are formulated from the very beginning within a dialogue among a large number of different actors and their perspectives. The context is set by a process of communication between various stakeholders. That requires great patience. But the problem is not formulated outside of that group and until that group comes to an agreement about what the problem is and how it will be carried out, no resources flow and no research activity can begin.

The second attribute relates to the fact that multiple actors bring an essential heterogeneity of skills and expertise to the problem solving process. Also, in Mode-2 kind we see the emergence of loose organisational structures, flat hierarchies, and open-ended chains of command. Universities are precisely the opposite type of such organisations. For the most part they are still highly hierarchical, fixed towards disciplinary structures. We find in Mode-2 almost the reverse of that.

The third attribute of Mode-2 is transdisciplinarity. If we had intended to use the term multi-disciplinarity or pluri- disciplinarity, we would have done so. Rather, we have chosen transdisciplinarity for a reason. What we were trying to convey by the notion of transdisicplinarity is that, in Mode-2, a forum or platform is generated and it provides a distinctive focus for intellectual endeavour, and it may be quite different from the traditional disciplinary structure. In a Mode-1 system, the focus of intellectual endeavour, the source of the intellectually challenging problems, arises largely within disciplines. This may still go on, but other frameworks of intellectual activity are emerging which may not always be reducible to elements of the disciplinary structure. Rather, it is in the context of application that new lines of intellectual endeavour emerge and develop, so that one set of conversations and instrumentation in the context of application leads to another, and another, again and again.

In many discussions on transdisciplinarity , priority is given to the kind of obstacles that people engaged in transdisciplinarity have to confront. Prominent among them is distrust over questions of quality. Questions such as "Aren't you lowering the quality of what you are doing?" are frequently raised in relation to any kind of inter- or transdisciplinary activities.

Nonetheless, transdisciplinarity has a semantic appeal which differs from what one often calls inter- or multi- , or pluri- disciplinarity. Note that the prefix - trans- is shared with another word, namely transgressiveness. Knowledge is transgressive and transdisciplinarity does not respect institutional boundaries. There is a kind of convergence or co- evolution between what is happening in the sphere of knowledge production and how societal institutions are developing. For example, we no longer are in the regime characterised by the grandiose type of nation state so characteristic of modernity, where there was a clearly structured, highly differentiated political, economic and social order with different functions that were taken up by the different sectors of society. What we see today is a resurgence, for instance, of NGOs and other ways in which various kinds of stakeholders organise in shaping social reality. This is why the transgressiveness of knowledge is better captured by the term transdisciplinarity.

There are two more important criteria of Mode-2 knowledge production: Accountability and quality control. Accountability differs from individual responsibility. While everyone should have an ethos of individual responsibility, it is necessary to have some sort of institutionalised responsibility, and this is what accountability in Mode-2 is all about. Accountability is an informal process but it has a formalised side to it. You know to whom you are accountable. There are certain procedures to make things visible that are otherwise invisible. And it brings in, different groups in society who want to know "what have you lately done for us?". It is this sense of accountability to different users that opens up the way to understand how scientific knowledge is being produced. Once there is awareness of accountability, and this has to become part of how future researchers are educated, then it can become a way to broaden the horizon of those for whom you are producing knowledge.

Quality control is indeed a very tricky criterion. In our first book, we readily admitted that in the way we described Mode-2 knowledge production, this is its Achilles' heel. Because what quality control in such a setting demands is not only scientific excellence. Scientific excellence is and remains the basis of producing good and reliable new knowledge. But there are other ingredients that go beyond scientific excellence in ways that are difficult to grasp because the context varies. There is no single criterion as there is in disciplinary quality control, where one can always fall back on the standards used in the discipline, allowing one to say: this is good physics, good biology, or good geology. You don't have this any more. And yet, somehow, you have to bring in these additional criteria of quality, of value-added quality. Actually, we should go beyond value-added; we should start to speak about value-integrated. There is something of a societal value that needs to be integrated into the definition of good science. The potential of transdisciplinarity lies precisely here: to obtain a better outcome, to produce better science. We will see how we can get there.

Science and its contextualisation

In Re-Thinking Science we describe the co-evolutionary processes in which society and science are engaged. Co-evolution should not be taken to mean harmony. Transgression and the changing balance between State and Market are highly relevant themes. The unprecedented level of education in our societies, the pervasiveness of modern information and communication technology, the realization that the production of uncertainty is an inherent feature of the co-evolutionary process mean that Society is moving into a position where it is increasingly able to communicate its wishes, desires and fears to Science. What happens then to science as result of this reverse communication?

First of all, it is a portentous, and not a trivial, change. This has not been sufficiently grasped. Let me illustrate. We are familiar with the idea of science communicating with society. Much of the debate about the public understanding of science presumes that non-scientists are not really up to date with the latest developments of science and need to be informed. We expect that form of communication. We are used to descriptions in common sense terms of beautiful discoveries, of developments in instrumentation, and so on. But once you allow for transgression, once you allow that institutional boundaries have become fuzzy, you open science to a flow of reverse communications. This is what we meant, in Re-Thinking Science, by contextualisation. Generally, there is a failure to grasp that science is, by its very success, bringing in a transformative factor. This is furthered by a parallel loosening of institutional structures and by the fact that, when society has ways to communicate with science, science cannot expect to stay the same.

The term contextualisation may again sound different in different ears, but let me explain it in a very simple, but perhaps unexpected way, what we meant: contextualisation means bringing people into knowledge production by asking one question: "where is the place of people in our knowledge?". Taking contextualisation seriously means asking that question even in those areas of knowledge production that seemingly are far away from domains occupied by humans. Of course, you can see a long line of people, especially if you work in a field such as molecular biology, or when research has something to do with genetic diseases, or when clinics are involved. Yet, we maintain that simply asking such a question in every sphere of research will alter the way that knowledge is being produced and make us more aware that a process of contextualisation is going on.

Asking the question about the place of people in our knowledge also implies an additional dimension, namely that researchers move not only in the context of application, but that they need to start thinking about the context of implication. What are the implications of what we are doing, of formulating problems in this particular way? To stress the importance of the context of implication is not a call for new foresight exercises, the kinds of things that have been tried and go on now for a variety of different purposes. It calls for something much more radical, namely, to start asking this question in the scientific laboratories, while recognizing that the question will be answered in a variety of different ways.

People occur, so to speak, in different variants. In one variant, they are ordinary people whom you encounter every day. These people are not statistical averages. They are real people and they are becoming more and more part of scientific organisations. NGOs, for example, to come back to an obvious example, depend for part of their funding on such people. This is also true for the research that some of the NGOs carry out. In the medical field, for instance, NGOs depend on funding that comes not only from the state, or from industry, but also from voluntary associations and private charities. So, in this variant, people are founders and supporters. They are loyal to science, but they want to have their say. People are voice.

In another variant, there are people who protest against what science or technology produces and which reaches them in what they consider unacceptable ways. Most European countries in the recent past have had their scandals, their crisis in public trust, their wave of protests and mobilization against something that science and technology, with the help of the market or the state, are offering them, but which they refuse. This is another way that real people come to influence the way in which scientific and technological development, as well as new regulations, take shape. This variant of people is on the verge of opting out. People exit.

There is still a third variant, that depends on the way in which people are conceptualised in the research process itself. This is obvious in some areas of science, as in the environmental field. You cannot do research on problems that have to do with the degradation of the natural environment without accounting for human intervention in these processes. But this should be interpreted to apply much more widely. Loyality to science can no longer be taken for granted. It may simply vanish when people do not feel that they have a place in the knowledge which is being produced, allegedly for their benefit. Loyality has to be earned, again and again. It has to be negotiated for. One way of doing so is to show that people are present, somehow - and be it only in an imaginary way - in the research process itself. People need to be made loyal again and again.

Thus, there are many different and, let me emphasise, legitimate ways of conceptualising people. You can imagine people in the abstract, as statistical aggregates. But, there are other questions that you might want to address for which it would be better to conceptualise people as active agents, people who have wishes, preferences, whose capacities can be enhanced and who interfere. We are arguing that you should be aware of this alternative in what you are doing, and make explicit which is the place you give to people in your production of knowledge. This is one way of tapping the potential of transdisciplinarity as well.

The production of socially robust knowledge

What are the consequences of the view that the boundaries between society and science have been truly transgressed on the traditional way we think about the demarcation between science and society? One of the implications of Mode-2, of course, is that it blurs and makes it harder to say where science ends and society begins. But, the whole epistemology that drives Mode-1 science is based on a very clear separation of science from society. Mode-1 epistemology is based on the idea of discrete areas of specialization structured on a model of communication that really has only two elements: the first one is that all research must be communicable in a form that can be understood by one's colleagues; and the second one is that it can attract a consensus, even if a limited one. Embedded in this model is a notion of reliable knowledge which comprises a whole series of relatively separate decisions about the integrity of a certain set of scientific findings, the limits of the integrity being dependent on the limits of the consensus achieved. Indeed, with the growth of specialization, many scientists agree that there is no overall consensus among the scientific community. There are only limited consenses held by groups of experts about where the consensus lies in their field of expertise.

But in a regime where the line between science and society is being transgressed, how could this epistemology still guide us? Is it sufficient once people are drawn into the production of science? And, if not, what is the epistemology that will guide us? More provocatively: can we find a way to move beyond merely reliable knowledge?

Reliable knowledge has served us well, and it is going to stay with us. Without the internal quality control of the peer group, science cannot be a sustainable enterprise because one needs a clear cut demarcation criterion: does it work or not? However, there are many more instances where reliable knowledge is no longer sufficient. The reaction of the public to the results of science are frequently contested. There are many controversies and there will be more to come, just because more educated people are also more critical, and society will continue to produce its own risks, since more options mean also more decisions to be taken. What we need is to strengthen by education people's critical abilities. We should be glad to have a highly educated and critical public to engage with in debate.

But what very often emerges when there is confrontation in the context of a controversy is that many researchers perceive the confrontation as an refutation of their work. It is a deep narcissistic insult to them because they work hard, and yet the result or the product - and it's good science, it's enthusiastic science, it is a beautiful piece of engineering ingenuity - is not sufficiently appreciated. It is refuted or contested. This is where our idea that something more is needed comes in. The answer we give (and it is an answer that needs to be filled out collectively, since there is no recipe about how to do it), is that what is needed in addition to reliable knowledge is socially robust knowledge. Robustness is a term that is familiar to engineers because some of them work on how to make buildings, for instance, more earthquake proof. Robustness is not an absolute concept, nor is it a relative concept either. It is

a relational concept. To go back to engineering, robustness depends where your building is located. Is it an earthquake prone zone or not? What kind of material is being used and which is the function of the building?

To continue with the metaphor: we cannot predict where the next controversy in the confrontation with the public will arise. But we can be sure that there will be future controversies. And yet, somehow, we have to try to anticipate such controversies and those instances where the products of science and technology might be refused or contested.

Re-thinking science takes place in the agora

Thomas Hughes, the eminent American historian of technology, has shown, in his book Rescuing Prometheus, the change in ethos amongst engineers, particularly in the area concerned with finding solutions to complex problems. Over the last decades more and more inputs, including those from various pressure groups, have been brought to bear on the problem-formulation, design and completion of large-scale projects. That makes sense to us. But there is more to it. This ethos now has acquired a kind of feedback loop in that the engineers now realise that you get a better technical solution if you bring in these views. This is quite a revolutionary interpretation of transdisciplinarity. It implies that more involvement on the part of society means not a better social solution, or a better adapted solution, or one that brings social tranquillity to a community, but a better technical solution. Could not the same conclusion be applied right across the scientific spectrum: that better scientific solutions emerge if there is dialogue with society than if there is not? I suspect that many researchers would have an instinctive bias against this conclusion. Indeed, many would argue that social inclusion will give you weak solutions. The evidence presented by Hughes suggests otherwise.

By now, it is perfectly accepted and considered highly desirable across a wide spectrum of institutions, from industry to policy makers, that innovation and much of what is the thrust behind innovation, comes from new links between producers of knowledge and the so called users. But in order to fill the potential of transdisciplinarity, the notion of users must be extended. If knowledge is transgressive, then the whole range of reverse communications must be opened.

What could be the appropriate structure in which a debate of this kind might take place? Going back to an old Greek term, we call it the agora. It requires the management of complexity in a public space, which is neither state, nor market, neither public, nor private, but all of this in different configurations. Indeed, the agora is everywhere. It is in your mind as much as in social or public political settings, in corporate structures or in the rules of governance as much as inside laboratories and how we relate to each other. It still recognizes disciplines, but it has moved beyond them to engage with - whom? - the imaginary layperson and imagined users, the public, citizens, in short, what we take to be society to whom we all belong.

Discusssion

The very idea of a discipline
Tim Moore
May 1, 2003 14:53 UT

One cannot effectively discuss trans- cross- inter- multi- etc- disciplinarity without some assumptions about what constitutes a discipline in the first place. This may be a regime of learning, of research, of

techniques and skills, or some combination of these. The trouble is that most, perhaps all, 'disciplines' (if we identify them, say, with university departments, research initiatives, etc) are multiple and often contested regimes. Perhaps the thing is, when a question arises, to try to see what approaches may be promising, wherever they come from, and to work on the ground to try to diminish impediments to trying them out. Such impediments are common, but I think that they are often more to do with mind-sets and institutional factors, and may easily arise within a supposed discipline. It is extraordinary, for instance, that Wiley seems to have felt it necessary to keep his work on Fermat's last theorem secret for seven years, and put on an appearance of carrying on as usual in his University department. It seems to me that this cannot be simply put down to a personal idiosyncracy or anxiety or ambition, nor to the 'nature' of mathematics as a discipline. The impediments to creative work are perhaps more detailed and messy, requiring whatever wit of circumvention we can summon. Maybe that's how the agora goes.

Etymology
Gloria Origgi
May 3, 2003 21:09 UT

Perhaps etymology can help us here in clarifying some nuances of the concept of “discipline”.

Disciplina in Latin refers to the training of a *discipulus*: it is mainly a pedagogical notion. My feeling is that we’re using the term in this very sense even in the contemporary debate. A "discipline" is not only a corpus of knowledge, but a method of training and molding young minds.

Perhaps, we should get rid of the whole idea of a discipline in this sense to be able to develop trans-, multi, inter- *what*? research projects.

etymology
Tim Moore
May 4, 2003 16:08 UT

Gloria's point is well-taken. On the other hand, pedagogical traditions since the middle ages have often themselves been multidisciplinary in something like the modern sense. So the ones to be rejected might be only those which have become ossified or too inward-looking.

Reply to Tim Moore
Helga Nowotny
May 8, 2003 21:09 UT

Of course, the word discipline evokes many different associations. I agree with Tim Moore that ‘most, perhaps all, disciplines…are multiple and often contested regimes’. But they all tend to rely on various mechanisms with which boundaries are drawn and re-drawn. Even within a ‘discipline’, we often find implicit or explicit distinctions which mark what is considered ‘more scientific’ and less, the ’hard’ vs. the ‘soft, or the theoretical vs. the ‘practical’. These distinctions are far from innocent, since they form the basis on which cognitive and social hierarchies are built. Individual ingenuity can sometimes get around them, but can we collectively be more audacious and subversive?

Disciplines, as Stephen Turner has suggested, have two elements to their definition. One is nominal: the discipline must be called a discipline. The other is related to the actual facts of employment: there must be persons trained in the name of the discipline and the beginnings of a labour market. What does this mean for cross-, inter-,multi etc. disciplinarity? The impediments mentioned by Tim Moore can arise within a discipline and between. But they will also continue to arise as long as disciplinarization assures privileged access to the market of students and their employment chances. Thus, interdisciplinarity, apart from other benefits, must also look beyond and find new opportunities for its students

Further considerations on etymology
Basarab Nicolescu
May 20, 2003 12:22 UT

As Gloria pointed out, the word "discipline" comes from the Latin word "disciplina", derived in its turn from "discipulus". It means "action of learning" and later "teaching", doctrine, methode", "education", and "military formation" . By extension, it means "principles, rules of life". Interestingly enough, this word meant, in the 12th century, "punishement", in the sense of mortification of the body. Towards 1549 it signifies the instrument of flagellation in the religious processions. It is also very interesting to note that the word "disciplinary" ("disciplinaire") first appeared in French in 1611. I would be interested to know when the word "disciplinary" first appeared in English. (Bibliographical source : "Le Robert - Dictionnaire historique de la langue française", Dictionnaires Le Robert, Paris, 1992, vol. I, p. 610.)

Reply to Nicolescu's Query
Julie Klein
May 24, 2003 21:59 UT

Basarab asked a good question, about the first use in English of “disciplinary.” I especially appreciate the question, because I often complain about the casual assumption that disciplines as we know them today have been around since the dawn of time.

I don’t have an absolute answer. The Oxford English Dictionary, though, provides credible clues. The first listing for the meaning of “instruction imparted to disciplines or scholars” is in 1382. The first listing for a “branch of instruction or education” is in 1386. The first listing for “instruction having its aim to form the pupil to proper conduct and action” is in 1434. The first listing for “the orderly conduct and action which result from training” is in 1509. The first listing for “The order maintained and observed among pupils, or other persons under control” is in 1450. Of added note, the first listing for “to subject to discipline,” derived from the French “discipliner” or Med. Latin “disciplinare” and Latin “disciplina,” is in 1382.

What is instructive about this chronicle is the fact that the earliest listings are in the fourteenth century, after the initial transition from the secular cathedral schools to the “universitas “ began and moving toward greater codifications of learning that would take place in the Renaissance.

Why "socially robust knowledge"?
Dan Sperber
May 3, 2003 23:16 UT

I agree with Helga Nowotny that “we should be glad to have a highly educated and critical public to engage with in debate.” In many domains at least, I too believe “that better scientific solutions emerge if there is dialogue with society than if there is not.” I see the value of re-thinking science in the agora (and isn’t it what we are trying to do here?). However the claim that what we need is not “merely reliable knowledge” but “socially robust knowledge” puzzles me.

To begin with, science has never been aimed at the production of “merely reliable knowledge.” It has been aimed at reliable *and relevant* knowledge. Various forms of relevance have been involved: explanatory relevance and practical relevance in particular. Practical relevance, which guides the bulk of funding and institutional support for research, has always been evaluated at a societal level (but not always democratically so, of course). Engaging in scientific inquiry has been and is, if anything, a move away from “socially robust knowledge” and towards knowledge that might not be understood by the society at large, and not even easily understood by the scientific community, or, if understood, might not be readily accepted. Galileo and others have paid dearly from such non-conformism. Historically, the most “socially robust knowledge” has been in the form of systems of religious beliefs that have been generally accepted, each within its own religious community, with very little change over centuries – except, of course, that religious beliefs are not knowledge.

I see also a tension, not to say a contradiction, between the plea for socially robust knowledge and Nowotny’s praise of the “transgressive” character of knowledge. Surely the more

transgressive contributions to knowledge go against socially robust ideas. Or am I missing something?

Reply to Dan Sperber
Helga Nowotny
May 8, 2003 21:05 UT

Sometimes, one is engaged in an argument from a specific position, risking to exaggerate its importance to others. In *Re-Thinking Science* we have a long argument with John Ziman, who is the lucid proponent of 'reliable knowledge'. For him, this constitutes something like the epistemological core of scientific knowledge, the production of which is carried out entirely within the scientific community. Practical relevance, of the kind mentioned by Dan Sperber, exists, but is based upon or derivative from reliable knowledge. For John Ziman and others, practical or relevant knowledge is merely wrapped around the hard epistemological core of reliable knowledge. We argue that while reliable knowledge remains indispensable (we all expect science and technology 'to work'), it is no longer sufficient, since wider society claims and re-defines what reliable and relevant knowledge it would like to have.

'Socially robust knowledge' has nothing to do with religious beliefs (they are not knowledge in this sense and I would strongly object to call any kind of dogmatism or fundamentalism 'socially robust'). Social robustness describes a process which allows the unforeseen to enter not only on the side of science dealing with Nature, but also with Society. Robustness is produced when research has been infiltrated and improved by social knowledge. Like any knowledge production, it is subject to frequent testing, feedback and improvement, because it is open-ended.

Still puzzled
Dan Sperber
May 9, 2003 18:13 UT

"Socially robust knowledge" sounds nice. Each of the three word of the phrase has positive connotations. However, if I understand Helga's response, none of them, nor their combination is to be taken quite in their ordinary sense. Science, in whatever mode, is a social activity. The fact that not everybody is involved does not make it any less social than it does for banking or karaoke. Moreover, scientific activity, unlike so many other social activities, does involve sharing the output of the activity with society at large, through education and through practical applications. Given that the funds for research come not from the scientists themselves (the time where science was, for a good part, in the hands of "gentlemen of leisure" is long gone), but from the state, industries, and so on, has meant that scientists have not been free to study whatever they wanted as much as they wanted, but had to focus of what was seen as relevant by other segments of society, political authorities, and so forth. Moreover, the social demand was always for robust knowledge (for what else do you pay scientists?). So what is special in the "socially" of Helga's "socially robust knowledge"? I am not sure, but isn't it that non-scientists should have a major say not only on the relevance of scientific findings, but also on their content and on their epistemic standing? It is not "robust" and it is not "knowledge" until it has been negotiated, possibly modified and in the end accepted by society at large. But, if this is what is meant, isn't the least transgressive contribution to knowledge the more likely to be socially robust? I still don't see how you can defend the transgressive character of knowledge and "socially robust knowledge" in the same breath.

Good question - Reply to Dan Sperber part I - A transgressive AND social robust knowledge
Helga Nowotny
May 14, 2003 11:35 UT

How can I defend the transgressive character of knowledge and socially robust knowledge at the same time, asks Dan, and it is a good question.

Social robustness is about making science, and the research process, more open to societal expectations and demands. It is also about inventing institutional arrangements and other social

forms that allow such negotiations to be carried out, hopefully with results that will benefit knowledge, and knowledge production, as a public good. This does not imply “anything goes”.

A long time ago, politics and science had to agree that ‘scientific truth’ cannot be politically negotiated and Merton’s famous defense of the scientific ethos against the fascist and totalitarian regimes of his day were along this line. Today, democratic societies are faced with finding incentives for the private sector to invest more in research, since practically everywhere, and in some countries with dramatic effect, public funding is leveling off or declining. Now call this ‘commercialisation’ or commodification’ of science and research, if you wish, but in the end it will depend on the fine-grained, yes “transdisciplinary” and often local negotiations of how specific problems are to be defined and which solutions are deemed satisfactory to all participants involved in knowledge production which is distributed throughout society.

What is special in calling for social robustness is to make this process explicit, to render it visible, so that interventions are possible when they are needed. Following Hirschman’s distinction, ‘voice’ is one of the options and we are far from having found satisfactory, not only democratically legitimate, but also democratically effective ways, of providing for it. What scientists (and politicians in a different way) obviously fear most is the ‘exit’ option. But what can no longer be taken for granted, neither by scientists, nor by politicians, is ‘loyality’. It has to be gained, and regained, not only after every scandal and controversy implicating science, technology and politics in ways which are often difficult to entangle but which reflect how the real world of ‘Science, Money and Politics’ operates (as Daniel S. Greenberg has so uncannily shown). Loyality itself needs a socially robust and sustainable basis, if science is to thrive as a social and cultural practice also in the future.

Good question - Reply to Dan Sperber part II - Robustness, Transgressiveness and Transdisciplinarity
Helga Nowotny
May 14, 2003 11:37 UT

Robustness, yes, ultimately will always be on the side of society, but the last thing I want is to see it being exercised in a crude way which can only be detrimental to all. This is where transgressiveness enters. The cunning of reason has invested knowledge production with being inherently transgressive. Knowlegde craves the yet unknown, it is driven by the ambitions of curiosity, it seeks to bring about the unpredictable and the novel. It has infected society to share at least partly its passions and obsessions. Most of us want science to go on exploring the unknown, technology or techno-science to come up with novel solutions, products, diagnoses and therapies, that will somehow increase well-being and material benefits, even if we become much aware of the down-sides of this endeavour. Most of us realize that much more has to be done to bring basic amenities and conditions for living in human decency to those parts of the world which still lack them in scandalous ways.

Transgressiveness is such a powerful force, because it is alive and active in each of us as a human being and allows us collectively to transform knowledge into humanitys highest achievements and most shameful defeats. Undoubtedly, part of the transgressive character of knowledge is tamed by working for the market and will be distorted by crass commercialization. Part of it is being tamed by the military for unprecedented high-tech exploits and ever more sophisticated weaponry. Part of it will be squandered by perhaps well-intentioned, but ill-conceived large scale planning efforts, be it publicly administered or channeled through large corporations, especially in the so-called developing countries. But part of it will continue to seep through all institutional walls and cross disciplinary as well as institutional boundaries. It will crop up locally, in unpredictable places and circumstances, subverting what economists predict about human motivation. It will not ‘speak truth to power’, because power sees no reason to even listen to it, but it will continue to whisper and eventually become a loud voice which can no longer be overheard.

Transdisciplinarity and the concept of the agora is an attempt to find a place, indeed many, heterogeneous places, to let the transgressivity of knowledge and the ultimate robustness of society encounter each other and to find solutions to problems that both must recognize as

somehow being also ‘theirs’. Negotiations must take place and new institutional arrangements be invented and implemented. There will a lot of local, regional and national variation. What may work in a university setting, may not be good for a research council or a local initiative. What may work in the US, might not work in France or Switzerland. ‘Voice’ must always be conceived in the plural. As with language, its production is inherently infinite, but still meets the constraints of grammatical rules and semantic content or how democratic institutions and the research process work at a given time and place. Transdisciplinarity may be conceptually vague, but it has the advantage of being adaptable under circumstances that may not all be foreseen, nor do I want them to be legislated. In this sense, transdisciplinarity is also an appeal to our collective imagination, to help bring about institutions that do not yet exist, but which carry the promise of reconciling robustness and the transgressivity of knowledge – not once and for all, but again and again.

The rhetorical risk
Dan Sperber
May 16, 2003 12:37 UT

Helga's answer is helpful, but it makes it even clearer that the words "socially robust knowledge" are chosen as much or more for their rhetorical appeal than for their descriptive adequacy. What is meant would be better described, it seems to me, as "socially entrenched knowledge." In any case, there is something unfortunate about the suggestion, carried by the expression "socially robust knowledge," that social entrenchment is an epistemic virtue.

I would not belabour this terminological point if the strong reliance on rhetorical effects in Helga's paper and responses to comments (some of which have been, it is true, no less rhetorical) did not, for me at least, detract from the main thrust of her argument. It underscores the risk that shifting part of scientific communication to the "agora" would give -- is already giving -- much greater weight to rhetorical devices in deciding issues, something that, as an empirical scientist, I find quite unappealing. This, of course, is only one consideration in the issue of deciding who should participate in scientific conversation and in what ways. It is not, however, a minor consideration, for, precisely, what we want to avoid is that scientific views of little merit should become socially entrenched because of their rhetorical appeal. This of course is already happening, as a perverse effect of the otherwise encouraging success of popular science publishing, and its feedback effect on scientific practice through its influence on funding agencies, on students, and on scientists themselves.

Time out on 'Rhetoric'
Steve Fuller
May 16, 2003 19:26 UT

Dan is beginning to sound like one of those ‘Mode 1’ people that I didn’t think ever really existed. First of all, the issue of rhetorical devices is invidious in this context, since everyone uses them – and not without reason. The suggestion that there is some clear distinction between ‘empirical’ and ‘rhetorical’ is untenable: Both are necessary features of any communicative and informative medium, and each normally enhances the other. However, whenever people insist on this distinction, it’s clear which side the ‘Angels’ are on – and it’s not rhetoric. This distinction is invoked to insinuate that someone is using words to mean something other than they seem, and perhaps even that they are not engaged in a morally appropriate inquiry. I myself accused Helga of this before, but I didn’t hide behind a bogus binary of empirical v. rhetorical. That’s too rhetorical even for me!

Consider all this rhetoric: I was struck by how Dan made a completely false (empirically speaking) statement about religious belief systems never changing – and then Helga immediately agreed with him, perhaps to express a common ethos. (Yet, one wonders how they imagined the history of modern science would have proceeded, were it not for mutations of the Christian belief system!) This to me is quintessential rhetoric, but it’s quite normal: Two speakers quickly restrict the domain of discourse by adopting some false assumptions that will hopefully lead to some fruitful outcomes. Again, I was struck by how Dan and Helga can easily talk about the pros and cons of ‘reliability’ as an epistemic value, given its elastic meaning from the pseudo-mathematical to the crypto-ethical. Of course, the possibility of empirical inquiry

presupposes that reliability can be given some kind of clear and workable analysis at some point, but for now the concept hangs as a nebulous promissory note without losing anyone any sleep – as they then move on to 'relevance'.

I say all this because accusing people simply of using rhetoric is pejorative without being informative. But stayed tune for more…

...but does transgressive knowledge require transgressive rhetoric?
Steve Fuller
May 16, 2003 19:28 UT

Having said that, I think that there are less metaphysically threatening ways of talking about 'transgressive knowledge' than Helga's last set of responses, which seemed to suggest that knowledge exists as some kind of 'vital force'. Here are some relatively domesticated ways of talking about these things: e.g. 'ecological validity' in social psychology, which has generated an interesting literature about when scientists take seriously features of 'real world' conditions and the feedback effects they generate. There is also 'participatory action research' in which the people under study help formulate the research questions. Traditionally this has been talked about in terms of academics steering away from narrow discipline-based agendas, and the word 'transdisciplinary' is quite appropriate in this context.

The problem with Modespeak is that it opens the door so widely that, in principle, almost anything can influence the research agenda, including a market-generated paymaster, and there is little to discriminate good from bad influence on research (either 'ex ante' or 'ex post'). Moreover, there's the other side of this coin, which is the way academics might help transform the non-academic entities attempting to influence their work. For example, in the increasingly ephemeral world of business, corporate managers could learn something about the dynamic-yet-stabilizing processes of universities as corporate entities (Fuller 2002: chap. 1). However, for now Modespeak clearly tends to portray Mode 1 guys like Sperber as in need of rehabilitation.

Fuller, Steve. (2002). Knowledge Management Foundations. Woburn MA: Butterworth-Heinemann

Reply to Steve Fuller
Dan Sperber
May 18, 2003 9:36 UT

Steve Fuller: *"Accusing people simply of using rhetoric is pejorative without being informative"*

Dan Sperber: Agreed. To express oneself with the goal of convincing others is to engage in a rhetorical activity. Nothing wrong with that, and I never accused anybody of doing so.

SF: *"The suggestion that there is some clear distinction between 'empirical' and 'rhetorical' is untenable: Both are necessary features of any communicative and informative medium, and each normally enhances the other."*

DS: Note that Steve is using in the second sentence the distinction he was objecting to in the first sentence. Even more relevant here: The idea that the descriptive (a term I prefer to Steve's "empirical") and rhetorical goals in communication normally enhance each other is wonderfully optimistic. Steve must never listen to, or read politicians. But forget politicians, we scientists often find ourselves having a hard time reconciling descriptive adequacy and persuasiveness, and we end up making, more or less consciously, choices of words that somewhat compromise one goal or the other (hoping that at the end of the day, both goals will have been adequately served).

SF: *"Of course, the possibility of empirical inquiry presupposes that reliability can be given some kind of clear and workable analysis at some point, but for now the concept hangs as a nebulous promissory note without losing anyone any sleep."*

DS: For most empirical scientists and also most philosophers of science, having and using clear reliability criteria is a central concern. Social scientists who don't loose any sleep, or any time at all, on such issues of reliability of knowledge are, it seems to me, out of touch.

DS; What I did, in the message Steve is responding to, was question Helga's use of the phrase "socially robust knowledge," saying that these words were "chosen as much or more for their rhetorical appeal than for their descriptive adequacy." (Not an accusation). I would be curious to know what Steve thinks of "socially robust knowledge" (the phrase and the idea)?

Reply to Dan on Rhetoric and Reliability
Steve Fuller
May 18, 2003 12:50 UT

I shall follow Dan's economizing practice of citing the bits of the previous speaker, since it helps convey a sense of focus to the issues.

DS:… we scientists often find ourselves having a hard time reconciling descriptive adequacy and persuasiveness, and we end up making, more or less consciously, choices of words that somewhat compromise one goal or the other (hoping that at the end of the day, both goals will have been adequately served).

SF: Actually I don't see things quite this way. When, say, Richard Dawkins uses an expression like 'selfish gene', he thinks he is saying exactly what needs to be said to the audience he wants to say it to. Each aspect of this process is empirical and rhetorical simultaneously. If people don't like what he says or misunderstand what he says, then that is also simultaneously an empirical and rhetorical error – though on whose part exactly is an open question. We don't need to be talking about the 'compromising' of one or the other aim of speech unless we presuppose some objective realm of true propositions which empirical science aims for (by its fallible means) and rhetoric aims to convey (by its separate fallible means). Perhaps you would say that Dawkins is 'forced' to talk baldly about 'selfish genes' because people don't know enough evolutionary biology to understand subtler formulations, but I would then interpret you as simply expressing a wish that ordinary people had the biologist's interest in asking biological questions.

DS: For most empirical scientists and also most philosophers of science, having and using clear reliability criteria is a central concern. Social scientists who don't loose any sleep, or any time at all, on such issues of reliability of knowledge are, it seems to me, out of touch.

SF: I never denied that there are lots of people worrying about something they call 'reliability' – the question is whether these worries have anything in common other than a word: The most precise understanding of this word is in the methodology of experimentation. There are also some philosophers who come up with abstract models of reliability that basically stick variables on intuitions but don't actually measure anything. And finally, there is much vaguer talk by sociologists about 'reliable processes' like peer-review, where 'reliability' is a polite way of talking about the 'old boys network'. Perhaps you think there is a deep concept hidden here. I'd like to hear about it, and how it influences any epistemic judgements you actually make.

More follows....

Reply to Dan on Socially Robust Knowledge
Steve Fuller
May 18, 2003 12:52 UT

DS: I would be curious to know what Steve thinks of “socially robust knowledge” (the phrase and the idea)?

SF: I did not make myself clear before: I agree with Helga here – ‘transgressive knowledge’ is often ‘socially robust knowledge’ in the sense of forcing the academic to validate her knowledge by something other than the standard research paradigms. In that respect, ecological validity in social psychology and participatory action research are examples of both ‘transgressive’ and ‘socially robust’ knowledge. This is, so to speak, the good side of Mode 2. As I indicated before, however, transgressive/socially-robust forms of knowledges are not NECESSARILY good things. And here I share some of your qualms about the extent to which research integrity and academic autonomy might be compromised. But once again, the Modespeak obscures more than clarifies. So, my advice is let’s drop worrying about the meaning of words and talk about what really bothers us.

More Word Worries
Rainer Kamber
May 20, 2003 13:46 UT

Having followed this thread I am stuck with the impression that there is some talking past each other going on. I would like to know what Helga, Dan, and Steve, respectively, mean by "transgressive".

Dan has said nothing further, although he conceded in his first reply to Nowotny that there may be a misunderstanding lurking. He said he believed that there is a tension "between the plea for socially robust knowledge and Nowotny’s praise of the “transgressive” character of knowledge. Surely the more transgressive contributions to knowledge go against socially robust ideas." What does this mean?

After reading Helga's replies to Dan it is unclear to me if she identifies "transgressiveness" with "social robustness". Could these concepts not be put in a few simple words? E.g. "Social robustness is about making science, and the research process, more open to societal expectations and demands." (Nowotny) Now, what is meant here by "science" or "research process"? Are extrascientific actors to generally have a voice in the selection of the subject matters of scientific knowledge production (e.g. the selection of "problems"? Or research questions?). Or does it mean that such actors are to contribute to knowledge production as such, i.e. the methodologically regulated production of knowledge? Or are extrascientific actors a "knowledge-resource" for research? Since, as Helga seems to claim, the control over knowledge production is threatended to be taken out of the hands of "science" by its "commercialisation" or "commodification" it needs to be claimed back by "the public". Well, if the "commercialisation" or "commodification" of scientific knowledge production is the problem, then why not simply claim the latter back for science proper. I take it from some of Helga's other remarks that this is not feasible since there isn't (and never was) any such thing as science proper. But then why exactly has "commercialisation" or "commodification" become troublesome in the first place? Is the latter, then, not the "normal" way that knowledge production is (and has been) done? And if this is so, is Helga's aim to claim knowledge production for the public because that is where it has belonged all along - quite independently of "commercialisation" or "commodification"? Mind you, I find the claims by Helga about the questions of "voice", "opting out", or "loyalty" (in her introductory essay) very confusing since they seem to utterly mix normative and descriptive claims in the mode 2 agenda. It seems to me that if empirical data says anything about people's attitudes and perceptions about science then it is that these attitudes and perceptions have, in fact, changed rather little in the last 40 years or so.

Steve says that "‘transgressive knowledge’ is often ‘socially robust knowledge’ in the sense of forcing the academic to validate her knowledge by something other than the standard research paradigms." But it seems to me so far that "transgressiveness" and "social robustness" of

knowledge are perfectly independent notions and I can't see how the transgressiveness of knowledge implies anything about its social robustness, or vice versa.

Response to Rainer on Modespeak Semantics
Steve Fuller
May 20, 2003 15:59 UT

Here's what I think is going on. In Modespeak, 'socially robust' is defined in terms of 'transgressive', not the other way around. Perhaps this is the source of confusion. I think the reason why Dan thinks 'transgressive' and 'socially robust' are inversely related is because he reads 'socially robust' in a monolithic way as 'consensual' and then he comes up with a position like Ernest Gellner's in which science and society stand somewhat in opposition to each other. However, in Modespeak, 'socially robust' means more like 'robust under substantially different social environments'. This concept is 'transgressive' of traditional academic research, whereby a knowledge claim is accountable only to academic peers – and not, say, the subjects/objects of knowledge, or the funders, or the larger society. So what is being 'transgressed' is the boundary between academic and non-academic. Each of the non-academic groups have their own criteria of validity and producing a knowledge claim that can survive across all those settings is 'socially robust'.

More and more confusing
Dan Sperber
May 28, 2003 21:52 UT

Steve Fuller, helpfully trying to correct my misapprehensions, writes:

"in Modespeak, 'socially robust' means more like 'robust under substantially different social environments'. This concept is 'transgressive' of traditional academic research, whereby a knowledge claim is accountable only to academic peers – and not, say, the subjects/objects of knowledge, or the funders, or the larger society. So what is being 'transgressed' is the boundary between academic and non-academic. Each of the non-academic groups have their own criteria of validity and producing a knowledge claim that can survive across all those settings is 'socially robust'."

Given that, indeed, "each of the non-academic groups have their own criteria of validity," and that these criteria typically diverge within society and even more across societies and cultures, the only true kind of "socially robust knowledge" by this criterion is the most robust knowledge by standard scientific criteria, that somehow achieves obviousness across social contexts just as does knowledge that snow is cold, that dogs bark, and that triangles have three sides. So mathematical theorems, heliocentrism, Hartley's explanation of the role of the heart, and other comparable scientific achievments survive across more "substantially different social environments" than, say, any piece of social science (none of which is robust by standard scientific criteria). These are the most socially robust pieces of scientific knowledge by the definition proposed, and this because they are the most scientifically robust to begin with. (And, by the way, what is "transgressive" about these superb instances of "social robustness"?) On the other hand, satisfying the non-academic, group-specific criteria of validity of some given social group is a sure way of not satisfying those of other groups and of not producing "socially robust knowledge." Very confusing. Or am I again missing something?

One more time on robustness
Steve Fuller
May 30, 2003 18:43 UT

I think you're missing something. Let's grant your point that first-order natural scientific knowledge is more socially robust than first-order social scientific knowledge. The main reason for this difference in robustness that is that natural scientists control the context of reception of their knowledge claims. In other words, if you want to dispute whether the heart works as Harvey says, then you had better studied some human physiology, and these studies are in the hands of the people who are promoting Harvey's ideas – that is in terms of how knowledge of

the heart is transmitted and authorized in human physiology. Your own naïve ideas about the heart don't count for much in the discussion.

I state this crudely because, in contrast, much less control of the context of reception exists in the social sciences. Serious doubts can be raised about whether economic theories and 'findings' apply to economic reality, etc. This is because the subjects of these theories have their own views and insights into the matters covered by the theories, and it is not clear prima facie that the social scientific theories carry more legitimacy than the views of the subjects of those theories. This is related to the ambiguous status of methodology in the social sciences. If you use very rigourous, lab-like methods, your results lack ecological validity and you're treating your subjects like rocks and trees. But if you take your subjects at their word, you're seen as unrigourous, etc.

Striking the right balance between these two extremes involves transgressing the academic-nonacademic boundary, implying some kind of hybrid methodological resolution. Part of the Mode 2 story is that as the natural sciences are increasingly subject to application-driven research, they are also adopting the pattern of work familiar to social scientists, where criteria of validity need to be forged with clients and stakeholders.

To go back to this troublesome word 'robust': Imagine two situations where something might be 'socially robust'. One is if it can alter its environment to enable it to survive easily, even if it means eliminating other things in the environment (e.g. the displacement of folk knowledge by scientific knowledge). Another is if it can alter itself to suit the environment in which it needs to survive (e.g. arrive at some folk/scientific hybrid). Your examples from the natural sciences are like the first sense, but the Modespeakers are interested in the second sense, which is becoming more prevalent.

Emancipatory Science, emancipated scientists?
Rainer Kamber
May 4, 2003 11:48 UT

Nowotny's project of transdisciplinary science is transdisciplinarity seen as a "forum or platform" which she later calls the "agora". It is here where the "re-thinking of science" is to take place. Nowotny and others have already rethought it: This new science and the scientists that carry it out answer to "society talking back to science". Transdisciplinary science and its scientists are to be "accountable" towards the users of scientifically produced knowledge. Accountability is to be achieved by a "contextualization of science", i.e. by scientists asking themselves "where is the place of people in our knowledge?"

What are the needs that drive this new utopia of science? What is the image of science that Nowotny wants to replace? One need she addresses is the "unity of knowledge" but it is not discussed further. The other is the desideratum of "joint problem solving" and this is where her project has its origins. Certainly, there are kinds of problems whose definition is a task that exceeds the capacities of the science system alone. Environmental problems, social problems, problems of development could be general examples. One scandalon, tacitly implied by Nowotny, could be science arrogating this task to itself - an arrogance that owes much to some of the older utopias of science (Bacon's for example) and that is still represented in expertocratic tendencies in the implementation of science policy today. But Nowotny wants to go further in claiming that transdisciplinary science, in the end, will be better science since it will produce better (i.e. "socially reliable") knowledge.

It seems that Nowotny's claims regard the relationship between science and society much more than the relationships between different areas in science. But do scientists really need a call for emancipation? Are most scientists still in fact blind, deaf, and dumb to the presuppositions or the implications of their work? Is it not policy-makers that are to be targeted here since, in the course of the 20th century, science has lost much of its internal steering capacities anyway? Doubtlessly scientists tend to underestimate the tacit role of internal factors and agendas when considering the selection of research targets or knowledge desiderata and much could be acheived in this area by strengthening the according reflective capabilites in science. But will this indeed lead to a more "socially robust knowledge"? Does not this latter concept tend to be misleading since it does not

account for science's own capacities to contribute to reform and emancipation? And how does the concept of "socially reliable" fit into democracies since, as much of the experience with "participatory science" amply demonstrates, the latter can by no means be identified with "democratic science"? Is there really a need for scientists to be much more socially or politically conscious in their work? And is science really to be governed after the paradigms of political or economical governance? It seems to me that the implications (and the historical precedents) of all this may still need more thorough reflection on the part of science studies.

Reply to Reiner Kamber
Helga Nowotny
May 8, 2003 20:51 UT

The last chapter of our book has the title “Re-Thinking Science Is Not Science Re-Thought’. I assure you that we have not re-thought everything and I have no prescriptive advice to offer. But I am convinced that, whether you like it or not, the process of what we call ‘contextualization of science’ continues unabatedly, because it results from co-evolutionary processes that link society and scientific knowledge production in specific ways. Hence, it is not a question of ‘achieving accountability through contextualization’, but the other way round: contextualization manifests itself, among other, in greater demands for accountability.

Obviously, accountability is no panacea. It can be distorted and distorting, it can stifle creativity if it is carried too far or if it becomes just another bureaucratic control mechanism. If anything, it is closer to a dystopia than to a new utopia of science. But it will not go away. It is up to emancipated scientists – of whom I wish there would be many more – to invent new forms and stable, future-oriented institutional arrangements that meet the demands for “joint problem solving”. Hence, this is not a question of being ‘socially or politically more conscious of their work’, nor of submitting science under ‘political or economic governance’. But it can be read as an appeal to ‘science’ and to some ‘scientists’ to emancipate themselves from their belief that they somehow stand outside ‘society’ – hence my claim that transdisciplinary science might in the end lead to ‘better science’- which is to be jointly defined.

Conférence de consensus
Abdelkarim Fourati
May 6, 2003 12:15 UT

Quelle serait la structure appropriée dans laquelle un débat de cette sorte (inter- et trans-disciplinaire) pourrait prendre place? Pour contribuer aux débats sur la question de conclusion du texte de Helga Nowotny, je vous propose le concept de "Conférence de consensus".

Inventée au Danemark en 1987, la conférence de consensus (ou conférence de citoyens) organise la rencontre entre des experts, des groupes d'intérêts constitués et un jury de citoyens extérieur à la controverse, chargé de produire des recommandations destinées aux décideurs politiques. Tous les observateurs de telles conférences sont frappés par la qualité des échanges, la capacité des non-spécialistes à «saisir les dimensions stratégiques de la recherche scientifique», à produire des recommandations mesurées qui «proposent des solutions de bon sens», à composer avec les différents intérêts et à faire émerger ce point commun improbable: la volonté générale d’une intelligence collective.

Qu'ils soient spontanés ou organisés, en actuel ou en virtuel sur Internet, ces colloques (ou forums) hybrides entre spécialistes et non-spécialistes déstabilisent le partage entre savants et citoyens ordinaires. Ils démontrent que la pensée disciplinaire n'est ni la seule possible ni la seule valable; que la capacité de diagnostic, d'interprétation des faits, d'exploration des solutions envisageables, n'est pas l'apanage des spécialistes. En outre, ces manifestations forcent à reconnaître que les profanes apportent une autre forme de savoir, une prudence, qui contribue à rendre la science plus objective, si l'on convient de penser qu’elle l'est d'autant plus qu’elle aura répondu à un maximum d'objections. Il est temps de reconnaître que l'inculture scientifique affecte aussi bien les scientifiques professionnels que les non-scientifiques. En effet, dans l'état actuel d'ultra-spécialisation, le niveau d'ignorance concernant un domaine particulier est pratiquement aussi élevé dans la collectivité scientifique, dont la plupart des membres travaillent dans d'autres domaines, que parmi les profanes. On n'a donc pas

affaire à un large fossé unique entre scientifiques et non-scientifiques, mais à une multitude d'hiatus particuliers entre spécialistes des disciplines et non-spécialistes dans chaque domaine. La science n'est pas une vaste île séparée du continent de la culture, mais un archipel éparpillé d'îlots, parfois plus éloignés les uns des autres que du continent. Un expert d'une discipline dans un certain champ est un non-expert dans presque tous les autres disciplines, et se trouve donc fort proche du profane total du point de vue de la culture scientifique en général. Aujourd'hui, la culture des cultivés, c'est de savoir des petits riens sur tout; celle des spécialistes, de savoir tout sur rien.

Si le sens commun n'est pas une base solide pour la pensée scientifique disciplinaire, il n'en existe cependant pas d'autre possible. La méthode scientifique est alors celle du sens commun critique. Quand un scientifique formule une hypothèse, en déduit des conséquences, collecte des données et transmet ses résultats à ses collègues; ses processus de pensée sont-ils, en dépit de la complexité de leurs objets, si différents des processus naturels de pensée? Einstein ne disait-il pas que "la science tout entière est un raffinement de la pensée de tous les jours"?

The Shift to Participation
Julie Klein
May 7, 2003 19:20 UT

Abdelkarim Fourati raised the question of the most suitable structure for debate. He proposed consensus conferences, developed in Denmark. I would add a few supportive comments. Participation of stakeholders is not new. In Danish agriculture, the tradition of self-organized and cooperative development dates to the 19th century. In the 1970s, though, a new rhetoric of "co-management and decentralization" in managing renewal resources and environments became evident. In 1980s and 1990s, participation began moving center stage in technology assessment and, in the late 1980s, new ideas for improving "planned participation" in environmental regulation emerged in both Denmark and The Netherlands. The concept of "consensus conferences" brought public debate into technology assessment. Similar efforts followed in other countries, including the Swiss PubliForums. I would add an additional example I have observed first-hand. The current project to decontaminate the Rocky Flats nuclear weapons facility in Colorado involves a wide range of stakeholders, including vociferous environmental groups. Project directors would be quick to echo Helga's caveat that transdisciplinary work takes patience. Every phase must be subject to stakeholder buy-in, slowing down the process. However, given that the final cleanup will not render the site completely "safe," their involvement is all the more crucial. They have to live with the long-term implications of the poisoning of their land after the technical "experts' have cashed final contract checks and gone home.

Many public authorities, Paulius Kulikauskas observed at the Zurich conference on Transdisciplinarity, are now eager to engage in experiments, demonstrations and pilot projects in the name of "transdisciplinarity," "sustainability" and "participation." Their interest is fueled in part by disappointment in traditional approaches to urban renewal. However, integrating concepts of participation and transdisciplinarity into general governance culture on a long-term basis is a complex task. Kongens Engave, a neighborhood in southwest Copenhagen, is the only area under the Danish Urban Regeneration Experiment that has both a locally-elected council and a government subsidy. When residents became involved in the planning process, they formed sectoral working groups in areas such as physical problems, housing, culture, employment and social issues. As a result of participation, criteria for success became more holistic and locally oriented. It took a lot of patience to get to that point, however.

I would also point to the shift from ineffectual technology transfer to cooperation in development activities. In the South, indigenous knowledge and accessible forms of traditional technology are valued. In the past, however, interactions between North and South tended to be one-way applications of knowledge delivered by a "first-civilization" to a "second civilization." They were not appropriate to local social, cultural, economic, and ecological realities. An imbalance continues, but, Hansjurg Mey and others pointed out at Zurich, transdisciplinary perspective has the potential for integrating Northern and Southern views. Discussions occur on two levels: the North-South gap and the gap between scientific elite and the majority.

Reply to Abdelkarim Fourati
Helga Nowotny
May 8, 2003 20:58 UT

Consensus conferences are interesting settings for what has been called the 'democratization of expertise'. It has become clear, however, that they too are made to fit into an overall structure of authority, carefully managed in subtle ways through language, timing, setting, framing of questions etc. But if experts are genuinely interested in listening to lay persons' views and critique, they should know – and accept – that this might also threaten their status as experts. For experts are mostly dealing with 'imagined lay persons' whom they imagine to fit into the overall structure of their knowledge and expertise.

Meeting – and accomodating – 'real' lay persons therefore predisposes their willingness to alter the image they have in their own imagination. In the end, they will remain experts, but perhaps with greater respect for the cognitive and social autonomy of lay persons.

La montée du spécialisme en médecine
Abdelkarim Fourati
May 6, 2003 13:56 UT

Helga Nowotny a raison de prendre dans son texte des exemples du domaine de la médecine. De fait, les sciences médicales doivent prendre une place importante pour repenser l'interdisciplinarité de façon générale, et cela au moins pour deux raisons: (1) la montée du spécialisme dans la pratique de la médecine, et surtout (2) le fait que la médecine se trouve au carrefour de la grande coupure entre les sciences physico-biologiques et anthropo-sociologiques. Justement, mon grand projet de recherche inter et trans-disciplinaire, depuis le début des années 1980, est la contribution à l'articulation des sciences bio-médicales et anthropo-sociologiques par les sciences cognitives.

La pratique médicale nous impose aujourd'hui, pire encore demain sûrement, une répartition des tâches, une division du travail, toujours plus poussée: des spécialisations de plus en plus pointues, eu égard au volume d'informations en croissance exponentielle. La médecine qui a commencé par être une branche unique, puis s'est divisé en plusieurs spécialités, lesquelles spécialités tendent à s'éclater à vue d'œil en des spécialités de plus en plus étroites… Cette tendance qu'a la médecine à s'émietter a bien entendu d'autres causes ; par exemple, le fait d'être un spécialiste en quelque domaine de la médecine a de nos jours un grand prestige social.

Mais la sur-spécialisation est désastreuse, d'une part, quand il s'agit de vouloir guider une recherche pluridisciplinaire ou de mettre en oeuvre des politiques concernant la médecine où la décision et l'action requièrent un jugement qui, finalement, doit être synthétique et non analytique. D'autre part, un malade n'est pas une simple juxtaposition d'organes ou de fonctions, mais une organisation vivante, cohérente, où les relations entre les parties sont au moins aussi importantes que les parties elles-mêmes.

L'introduction des nouvelles technologies pour traiter l'information médicale et aider à la pratique médicale, n'a pas encore apporté tous ses fruits. Comme le suggère le professeur François Grémy: "l'essentiel de l'impact des sciences de l'information en médecine est d'abord et avant tout culturel. Si les premiers contacts entre elles et la médecine n'ont pas été décisifs, et ont même été grevés d'échecs, c'est qu'ils ont été l'occasion de l'affrontement de deux modes de pensées très différents au départ. Les sciences de l'information ont représenté pour les médecins un miroir assez cruel des faiblesses méthodologiques de la médecine... Ceux parmi les médecins qui ont eu le courage de faire face à ce constat, et qui ont compris l'apport hautement significatif du nouvel outillage mental que l'informatique représente, se sont livrés à une remise en cause profonde de leur mode de pensée. Ils ont compris que devenait nécessaire un nouveau regard de la médecine sur elle-même: une méta-médecine...".

Bref, une révolution scientifique en médecine est nécessaire pour résorber la crise de croissance qui la traverse depuis la fin du XXe siècle; mais elle ne peut se faire que par une réorganisation des disciplines médicales utilisant des « niveaux méta » de la connaissance médicale, et par une intégration adéquate des sciences cognitives…

Reply to Abdelkarim Fourati
Helga Nowotny
May 8, 2003 20:56 UT

Medicine - and modes of doing medicine - have always been interesting, since diverse settings and practices have to be brought together: the bedside, the hospital, the clinical lab or, to put it differently, the patient, the sick or even the dead body, as well as experimental medicine in which one can control and manipulate changes. John Pickstone in *Ways of Doing* argues that medicine also allows different styles, or cultures of inquiry to be brought together, ranging from the biographical/hemeneutical (the patient's history as told by him or herself), the comparative/analytical in the hospital setting, but also the synthesis/experimental style of making and inventing new things. While specialization is always threatening when it is not put back to fit the patient as human being, it is obvious that we need 'ways of seeing' and 'ways of doing' that allow to bring the various cultures or styles of inquiry together again.

The Transition to Transdisciplinarity
Julie Klein
May 6, 2003 17:48 UT

It's good to have Helga Nowotny on board, since she brings to the seminar a major historical development in the history of interdisicplinarity. I'd like to start where Helga starts, with her comment that transdisciplinarity is a theme which "resurfaces time and again." Recently, it has taken some striking turns. The term is conventionally dated to the first international conference on interdisciplinarity, held in France in 1970. The definition adopted by conference organizers was a comprehensive framework that transcends the narrow scope of disciplinary worldviews through a comprehensive and overarching synthesis. General systems, structuralism, Marxism, policy sciences, feminism, and sociobiology have been leading examples. Other definitions emerged in the ensuing decades, including a new structure of unity informed by the worldview of complexity in science (in the work of Basarab Nicolescu and the Paris-based CIRET), a new mode of knowledge production that fosters synthetic reconfiguration and recontextualization around problems of application (in the original Gibbons, et al. Mode 2 thesis), and collaborative partnerships involving public and private sectors in research on problems of sustainability (in the Zurich 2000 conference on transdisciplinarity). In the past, the term was not used often in humanities, but it has been appearing increasingly as a label for new knowledge formations rooted in cultural critique (in women's studies, cultural studies, and a variety of other fields that bridge humanities and social sciences).

While there are significant differences in some of these definitions – around whose philosophy to follow and which problems will be addressed -- in all cases the discourse of transdisicplinarity is truly "trangressive," to borrow Helga's emphasis. Today's transdisciplinary initiatives demand movement beyond older forms of interdisciplinary cooperation and a radical blurring of all boundaries (not just the divides of disciplines but the gap between the academy and the agora). They also underscore the heterogeneity of knowledge. There is no longer, as Helga points, out a single hierarchical formation. These initial threads for our second seminar recall to mind several developments over the course of the Sperber seminar.

The more we talked, the more prominent problem-focus became as a driving force in our conceptualization of interidsicplinarity. The nature of problems differed (from abstract intellectuality to the Lebenswelt). The scope differed (from small intellectual questions to large-scale social issues). The structures in which problems were addressed differed (from small projects to centers to the formation of new fields). Yet, problems (not interdisciplinarity per se) emerged as a common point of reference. We also moved to a position of heterogeneity. A variety of structures, concepts, and methods are now available to the researcher, even if particular cognitive and social constraints dictate one choice over others. And now, with transdisciplinarity, we kick the door open more, problematizing not only disciplinarity but interdisciplinarity as previously understood. As we talk this month, I hope we'll also keep a question that Dan raised in the first seminar alive. How radical are the changes we are discussing? Is there indeed a fundamental change taking root?

Reply to Julie Klein
Helga Nowotny
May 8, 2003 21:00 UT

Thank you for linking this discussion back to the previous one with Dan Sperber and putting 'problem focus', the joint definition of problems and working for a joint solution, back to where they belongs: center-stage. We are only at the beginning, I submit, to understand in how many different ways a problem might be sliced or to get a better glimpse into the different ways of framing it – and beginning to analyze the consequences. Problems (and their definition) can be 'owned' by certain groups, or so they may claim, and some 'solutions' will merely shift it to another department or another area of responsibility. This is where a transdisciplinary approach holds great potential in making these tacit assumptions explicit and rendering some of the invisible moves visible.

Heterogeneity, as mentioned by Julie, can already be overwhelming. Can we, should we, go one step further and 'kick the door more open'? But is this not one of the deeper roots of scientific curiosity as well? Are we not obliged, in the sense of obliging ourselves, to keep raising questions, even if ready answers are not yet at hand? Should we not start to think what the transition to transdisciplinarity would mean – for instance in designing a curriculum, with problem-focus and problem-choice at the center, with all the implications to our students and ourselves?

Fundamental changes in knowledge production?
Grit Laudel
May 9, 2003 3:15 UT

Julie asked a clear question which hadn't yet been answered: "How radical are the changes we are discussing? Is there indeed a fundamental change taking root?"

One of the big problems with the Mode 2 concept is its theoretical fuzziness. It never became clear in their texts if the authors claim a world-wide displacement of Mode 1 knowledge production by Mode 2 knowledge production. In the subsequent reception of the concept by STS researchers it was mainly interpreted this way (e.g. Weingart 1997; Godin 1998, Shinn 1999). To answer Julies' question, up to now, there is no empirical evidence for a fundamental change that encompasses the whole science system. The authors neither give a proof for such a fundamental change in their 1994 book, nor in their 2001 book. The lack of empirical proofs was one of the major critiques, formulated by one of the panel members, Peter Weingart (1997). Given this poor empirical background, one could just ignore Mode 2. But there is a danger that comes from a completely other side: Science policy is starting to overtake this concept and to turn it into a "self-fulfilling prophecy" (Shinn 1999: 172-173, Gläser 2000: 462-463). Transdisciplinarity, socially robustness might then become criteria for funding research whether or not it makes sense.

Gläser, Jochen, 2000. Limits of change: cognitive constraints on "postmodernization" and the political redirection of science. Social Science Information 39: 439-465.

Godin, Benoit, 1998. Writing Performative History: The New New Atlantis? Social Studies of Science 28: 465-483.

Shinn, Terry, 1999. Change or Mutation? Reflections on the Foundations of Contemporary Science. Social Science Information 38: 149-176.

Weingart, Peter, 1997. From "Finalization" to "Mode 2": Old Wine in New Bottles? Social Science Information 36: 591-613.

Transdisciplinarity: A brave new social epistemology?
Steve Fuller
May 7, 2003 0:04 UT

I must not be alone in finding the entire Modespeak strategically vague on issues relating to the future of academic knowledge production. (Perhaps that's how they like it in Brussels.) Helga Nowotny talks a lot about 'radical' and 'revolutionary' transformations occurring, but really is there anything more to her endorsement of 'transdisciplinarity' than the de-privileging of the university as the main site of knowledge production? And what's so good about that?

At least, 'interdisciplinarity' had the virtue of supposing that whatever forms of knowledge had not been covered by traditional academic disciplines could be tackled by combining – and perhaps even transforming – two or more disciplines. In contrast, 'transdisciplinarity' seems to imply much more than the obvious idea that many socially relevant problems arise outside the research agendas of academic disciplines. It also seems to deny any special role for the university in resolving these problems or capturing the knowledge that is produced in the process.

Somewhat in anticipation of my October paper for this conference, I think it's very important to defend the university as more than a glorified car park (a.k.a. 'agora') that provides a mutually convenient location for the state, industry, and experts to manufacture some mutually beneficial knowledge. The university is in the business of producing knowledge as a 'public good', which means (among other things) that whatever knowledge is produced is made as widely available as possible. This charge is much more proactive than allowing knowledge to 'seep' (or should I say 'trickle down') from its original networks to those lucky enough to capture it. Yet, this state-of-affairs appears to be an aspiration of Modespeakers like Nowotny.

I am very struck by the lack of attention to power relations in Nowotny's discussion of the agora. What enabled the agora to function as an exemplar of democratic governance was that only Athenian citizens – i.e. mutually recognized peers – could participate. In the agoras envisaged by Nowotny, there are often considerable power asymmetries among the parties. For example, when Nowotny says that the best scientific solution is the one that takes society into account, she appears to mean something much less egalitarian than it sounds – namely, the incorporation of potential consumers in the design of a new product. In this way, we 'anticipate future controversies where the products of science and technology might be refused and contested'. If this is so, we have reached an Orwellian situation – perhaps befitting the bureaucratic world where Modespeak thrives: 'Giving voice' becomes identical with co-optation, and the mark of democratic science governance is that criticism is not expressed and addressed but pre-empted and contained.

> **A Little Less Generalization, Please**
> Julie Klein
> May 7, 2003 21:31 UT
>
> With a Huxley-inspired flourish -- mocking transdisciplinarity as a “brave new social epistemology” -- Steve Fuller lumps together a complex set of issues into “the entire Modespeak” and its community of “Modespeakers.” A little generalization is in order. Steve asserts that transdisciplinarity de-privileges the university as the main site of knowledge production and capturing the knowledge produced in problem solving. It is about a great deal more, and even extreme versions should remind us of the dangers of academic myopia. The university, from its inception, has deprivileged many forms of knowledge that expand human consciousness and contribute productively to solving human problems. How many struggles should I cite? The attempt to legitimate alternative practices in health care? The attempt to legitimate the knowledge of other cultures? The attempt to use the Internet for building new public spaces? Furthermore, if the university is, as Steve says, in the business of “producing knowledge as a 'public good,’” then why does the ”public” continue to be deprivelged in the status hierarchy of academic knowledge? Why do academics continue to write in a language inaccessible to a wider sphere? Why do community service and political work continue to be discounted in the academic reward system? Why do the walls keeping the surrounding community at bay remain high, theorizing into jargon-ridden abstraction “socially relevant problems”?

As for Helga's appealing to something less egalitarian than it sounds – "the incorporation of potential consumers in the design of a new product" – the original Mode 2 hypothesis in NEW PRODUCTION OF KNOWLEDGE was heavily oriented to industrial application. True. However, in RETHINKING SCIENCE, Helga and colleagues responded to some of the criticism and moved in a wider direction. We ought to be looking there and admitting an even larger family of examples into our discussion.

As for Steve's charge that there are often "considerable power asymmetries among the parties" in the agoras Helga envisages, his counter history lesson is not convincing. Steve tells us "What enabled the agora to function as an exemplar of democratic governance was that only Athenian citizens – i.e. mutually recognized peers – could participate." If you were a woman or a slave, you were not a "mutually recognized" peer, hence ineligible for citizenship and its attendant privileges. A clear example of preemption and containment, if ever I saw one. One of the benefits of the new push for transdisciplinarity, for all its accompanying difficulties and legitimate points of critique, is giving a say to once-silenced voices.

I'd add that the explosion of new internet forums should remind us of the need for agreement on rules of communication in such forums. Spirited debate is one thing. Demeaning the notion of the agora as the glib notion of "a glorified car park (a.k.a. 'agora')" does not contribute productively to understanding. Nor does the over-generalization of an Orwellian world "where Modespeak thrives," or mocking 'Giving voice" as nothing more than "co-optation." A little less satiric generalization and more complexity of facts in argument, please

Pro Agoraphobia
Steve Fuller
May 8, 2003 5:25 UT

With all due respect to Julie, I am only working with the material I am given. Transdisciplinarians are the classic lumpers, mixing so-called emancipatory science with client-driven research. The Modespeak literature does little to distinguish the two. Moreover, the fact that 'transdisciplinarity' has many different meanings does not necessarily mean that the phenomena it refers to is complex. It may simply mean that the term is vague, intentionally or otherwise.

Nevertheless, as I expected, transdisciplinarity is instinctively anti-university – at least judged by Julie's response. The points she raises about the shortcomings about universities are of course correct, but transdisciplinarity as conceived by Modespeakers is at best a stopgap solution to them. Ultimately one needs to INSTITUTIONALIZE inclusiveness and openness and all the other democratic virtues, and universities have been historically the best knowledge producing entities to do that. 'Affirmative action' is something that universities – not think tanks, R& D divisions, research parks, or even self-organizing networks -- institutionalise. Moreover, Modespeakers talk up a storm about networking but little about consolidating the knowledge that networks produce so that it is made generally available.

Julie's response to my critique of 'agora' is interesting because she brings up the hidden asymmetries in the concept that are no less pertinent to transdisciplinarity. What she says is unwittingly one more reason for abandoning the word 'agora'.

I'm also sorry that Julie thinks I am overstepping internet etiquette, but I actually do think – until proven otherwise – that 'giving voice' is tantamount to 'co-optation' in Modespeak. Or at least, Modespeakers can't really tell the difference between the two states. (Maybe it's just me, but I would like to go on the record as having said that.)

Finally, I like examples just as much as the next person, but there is no inherent virtue in proliferating examples, especially when it's not clear what they have to do with each other – except the label 'transdisciplinarity', in this case. I mean this not only as a conceptual point. It has political implications as well. The proliferation of 'agora' in scientific decision-making is only superficially democratic. One of the cleverest ways the UK government has to do want it wants is to run multiple consensus conferences, focus groups, internet polls, town meetings, etc. Of

course, each of these events reaches somewhat different conclusions, and so the government can decide what it wishes – all along claiming popular support.

Reply to Julie Klein
Rainer Kamber
May 8, 2003 19:52 UT

Yes, "the university" presumably has done some of the things Julie claims it has - as has any social institution that is founded on specific norms regarding external access to its resources. If one of the basic and typical resources of academic research is the ability to produce reliable knowledge in a (more or less) systematic fashion then that includes the adherence to methodologically basic standards regarding all knowledge claims raised within it. Any party will obviously be excluded from this resource that is unable (for whatever reasons) to adhere to such internal standards. Thus academia cannot reasonably be viewed as some kind of organism that functions mainly through the oppression of "other" kinds of knowledge producers: it simply applies its internal norms regarding knowledge production to all knowledge claims, be they internal or external. To simplify such complicated matters even more I want to suggest that this makes reasonable sense, as it does for any other social institution. It is a completely different matter wether these standards are in any way justified (or, for that matter, wether they are justifiable at all). To my mind and within some kind of "naturalized" framework it seems reasonable, too, to assume that these standards arose in an inductive manner out of experience about how best to go about producing reliable knowledge. (I am, of course, perfectly clear about the fact that knowledge is not the only thing "the university" produces.)

Obviously, what Helga Nowotny claims is that (1) reliable knowledge is not enough - without giving more than very general opinions about the contemporary character of the relationship between science and society (so much for "generalizing"). She also explicitly claims (2) that socially robust knowledge will in some way represent better knowledge - mainly by argueing through an analogy about technical (i.e. applied) knowledge. But "better" regarding what standards? And does the analogy hold? I simply cannot help but take a stand with Steve Fuller here since I, too, feel that the blame about too much generalization has not been accurately directed.

A remark regarding the agora. I believe that what Steve has suggested was exactly that it remains an open question wether the concept of the agora that Helga Nowotny envisages will be able, in principle, to avoid preemption and containment. One of the more disconcerting characteristics in participatory experiences in knowledge production seems that, e.g., it often lacks basic procedural structures to ensure the representative participation of those concerned in any given area of research. Furthermore, much experience also seems to show, that solutions reached through participatory knowledge production not only are extremely resource-intensive but that it remains often unclear wether this format was actually able to contribute substantially to sustainable solutions.

To be clear: I am convinced that the application of participation in knowledge production can contribute positively to the development of science. But I am so far simply unconvinced regarding Nowotny's claim that it will play any substantial role in this development - or that it even should take on such a role.

Reply to Steve Fuller
Helga Nowotny
May 8, 2003 20:53 UT

I don’t know what I wrote, or have failed to write, to become the target of Steve Fuller’s insinuations and attack. Let me therefore go through your text, Steve.

1. Brussels and the de-privileging of the university: I don’t know what they like or not in Brussels, nor do I speak for them - whoever ‘they’ are. I fail to see why transdisciplinarity would de-privilege the university. Quite to the contrary, if more transdisciplinarity would be practiced inside, this would greatly boost universities. Or what exactly are their privileges?

2. I don't understand why anything I said would 'deny any special role for the university in resolving these problems or capturing the knowledge that is produced in the process'. Again, my intervention is an appeal also to universities to capture or re-capture their capacity in problem-solving, even if departemental lines have to be crossed or if you need to speak to people outside the university.

3. If you want to equate the university with a glorified car park, you may do so. I don't. Universities are, as Steve puts it "in the business of producing knowledge as a 'public good, which means (among other things) that whatever knowledge is produced is made as widely available as possible". Agreed. But universities are not the only ones to produce knowledge, not even as a public good, nor do they hold a monopoly on it. And how will you make the knowledge produced widely available and to whom without engaging in a kind of agora-like behaviour and setting and without resorting to some kind of transdisciplinarity?

4. I did not realize that it is mandatory to mention power asymmetries everywhere, but Steve need to be only half-disappointed: they are to be found on p. 211 of Re-Thinking Sciene. Power does matter and who would deny it? But what follows from this observation? Orwell can be conjured up at any time, just as abuse and co-optation always remain possibilities. In fact, we might end up in a state of totalitarianism again. But I do not think that this will be brought about, because we 'give voice' to people who did not speak before or who do not have the means or access to articulate themselves.

Finally, you do not have to like 'Modespeak', Steve, but please, don't give me 'Oldspeak'.

Pro-Exemplar: Replying to Steve
Julie Klein
May 8, 2003 21:32 UT

I am reminded of the objection to generalization that arose in the Sperber seminar, from individuals impatient with etymological and epistemological abstraction. While I enjoy the latter, their push to particularity was welcome. It moved us to a useful discussion of specific contexts and formations, capped by several astute comments from Jochen Glasser. I take Steve's point about transdiscipinarians being "classic lumpers," but somehow missed the announcement of a solidarity among "Modespeakers." (Who else do you have in mind, Steve?),

Moreover, while acknowledging Steve's point that the multiple meanings of transdisciplinarity might render it vague – hence, terminological muddle -- the phenomena at stake indeed complex, a realization that Basarab Nicolescu has done the most to teach us.

I don't share Steve's view that transdisciplinarity is "instinctively anti-university," either, but do buy his argument about the necessity of institutionalization. In that vein, I'd like to invite members of this seminar to contribute examples of institutionalizing inclusiveness, openness, and kindred virutes outside the university in a forum we are presumably to be testing – the Internet. Are there electronic communities where 'giving voice' is not tantamount to 'co-optation? New information technologies facilitate networking on an unprecedented scale, and in the name of this month's topic. A search of the Internet reveals a multitude of websites using the descriptor "transdisciplinary." There are sites dedicated to learning assessment, arts education, distance education, mental health, rehabilitation, special education, children with multiple disabilities and pain management. The term also appears on sites for engineering problems, ecological economics, human population biology, language and thought, preparation for teamwork and collaboration, systems science, cybernetics and infomatics, and knowledge organization. Do they have any promise of transformative change or are they only minor and fleeting conversations?

Two sites, in particular, might provide a comparative test case. The first, the Centre International de Recherches et Etudes Transdisciplinaires (CIRET) is a vitual meeting space for specialists from different sciences and other domains of activity, including art, industry, and education. CIRET publishes a journal devoted to transdisciplinarity, disseminates results of UNESCO-sponsored international colloquia (including the First World Congress on Transdisciplinarity in

Arrabida in 1994 and the 1997 Locarno Congress on “The Transdisciplinary Evolution of the University”), and presents theoretical works on the nature of transdisciplinarity and reports on practical developments in France, Spain, Romania, Brazil, and other countries. (). The second, the Swiss Academic Society of Environmental Research and Ecology (SAGUF), involves researchers and non-academic partners in transdisciplinary projects in Switzerland and abroad. SAGUFNET provides bibliography and information services, a forum for discussions, links to other pertinent sites, and an introduction to the topic of transdisciplinarity written by Christoph Küffer (http://www.transdisciplinarity.ch). Both SAGUFNET and CIRET are multi-lingual international electronic sites, so, to reiterate, provide a good tests of one of the questions we are to be exploring.

the Question of Inherency: Replying to Rainer
Julie Klein
May 8, 2003 21:35 UT

Rainer wrote -- “Thus academia cannot reasonably be viewed as some kind of organism that functions mainly through the oppression of "other" kinds of knowledge producers: it simply applies its internal norms regarding knowledge production to all knowledge claims, be they internal or external.”

I agree that is not a „main function,“ but the history of the academy is replete with strong and sustained efforts to repress some forms of knowledge and their producers. Perhaps I misunderstand what you meant (and please correct me if I have), but I don’t see that as a "simple" application of norms to all claims, raising the questions of whether there are incommensurate claims and whether justification is not separate but inherent in any knowledge practice (akin to question raised by others about whether responsibility is inherent in science).

Transdisciplinarity: the great meme machine
Steve Fuller
May 12, 2003 16:09 UT

I thought I would have some peace this weekend in Berkeley at a conference on ‘the utility of the history of science to scientific practice’ (how Mode 2!). But Helga seems to have sprung into action, perhaps by the unflattering light in which I cast transdisciplinarity. But of course, she may have been previously busy…

First, I’m not sure what Helga means by ‘Oldspeak’, but we’re all in serious trouble if talk of power asymmetries has become passe’ in Modespeak. My reference to ‘Brussels’ is about the buzzword-status of Modespeak in European science policy circles (including even the UK). Call it ‘memes’, except as several interlocutors have remarked, the distinctiveness of the spreading jargon is not matched by an equal clarity of the conveyed concepts.

This state-of-affairs is not accidental. There is an advantage to keeping the parameters of transdisciplinarity vague – and not simply because the world is a rapidly changing, heterogeneous place (when hasn’t it been?). Rather the language needs to be adaptive in a changing political environment. In the European context, there are many old social democrats trying to reinvent themselves as neo-liberals, and Modespeak is very good for that purpose, since its political horizons range from a pure contract-based, market-driven research agenda (neo-liberal) to a more traditional welfarist, socially responsible research agenda (social democrat).

In this respect, I find the radical-sounding rhetoric that Helga invokes in her responses somewhat misleading (e.g. ‘breaking the door open’), since the reality of transdisciplinarity on the ground tends to be much more accommodating, flexible, even pliable. Moreover, for all the rhetoric of transcending traditional limitations, especially in academia, Modespeakers don’t show a lot of interest in – or perhaps they believe it’s impossible to gauge – the overall effects of these multiple cross-cutting alliances, networks, etc. Is knowledge really flowing in some more equitable, democratic fashion, or is it more an opportunistic ‘capturing’ of knowledge by those who can?

For example, while I strongly endorse the use of consensus conferences and citizens juries, I also believe that they should be binding on legislators in some constitutionally defined fashion, with the possibility of future reversal, if the consequences of pursuing their decisions turn out to be negative. What I don't endorse is the endless proliferation of 'voicings' that serve no clear political purpose than simply allowing the public to let off steam and allowing politicians convenient excuses to do whatever they want. Is innovation at the level of consultation matched by innovation at the level of decision-making?

Put it this way: The problem with letting a thousand flowers bloom is that some of them may turn out to be weeds and strangle the other flowers. But this is only a problem for gardeners and not seed merchants. I worry that like the Greek Sophists, who did their best business in the agora, Modespeakers are more merchants than gardeners.

In defence of an idea of the university
Peter Plöger
May 13, 2003 21:51 UT

I thank Steve Fuller for directing our attention to the challenge of socially relevant problems that the academic world has to face – but often doesn't. Indeed, I think these are the kind of problems that should be the starting point for any knowledge production that is intended to be transdisciplinary, as it is the most urgent problems that cannot be solved by one discipline alone. To put it differently, transdisciplinary knowledge production must be sensitive to social relevance. This seems to be a different notion of our key word than the one oriented at the "context of application" Helga Nowotny proposes.

You can use "modespeak" to talk about a lot of sympathetic things: giving voice to the yet unheard, giving those affected by the products of science a share in decision-making, opening up borderlines, building networks, ... You cannot use "modespeak" to talk about all this as desirable ends. However, all this is desirable and even necessary "to produce a better science". If that was what a considerable part of academic knowledge production really was like (which still remains to be shown by the advocates of the Mode 2-thesis), society would surely be better off. If that is not, why are there no normative considerations on the future of the academy and its relation to society? Transdisciplinarity, after all, is a normative notion as well. Seen from this point of view, "modespeak" is wishful thinking without talking about the wishes.

As a site for the "normative type" of transdisciplinarity oriented at social relevance, the university is best suited (although there might be better sites to come). Universities, with their combination of research and liberal education on a background of a multitude of academic fields, provide in principle an ideal basis for the production and dissemination of transdisciplinary knowledge. In this interpretation of transdisciplinary knowledge as a societal need, it is the most precious public good the universities can produce. In practice, modern universities face a host of serious structural challenges: - internal differentiation, partly effected by the demand for vocational training for a highly specialized job market, makes it harder to teach academic disciplines and necessitates new forms of self-governance; - student numbers have long exceeded the universities' capacities; - competition for resources favours applied research, and in consequence forces the universities to restructure their curricula; - etc. All this is of ambivalent value to knowledge production and dissemination. However, I suspect that these developments, taken to their very consequences, might impair the function of the university as a site for producing socially relevant knowledge that ignores disciplinary borders. That means, it would impair its function to counterbalance one-track social change that decreases the ability of our society to cope with the multi-faceted problems it faces. Again, Steve is right to ask how the challenges that universities are forced to cope with, relate to the changing structures of knowledge production described by the Mode 2-thesis. If there is indeed a positive correlation then he has been right in warning us about the ambivalent nature of mode 2-science and the threat it poses to the integrity of the universities. It seems to be time to pose an old question anew: What idea of the university do we have?

What is 'Oldspeak'? Reply to Steve Fuller PART I
Helga Nowotny
May 14, 2003 11:46 UT

I have tried to figure out where the disagreements between Steve and myself lie. Maybe this is what I have been busy with, not knowing that I would unintentionally thereby disrupt Steve's quiet weekend…

First, it is a matter of personal style and temperament. I do not like insinuations and the agressiveness they carry, since each insinuation is slippery, giving rise to some subtle, only half-stated accusation. Then, I either have to defend myself against something that has never clearly been stated, nor has it explicitly been raised against me (in this case, why should I defend myself) or I ignore it and will be seen as arrogant. But this is merely a difference in personal style.

More substantively, I came to the conclusion that the disagreement is about thinking in dichotomies or not. Maybe "Modespeak" is a way to overcome dichotomies. For Steve, you are either a 'gardener' or a 'seed merchant', and he leaves no doubt what is the morally preferable choice. You are either for 'universities' or you are 'de-privileging' them. You are for the commercialization of research, or you are defending the very idea of science. You are either an old social democrat trying to reinvent yourself as a neo-liberal, or you are an old traditional welfarist with a social responsible research agenda. You are for 'Brussels' or against 'Brussels', for reasons that, as Steve tells us, have to do with the "buzzword status of Modespeak in European science policy circles". The underlying pattern of thinking in dichotomies is always the same: you are either for us, or against us. You are either on the 'good' side or you belong to the other, derogatory category. This is what I call "Oldspeak". It may be enjoyable up to a point, but I think it carries some grave risks.

Reply to Steve Fuller PART II
Helga Nowotny
May 14, 2003 11:49 UT

The greatest risk is that it may impede us collectively to move forward, since it is easy to remain stuck in the old categories and memories of battles fought in the past. I do not believe that the world can be carved up in such simple dichotomies.

Yes, it is heterogeneous, complex and messy. It keeps on changing, and so do the circumstances in which we employ language. Yes, power asymmetries matter, but we have to take them into account and move on nevertheless. Yes, we should worry about possible abuses and threats to the democratic order, but I also believe that we have an obligation not only to warn, but also to act in whatever limited environment our actions might make a difference, on however small a scale.

If, to take Steve's example, consensus conferences and other forms of encouraging cross-cutting alliances merely provide politicians with convenient excuses to do whatever they want, why do we not speak up publicly as social scientists or STS persons and act accordingly? Many of these "endless proliferations of voicings" would not proliferate at all, were it not for the active professional involvement on the part of a considerable number of social scientists who gain their living this way, probably on short-term contracts which they therefore have good reason to denounce.

But if we, as social scientists, had the courage to speak up publicly that these exercises are only a waste of public funding and a political scam, the general public, the media, research councils and politicians would rightly expect us to come up with alternative proposals. If they consist in making them "binding on legislators", fine, but then we will first need to consult with lawyers and politicians alike. We would have to engage in some kind of transdisciplinary exchange, defining jointly what the problem is, what our experience as social scientists with these many experiments has been and argue for another solution that will hopefully prove to be more socially robust than the present fight between "weeds" and the flowers they strangle.

Maybe, this makes me sound like an old-fashioned social democrat, nostalgic for long passé ideas of social engineering. But I am only trying to find the consistency in Steve's arguments. Mine have been stated, maybe too vaguely, in "The Potential of Transdisciplinarity" , in my other writings and in my replies in this debate. I do not have 'the solution', but I have strong ideas about the direction in which I think we should go.

What a pleasant surprise -- some agreement!
Steve Fuller
May 15, 2003 17:41 UT

Let me thank Helga for putting up with my 'impolite' manner – it's an old Enlightenment thing (Goldgar 1995). However, I think it gets results.

First, I don't see an inherent problem with postulating binary oppositions, their harshness notwithstanding. After all, I seem to recall that Mode 1 vs. Mode 2 is a binary that managed to get this entire discussion off the ground. And I didn't invent it!

I must say, though, overall I am pleasantly surprised by the general tenor of Helga's remarks, which include a denunciation of the 'endless voicings' and a restatement of social democratic ideals, relatively unadulterated by neo-liberalism. I am surprised because the short-term contract research environment that we both condemn – in which these voicings flourish -- is what I think most people think of as 'transdisciplinary' research. In this connection, I am surprised that Helga in her capacity as spokesperson (I hesitate to put it more strongly) for transdisciplinarity has not done more to distance herself from these developments, with which her work is often associated.

I myself do criticize this work in print – along with providing some kind of analysis of the situation (e.g. Fuller 2000a: chap. 5; Fuller 2000b: chap. 7; Fuller and Collier 2003: New Introduction). But I take the point that it is necessary also to become involved with constructive proposals. I have been a strong advocate of consensus conferences in the UK, often against resistance of social science colleagues who basically want to pursue the 'endless voicings' paradigm (Fuller 2001). I have also been supportive of Japanese STS colleagues with a similar agenda. Peter Ploeger from Bielefeld, who has recently entered the discussion, is someone who has actually tried to marry concerns about the future of the university with the consensus conference format.

Helga is exactly right that the issue of institutionalization eventually brings us to issues concerning lawyers and politicians. And this is often the main obstacle because they people don't want their hands tied -- or at least we have not found a way of persuading them that it's in their interest to have their hands tied! Instead, they'd like to see consensus conferences as an all-purpose social technology, a kind of glorified focus group. As long as our social science colleagues encourage this perspective, we'll have an uphill struggle.

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Pour une Transdisplinarité Unie
Debono Marc-Williams
May 7, 2003 21:38 UT

English Abstract: I agree with the author that knowledge is transgressive and that the potential of transdisciplinarity is to introduce new links between producers (and users) of knowledge. The consideration of new metaplastic paradigms in which all different forms of perception are taken into account could be a way to transgress such a knowledge. However, interdisciplinarity, even as well developed as in cognitive sciences, is not sufficient to treat the transversality of knowledge. This has been clearly shown in the manifesto of transdisciplinary published by B. Nicolescu who develop cross-arguments about the different levels of reality implied throughout the value-chain of the transdisciplinary act.

Je suis d'accord avec l'auteur sur le fait que la connaissance est transgressive et que le potentiel de la transdisciplinarité consiste a introduire de nouveaux liens entre producteurs - et utilisateurs – de connaissances. La considération de nouveaux paradigmes métaplastiques au sein desquels les différentes formes de perception seraient prises en compte irait dans le sens de cette transgression. Tel parait-être le cas des réseaux art-cognition ou observant les comportements émergents des systèmes complexes. Tel est aussi l'enjeu des sciences cognitives cherchant a comprendre la nature de la conscience. Toutefois, la pratique interdisciplinaire, telle qu'elle est présentée, ne peut répondre stricto-sensu à cette attente dans la mesure où les niveaux de réalité visés ne me semblent pas correspondre à ceux qui permettraient d'ébaucher une ascèse vers la transgression.

De fait, le préfixe trans signifie implicitement adopter une perspective transversale, se saisir de ce qui se situe entre et au-delà de, ne pas quitter le lien générique qui permet d'édifier des ponts naturels entre les disciplines et aller vers une transposition des grilles de lecture de la réalité. Cette approche de la transdisciplinarité distinguant les niveaux d'organisation des niveaux de réalité et intégrant le tiers-inclus (qui n'est pas un 'third attribute of Mode-2') a été clairement explicitée dans le manifeste de Basarab Nicolescu (http://perso.club-internet.fr/nicol/ciret/index.htm). Je ne doute pas que l'objectif de Helga Novotny soit comme le nôtre d'aller dans le sens d'un décloisonnement des disciplines. Les processus co-évolutifs qu'elle cite ou encore ce que René Thom désigne comme un principe général tendant à universaliser le concept 'mathématique' de transversalité en sont de bons exemples.

C'est pourquoi nous devons, en aval d'une réflexion épistémologique, aller ensemble vers la mise en place de schémas auto-cohérents observés dans nos disciplines respectives, les articuler à des modèles communs, puis valider ces prédictions par une série d'expériences interactives réalisées dans et entre ces disciplines. Ainsi, nous pourrons ouvrir, par la superposition de champs expérimentaux concrets, des voies de recherche nouvelles permettant d'appréhender les phénomènes dans leur globalité, sans pour autant renier la spécialisation de chacun.

A Broader View of Transdisciplinarity
Joseph Brenner
May 8, 2003 7:06 UT

The title of this new thread is taken from a "Manifesto" signed by a few people consequent on the Conference on Transdisciplinarity in March, 2000. The views expressed in it may be controversial, as are all views, but they are those of a substantial number of thinkers, grouped around the International Center for Transdisciplinary Research and Study in Paris (CIRET;http://perso.club-internet.fr/nicol/ciret/) Summarily, this approach to transdisciplinarity seeks a balance between real-world applications and a logico-philosophical basis that avoids a potentially reductionist pragmatism. People who would like to discuss some of the "academic" aspects of transdisciplinarity (which may in fact be much more "practical" in the long term), may wish to comment. Also, it might be useful if the above site were added to the links of the "interdisciplines" home page.

a few words from a fascinated observer
Karen-Claire Voss
May 8, 2003 13:52 UT

A few words from a fascinated observer:

While I applaud this recent interest in transdisciplinarity from what I have read of the discussion on this site so far it regrettably seems to me that transdisciplinarity is being regarded as just the latest in a series of fleetingly fashionable concepts. There is nothing in Helga Novotny's presentation of transdisciplinarity, for example, or in any of Julia Klein's writing, which indicates that unlike disciplinary, interdisciplinary or multidisciplinary approaches, the transdisciplinary approach actually demands (and if used as it should be inevitably results in) ontological change on the part of the researcher. In what way? Using the transdisciplinary approach requires that the long forgotten Subject be included and the implications and effects of that are endless.

Of course even my making a statement like the one I just made in a context like this is considered completely out of bounds. Indeed, it is out of bounds. However, hasn't Novotny remarked on the "transgressive" character shared by knowledge and transdisciplinarity? Transdisciplinarity does have a transgressive character which is precisely what makes so many persons within the academy extremely uncomfortable with it and why discussions of it are so frequently limited to talking about its implications in terms of things like the distribution of knowledge (directional—i.e., from the top down or not, and otherwise—e.g. across various groups such as "laypeople" and "academics.") as though knowledge was some kind of commodity, some object one could purchase or obtain). The examples of transgressiveness that Novotny offered—that of the "resurgence of NGOs" and the "other ways in which various kinds of stakeholders organise in shaping social reality" do absolutely nothing to convey the profoundly radical character of transdisciplinarity.

These are some thoughts off the top of my head as it were, but I fervently hope that they will function to spark some meaningful discussion about the issues I have raised.

Finally, since most of the persons who will be reading this do not know who I am, let me say by way of introduction that I am a historian of religions who specializes in esotericism, who encountered transdisciplinarity ten years ago, and who subsequently (and as a direct result) abandoned her hitherto relentless climb up the academic ladder as well as her efforts to "establish" herself in the academy as yet another "academic entrepreneur." Instead, I ended up moving to Istanbul where I work as an independent scholar and writer. I am utterly devoted to "learning how to "conjugate the verb 'to be,'" as Basarab Nicolescu once put it.

Karen-Claire Voss Former Adjunct Professor of Religious Studies San Jose State University, San Jose, California Independent Scholar and Writer, Istanbul Member of CIRET

A reply to a fascinated observer
Clive Graham
May 18, 2003 4:41 UT

I have for some time sensed a division concerning transdisciplinarity. Transdisciplinary knowledge production is clearly defined in The New Production of Knowledge (1994). Indeed, critics do not deny the transition from Mode 1 to Mode 2, just the timing and novelty of it. In Rethinking Science (2001), Nowotny et al extend Mode 2 from contextualised to transgressive knowledge production although this work does not expand upon the earlier definition of transdisciplinarity.

Transdisciplinary knowledge production advanced by Basarab Nicolescu is not based on observation but on a vision. This does not make Nicolescu’s concept any less worthy. But in CIRET's Moral Project (1987 modified 1999), Nicolescu refers to transdisciplinarity as “the return to a Golden Age”. But is not this a utopian concept? And CIRET defines transdisciplinarity as opposed to all globalising projects while seeming to promote it as a global meta-narrative. I genuinely admire Nicolescu’s concept of the included middle to break the disciplinary bind of classical scientific logic, but the rest seems most inappropriate in a post-modern world and may explain why Nicolescu unwittingly attracts a fringe following which he has seen necessary to reject in Manifesto of Transdisciplinarity (2002).

I am unable to agree with Karen-Claire Voss’ assertion that Helga Nowotny ignores the “subjective”. I regard both New Production and Rethinking Science as excellent ethnographies. The analysis of observations in Rethinking Science especially does not convey the detached

objectivity of the quantitative researcher. Rather, we are provided with insight into the emerging dominant assumptions and practices of new knowledge production. Our understanding is enriched by the “subjective” robustness of her examples. Certainly, the detailed complexity of Mode 2 society conveys the sense of radical transdisciplinarity. Further, in The Potential of Transdisciplinarity she states “it is a portentous, and not a trivial, change”, and warns of criticism likely to be encountered when practising transdisciplinarity aligned with, what I interpret as, the ontological change on the part of the researcher. While Karen-Claire Voss might respond that I have missed the point of “the profoundly radical nature of transdisciplinarity”, surely, in a post-modern world, we can live with more than one reality of what constitutes transdisciplinarity. For me, the fundamental difference between observed and visionary transdisciplinarity is that the former moves within the context of what is occurring while the later moves within the context of what should occur. I can work with both.

In 2000, I was approached to assist in the formulation of a new doctorate degree based on Mode 2 knowledge production and transdisciplinary research. I am now in the enviable position of being able to observe transdisciplinary thinking and research in application. I doubt it is possible to embrace all 8,530+ disciplines (Crane and Small 1992) in transdisciplinary pursuit. Reality necessitates the unification of knowledge across disciplines relevant to a particular context.

The contributions of Nowotny, Klein, Nicolescu and others have forged a radical reconstruction of knowledge production and transdisciplinarity here far from Europe, North America and Istanbul. More will surely follow. Embrace it.

First reply to Clive Graham
Basarab Nicolescu
May 19, 2003 7:53 UT

The reply of Clive Graham to Karen-Claire Voss contains remarks which are refreshing and stimulating, especially the one refering to the necessary relation between theory, practice and vision of transdisciplinarity and the associated division between the two main streams of transdisciplinarity today. I will comment in a different letter to this precise point. For the moment, allow me to correct some wrong statements about the CIRET's Moral Project. You write, Clive, that "...in CIRET's Moral Project (1987 modified 1999), refers to transdisciplinarity as "the return to a Golden Age"". This assertion IS NOT contained in our Moral Project, which you can read in its integrity, in English translation, on the page http://perso.club-internet.fr/nicol/ciret/english/projen.htm Are you so kind, Clive, to mention the exact place where you did find that I refer to transdisciplinarity as "the return to a Golden Age"? Needless to say, I never made such a claim. Second point: the CIRET Moral Project is not mine. It was formulated and signed in 1987, when CIRET was founded, by 52 personalities coming from different academic disciplines. Third point : there is not and cann't be a "modified" version of this Project, for the simple reason that it is a legal document, deposed at the Prefecture de Police in Paris, as an appendix to our bylaws, at the moment when our non-profit organization, governed by the Law of 1901, was declared. As such, this document can not be modified. It is true that this Project was complemented in 1994 by the Charter of Transdisciplinarity adopted by the participants at the First World Congress on Transdisciplinarity, which took place in Convento da Arrabida (Portugal). This Charter is signed now by several hundreds of transdisciplinary researchers, most of them not being members of CIRET. You can read this Charter, in its English translation, on the page http://perso.club-internet.fr/nicol/ciret/english/charten.htm Of course, here also, no reference is made to transdisciplinarity as "the return to a Golden Age". Basarab Nicolescu

Theoretical, Phenomenological and Experimental Transdisciplinarity
Basarab Nicolescu
May 20, 2003 11:10 UT

The distinction made by Clive Graham between "observed" transdisciplinarity (which concerns "what is occuring") and "visionary" transdisciplinarity (which concerns "what should occur") is certainly useful, even if the words "observed" and "visionary" are ambiguous and not used in the scientific terminology.

I propose to use instead the well-established terminology in hard sciences (e. g., in quantum physics) which distinguishes theory, experiments and phenomenology. The word "theory" implies a general definition of transdisciplinarity and a well-defined methodology (which has to be distinguished from "methods" : a given and single methodology corresponds to a great number of different methods). The word "experiments" implies performing these experiments following a well-defined procedure allowing any researcher to get the same results when performing the same experiments. Finally, the word "phenomenology" implies building models connecting the theoretical principles with the already observed experimental data, in order to predict further results.
I will classify the work done by Michael Gibbons and Helga Nowotny as "phenomenological transdisciplinarity", while my own work (from 1985), as well as the one of other eminent researchers like Edgar Morin, as "theoretical transdisciplinarity". In its turn, the "experimental transdisciplinarity" concerns a big numer of experimental data already collected not only in the framework of "Mode 2 knowledge production" but also in many fields like education, psychoanalysis, the treatment of pain in terminal diseases, the tobacco addiction, art, history of religions, etc. The reduction of transdisciplinarity to only one of its aspects is very dangereous because it will transform transdisciplinarity in a temporary fashion, which I predict that will disappear soon as many other fashions in the field of culture and knowledge. The huge potentialities of transdisciplinarity will never be accomplished if we do not accept the simultaneous and rigorous consideration of the three aspects of transdisciplinarity. These simultaneous consideration of theoretical, phenomenological and experimental transdisciplinarity will allow both a unified and non-dogmatic treatment of transdisciplinary theory and practice, coexisting with a plurality of transdisciplinary models.

Reply to "A few words from a fascinated observer"
Clive Graham
May 20, 2003 14:07 UT

In CIRET “Moral Project”, (http://perso.club-internet.fr/nicol/ciret/english/projen.htm) the statement is made that transdisciplinarity rejects all globalizing projects. I perceive this as anachronistic modernity given post-modern globalization. In “A New Vision of the World Transdisciplinarity” (http://perso.club-internet.fr/nicol/ciret/english/visionen.htm), the title implies a meta-narrative to me. However, in reducing my contribution to 500 words I inadvertently connected two phrases that originally read: *“Nicolescu refers to transdisciplinarity as the answer to failed social revolution, and “the return to a Golden Age” if we retain disciplinarity. But is not this an illusionary concept? I am the living beneficiary of social revolution. I don’t regard it as a failure. I am neither sad nor empty”* , etcetera. I apologize if I attribute this incorrectly.

However, my contention that there appears a divide between observed and visionary transdisciplinarity remains. Helga Nowotny does not detail a transdisciplinary methodology, presumably because each Mode 2 context generates a unique transdisciplinary method.

Basarab Nicolescu delivers a profound methodology employing Lupasco and Godel which delivers a method for transdisciplinary thinking, the precursor for transdisciplinary knowledge production. His analogous thinking is employed by the doctoral students in my workshops for approaching research problems from new and multiple perspectives.

I only know Karen-Claire Voss through “The University as a Space of Possibility” which is an inspirational work. However, her “Few Words” gave me the impression that, unless we go all the way with the visionary school, we are falling short. If so, I don’t agree, given the post-modern condition. It is this ‘either-or’ that I sense, from the distance of Australia, between the Nicolescu vision and the analyses of Nowotny and colleagues.

For me, Gibbons et al (1994) posit contextualized transdisciplinarity; Nowotny et al (2001) expand the concept to transgressive transdisciplinarity; Nicolescu (1993 to 2002) expands the concept further to transdisciplinary hyper-reality in as many dimensions as necessary. I conceive a continuum between the three concepts when defining a problem in context, thinking about the solution in hyper-reality, and testing the solution in the transgressiveness of Mode 2 society.

However, many of my colleagues reject transdisciplinary knowledge production because it carries a “New Age” connotation forged in part, if unwittingly, by visionary grand narratives. Although, Basarab Nicolescu renounces the connotation forcefully in “Manifesto” (2002), meta-narrative visions and allusion to “planetary and cosmic dimensions” (Charter), for example, regrettably keep alive this perception. I know they are just words, but such vocabulary is seized upon by Mode 1 advocates to discredit transdisciplinary knowledge production as “New Age”. That is why the works of Helga Nowotny, Julie Klein and others, developed from practice, are so important for sustaining the development of transdisciplinarity.

I hope I have explained my position and the relevance of both observed and visionary transdisciplinarity to my post-modern world. Kind regards.

One remark on history
Peter Plöger
May 13, 2003 21:53 UT

Since the development of mode 2 knowledge production is a historical development – albeit a very recent one – let me make a brief point on history here. As Steven Shapin (1990) has pointed out, the history of scienctific disciplines shows that they are in a permanent process of reshaping, their borderlines being re-scrutinized over and over again. The same is true of the demarcation between academic disciplines and society, according to Shapin. This implies two things: 1. there are no real borderlines at all, but dynamic zones of transition; 2. transgression is not such a new phenomenon, but various kinds of transgression must have occured quite frequently in the history of science and society. Another hint is given by Joseph Ben-David (1971). He maintains that the conditions for the success of science were not its allegedly stable methodological or epistemological “equipment” but that they depended on the relations science had to its social environment. As these were constantly changing throughout history, so were the conditions for scientific development. These points indicate that one has to take a closer look on how historically new the phenomena of mode 2 knowledge production really are and on the question of how stable they may become.

Ben-David, Joseph (1971): The scientist’s role in society. Englewood Cliffs.

Shapin, Steven (1990): “Science and the public”. In: R. C. Olby et al.: Companion to the history of modern science. London & New York.

Moving Past Dichotomies in “Brussels”
Julie Klein
May 16, 2003 21:03 UT

In the midst of the war of dichotomies, I want to put the question of problem choice on the table. The history of problem-focused interdisciplinary research dates to the 1940s, in agriculture and defense-related research. During World War II, centers and laboratories were established for military projects (e.g., building an atomic bomb, solving problems with new radar systems, and designing a new turbo engine). Since the 1970s, industrialized nations have been allotting increasing amounts for multi- and interdisciplinary research in areas of intense international economic competition, especially engineering and manufacturing, computers, biotechnology, medicine, and even defense-related research. The current momentum for transdisciplinary research on “important” problems needs to be seen in light of this history.

In Europe, or “Brussels” as Steve has renamed it, a new rhetoric of transdisciplinarity arose around the issue of problem solving at both national levels and in the European Union, which took a proactive stance. The EU’s Third Research Program promoted interdisciplinarity. The Fourth Program advocated task forces and an emerging transdisciplinarity. The Fifth Program began building an infrastructure for transdisciplinary research aimed at fostering new structures for improving quality of life for European citizens. This stance illustrates the complicated politics of problem choice. Economic and technological developments are targeted alongside issues of sustainability. The most explicit commitment to sustainability was the program on “Quality of Life and Management of Living Resources.” Three previously separate strands of life-science based activities were brought together. Agro-food, biomedicine, and biotechnology were fused in a new structure focused on food, nutrition

and health; control of infectious diseases, the “cell factory” (biotechnological developments and applications), environment and health, and sustainable agriculture, fisheries and forestry, and integrated development of rural areas.

All problems are not of equal status, however. Problems of sustainability take a back seat to problems of the marketplace in Europe, the United States, and other nations-- whether in the name of inter- or transdisciplinarity. Bryan Turner's analysis of the medical curriculum inevitably comes to mind. Turner called attention to the difference between instrumental bridging of specialist knowledges and a critically grounded conceptualization. When interdisciplinarity is conceived as a short-term solution to problems, as in many research centers focused on social and economic problems, questions of epistemology are replaced by the pragmatics of reliability, efficiency, and commercial value. Interdisciplinarity in social medicine and sociology of health emerged as an epistemological goal. Researchers focused on the complex causality of illness and disease and on the corresponding assertion that any valid therapeutics must be based in a holistic view of the patient. An epistemologically creative and critical holds out the promise of a more comprehensive map of knowledge and, I would add to Turner, decidedly different priorities in problem choice. The same may be said of the current discourse of transdisciplinary problem solving.

For Turner, see "The Interdisciplinary Curriculum: From Social Medicine to Postmodernism,” published in the journal Sociology of Health and Illness in 1990 (12:1-23)

Warrants for action versus scientific validity
Davydd Greenwood
May 20, 2003 2:16 UT

I greatly enjoyed Nowotny's essay and experienced much of it as a vindication of my 3 decades of struggle to maintain a transdisciplinary perspective in a university world of silos and bunkers.

The institutional problems outlined here are ones that Morten Levin and I have written a good deal about in recent years but we do so from an "action research" perspective. In the case of action research, based as it is on phronesis, the problems studied are the vastly complicated real world problems that no discipline can handle. But in action research, the local stakeholders are part of the research team, setting the agenda, learning and conducting research, and collaborating in the interpretation of results, the design of actions, and the evaluation of outcomes. What constitutes "validity" is that the knowledge and action plans so generated persuade knowledgeable local stakeholders who know their life situations better than anyone to act on the knowledge. We take this to be a higher form of validity in social science than any significance test or peer review. How many sociologists would risk their mortgage on the results of a regression analysis?

> **Action research and new technologies**
> Gloria Origgi
> May 23, 2003 13:52 UT
>
> As an example of the « action research » perspective that Davydd Grenwood evokes, I would like to mention our interdisciplines project. This is quite a “light” project, but it is innovative in gathering different “models” of communication, something in between the on line forum and the peer review: a sort of collaborative publishing platform.
>
> It has been realized by a very heterogeneous team: a group of researchers in cognitive science and philosophy and a team of technical experts in publishing and new technologies. We have conceived together the whole project, starting from a quite fuzzy idea of a “portal of virtual conferences”.
>
> As researchers in humanities, we brought our *savoir faire* about workshop organization, scientific standards of communication, etc. etc. and the other team brought its experience about on line publishing and distribution of content. I am not sure that I would have been able to think things in this way in a purely academic environment and I think that an intelligent development of new technologies of information and communication is transdisciplinary in this sense: it

requires the making of new, heterogeneous teams, that breaks the traditional frontiers between non-profit and profit research and knowledge production.

In this perspective, I would bet on new technologies of communication and information as the ideal environment to develop true, action-based, world-oriented research projects.

Convergence and differences between the two approaches of transdisciplinarity
Basarab Nicolescu
May 21, 2003 10:03 UT

I agree with many of the statements made in the beautiful and clear essay of Helga Nowotny. I will mention only few of them, underlying at the same time, in the spirit of a constructive dialogue, our differences. 1. I fully agree that the "underlying need" of transdisciplinarity is "the loss of what is felt to have been a former unity of knowledge". But I don't understand at all why "the joint problem solving" is the aim of transdisciplinarity. Certainly "the joint problem solving" is ONE THE AIMS of transdisciplinarity but not THE AIM. The use of the singular seems to me dangerous, as in religion, as allowing unnecessary wars and improductive dogmatism. 2. I totally agree that knowledge is "inherently transgressive" and that there is a deep relation between "transdisciplinarity" and "trangressiveness". But what we really mean by "transgressiveness"? I wrote myself a lot of articles (alas, in French!) on this subject. Is this trangressiveness concerning society or, in the first place, the human being which is (or has to be) in the center of any civilized society? In more philosophical terms terms, is transdisciplinarity and its associated transgressiveness concerning the Subject/Object interaction or only the Object? 3. I am somewhat perplexed by the abrupt assertion "Mode-2 is transdisciplinarity". "Mode-2" is certainly part of transdisciplinarity, but not ALL transdisciplinarity. Again, the use of the singular here is disturbing. Are we allowed to identify "production of knowledge" and "knowledge"? 4. I am also somewhat perplexed to read that "the potential of transdisciplinarity lies precisely here: to obtain a better outcome, to produce better science". This is certainly a potentiality of transdisciplinarity but certainly not THE potential. I certainly fully agree with the notion of "social robust knowledge", but where am I, where are you, where are us in all this? The "social robust knowledge" has to be at the service of human being and not the other way around, isn't it? In other words, the Subject/Object interaction seems to me at the very core of transdisciplinarity and not only the Object itself. 5. I fully agree with the notions of "process of contextualisation" and "context of implication", but why transdisciplinarity has to be reduced only to (hard) science? The Subject is again obviously missing in such a view. Such questions and remarks are in fact shared by many transdisciplinary reasearchers all over the world. Some of them expressed similar views in a document presented at the Zurich conference (2000) and concerns precisely a "broader view of transdsisciplinarity" (Joseph Brenner also refers to this document). It was signed by 6 participants, only 2 of them being CIRET members. I was myself a member of the International Scientific Committee of the Zurich meeting and I worked hardly for the organization of the conference, but I cancelled my participation when I was convinced that all was centered on "joint problem solving", everything else having only a marginal place. The document can be found on the site of Ron Burnett http://www.eciad.bc.ca/~rburnett/communications.html and I suggest the moderators to include it in the Bibliography of our seminar. It is my deep conviction that we can arrive at a formulation of transdsisciplinarity which is both unified and diverse : unity in diversity and diversity through unity is inherent to transdisciplinarity. Our agora (following the nice suggestion of Helga of the appropriate structure for a debate on transdisciplinarity) can help us on this way.

Redefining the 'trans' in transdisciplinarity
Ron Burnett
May 23, 2003 7:02 UT

Helga Nowotny has written an important piece that summarizes a great deal of what is important in the debate on disciplines, boundaries and new forms of knowledge production and research. I am interested in the 'transgressive' nature of transdisciplinarity and want to take up a theme developed by Basarab Nicolescu, that what we are dealing with here needs to be thought about in the plural, that is, disciplines in motion and the consequences of a social and political context that now needs a radical reinterpretation of what disciplines mean. The "Trans" in transciplinarity is about transition and movement, where the rigour of research is matched by concerns for connections and inter-relationships. This is a middle zone of exchange and to me is about shifting the ground of research in both the sciences and the arts from a concentration

on disciplinary needs and history, to an emphasis on how that history is connected to the goals and aspirations of other disciplines. In other words, no one discipline can make strong claims anymore about its own direction, value and output in isolation from what is happening in sometimes contiguous and oftentimes opposite areas of research. This as Nowotny points out is difficult, but I feel is essential if we are to redefine the nature, direction and orientation of research and to make it directly more relevant to the present social and politcal context. I would therefore propose that transdisciplinarity is different from other forms of inquiry and requires researchers and teachers and learners to approach the subjects they are interested in a more 'trans'-itory fashion. I believe that the impact of this shift will be felt to the greatest degree in the sciences. Objects of study can no longer be researched without referring to arguments in ethics, the environment and issues of sustainability. None of these can be discussed in isolation of related issues of a political and social nature. The 'trans' in trandisciplinarity is about recognizing the wholistic shape of this process of investigation which trans-forms our very definitions of research.

Definition of Transdisciplinarity
Basarab Nicolescu
May 29, 2003 19:37 UT

It seems to me that many of the problems we have on this Internet Forum on Transdisciplinarity come from the fact that we use different definitions of transdisciplinarity or, even worse, we discuss transdisciplinarity without defining what this word means. Let me recall the historical fact that Jean Piaget was the thinker who first coined the word "transdisciplinarity", around one year before the workshop "L'interdisciplinarité - Problèmes d'enseignement et de recherche dans les universités", hold in Nice (France) from 7 to 12 of September 1970 (Proceedings published by OCDE in 1972). It is true that several other contributors (Erich Jantsch, André Lichnerowicz, Guy Michaud, Pierre Duguet, etc.) use this word in their contributions, but Guy Michaud, one of the organizers of the meeting, and also André Lichnerowicz, confirmed to me that it was Piaged who invented the word and asked to the others to think about its meaning. It is amusing to mention also the fact (see page 13 of the Proceedings)that the word "transdisciplinarity" was initially proposed by Piaget to figure IN THE TITLE of the meeting, but OCDE authorities decided not to use it in order to better adapt to the different systems of teaching in the different countries represented in OCDE... In his contribution, Piaget gives the first known definition of transdisciplinarity (I give it in French, in order not to distort his words) :

"Enfin, à l'étape des relations interdisciplinaires, on peut espérer voir succéder une étape supérieure, qui serait "transdisciplinaire", qui ne se contenterait pas d'atteindre des interactions ou réciprocités entre recherches spécialisées, mais situerait ces liaisons à l'intérieur d'un système total sans frontières stables entre les disciplines" (page 144).

In her stimulating contribution to this Forum, Helga does not give a definition of transdisciplinarity but she gives sufficient hints to point towards such a definition. When she writes "Transdisciplinarity is about transgressing boundaries", she is in obvious agreement with Piaget, even if she does not say explicitly that the "boundaries" in question are the boundaries BETWEEN disciplines. The assertion "The third attribute of Mode-2 is transdisciplinarity" is more ambiguous, because it allows the reader to think about an identification between new "Mode-2 production of knowledge" (defined in the book "The New Production of Knowledge") and "transdisciplinarity", identification that would be in obvious contradiction with the Piaget's definition. Moreover, it is not clear if the new mode of production of knowledge - the Mode-2 - corresponds to new knowledge or is, in fact, recombination of elements of old knowledge. I gave myself a definition of transdisciplinarity in my book "Manifesto of Transdisciplinarity" (SUNY Press, New York, 2002): transdisciplinarity is the new "in vivo" knowledge, founded on the following three postulates : 1. There are, in Nature and in our knowledge of Nature, different levels of Reality and, correspondingly, different levels of perception; 2. The passage from one level of Reality to another in insured by the logic of the included middle; 3. The structure of the totality of levels of Reality and perception is a complex structure: every level is what it is because all the levels exist at the same time.

I think that there is no obvious contradiction between the three above definitions but perhaps only a different degree of generality. I formulated my own definition in order to agree with what I know to be essential both in hard and soft sciences. I would be very interested to know what the other participants

at this Forum, and especially Helga, think about this problem of agreement or disagreement between the above three definitions of transdisciplinarity.

L'au-delà des disciplines
Edgar Morin
May 30, 2003 16:56 UT

Les disciplines sont pleinement justifiées intellectuellement à condition qu'elles gardent un champ de vision qui reconnaisse et concoive l'existence des liaisons et des solidarités. Plus encore, elles ne sont pleinement justifiées que si elles n'occultent pas de réalités globales. Par exemple, la notion d'homme se trouve morcelée entre différentes disciplines biologiques et toutes les disciplines des sciences humaines : le psychisme est étudié d'un coté, le cerveau d'un autre coté, l'organisme d'un troisième, les gênes, la culture, etc.. Il s'agit effectivement d'aspects multiples d'une réalité complexe, mais qui ne prennent sens que s'ils sont reliés à cette réalité au lieu de l'ignorer. On ne peut certes créer une science unitaire de l'homme, qui elle-même dissoudrait la multiplicité complexe de ce qui est humain. L'important est de ne pas oublier que l'homme existe et n'est pas une illusion "naïve" d'humanistes pré-scientifiques. On arriverait sinon à une absurdité (en fait on y est déjà arrivé dans certains secteurs des sciences humaines ou l'inexistence de l'homme a été décretée puisque ce bipède n'entre pas dans les catégories disciplinaires). Une autre conscience, celle de ce que Piaget appelait le cercle des sciences, qui établit l'interdépendance de facto des diverses sciences, est également nécessaire. Les sciences humaines traitent de l'homme, mais celui-ci est, non seulement un être psychique et culturel, mais aussi un être biologique, et les sciences humaines sont d'une certaine facon enracinées dans les sciences biologiques lesquelles sont enracinées dans les sciences physiques, aucune de ces sciences n'étant évidemment reductible l'une a l'autre. Toutefois les sciences physiques ne sont pas le socle ultime et primitif sur lequel s'edifient toutes les autres ; ces sciences physiques, pour fondamentales qu'elles soient, sont aussi des sciences humaines dans le sens ou elles apparaissent dans une histoire humaine et dans une société humaine. L'élaboration du concept d'énergie est inseparable de la technicisation et de l'industrialisation des sociétés occidentales au 19ème siècle. Donc, dans un sens, tout est physique, mais en même temps, tout est humain. Le grand problème est donc de trouver la voie difficile de l'entre-articulation entre des sciences qui ont chacune, non seulement leur langage propre, mais des concepts fondamentaux qui ne peuvent pas passer d'un langage a l'autre.

A philosopher's reflections on his interactions with a neuroscientist

Pierre Jacob (CNRS, Institut Nicod)

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Abstract: What is the philosophy of the cognitive sciences all about? Why should one want to be a philosopher of the cognitive sciences? In my view, there are two broad ways one can think about these questions. One can approach the cognitive sciences either as a philosopher of science primarily interested in the nature of cognitive scientific explanations or as a philosopher of mind primarily interested in the nature of minds. I am interested in both. In this paper, I shall tell the two-tier story of my collaboration with the cognitive neuroscientist, Marc Jeannerod. I will describe how we worked together towards the creation of the Lyon Institute of Cognitive Science and how we came to write a book together on human vision.

Introduction

In the course of the past ten years, I have been engaged in collaboration with the cognitive neuroscientist Marc Jeannerod both in setting up an interdisciplinary institute and in writing a book on vision. I have been asked by the organizers of this web seminar on interdisciplinarity to recount and reflect on this experience, as an example of interdisciplinary interaction. I do so sketchily, and will be happy, in answer to questions, to go more into details and issues that might be relevant to the general topic of the seminar.

Cognitive science — or should one say "the cognitive sciences"? — lies at the interface between the natural non-human sciences and the human social sciences. Notice that noun phrases referring to scientific disciplines or academic areas can often be used either in the singular or in the plural. So it is e.g., with "the cognitive sciences" and "the neurosciences". In what follows, I will use the singular when some common research goal is involved and the plural to emphasize the relevant heterogeneity of the concepts and empirical methods used by the various disciplines involved.

Whereas the social sciences make liberal use of such human mental representations as intentions, perceptions, beliefs and desires in their various attempts at explaining human actions, the natural sciences are reluctant to explain the behavior of anything — from elementary particles, stars, galaxies, to molecules and cells — by attributing to them representations. To a naturalistically inclined philosopher of mind, like me, who pays proper respect to the natural sciences, cognitive science offers the prospect of providing scientific respectability to mental representations as "theoretical entities" (as philosophers of science call such things). I would not like to denigrate the social sciences. Nor am I willing to embrace a radical form of methodological dualism between the natural and the social sciences.

Ten years ago, on March 3, 1993, I took an early TGV from Paris to Lyon. I had been invited by Marc Jeannerod to attend a small interdisciplinary meeting designed to launch a discussion of how best to integrate linguistic and philosophical research into a projected Institute of Cognitive Science. In 1993, Marc Jeannerod, who was trained as a neurophysiologist and who has done seminal work in human neuropsychology and psychophysics, was the head of a research unit called "Vision and motricity", located in Bron (on the eastern outskirts of Lyon). In what follows, I will tell the twofold story of our collaboration: first, I will describe the science policy process whose turning point was the creation of

the Lyon Institute of Cognitive Science. Then, I will move to issues of scientific substance and evoke my collaboration with Marc Jeannerod in writing a book on vision.

The creation of the Lyon Institute of Cognitive Science

Whereas cognitive science programs developed in the 1970's in the US, in the UK and elsewhere, it was not until the late 1980's that the French scientific community came to recognize the scientific potential of cognitive science. Several eminent neurobiologists and neuroscientists such as Jean-Pierre Changeux, Michel Imbert and Alain Berthoz played a critical role in this process. In the late 1980's the Lyon-based neuroscientist André Holley (working on olfaction) became the head of the CNRS program called "Cogniscience". Then by the mid-1990's, Marc Jeannerod had convinced the General Director of CNRS that CNRS should create its own Institute of Cognitive Science and furthermore that it should be located in the Lyon area.

In the Fall of 1995, I became the head of a small interdisciplinary CNRS unit (EP 100) called "A modular approach to cognitive processes: memory, language, action", based in Lyon, involving a dozen members, and whose goal was to pave the way for the creation of the new Institute of Cognitive Science.

The group involved three neurobiologists from André Holley's lab working on olfactory memory in rats, two cognitive neuroscientists (including Marc Jeannerod) a cognitive psychologist (working on human memory and especially on face recognition), a cognitive psychiatrist (who examines shizophrenic patients at the Vinatier Hospital), three syntacticians (working on the fundamental properties of universal grammar within a Chomskyan framework), and a naturalistically inclined philosopher of mind (myself). Marc's sense of irony was, I think, tickled by the opportunity to do a little social experiment, i.e., to test Plato's idea that philosophers should run labs.

From the Fall 1995 until early 1998, we held a weekly interdisciplinary seminar in one of the small rooms provided for us by the Medical School of the Lyon University Claude Bernard in the Rockefeller Center, near the subway stop Grange Blanche. During these sessions, one of us would attempt to give the others a precise sense of what he or she was doing.

One of my most memorable experiences at the time was a visit to the cognitive neuroscientist Driss Boussaoud's lab. Driss was recording single neurons in the premotor cortex of a macaque monkey engaged in a task in which he was required to press a lever with his left hand if and only if he would see a red square on the upper right corner of the screen in front of him. The occurrence of the red square would always be preceded by the occurrence of a green circle in the lower left corner of the screen a few milliseconds earlier (that would prepare the monkey for the motor instruction). Driss was interested in disentangling the respective contributions of visual attention and motor intention to the preparation of action. For an hour, I watched the monkey who was quietly sitting with his back turned onto me. A thin electrode was coming out a metal plate covering his skull. He occasionally pressed a lever and sipped apple juice when he succeeded in the task. I paused over whether I was ready to move into moral philosophy. I decided that I was not.

In addition to our weekly interdisciplinary attempt at talking across disciplines, on Thursday afternoon once a month, we would invite two speakers on a common topic of relevance for the cognitive sciences. We selected the speakers either because they would approach a single topic from the perspective of two different disciplines (e.g., philosophy and neuropsychology or linguistics and cognitive psychology) or because they had some important theoretical or methodological disagreement. The topics included phenomenal consciousness, blindsight, implicatures in human verbal communication, numerical cognition, mirror neurons, visual imagery, the empty subject parameter in Romance languages, the cognitive study of religion, the memory of proper names, spatial cognition and autism.

The design of an Institute of Cognitive Science faces one major challenge: how to promote the collaboration between neuroscientists (i.e., biologists), computer scientists and social scientists? How to integrate the social sciences into a cognitive science context? Within the French institutional framework of CNRS, the challenge was to create a single CNRS research unit across the boundaries between at least three separate Departments: the Departments of the life sciences (SDV), of the computer sciences (STIC) and of the human and the social sciences (SHS).

Marc and I agreed on three related assumptions: (a) an institute of cognitive science should not be an institute of cognitive neuroscience. (b) Cognitive psychology is really the core of the cognitive sciences. (c) Linguists working within the tradition of generative grammar should provide a major input to the social sciences within an institute of cognitive science.

The first assumption is almost definitional, but the other two require a few justifications.

• First, in the past thirty years or so, in the study of motor cognition, perception and memory, cognitive psychology and cognitive neuroscience have increasingly come to share more experimental paradigms (e.g., brain imaging techniques).

• Secondly, as a result of the cognitive revolution of the 1960's, cognitive psychology has been an integral part of the computational sciences as psychologists have accepted as a constraint to offer computational models of the various human cognitive processes that they study.

• Thirdly, one of the major inputs to the cognitive revolution has unquestionably been the Chomskyan revolution in linguistics. In a nutshell, in the Chomskyan framework, the study of the language faculty is the study of that fundamental piece of human **knowledge**, which allows a human child to learn the grammar of the language spoken by members of her linguistic community on the basis of her linguistic experience. While psychologists study memory, perceptual and motor **processes**, generative linguists study systems of **knowledge** that are distinctive of human cognition — something important to the social sciences within a cognitive scientific environment. To put it mildly, our assumption was not widely shared among French linguists and other social scientists who were in a position to affect the orientation of the projected Institute in the years 1995-1998 — either in the CNRS section devoted to the study of language or at the Head of the Department for the social and the human sciences.

Ultimately, in February 1998, roughly fifty cognitive scientists moved into a brand new building on 67, boulevard Pinel, in the neighborhood of the Psychiatric and the Neurological Hospitals in Bron. From February 1998 until the end of 2002, Marc Jeannerod has been the Director of the Institute. In January 2001, I left the Institute to become the director of the Institut Jean Nicod in Paris, a newly created

interdisciplinary research unit at the interface between philosophy, the cognitive and the social sciences.

In retrospect, the Lyon Institute of Cognitive Science is, in my view, one of the best things that have happened to the French cognitive science community: it offers social scientists the unique opportunity to collaborate with experimentalists from the various areas of the cognitive sciences and to test some of their favorite social scientific theories. Two problems, I think, have arisen and will persist. On the one hand, within the French context of CNRS, it has proved difficult to hire the best scientists for the required special slots, on the basis of an international search committee, and not to depend excessively on existing CNRS research positions. On the other hand, it has turned out that the agenda of the best theoretical linguists is to keep doing theoretical linguistics, not to rush into experimental collaboration with psychologists and/or neuroscientists.

What is cognitive neuroscience?

Science policy was not our primary, let alone exclusive, topic of common concern. Soon the new cognitive neuroscience of vision became our favorite topic of discussion. In fact, in order to understand how a cognitive neuroscientist and a philosopher of mind could come to write a book on vision together, it is important to say what cognitive neuroscience is.

Cognitive neuroscience is the biological roots of the current cognitive sciences. Cognitive neuroscience, however, is only one component of the neurosciences. The goal of the neurosciences is to understand what is arguably the most complex physical object presently known in the universe: the human brain. A human brain contains in the order of one hundred billions neurons, which are involved in roughly one million billions synaptic connections. At the most elementary biological levels of organization of brain functions, molecular neurobiology tries to understand the chemical composition of neurons and the molecular structure of neurotransmitters that are involved in the communication between neurons. At a more complex level of organization, different functional areas of the human brain have been recognized and the connections between them have been mapped by means of the combined methods of neuroanatomy, histology and neurophysiology. A human brain does not arise out of an act of special creation. It results from two historical processes: the phylogenetic history of the species homo sapiens sapiens and the ontogenetic development of each human individual. Thus, the neurosciences involve the comparative study of the differences and the similarities between features of the human brain and features of the brains of members of other species — in particular non-human primates. And they involve the study of the genetic bases, the embryological and epigenetic developments of the plasticity of the human nervous system.

Current cognitive neuroscience is really motivated by the attempt to map specific cognitive activities onto specific brain areas. It in turn involves a heterogeneous ensemble of experimental techniques:

• electrophysiological recordings of either single neurons or assemblies of neurons in the course of a cognitive task in various animals either awake or anaesthetized;

• the neuropsychological study of impairments selectively produced by brain lesions in human patients;

• the psychopathological study of mental diseases in human patients without recognized brain lesions;

• the use of various techniques of brain imaging to study the activation of particular brain areas in normal human subjects during a cognitive task;

• the psychophysical study of the perceptual and/or motor responses of normal human subjects upon detection of an experimental stimulus.

Because its goal is to map selective brain areas onto cognitive activities, more than any other discipline within the neurosciences, cognitive neuroscience is likely to be of direct relevance to the social sciences. So in particular, cognitive neuroscience has brought new insights to our understanding of concepts of fundamental importance for the philosophy of mind such as **perception** and **action.** In the last twenty years, cognitive neuroscience has discovered that single neurons in particular areas of the brain of macaque monkeys fire preferentially in response to the perception of object-directed actions involving particular movements of the fingers or to the sight of a full face rather than to the same face viewed in profile. In the following, I shall concentrate on the concept seeing.

How a cognitive neuroscientist and a philosopher came to write a book together

Strong interdisciplinarity is exemplified when two or more well-established scientific disciplines merge and give rise to a new scientific discipline. Outside the cognitive sciences, molecular biology grew out of the interplay between genetics and biochemistry. Within the cognitive sciences, linguistics and cognitive psychology gave rise to experimental psycholinguistics. Marc and I were never involved in strong interdisciplinarity, since we never designed, let alone performed, a novel experiment together. Instead, we spent five years discussing the significance of many experimental papers in electrophysiology, neuropsychology and psychophysics.

— *The two visual systems hypothesis*

The main result of our collaboration has been to clarify the “two visual systems” model first proposed by Mortimer Mishkin and Leslie Ungerleider (in the early 1980’s) and later revised by David Milner and Mel Goodale in their book, The Visual Brain in Action (Oxford University Press, 1995). On the basis of lesions in the brain of macaque monkeys, Ungerleider and Mishkin discovered that, in the primate visual system, there is a bifurcation between two anatomical pathways the ventral pathway projects the primary visual cortex onto inferotemporal areas. The dorsal pathway projects the primary visual cortex onto parietal areas. Ungerleider and Mishkin further hypothesized that the former is involved in the visual recognition of objects (the **What** system) and the latter is involved in localizing objects in extrapersonal space (the Where system). On the basis of neuropsychological evidence, Goodale and Milner conjectured that the ventral pathway underlies “vision-for-perception” and the dorsal pathway underlies “vision-for-action”. The basic idea of the two visual systems model of human vision is that humans can see one and the same object in two fundamentally different ways: they can build a perceptual appreciation of it and they can use visual information in order to act upon it.

Arguably, it is part of the human commonsense conception underlying what psychologists call “mindreading” that **seeing** leads to **knowing**. Human children seem able to understand early on that whether or not some object or event falls within the direction of one’s gaze makes a difference to one’s knowledge about the object or event. There is currently much discussion about whether apes share this commonsense human conception.

As a philosopher of mind with an interest in the philosophy of perception, I had assumed (a) that the goal of the human visual system is perception; (b) that human visual perception has a distinctive kind of phenomenology and (c) that the goal of perception is knowledge of the world. Although I had been exposed to the phenomenon of blindsight, I had failed to appreciate its full implications for a scientific understanding of human vision. Blindsight patients have a lesion in their primary visual cortex. As a result, visual inputs are disconnected from the rest of their visual cortex and they feel no visual phenomenal experience in their blind hemifield. However, in the 1970's, it was discovered by a number of neuropsychologists (including Lawrence Weiskrantz, Marc Jeannerod and several collaborators from Lyon) that blindsight patients have surprising residual visuomotor capacities without visual phenomenal awareness of the stimulus.

By virtue of interacting with Marc, I came across a wide array of empirical work from the cognitive neurosciences of vision that converges on the conclusion that it is not true that the goal of the human visual system is to give rise to visually based knowledge of the world. Instead, much visual processing in humans is devoted to the guidance of object-directed actions. It dawned upon me that this evidence provided new arguments for the **representational** account of the visual mind. The cognitive neurosciences of vision were ready for some conceptual analysis. Philosophers had much discussed the idea that, whereas thoughts have conceptual content, perceptual representations have nonconceptual content. I wanted to argue that human hand actions directed towards objects are guided by special visual representations — visuomotor representations. The task was to analyze the differences between the nonconceptual content of visual percepts and the nonconceptual content of visuomotor representations. For example, the size, shape and location of an object are relevant for grasping it. But its color and texture are not.

One crucial area of the cognitive neurosciences turned out to be of great relevance for the assessment of the two visual systems model of human vision: it is the study of the normal unfolding of human visually guided actions of grasping objects. The dexterity of the human hand (and to a lesser extent of non-human primates) allows them uniquely to reach, grasp and manipulate objects. The action of grasping involves two components: a reaching component guided by a visual representation of the target located relative to the agent's body and a grasping component guided by a visual representation of the size, shape and orientation of the target. Marc Jeannerod and others discovered that during reaching (i.e., transportation of the hand to the target), there is an automatic process of grip formation whereby the preshaping of the finger grip is programmed much before the hand contacts the object. At about 60% of the reaching phase, the opening of the fingers reaches its peak, which is called "maximum grip aperture" and which is linearly correlated with the size of the target. This visuomotor process can be selectively disturbed by brain lesions as in optic ataxia.

In his previous work (e.g., in his BBS 1994 paper "The Representing brain. Neural correlates of motor intentions and imagery" and in his 1997 book, The Cognitive Neuroscience of Action, Blackwell), Marc drew a functional distinction between two kinds of visual processing, which, borrowing from the study of language, he labelled respectively the "semantic" and the "pragmatic" processing of visual information. Whereas the former leads to visual identification and recognition of objects, the latter underlies visually guided actions onto objects. In the forthcoming book that Marc and I wrote together and that is entitled Ways of seeing, we try to decompose further the distinction between the semantic and the pragmatic processing of visual inputs. In the process, we argue for two particular qualifications of the two visual systems model of human vision.

• *Seeing actions: the interface between visual and social cognitions*

First of all, we argue that the two visual systems hypothesis is restricted to the visual processing of objects that humans can reach, grasp and manipulate with their hands. But, or so we argue, human vision is not restricted to seeing **objects** that can be grasped between the thumb and the index finger.

Nor should human actions be restricted to grasping objects. Indeed, we argue that the "visuomotor transformation" that allows a human being to grasp an object is but a low level of human pragmatic processing of visual information. A higher-level of pragmatic processing of visual information is involved in the use and the recognition of tools, and in pantomimed actions with complex tools. After a lesion, apraxic patients may be impaired in their use and recognition of tools, and their understanding of actions with tools. But their visuomotor transformation is intact.

In addition to graspable objects, humans can also see holes, shadows, substances, gases, events and actions. They can see other humans act. Seeing a human act involves seeing an action, which is an event. It also involves seeing a human body composed of various articulated parts, including the face, which can carry much social information. Furthermore, much recent work has showed that humans have a special visual sensitivity to the perception of biological movements.

Human actions that humans can see can in turn be directed either towards inanimate or towards animate objects, including conspecifics. So in the last chapter of the book, we argue that the human brain contains two complementary systems: one is for the visual perception of object-oriented actions and the retrieval of motor intentions. The other is for the visual perception of human actions directed towards conspecifics and the retrieval of social intentions, i.e., intentions to affect a conspecific's behavior, such as causing submission, fear or sexual desire. We submit that these two complementary perceptual systems send inputs to the human mindreading system, which in turn is a crucial component of human social cognition.

• *Visuomotor representations vs. Visual percepts*

Secondly, we offer an explanation of the basic dissociation between visual perceptual processing and visuomotor processing of one and the same visual input. This dissociation is at work in the contrast between the visual behavior of respectively visual agnosic patients and optic ataxic patients. It is also exemplified by normal subjects' responses to illusory stimuli.

There are three constitutive features of visual perception. First, to perceive objects in a visual array is to process their spatial relationships. Second, to perceive objects is to represent their relative sizes, shapes and orientations. Finally, the job of visual perception is to enable an individual to recognize objects, apply relevant concepts, and mentally classify them. So what is important to visual perception is the representation of those enduring properties of objects that allow recognition from many different points of view on many different occasions.

By contrast, the visual information relevant to reaching and grasping an object is its position relative to the agent's physical position at a given time. For the purpose of grasping an object, what must be represented is its absolute (non-relative) size and shape. So two kinds of visual features of objects are relevant to the visuomotor processing: geometrical properties such as size, shape and orientation, and the distance of the object relative to the agent, which in turn changes during the action.

Philosophers have argued that beliefs and visual percepts have a mind-to-world direction of fit: if what a belief or a percept represents fits a fact in the world, then the belief or the percept is veridical; otherwise not. Intentions and desires, on the other hand, have a world-to-mind direction of fit: if what obtains in the world fits what the intention or the desire represents, then the intention or the desire is fulfilled; otherwise not. We argue that the function of visual percepts is to provide visual information relevant to the formation of beliefs. Following Ruth Millikan's idea that there are what she calls "pushmi-pullyu" representations (after the Pushmi-Pullyu, an imaginary two-headed animal in Dr Doolitle's stories), that fall in between beliefs and intentions, we argue that visuomotor representations have a **hybrid** direction of fit in virtue of which they provide motor intentions with visual information about affordances for action. We argue that the contrast between visual percept and visuomotor representation is corroborated by the double dissociation between the perceptual impairment of apperceptive visual agnosic patients and the visuomotor impairment of optic ataxic patients. The former cannot visually recognize the size, shape and orientation of an object that they can grasp between their thumb and index finger. The latter cannot reach and grasp objects whose size, shape and orientation they can visually recognize.

Marc and I wrote our first paper on the two visual systems hypothesis in 1998. We started writing a full book in the Summer of 2000: we decided to start by writing each selected chapters. Then each of us revised the other's writing. Much of our initial collaboration was devoted to the analysis of dissociations between perceptual responses and visuomotor responses to illusory stimuli in normal human subjects. At the time, a new experimental paper on this topic would be published every other week or so. The question was whether the new psychophysical data were consistent with the two visual systems hypothesis, which was essentially based on neuropsychological evidence. I was a recent convert to the theory. In many conversations and email exchanges, I tried to argue that the evidence was compatible with the theory. Marc was more sceptical: he was more willing than I was to give up an overall framework in the face of prima facie recalcitrant evidence.

In 1995, I was a philosopher of mind whose questions were broadly metaphysical in character: could one naturalize intentionality? Until I spent time in Marc's lab, it had certainly not occurred to me that by studying the calibration of the human finger grip in either a task of manual estimation of the size of an object or in a grasping task, one could get deep insight into the visual human mind. The human finger grip offers an elegant design for the experimental study of selected features of the human mind. When they are not immersed into historical questions, the best philosophers are, I think, irresistibly drawn towards "big pictures". I do feel the impulse. But in the future I will also keep trying to ask questions about features of the human mind that can be put to an experimental test. As I see it, within a cognitive science context, the minds of philosophers of mind are bound to remain divided, if not hybrid.

Discussion

Limits to interdisciplinarity: The case of generative grammar
Dan Sperber
Jun 2, 2003 12:05 UT

I was a member of the scientific committee that oversaw the development of the Lyons Institut des Sciences Cognitives. I agreed that “one of the major inputs to the cognitive revolution has unquestionably been the Chomskyan revolution in linguistics” and I sympathized with Marc Jeannerod’s and Pierre Jacob’s view that “linguists working within the tradition of generative grammar should provide a major input ... within an institute of cognitive science.” Pierre alludes to the fact that “to put it mildly” this aspect of the project failed to gain institutional support. (Actually, the way in which generative linguistics has been held in suspicion and at bay by the French linguistic establishment would be a story worth exploring in its own right.) Another difficulty, which was left in the background because institutional obstacles had taken center stage, is the fact that, with very few exceptions, generative linguists show very little interest for interdisciplinary interactions. They have some good reasons for this. Because of the very success of their program (at least in the sense of having generated new issues, hypotheses, and tools which have changed the whole field, even for non-Chomskyans), their work has become quite technical and quite demanding. Its methods and arguments are in good part specific. Evidence and arguments from other disciplines, for instance experimental psycholinguistics, developmental psychology, or neuropsychology, that, in principle, might be relevant, are hardly ever used, probably because they don’t address issues at the level of fine-grainedness at which most generative linguists are working. So, even if one might wish that these linguists were more interested in interacting with neighboring disciplines, they have reasonable grounds, at this stage in the development of their field, to work in a strongly mono-disciplinary fashion. Interdisciplinarity is not always better.

Now turning back to the issue that was facing Marc and Pierre (and leaving aside the cost, in this case, of confronting strong institutional obstacles): Was their recognition of the historical and intellectual place of generative grammar in the cognitive sciences sufficient reason to try and make it a central ingredient of an institute of cognitive sciences that had limited resources and that had, as it raison d’être to foster interdisciplinary interaction, when, for in part good reasons, generative linguists are sure to keep to a large extent to their own? This is a genuine question on my part. I also pose it to illustrate the point I made in the previous discussions that interdisciplinarity is not a goal and a good in itself, even if it is of crucial importance to scientific advances in many areas.

> **Specific Obstacles for Interdisciplinarity**
> Rainer Kamber
> Jun 3, 2003 9:48 UT
>
> Dan, you propose three reasons for the lack of "interest for interdisciplinary interactions" among generative linguists:
>
> That "their work [1] has become quite technical and quite demanding. [2] Its methods and arguments are in good part specific. [3] Evidence and arguments from other disciplines [...] are hardly ever used, probably because they don’t address issues at the level of fine-grainedness at which most generative linguists are working."
>
> I like these suggestions, and it seems to me that none of them have been discussed earlier. Although [1] does not seem to constitute an adverse motive for generative linguists about interdisciplinary interactions since it poses an obstacle for outsiders rather than for insiders, [2] and [3] suggest that the specificity or "fine-grainedness" (what exactly does "fine-grainedness" mean here?) of "methods and arguments" produce such an adverse motive in generative linguists. (I suppose that, by "arguments" you mean theoretical and experimental hypotheses, theorems, standards about relevant evidence etc.?)

So, one hypothesis supported by this kind of evidence would be this: The combination of scientific (or institutional) success with highly differentiated methods and a certain amount of empirically based theory-building within some research area is adverse to interests in interdisciplinary interactions. Since, with these elements, we have some of the important building blocks of disciplines in general the hypothesis may be spelled differently (and much more simplifyingly):

The stronger the discipline the smaller the interest in interdisciplinarity.

It seems that what you suggest is that the scientific and institutional history of generative linguistics constitutes evidence for this quite fundamental (although extremely general) hypothesis about obstacles for interdisciplinarity.

Reply to Dan
Pierre Jacob
Jun 3, 2003 9:57 UT

Dan asks: should generative linguists (GLs) be an important component of an institute of cognitive science? Yes, they should. Why? The short answer is: human language is one important part of human cognition. GLs do some of the best research into the fundamental properties of the human language faculty. Therefore: GLs should be an important part of an institute of cognitive science.

Dan agrees that GLs have made surprising discoveries about the grammatical properties of many human languages. Secondly, they have made testable claims about such properties as the modularity and domain-specificity of the human language faculty that allows a human child to acquire his knowledge of the grammar of his native tongue on the basis of his linguistic experience. Thirdly, GLs has paved the way for a research program designed to investigate what is unique to human cognition. Thus, developmental psychologists have launched the investigation of the initial state that allows a human child to acquire his knowledge in cognitive domains other than language, such as numerical cognition, naive physics, naive psychology, and so on. So the longer answer is: if research into the cognitive architecture of the human mind/brain ought to be conducted within an institute of cognitive science, then on purely scientific grounds, GLs should be part of it.

Dan raises the question: does generative linguistics have the potential for generating genuine interdisciplinarity within a cognitive science environment? Well, although the topic of this web seminar is interdisciplinarity, like Dan himself, I do not think that interdisciplinarity at all costs should be valued for its own sake, as an intrinsic epistemic, deontic or moral value. As I see it, the challenge of an institute of cognitive science is to help foster interesting, not fully predictable, interchanges between scientists working on different areas of human and non-human cognition with different tools.

One possible obstacle to the integration of GLs within a cognitive science environment, as Dan sees very well, is the ‘very success’ of generative linguistics. Generative linguistics is an empirical enterprise, i.e., its hypotheses are testable. Nonetheless, in practice, GLs work more like mathematical logicians writing derivations than like experimental scientists and they need peers. Nonetheless, in my opinion, developmental psychologists, psycholinguists, neuropsychologists and cognitive neuroscientists should have easy access to people doing the best research into the fundamental properties of the human language faculty. It would, however, simply be wrong not to recognize that some GLs have been involved in some interdisciplinary work. First, as noticed in my paper, experimental and developmental psycholinguistics are in part grown out of generative linguistics. Secondly, some GLs (e.g., Alec Marantz at MIT) have turned to experimental neurolinguistics. Some have recently examined the different electrical patterns of EEGs (electroencephalograms) generated by violations respectively of syntactic and semantic constraints.

As for the diffidence (to put it mildly) of the French linguistic establishment towards generative linguistics (at the period under consideration), it is a political and ideological problem. I wish to register my strong opposition to it.

Grain, strength, and interdisciplinarity
Dan Sperber
Jun 4, 2003 12:56 UT

Grain, strength, and interdisciplinarity

Compare the two sentences:

(1) What did Henry buy? (Answer: *the book that impressed Joan*)

(2) *Who did Henry buy the book that impressed? (Answer: *Joan*)

English speakers know that (1) is grammatical and (2) is not, even though they may never have paid any attention to such fact, and even though the underlying rule is neither obvious nor explicitly taught. Linguists explain such facts in terms of underlying rules and constraints, knowledge of which is part of speakers' linguistic competence. Evidence in the form of such made-up examples and the intuitions they trigger has been quite sufficient to test and refine linguistic hypotheses. Of course, such hypotheses have, in principle, implication at the level of mental and brain processes, and even at the genetic level if the hypothesized constraints are, at least partly, innate. So in principle, these hypotheses might inspire further hypotheses and be testable through their consequences at these levels. However, at present, there is nothing even remotely so fine grained (i.e. explaining and being testable through such minute and precise data) at the psycho- or neuro-linguistic levels, let alone at the genetic level. Hence research on grammar proceeds in relative autarchy.

Does this generalize, as Rainer suggests, to: "The stronger the discipline, the smaller the interest in interdisciplinarity"? I don't think so. What is lacking in this case is a commensurate practical ontology. For reasons of grain, as just explained, most facts studied by linguistic don't have, in practice, corresponding facts to be presently investigated by neighboring disciplines.

Consider, in this light, the case of the relationships between the neurosciences and psychology. Inasmuch as "strength" can be assessed, most would agree, I guess, that neurology is stronger than psychology, and that this difference in strength is not dwindling. Yet the interactions between the two disciplines are rapidly developing, and in a very fruitful manner. Why? Because neurology has been able to study more and more fine-grained neurological phenomena to the point where, in many cases, they correspond to phenomena studied by psychology. Thanks to this relative miniaturization of the objects that can be investigated by neuroscientists, psychologists are more and more in a position to formulate non-trivial hypothesis with clear and testable neurological consequences.

So, I would suggest as an alternative to Rainer generalization: the smaller the ontological overlap, the smaller the interest in interdisciplinary interactions.

Reply to Pierre: ideal and real institutions
Dan Sperber
Jun 4, 2003 13:19 UT

I remember Noam Chomsky explaining that scientists are, and with good reasons, like the guy who is looking for his keys under the street lamp not because he believes that this is where he has lost them, but because this is where he can look better. The point for scientsits, Chomsky was arguing, is not whether an issue is intrinsically important, but whether important things can be said about it at present. I agree with Pierre that "human language is one important part of human cognition" and that generative linguists "do some of the best research into the fundamental properties of the human language faculty." This is not enough however to conclude

that, at this stage in the field, generative linguists “should be an important part of an institute of cognitive science.” The issue is: will their presence in such a context yield important scientific benefits for other cognitive scientists as well as for themselves. The answer is, well, maybe, sometimes, yeah, it might happen. In an ideal institute of cognitive science, where you could recruit as many researchers as you wanted, I would definitely have a strong generative linguistic team, just on this odd chance, and of course, I would look for the rather exceptional generative linguist who has already shown an active interest in interdisciplinary research. But in a real and more modest institute, like that of Lyons, where resources are limited and where you have to choose whether to hire, say, one more linguist or one more specialist of child development, developing the generative linguistic side does not seem to me an obvious priority. Also, most generative linguists themselves would, for scientific reasons, rather be part of a team of ten linguists that one of the two linguists in a team of ten cognitive scientists.

Reply to Dan on ideal and real institutions
Pierre Jacob
Jun 8, 2003 10:19 UT

I accept Dan's point that in practice, the integration of generative linguists (GLs) within a cognitive science environment raises special problems.

I still think, however, that in spite of the limited resources of real institutions, an institute of cognitive science would be far better off with (good) GLs than without GLs. Why?

Here, I want to turn to some of Dan's interesting remarks on "Grain, strength, and interdisciplinarity". We agree that the human language faculty is an important area of human cognition to which GLs have made major contributions. Dan points out that the cognitive neurosciences have been able to match the fine-grainedness of many of the phenomena approached in cognitive psychology, unlike most of the important phenomena discussed by GLs. I venture two possible reasons for this difference:

(1) On the one hand, in the past forty years, cognitive psychology has mostly studied mental processes (or processings) of various kinds. Time reaction studies have been a crucial component of the tool-kit of cognitive psychologists. On the other hand, much of the knowledge of the brain that derives from cognitive neuroscience is knowledge of where in the brain different kinds of mental processes take place. It has been very fruitful and not too difficult to combine cognitive neuroscientific knowledge of where a process takes place in the brain with the psychological knowledge of how long it takes. By contrast, GLs study a system of knowledge, not mental processes. Time reactions do not say much about knowledge systems because too many unknown factors may interact in mapping the knowledge system onto observable behavior.

(2) One other problem for the integration of linguistics with the rest of cognitive science is that GLs have discovered properties of the language faculty that are very surprising given human commonsense intuitions about language. Humans have a wide variety of commonsense beliefs about language (and also vision): for example, that human children are taught to speak. Now, GLs have accumulated an impressive computational picture of the human language faculty (involving complex theoretical notions like c-command, binding or long distance dependencies, etc., with no counterpart in commonsense beliefs about language).

Cognitive scientists, who are not theoretical linguists, share the common sense beliefs about language. So (and here I come back to my response to Dan on real vs. ideal institutions), it is both difficult and important that cognitive scientists, who are not linguist, be informed of how much theoretical advances in the study of the language faculty may run counter to their commonsense intuitions about language.

"Interdisciplinarity" in a One-Way World
Rainer Kamber
Jun 3, 2003 9:54 UT

In Pierre Jacob's highly insightful and exciting story about the Lyons Institut des Sciences Cognitives (LISC) one question is raised, to my mind, immediatly. Pierre seems to describe and discuss mostly one-way communication e.g. from the (strongly empirically oriented) Cognitive Neurosciences (CNS) to the (theoretically oriented) Philosophy of Mind (PM). It has not become clear to me in what way reverse communication has actually and genuinely taken place, from PM to CNS. Apart, that is, from the editorial work on the book which, in this case, does not seem to constitute a strong example of actual interdisciplinary knowledge production since here, too, theoretical work was mainly interpretatorial regarding empirical evidence gathered in the CNS. In what way has PM actually contributed to the empirical CNS in the case of LISC?

Since Pierre suggests a parallelism in CNS and PM regarding the "two visual systems hypothesis" one could speculate as to wether the epistemological dichotomy of "knowing that" and "knowing how" has actually contributed, e.g., to the formulation of empirical testable hypotheses or the the design of experimental settings. But I wonder if what we call PM in such a context would not more accurately be labelled "Theoretical CNS". The two fields, as described by Pierre within the context of the history of the LISC, seem to stand in the kind of relationship that is similar to the one between empirical and theoretical Biology. I admit, of course, that theoretical Biology is not identical to the Philosophy of Biology. But there seems to be enough conceptual and theoretical affinity between these fields as to allow scientific cooperation without the impediments usually experienced in interdisciplinary cooperation.

> **Reply to Rainer Kamber**
> Pierre Jacob
> Jun 4, 2003 9:30 UT
>
> I agree with Rainer Kamber that the joint work with Marc Jeannerod which I succinctly described in my paper is not an example of strong interdisciplinarity. I did say so in my paper on the grounds that we did not perform a novel experiment together. We did, however, make empirical predictions that can be tested. For example, we predicted that if an apperceptive agnosic patient could grasp simultaneously a pen in her right hand and its cap in her left hand, she could not match their relative shapes, sizes and orientations so as to fit the former into the latter.
>
> I also think that at several points in our joint work, my own representationalist proclivities did have an impact on our interpretation of some of the empirical results from cognitive neuroscience. If people are interested in the details, I can supply some of them later in the seminar.
>
> As a philosopher of mind, in the joint work reported in my paper, I was not primarily driven by the quest for interdisciplinarity for its own sake. I wanted to examine the question of how a broad array of empirical phenomena from electrophysiological recording of cells in the monkey, the neuropsychological examination of brain-lesioned human patients and psychophysical responses of healthy human subjects fits a particular theoretical framework, i.e., the two visual systems model of human vision. What I found challenging was (1) to try and provide a detailed analysis of the contrast between processing one and the same visual stimulus according to whether the task is perceptual or visuomotor; (2) to clarifiy the scope and limits of the two visual systems model of human vision.
>
> In response to Rainer's query about whether I embrace the view that philosophy of mind (as I see it) is really 'theoretical cognitive neuroscience', I want to say that I am a pluralist. As I alluded to in the paper, I started with questions that are broadly metaphysical in character such as: 'can one naturalize intentionality?'; 'is the content of a mental representation a causally efficacious property?'. In the joint work on vision, my questions became much narrower. I would not want by any means to disparage the virtues of broad metaphysical questions, in the philosophy of mind or elsewhere in philosophy, on the grounds that they are not open to empirical tests. After a good look at some empirical work in the area of human vision, there

remain a lot of exciting traditional metaphysical questions to be asked about the human mind. Just to mention one that I still find fascinating: how can our thoughts be about other things, some of which exist in space and time, others not?

The role of Philosophy of Mind in Cognitive Science
Gloria Origgi
Jun 4, 2003 15:21 UT

I am a philosopher who has interests in empirical research on cognition. In my experience, the role of philosophical reflection in the interaction with cognitive sciences cannot be reduced to that of "theoretical cognitive science", as Reiner Kamber suggests in his comment on : "Interdisciplinarity in a One-Way World".

Genuine philosophical problems may inform empirical research, but they do not have their origins in cognitive science: rather they belong to a well established tradition of philosophical reflection about human mind, the relation of the mind to the world, the representational power of the mind. What makes a state of mind a representational state is one of the oldest philosophical questions ever asked. Pierre's position of intentional realism - that is the claim that mind is a representational system whose function is to deliver representations of the environment for the benefit of the individual whose mind it is (cf. Pierre Jacob (1997) *What Minds can Do* Cambridge UP) – challenges the strong "Cartesian" tradition of a dualist ontology that treats minds and things as two different substances with irreducible different properties. These questions have become part of the scientific contemporary debate in the last 30 or 40 years, but they still belong to a lively philosophical tradition of questioning about the place of intentional/semantic properties in nature.

But Pierre's paper has a strong implication for philosophical research. In the last paragraph, he seems to suggest that philosophers should try to resist the temptation of the "big pictures" and stick to those hypotheses about the human mind that have empirical predictions. Is this an unavoidable consequence of a closer collaboration between cognitive science and philosophy or there is still place for a serious, autonomous philosophy that doesn't reduce to the formulation of testable hypotheses?

Reply to Gloria Origgi
Pierre Jacob
Jun 6, 2003 11:00 UT

I agree with Gloria that not every problem in the philosophy of mind derives from empirical results in cognitive science. Chomsky often reminds us that some important empirical work in cognitive science derives from philosophy. I think he is right, not just about his own work.

For example, work in developmental psychology on infants' perception and reasoning about physical objects (by Elizabeth Spelke, Renée Baillargeon, Susan Carey and others) derives from straight metaphysical investigation into the individuation of objects (by Quine, Strawson or Wiggins).

I rather like the idea that some of the best work in cognitive science has consisted in turning some of the best metaphysics into an empirical enterprise (which is what the developmental psychologists I just mentioned have done).

Gloria asks whether my paper implies that "philosophers should resist the temptation of 'big pictures'" and stick to empirical work. My answer is: Yes and no. In the last paragraph of my paper, I just tried to describe a divided (or hybrid) state of mind that I think can be ascribed to many philosophers of mind with a genuine interest for empirical work in cognitive science. In the paper, I resisted the temptation to state norms. On the one hand, the impulse towards 'big pictures' is, I think, a genuine part of the philosophical task. On the other hand, now taking a normative standpoint, I do think that resisting one's impulse is a cognitive virtue. It is not impossible, but it is hard work, both to satisfy one's impulse for 'big pictures' and to meet intellectual standards. Obviously, it can be done, since it has been.

Doing fieldwork
Christophe Heintz
Jun 9, 2003 23:12 UT

Pierre Jacob writes that he 'will be happy, in answer to questions, to go more into details and issues that might be relevant to the general topic of the seminar'.

By volunteering for the 'job' of 'informant', Pierre gives us a nice opportunity to conduct online fieldwork. So I wish we could indeed go into the details of how his interdisciplinary projects arose, grew up and finally ended in the production of interdisciplinary results (an institute and a book).

Questions for Pierre: In the case of the book, I can see different factors that allowed interdisciplinary collaboration. First, you and Marc Jeannerod were part of a common lab (the one you created), so you had the institutional framework. But on the other hand, what led you to talk to each other was not primarily interdisciplinary research on visual cognition, but your common management of a research lab. It is during **informal** talks, i.e. not institutionalised within an interdisciplinary program, that you designed your project. So how much does your collaboration owe to institutional framework and how much to friendship or contingent individual facts? Also, you explain that collaborative interdisciplinary research is rendered necessary by these dual explanations going on in current cognitive science: functional and biological. So eventually, your interdisciplinary work fits in an **already existing paradigm**: physicalism plus functionalism. To which extent did this paradigm *a priori* motivated your research?

I don't claim that there is a recipe for interdisciplinary research, but maybe we can try to pinpoint some of the **facilitating conditions** that allowed Pierre Jacob and Marc Jeannerod to do their research.

Has anyone already designed a questionnaire to be filled up by interdisciplinary researchers? I think it would be fruitful to post it in the discussion of that seminar. And I'm sure Pierre would agree to fill it up.

> **Reply to Christophe Heintz**
> Pierre Jacob
> Jun 11, 2003 10:11 UT
>
> First of all, in response to Christophe's detailed questions, I want to repeat a point I made in my paper, i.e., that joint work with Marc was not strongly interdisciplinary. We discussed the significance of a wide range of experiments for the two visual systems model of human vision. We made a couple of testable predictions. But we did not conduct joint experimental work.
>
> Since I am not an experimentalist, this raises the questions not raised by Christophe: can a non-experimentalist make sense of experimental papers? If so how? I will keep the questions in mind for further discussions.
>
> Christophe asks about the respective contribution of institutional arrangements and friendship to the development of collaborative work with Marc. By virtue of being in the same research unit, not only did we start intense discussions of scientific substance between the two of us, but we also attended public seminars many of which were relevant for the topic of our investigations. So conversations between the two of us extended into public seminars where ideas could be further probed and tested against other very knowledgeable people.
>
> Christophe asks about the role played by our common acceptance of an existing framework for our collaboration. He mentions physicalism and functionalism. He is right that physicalism and functionalism are prevalent ontological biases in the cognitive science community. Although we accept this high-level ontological framework, it did not, I think, play a crucial role. It is, I think, pitched at too high a level of abstraction. I am not sure that we had a single common motivation for examining the two visual systems model of human vision.
>
> In 1997, Marc published The Cognitive Neuroscience of Action. That same year, I published What Minds Can Do. Marc seemed quite interested in philosophers' discussions about what it is

to have a mind and how to understand the content of mental representations within a naturalistic framework. I am quite sure though — but of course, Marc should speak for himself — that Marc thought that several influential versions of the two visual systems model were wrong on empirical grounds and that he wanted to provide a better picture of how the human visual system really works. Reading Marc's 1997 book was my true introduction to what became the two main topics of our common book: that not all human vision is visual perception, and to try and provide a representationalist account of the contrast between the visual perception of objects and visually guided actions directed towards objects.

The exception or the rule
Ira Noveck
Jun 13, 2003 14:14 UT

Pierre's text (and work) amply demonstrates the good that can come out of interdiscipinary work. I for one have profited from Pierre's policy work by being employed (happily) at the Institut des Sciences Cognitives and by having undertaken interdisciplinary work as a result. But I think Pierre's text treats interdisciplinarity (in this context at least) too loosely. One needs to make a distinction between (at least) two kinds of interdisciplinary work.

One sort concerns technique. This is where a classically trained psycholinguist, e.g., has the occasion to learn about new measures like Evoked Potentials and to launch himself into new literatures that would otherwise remain at the margins of his research (I choose the male pronoun randomly). This requires an investment in time, but its added value is rather incremental in nature. That is, the new sorts of data (coming out of the same questions that concerned him beforehand) could contradict or strengthen established findings (or even provide new accounts for such findings). This is what is now going on I think in fMRI work, where the rigor of cognitive psychology brings out the best in imagery techniques (and vice versa). This is all well and good (and I think the norm with respect to innovation around l'Institut).

The other concerns theory. This is exemplified by Pierre's and Marc's efforts. This is the case where theoretical approaches are confronted by empirical ones. What Pierre described was a kind of conceptual change reserved for those looking to challenge established views. This I think is especially valuable because it brings theories in line with data and makes both the better for it. However, I think this is rather rare and it is hard work. I also think that it is asymmetrical in that it is much easier for a theoretically minded person to catch up with empirical findings than for an experimentalist to challenge theories on purely theoretical grounds. (This partly explains my posting to Dan, where I suggested a specific role for philosophers in interdisciplinarity). One will find few practiced experimentalists taking Pierre's leap in the other direction.

The theoretical-experimental gap partly explains Pierre's comment on the generative grammarians and their role in the Institut. They are ideal people to have around because they could vet linguistic hypotheses and their corresponding materials. However, one will find few experimentalists who will make the leap towards understanding the nuts and bolts of Transformational Grammar.

This gap I think is essential for understanding ways to facilitate interdisciplinarity in the Cognitive Sciences. (Physicists would point out that this Theoretical-Experimental distinction is pretty well established in their discipline and that it does not block leaps from one side to the other.) If one does not see the value of(even benevolent) philosophers taking the lead in aligning theory and data, real theoretical training ought to be fostered across the Cognitive Sciences.

Reply to Ira
Pierre Jacob
Jun 18, 2003 15:05 UT

I agree with Ira about almost everything. I think his point about the contribution of theoretical linguistics to cognitive science is well-taken. One set of issues (mentioned in my response to Dan on "Grain, strength, and interdisciplinarity"), which might be well worth exploring further are:

a) much cognitive neuroscience has been devoted to the question of where in the human brain is mental process P taking place.

b) Much cognitive psychology has been devoted to the question of how long does it take for mental process P to be completed.

c) Most theoretical linguistics is concerned with the question: what is the structure of linguistic knowledge?

It's hard to map a relevant true response to c) onto established neuroscientific and psychological experimental methods designed respectively for answering questions about where and how long mental processes take place. Perhaps I am a little less inclined than Ira seems to be to praise the intellectual virtues of what he calls the move from a theoretical framework to empirical data. Here, the problem to be discussed (mentioned in passing in my response to Christophe on "Doing fieldwork") is: what can a non-experimentalist manage to extract from his reading experimental papers? Response: certainly not what an experimentalist can extract. For example, it's almost impossible for a philosopher of mind (at least for me) to tell whether an experiment has been well-done. Suppose the question was: were the people doing the experiment right to stop doing it because they got a real piece of data? I for one would have no clue. So the question arises: is an experimental paper open to different kinds of readings? Is it open to different kinds of understanding? If so, how should we capture the kinds of understanding in question? Are there different levels of understanding? Should the variety of understandings be captured as a matter of a difference in fine-grainedness? Is the transition from one level to the next (either upwards ot downards) a continuous or a discontinuous process? Are there local slots in one's understanding to be filled by further information? Is it some holistic process of interpretation? Should one think of the transition on the model of how one moves from a general existential proposition to a singular proposition by getting more contextual information? Can one stand at two different levels of understanding simultaneously? Does one lose some information as one changes levels (if difference in understanding is a matter of levels)?

Interdisciplinarity and Its Enemies
Craig Hamilton
Jun 17, 2003 14:25 UT

Dan Sperber's descriptions back in April of the Culture and Cognition program at the University of Michigan, and Pierre Jacob's descriptions this month of the Institute of Cognitive Science in Lyon, reveal some of the hard problems interdisciplinary research faces. While the problems they discuss are too complex to review here, a series of questions can highlight them: What is it? Why is it needed? What is its function? Who runs it? Who works in it? Why do they do it? What gets studied? Why does it get studied? How does it get studied? Who pays for it? Who gets the credit when it works? Who gets the blame when it does not? (Either the interdisciplinary research, or the center housing the research itself, is the 'it' here.)

Every interdisciplinary scholar will recognize these questions, especially if they work at a university that for ages has divided knowledge into departments. Because these questions are again and again put to interdisciplinary scholars, they can easily turn a promising enterprise into a disillusioned one. Interdisciplinary research seems forever vulnerable to this sort of up-and-down cycle although the same would be true if enough questions were put to any university department. I say this as someone from the University of Nottingham's Institute for the Study of Genetics, Biorisks, and Society, a new interdisciplinary institute that yokes the humanities to the social sciences to study biotechnology and society. As Pierre suggests with his example from Lyon, while doing the research itself is not always easy, one thing is even harder: "it has proved difficult to hire the best scientists for the required special slots [...] and not to depend excessively on existing CNRS research positions." I am certain this true. It can feel strange at an interview for an overtly advertized interdisciplinary job at the CNRS to face close-minded sceptics rather than open-minded enthusiasts. With friends like these, the young interdisciplinary scholar might wonder, who needs enemies? As I see it, someone like Pierre demonstrates how to make progress in science. When an expert in one field (philosophy) listens to an expert in another field (neuroscience), new answers to old problems can be found. This requires a skill

in listening (Pierre and Marc have been talking for ten years), but it may be in very short supply. It also requires open minds, which may be in even shorter supply now.

Response to Craig Hamilton
Pierre Jacob
Jun 24, 2003 16:41 UT

I for one don't feel that I can offer relevant responses to Craig Hamilton's batch of wh-questions about interdisciplinarity in the first paragraph of his comments.

In response to Craig's point about interdisciplinarity and open-mindedness, I'd like to make two comments.

First, insofar as interdisciplinarity requires learning novel things, it does require open-mindedness. As I see it, however, science requires a delicate balance between open-mindedness and dogmatism. Any science does. You don't give up a good theory in the face of what seems prima facie like counter-evidence until you have tried hard to accommodate the evidence and, as much philosophy of science has noticed, unless you have an alternative theory.

It is hard to give a general (let alone a formal) account of the required balance between open-mindedness and dogmatism in science. Much of the controversy in the philosophy of science between Karl Popper and Rudolf Carnap is about this. Carnap developed an inductive logic based on the assumption that it is rational for a scientist to select the theory that has the highest degree of probability relative to the evidence. Against Carnap, Popper argued that, among two or more hypotheses, it is rational to choose the boldest, i.e., the most improbable, not the most probable, given the evidence. I just mention this dispute as evidence that the issue of the balance between open-mindedness and dogmatism in science is a complex one.

I don't think interdisciplinarity makes scientific work any different: it too requires a certain amount of dogmatism. Even if you're involved in some interdisciplinary project, stick to good theories as long as you can. Secondly, one can certainly sympathize with the agony of a young scientist who applies for a position advertised as an interdisciplinary slot and who is being interviewed by an 'interdisciplinary sceptic'. But as I said in response to an earlier comment of Dan's, I am not convinced of the value of interdisciplinarity for its own sake. I am not sure that interdisciplinarity per se has intrinsic epistemic or moral value. So it all depends on which science has the best resources to approach the relevant problems.

It is worth investing time in learning new things if you have reasons to believe that it will contribute to the solution of a given problem. It is worth encouraging a young scientist to cross disciplinary boundaries if you have reason to believe that it will help her solve an interesting and well-posed problem. Otherwise, let her look for her lost keys where there is light (in Chomsky's joke mentioned by Dan, unless it was Tom Kuhn's).

Raconter n'est pas réfléchir
Abdelkarim Fourati
Jun 17, 2003 14:46 UT

Dans le premier texte de ce colloque, présenté en avril 2003, Dan Sperber disait: « Normalement, nous ne discutons pas entre nous de l'interdisciplinarité en elle-même. Ce que nous faisons, c'est travailler sur des problèmes qui se trouvent relever de plusieurs disciplines, et pour cela nous établissons des collaborations entre philosophes, psychologues, neuropsychologues, linguistes, anthropologues, etc. Cependant, de même que tant d'autres chercheurs, étudiants, et responsables d'institutions scientifiques, nous avons de bonnes raisons de prendre le temps de réfléchir à l'interdisciplinarité elle-même ». Cette remarque de Sperber peut s'appliquer aux travaux de Pierre Jacob. En effet, raconter son expérience de collaboration avec l'équipe de Jeannerod, ce n'est pas réfléchir rationnellement - et encore moins repenser - sur l'interdisciplinarité (sujet du colloque virtuel), même si cette expérience était fructueuse et dans le cadre des sciences cognitives qui sont, par leur conception même, inter- et transdisciplinaires.

Monsieur Jacob, en tant que philosophe naturaliste de l'esprit, n'a pas discuté le rôle qu'ont - ou devraient avoir - la philosophie des sciences, l'épistémologie et la sociologie des sciences dans les processus qui mènent à une recherche interdisciplinaire? Quelle est la place des sciences cognitives parmi les disciplines scientifiques en général? ... En effet, la philosophie n'est plus - si elle l'a jamais été - la reine des sciences, dictant les directions de recherches. Rappelons en effet que la philosophie des sciences (et l'épistémologie) dans son acception classique, bien qu'elle se voue à l'examen critique des conditions et méthodes de la connaissance scientifique; et qu'elle examine la validité des formes d'explication, la pertinence des règles logiques d'inférence, les conditions d'utilisation des concepts et théories scientifiques; elle se refuse d'examiner les résultats, c'est-à-dire les connaissances en elles-mêmes: d'où ses insuffisances. Et bien qu'elle se refuse cette tâche importante, cette philosophie des sciences se pose en tribunal extérieur/ supérieur à la science, apte à la juger pour violation des règles et des normes!

D'autre part, on oublie souvent le rôle de l'organisation des connaissances elles-mêmes, dans le système de recherche/ enseignement et l'utilisation pratique de ces connaissances (disciplinaires ou pluri-disciplinaires). Autrement dit, les chercheurs/ enseignants et les acteurs/ praticiens travaillent avec des connaissances (de leurs disciplines, parfois même en pluridisciplinarité), sans vouloir ou pouvoir passer au niveau de la réflexion rationnelle proprement dite, pour travailler sur les connaissances elles-mêmes, et delà ils négligent ce qui relie et ce qui se trouve entre leurs disciplines respectives (inter- et trans-disciplines). Ici je rejoins l'image donnée par Dan Sperber: « … vers une organisation "post-disciplinaire" du savoir. Il ne peut s'agir de revenir de la spécialisation actuelle à une omni-compétence qui n'est plus possible depuis longtemps. En revanche, on peut imaginer que la formation et la spécialisation se feront bien plus « à la carte » et que les scientifiques soient organisés non pas en disciplines autonomes, mais en un réseau continu avec des zones plus lâches et d'autres plus denses évoluant assez rapidement ». Dans cette conception, les zones plus lâches correspondent aux connaissances-réflexions sur les disciplines et les zones plus denses correspondent aux connaissances elles-mêmes…

Réponse à d'Abdelkarim Fourati
Pierre Jacob
Jun 19, 2003 17:23 UT

Premièrement, les commentaires d'Abdelkarim Fourati se prêtent, selon moi, à la question suivante : peut-il à la fois critiquer sévèrement l'épistémologie et la philosophie des sciences et me reprocher de ne pas adopter leur point de vue ?

Selon lui, l'épistémologie et la philosophie des sciences ont tort de prescrire des normes à la démarche scientifique sans se donner la peine d'examiner des exemples effectifs de découvertes scientifiques. Pour défendre la démarche normative de l'épistémologie et de la philosophie des sciences, je dirais qu'on ne peut ni confirmer ni infirmer une norme en décrivant un fait. On ne peut pas, par exemple, réfuter la norme selon laquelle on ne doit pas rompre sa promesse en exhibant des promesses non tenues. Lorsqu'elles sont bien faites, l'épistémologie et la philosophie des sciences effectuent, je crois, un délicat ajustement entre l'examen descriptif du développement des connaissances scientifiques et les prescriptions normatives : c'est ce que le philosophe Nelson Goodman nommait "l'équilibre réflexif".

Deuxièmement, à la question d'Abdelkarim Fourati sur les relations entre les sciences cognitives et d'autres sciences, j'ai répondu succinctement dans l'article qu'il discute : selon moi, les sciences cognitives sont à l'interface entre les sciences de la nature et les sciences humaines et sociales. A la différence des sciences de la nature, les sciences humaines et sociales expliquent les comportements humains en attribuant aux agents des représentations mentales. Les sciences cognitives commencent à conférer une respectabilité scientifique à la notion même de représentation mentale. Troisièmement, le contraste entre raconter une expérience et réfléchir est trop simple.

a) Il n'est pas exact qu'en rapportant une expérience, on n'y réfléchit pas. Raconter une histoire, c'est offrir un certain type d'explication. Plusieurs versions différentes d'un même événement peuvent être en désaccord. Certains faits objectifs premettent souvent de préférer une version à une autre.

b) Comme l'ont souligné les philosophes fonctionnalistes, on peut offrir plusieurs explications causales correctes distinctes d'un même fait. Par exemple, on peut faire comprendre le processus de suppression d'une douleur en mentionnant la formule chimique du médicament avalé ou en mentionnant sa propriété d'être un antalgique. Sans doute, faute d'avoir une composition chimique déterminée, un médicament ne pourrait pas être un antalgique. Mais il n'est pas toujours indispensable de connaitre la formule chimique d'un médicament pour comprendre pourquoi il a contribué à supprimer une douleur.

c) Enfin, toute explication n'est pas une explication causale. Par exemple, expliquer la signification d'un mot en donnant un synonyme n'est pas fournir une explication causale.

Répondre à Pierre Jacob
Abdelkarim Fourati
Jun 26, 2003 14:26 UT

Pour répondre à Pierre Jacob, permettez-moi une précision: j'ai parlé dans mon intervention « d'insuffisances de la philosophie des sciences et l'épistémologie dans son acception classique ». Autrement dit, elles sont nécessaires mais insuffisantes vu l'ambiguïté de la relation entre l'épistémologie et les sciences cognitives. Edgar Morin [La méthode. 3. La connaissance de la connaissance, Editions du Seuil, 1986, p.23] disait, à juste titre: « La relation entre l'épistémologie et les sciences cognitives ne peut être qu'étrange. Pour ces dernières, l'épistémologie est une des sciences qu'elles embrassent. Pour l'épistémologie, les sciences cognitives sont quelques-unes des sciences qu'elles examinent. Pour nous, ici, il s'agira d'assumer la complémentarité des deux points de vue antagonistes… ». Donc défendre la démarche purement normative de l'épistémologie et de la philosophie des sciences, c'est se priver de l'autre face de l'épistémologie qui est plus proche des sciences aussi bien de la nature qu'humaines et sociales. De fait, en tant que médecin hospitalo-universitaire, j'étais et je suis toujours largement en faveur (et même incitateur) à l'utilité de l'enseignement de l'histoire de la médecine, épistémologie médicale et socilogie médicale, pour mieux comprendre la médecine.

D'autre part, je suis complètement d'accord avec Pierre Jacob que: « les sciences cognitives sont à l'interface entre les sciences de la nature et les sciences humaines et sociales. A la différence des sciences de la nature, les sciences humaines et sociales expliquent les comportements humains en attribuant aux agents des représentations mentales. Les sciences cognitives commencent à conférer une respectabilité scientifique à la notion même de représentation mentale ». Reprenons le cas de la médecine: j'ai déjà dit dans une intervention précédente, en mai 2003, sur « Le potentiel de la transdisciplinarité » de Helga Nowotny, que: 1- « La médecine se trouve au carrefour de la grande coupure entre les sciences physico-biologiques et anthropo-sociologiques. Justement, mon grand projet de recherche inter et trans-disciplinaire, depuis le début des années 1980, est la contribution à l'articulation des sciences bio-médicales et anthropo-sociologiques par les sciences cognitives ». 2- « L'introduction des nouvelles technologies pour traiter l'information médicale et aider à la pratique médicale, n'a pas encore apporté tous ses fruits. 3- « Il devenait nécessaire un nouveau regard de la médecine sur elle-même: une méta-médecine… Une révolution scientifique en médecine est nécessaire; mais elle ne peut se faire que par une intégration adéquate des sciences cognitives ».

Ici je pose la question: l'épistémologie normative, comme la définit Pierre Jacob, peut-elle me prescrire la façon la plus adéquate, pour l'utilisation des sciences cognitives en médecine, sans une réflexion de la médecine sur elle-même, c'est-à-dire sans connaître les particularités de la médecine, de ses méthodes et ses résultats, surtout si ses méthodes restent encore implicites dans la pratiques des spécialistes. De plus, comment l'épistémologie normative peut-elle articuler les différentes disciplines de la pratique et de la théorie médicales, sans les avoir analysés en profondeur, et se contenter d'une vue superficielle de l'extérieur et de loin… (un médecin peut-il diagnostiquer une maladie grave, sans examens cliniques et complémentaires).

Interdisciplinarity, systemic, cross fertilization

Christophe Menant
Jun 17, 2003 21:18 UT

Pierre Jacob's article presenting the collaboration between a Cognitive Neuroscientist and a Philosopher in the field of visual information processing is interesting and informative.

It shows that "soft interdisciplinarity" is a reality bringing true added value. Also, one may even consider looking at an additional step within the cognitive process as presented. A step positioned between visual perceptual processing and visuomotor processing. It is about the "decision making" by the subject that will generate the action of grasping or not the object. This step is a specific event in the cognitive field, and it allows interdisciplinarity. It brings on stage items regarding biology, philosophy, anthropology and psychology. (But I have not read "Ways of seeing". Do you explicit such a step in the book? If so, forgive me for just repeating what you may have already said).

This subject of perception/decision/action is of some interest to me by the way of a systemic approach (as some of you know): a system submitted to a constraint manages the perception/action transition by generating meaningful information related to the satisfaction of the constraint. (http://www.short-theory-meaning.fr.st/)

This brings us to another point I feel interesting relatively to interdisciplinarity. It is about the opening allowed by the usage of a systemic approach. A system, understood as an ensemble of elements linked by an ensemble of relations, leaves to each discipline some freedom of choice for wordings the elements and the relations. Such approach eases possible alternate readings of a given subject. Going back to Dan Sperber's example about psychologists and anthropologists having difficulties to reach a synergy on a given subject: would a systemic presentation have been of some help in making available to them a common ground for the view that there are fundamental differences in the modes of thought of members of different cultures ? (Point they were clearly illustrating by their behavior).

A last point is about cross fertilization vs interdisciplinarity. Among the tools that can be used for interdisciplinarity is cross fertilization: concepts, ideas, tools, implemented in one field can be used by other fields for the build up of new knowledge. This is not new and it has been on going for long. The new element today is that Internet makes it more and more a possibility. Forums are a good example. But the last step of the process remains the openness of mind, the curiosity of the participants. And perhaps this is a key point that needs to be valorized for interdisciplinarity.

> **Reply to Christophe Menant**
> Pierre Jacob
> Jun 22, 2003 15:30 UT
>
> Christophe Menant makes the interesting suggestion that we examine the decision-making process whereby an agent makes up her mind about whether or not to reach and grasp an object. In the book with Marc, we do not have anything to say about the decision process itself: we say something about what happens once the agent has decided to grasp a target, not about how the agent comes to this decision.
>
> However, we do make the following claims that are, I think, relevant to Christophe Menant's observation: we claim that reaching for and grasping an object (e.g., a glass of beer) is an action or a piece of intentional behavior. Thus, on our view, reaching for and grasping an object is unlike e.g., the pupillary reflex or what the philosopher O'Shaugnessy calls 'sub-intentional acts' (e.g., moving your tongue in your mouth as you read), none which are genuine actions caused by some of the agent's mental representations. By contrast, we claim that the agent's hand and arm movements by which he grasps an object are caused by some of his mental representations.
>
> We further claim that whereas not every intentional action is caused by some of the agent's beliefs and desires, it is caused by some of the agent's intentions - at least by the agent's motor intention. Furthermore, in most cases it depends on visual information about the target of

prehension. We claim that the visual information supplied to the agent's intention results from visuomotor (or low-level pragmatic) processing of the visual properties of the target that are relevant for the action of prehension.

To reiterate a point made earlier, as a philosopher of mind, I was eager to argue both that a representational account of the visual mind has the resources to clarify some of the puzzles of visually guided actions and that it fitted with the two visual systems model of human vision.

Rapports entre les sciences cognitives et l'encyclopédisme
Martine GROULT
Jun 20, 2003 13:23 UT

Lorsque 2 disciplines scientifiques constituées donnent naissance à une nouvelle discipline, ce que vous qualifiez justement d'interdisciplinarité forte, les éditeurs de l'Encyclopédie l'ont appelé (par son nom...) : la création. Ce processus a constitué ce qu'il est convenu de considérer comme la philosophie de l'Encyclopédie. C'est tout du moins l'objet de mes recherches au CNRS, ce qui explique le vif intérêt que je porte à ce séminaire.

Si j'ai bien compris, une large part de l'interdisciplinarité, telle que vous la définissez, revient à saisir les chemins de la visualisation ou plus exactement de l'esprit en train de voir c'est-à-dire de comprendre. Selon mon interprétation, les chemins appartiennent à la science et la saisie à la philosophie. Ainsi, si j'ai bien suivi votre exposé, lorsque les sciences se trouvent sur la voie de l'invention, l'esprit humain travaille "la compréhension des intentions motrices" et lorsque la philosophie analyse les actions humaines de tous les points de vue (historique, anthropologique, psychologique), l'esprit humain travaille "la compréhension des intentions sociales". Intentions motrices et intentions sociales appartiennent au travail de l'esprit. Ce travail de l'entendement constitue pour l'Encyclopédie (et la philosophie du Discours préliminaire plus particulièrement) la voie de la transcendance qui avance par propriétés. Je mélange intentionnellement cerveau et entendement car c'est mettre ensemble science et philosophie c'est-à-dire considérer les intentions comme des faits scientifiques et non comme des idées non vérifiables.

Aussi, il me semble qu'à ce qui constituait la philosophie de l'Encyclopédie, il a été attribué une discipline : les sciences cognitives. Si tel est le cas et si cette logique encyclopédique à laquelle je fais référence est bien une des sources des sciences cognitives, j'aimerais avoir votre avis sur les rapports entre l'encyclopédisme et les sciences cognitives?

Pensez-vous alors envisageable de faire remonter au 18e siècle, plutôt qu'au 19e comme le précise Dan Sperber, l'organisation de la science moderne qui a conduit aux sciences cognitives nées de l'interdisciplinarité? Et pensez-vous organiser un jour un séminaire sur les rapports entre l'Encyclopédie et les sciences cognitives, ce qui reviendrait selon mon interprétation à identifier l'expression "d'interdisciplinarité forte"?

Réponse à Martine Groult
Pierre Jacob
Jun 23, 2003 10:13 UT

Premièrement, Martine Groult propose de nommer "la création" le processus par lequel deux disciplines scientifiques donnent naissance à une troisième discipline. Malgré l'autorité de l'Encyclopédie, je demeure réticent. L'emploi de ce terme me paraît inapproprié pour deux raisons.

a) L'application de ce terme me semble plus appropriée à la démarche artistique qu'à la démarche scientifique - du moins à la démarche des sciences empiriques ou expérimentales. Les artistes créent des ¦uvres d'art. Dans une certaine interprétation- constructiviste ou intuitionniste - des mathématiques, en démontrant un théorème, un mathématicien "crée" une vérité. Mais dans l'interprétation platonicienne, en démontrant un théorème, un mathématicien "découvre" une vérité. Dans les sciences empiriques ou expérimentales, dire d'un astronome, d'un chimiste ou d'un immunologiste qu'il a "créé" plutôt que "découvert" l'anneau d'une planète,

la structure d'une molécule ou celle d'un anti-corps me paraît avoir des implications "constructivistes" superflues.

b) Il serait fourvoyant de prétendre que le syntagme défini "la création" (composé de l'article défini) s'applique convenablement au processus d'engendrement d'une nouvelle discipline à partir de deux disciplines existantes. Cela laisse entendre non seulement que ce processus épuise à lui tout seul les variétés possibles des processus de création, mais de plus qu'il suffirait de mélanger aribtrairement deux catégories de discours pour mériter l'étiquette "interdisciplinarité". A certains de ses partisans contemporains qui font valoir que l'astrologie résulte du croisement de l'astronomie et de la "psychologie des profondeurs", on est en droit d'exiger des preuves empiriques du processus de fécondation "interdisciplinaire".

Deuxièmement, si le travail interdisciplinaire sur la vision effectué avec Marc Jeannerod a consisté "saisir" ce que Martine Groult nomme "les chemins de la visualisation", alors ceux-ci ne doivent pas être définis comme "l'esprit en train de comprendre". Nous soulignons l'existence d'une dualité dans la vision humaine. La perception visuelle est au service de la compréhension et de la connaissance du monde. Mais la vision humaine sert aussi à guider des actions manuelles de préhension. En utilisant la terminologie de Martine Groult, on pourrait dire que les représentations visuomotrices relèvent autant du "travail de l'esprit visuel" humain que la perception.

Troisièmement, nous soutenons que la perception visuelle des actions se sous-divise en deux sous-systèmes complémentaires : l'un est spécialisé dans la perception des actions dirigées vers des objets ; l'autre est spécialisé dans la perception des actions dirigées vers des congénères. Le premier contribue à la compréhension des intentions motrices (qui servent aux agents à organiser leurs actions en direction des objets). Le second contribue à la compréhension des intentions sociales (par lesquelles les agents visent à modifier le comportement de leurs congénères). Mais je ne reconnaîs cette distinction ni dans la dualité proposée par Martine Groult entre les sciences et la philosophie ni dans une hypothétique "voie de la transcendance qui avance par propriétés".

The cognitive bases of disciplines
Christophe Heintz
Jun 23, 2003 23:03 UT

Pierre makes a point to show that our cognitive apparatus contains some highly specialised devices that have specific functions. While Pierre studied how the processes that allow visual cognition can be both functionally and architecturally analysed, I believe that a similar analysis is possible for scientific cognition. This is highly controversial since science is taken to be one of the most ‘isotropic’ phenomena: science takes on board whatever evidence that may prove relevant for scientific theorizing. But of course, I do not mean that scientific thinking a priori restricts the evidence to take into considerations. What I mean, rather, is that the structure of science – i.e. its disciplinary organization- may have some bases in the structure of our mind.

Examples:

- Atran’s *Cognitive Foundations of Natural History* is a detailed examination of the relation between a hypothesised universal capacity for thinking about plants and animals and the history of Zoology, Botany and Natural History. Atran shows that the history of the scientific disciplines dealing with life is highly related to an innate mental device generating ‘naïve’ thought about living organism (in terms of essence, hierarchical classificatory concepts, etc.). While contemporary life sciences are at odd with naïve theories, in practice the naïve apprehension of living organism is still at work in scientific cognition, even if naïve thoughts are then reinterpreted in current theories.

- Mathematics: even thought it may be difficult to point out a single specific ability to do mathematics, sub-disciplines may have evolved under the constraint of cognitive abilities. Indeed, cognitive psychologists working of mathematical cognition have been pointing out cognitive abilities that underlie our mathematical intuitions. E.g. Gallistel shows that we have an innate capacity to apprehend continuous (vs. discrete) quantity. He asserts furthermore that this capacity is certainly at

the origin of the development of the calculus. Arguably, Logic also is strongly related with an innate capacity to reason.

- Anthropology: it appears to me that our abilities to attribute mental states (desires, beliefs, etc.) to others – which is currently studied under the names of Mindreading, Theory of Mind or Folk Psychology – is at the heart of the method of anthropology (participant observation). In fact, it seems to me that mind reading is the cognitive ground of much of the practices in the Social and Human Science, at least, in their interpretative components.

The abilities lately posited by cognitive psychologists curiously mirror scientific disciplines. Cognitive psychologists thus talk about naïve physics, naïve biology, naïve psychology, and common sense itself has been studied as a kind of naïve logic (e.g. Macnamara).
There can be two explanations of this phenomenon:
1. Cognitive psychologists have delimited the scope of their theories about capacities by using currently known scientific disciplines rather than by genuine cognitive objects.
2. Disciplines have developed on the basis of naïve theories. Disciplinary boundaries are not just the produce of historical contingencies, but also of psychological contingencies.
I think that both points hold. But while it is to the cognitive psychologists to clarify the first point, it belongs to 'science students' to investigate the plausibility of the second point.

Folk theories and scientific theories
Gloria Origgi
Jun 26, 2003 17:21 UT

Christophe raises a very interesting issue for the whole topic of interdisciplinarity, that is, the relation between our cognitive folk theories such as folk physics or folk psychology that all children develop in the first years of their life, and the scientific theories that stabilize in a population and are culturally transmitted.

Christophe's point is a sort of defence of the "naturalness of disciplinarity", when he claims that the structure of our science may somewhat be constrained by the structure of our mind. In a sense, he echoes the famous question of the neuroscientist Warren McCulloch, in his famous 1965 paper: "What is a number that a man may know it, and a man that he may know a number?" (By the way, nowadays we would consider the possibility that even women may know a number…)

This is definitely a fascinating project of research, and has been recently explored for example by Ian Hacking. His idea is that each scientific discipline that has stabilized in the history of science has its root in a specific cognitive capacity (see his recent book: *Historical Ontology*, Harvard UP). Hacking defines himself as a "dialectical realist", preoccupied by the interactions between what there is (and what it comes into being) and our conceptions of it.

Nevertheless a question remains open about which epistemic criteria we may adopt to pry apart cognitive constraints that give rise to genuine scientific knowledge from those that give rise to very robust pieces of culture such as astrology, theory of humours, homeopathy, etc. Are these criteria completely normative and historical? Is Christophe suggesting that the study of cognitive constraints on scientific theorizing can tell us something about the epistemic value of such theories?

Reply to Gloria
Christophe Heintz
Jun 30, 2003 0:05 UT

There are, indeed, different ways to tackle the problem of scientific disciplines.

1. The old way: you (more or less dogmatically) assert what justifies discipline boundaries and prescribe a consequent behaviour. e.g. The Ontology is such and such, and discipline boundaries should reflect ontological differences. For instance, life is different from inanimate

things, so Biology and Physics should not be conflated – the reductionnist project is absurd. Or: there is no genuine justification for discipline boundaries; scientific investigation should free itself from such a bridle.

2. The naturalistic way: You attempt to give a **causal explanation** of the disciplines and restrain from normative consideration. Causal explanations, it seems to me, can either show the causal power of socio-historical events on the drawing of boundaries or point out cognitive constraints on the making of disciplinary boundaries and specify their causal actions (e.g. by rendering cultural stabilisation possible via cognitive attractors). The social analysis may lead you to analyse, e.g., power relations. For instance you may say that Sociology appeared in France as an autonomous disciplines, rather than a sub-discipline of psychology, because Durkheim fought to have his own chair. You may even, if you work in the Durkeimian tradition, observe and explain isomorphic relations between the disciplinary organisation of scientific knowledge and power relations. So Sociology is a full discipline as Psychology because Durkheim has the same power/prestige as the person who holds the chair of psychology.

My previous message was intended to be a naturalistic attempt to answer to the problem of scientific disciplines. But rather than taking the socio-historical causes, I wanted to point out the possible role of cognitive causes. I did that by showing the possible isomorphic relations between the disciplinary organisation of scientific knowledge and cognitive abilities. Following this naturalistic attempt, my answer to Gloria's question: "Is Christophe suggesting that the study of cognitive constraints on scientific theorizing can tell us something about the epistemic value of such theories?" Is NO. Reciprocally, such an analysis CANNOT, by itself, "pry apart cognitive constraints that give rise to genuine scientific knowledge from those that give rise to very robust pieces of culture such as astrology, theory of humours, homeopathy, etc. **It is the work of science itself to tell when a cognitive process is reliable and when it is not.** For instance, the Muller Lyer illusion is discovered to be an illusion because we rely on the scientific knowledge of length, not because we know that our visual apparatus is flawed!

3. The renewed old way: You take the input from science on board and attempt to provide modest prescriptions (you may also want to use notions such as Truth, à la Goldman). Tackling the problem of disciplines this way leads to questions such as 'when does an interdisciplinary program prove fruitful?' and you try to answer with case studies; or you assume that interdisciplinarity can be fruitful sometimes and you wonder 'what are the facilitating conditions for interdisciplinary research?' (cf. my posting 'Doing fieldwork').

▽Are 'visuomotor representations' genuine representations ?
Maria Rossi
Jun 26, 2003 8:51 UT

This note bears on the concept of 'visuomotor representation', which seems to relate to an important theoretical problem (cf. 'Visuomotor representations vs. Visual percepts'). You wrote: "I came across a wide array of empirical work from the cognitive neurosciences of vision that converges on the conclusion that it is not true that the goal of the human visual system is to give rise to visually based knowledge of the world. Instead, much visual processing in humans is devoted to the guidance of object-directed actions. It dawned upon me that this evidence provided new arguments for the representational account of the visual mind. (…) I wanted to argue that human hand actions directed towards objects are guided by special visual representations — visuomotor representations. The task was to analyze the differences between the nonconceptual content of visual percepts and the nonconceptual content of visuomotor representations."

Nonetheless, is it legitimate to conceive of as genuine 'representations' the capacities you are referring to with the concept of 'visuomotor representations' (rather than as non-representational anchoring mechanisms)? Is this a parsimonious explanation? Where are the boundaries? The reply to these questions depends on the concept of 'representation' you are working with. Therefore, such question can be rephrased under this form: How do you construe and use the concept of 'representation' so as to explain the relationship between cognitive mental states and visuomotor capacities (such as ocular fixation, manual grasping and reaching) ?

This problem arises because many analyses or features of the concept of ‘representation’ does not seem to be obviously satisfied by a sensory-motor state – in particular, for instance, these features: ‘to be a productive and systematic mental state’ (have compositional semantics), ‘to be a conceptual description’, ‘to have (narrow) phenomenal content’, ‘to have truth conditions’ and so forth. Even if you might claim that “sensori-motor representations” can be functionally and pragmatically evaluated (= may have some form of correctness conditions), can these states be productive?

(By the way, the question is related to the general topic of interdisciplinarity, because it might not have been studied without a collaboration between, at least, philosophy of mind and cognitive neuroscience.)

Reply to Nicolas Bullot
Pierre Jacob
Jul 5, 2003 21:47 UT

Nicolas' question is an excellent one. Contrary to what Nicolas implies in his third paragraph, I don't think that all representations must be productive and systematic. The reason is that productivity and systematicity are features of representations with conceptual content and not all representations have conceptual content. Thoughts (beliefs, judgments) and utterances do: they are productive and systematic. But on my view, perceptual representations, mental images and pictures are neither productive nor systematic.

Why should we postulate perceptual representations with non-conceptual content? Briefly, the psychological explanation of e.g., a person's visual perception requires the postulation of internal states with correctness conditions whose informational richness and fine-grainedness outstrip her conceptual mastery. On the one hand, a person's ability to make color and/or shape discriminations is not matched by her ability to name and/or conceptualize shades of colors and shapes. On the other hand, although you may know that the two line segments with different arrow configurations in the Müller-Lyer illusion are equal, nonetheless you perceive them as unequal. Thus, the content of your incorrect percept is impervious to what you know.

In the book with Marc, we present arguments for bifurcating non-conceptual content into perceptual content and visuomotor content. The main argument in favor of the existence of visuomotor representations with a non-conceptual content different from the content of visual percepts proceeds in two steps. First, you find dissociations between responses to perceptual tasks and responses to visuomotor tasks. Secondly, you show that the responses to visuomotor tasks are not simple reflexes but genuine actions whose psychological explanation requires the postulation of mental representations (different from the representations engaged in the perceptual task). There are two sorts of evidence for the dissociation between perceptual and visuomotor responses. On the one hand, the neuropsychological examination of visual agnosic patients with a lesion in their ventral pathway shows that they cannot visually process the sizes, shapes and orientations of objects whose relative spatial locations are coded in so-called allocentric coordinates, for the purpose of perceptual identification. However, they can visually process the size, shape and orientation of an objects whose location is coded in egocentric coordinates when they are required to grasp it. On the other hand, there is psychophysical evidence that healthy human subjects respond differently to an illusory display according to whether the task is a perceptual estimate or a visually guided action of prehension. Consider the Titchener circles illusion: two disks of equal diameter look unequal if the first one is surrounded by circles smaller than it and the second one is surrounded by circles larger than it. The former looks larger than the latter. Briefly, the evidence is that the perceptual illusion arises from the automatic comparison of the relative sizes respectively of a disk and of the surrounding circles. But if the task is grasping one of the disks, then what matters is the distance between the target disk and the surrounding annulus, not the relative size of the circles in the annulus. Since the annulus of circles surrounding a 3D graspable disk is itself a 2D stimulus, it follows that the visuomotor processing can be fooled by selective features of the visual display. To say that it can be fooled is to say that the visuomotor processing generates a representation of the display.

The Role of Information Science in Interdisciplinary Research: A Systemic Approach

Catherine Garbay (CNRS)

(Date of publication: 1 September 2003)

Abstract: This paper focuses on the key role of Information and Communication Technology for the development of interdisciplinary programs in contemporary scientific research. An integrated model of the relation between humans and artifacts is discussed that leads to a new perspective on human affairs, technical matters and knowledge construction.

(Translated from French by Marcel Lieberman)

Introduction

This article aims at bringing to light the key role played by the information processing sciences, especially as regards their recent evolution, in the development of an interdisciplinary approach in scientific research. To this end I adopt a systemic view that encompasses the following points:

1. the places, moments and forms of interaction between humans and technical artefacts are increasing, leading to a reconsideration of the status conferred to technical systems, their users and their environments;

2. new objects of study, new research goals, emerge at the same time, challenging disciplinary boundaries;

3. multiple viewpoints are thus developed, which will gradually become part of the new trends of explanation, modeling and confrontation brought about by the information sciences;

4. lastly, new instruments of collective practices arise, which are capable of profoundly changing the approaches of interdisciplinary work.

I will develop here the first three points.

Ever Increasing Interactions between Humans and Technical Artefacts

While the places, forms and moments of connection between technical artefacts and humans increase, new tensions arise that deeply question the status itself of technical systems, humans, their environment and their relationship.

New methods for recording information increasingly call on our sensory, perceptive and interpretive faculties; new methods for distributing information contribute to the weaving of social bonds around new, not purely utilitarian, values; and lastly, new methods of use lead to the development of hybrid realities that intertwine humans with technical artefacts. One thus slides from information technology towards knowledge technology; from the search for performance to the search for meaning; from the principle of intelligibility to the principle of integration, from the communicational paradigm towards the analysis of the mediations that are established within networks, and which participate in the emergence of new communities.

New tensions also arise. As the possibilities of observation and measurement expand, increasing the amount of information to manage, it becomes more and more difficult to determine the contexts of operation and use, and to construct the know-how and corpus of knowledge needed for its utilization. Even the legitimacy of certain forms of knowledge—more specifically, the principles of explanation and causality which form the basis of the conception of technical artefacts—is brought into question when confronted with a rationality that's presented as both situated and multiple: the main trait of both immediate experience and the situation; a trait intertwined with the dimensions of subjectivity, intentionality, and the implicit; a spatio-temporal anchor of memory and knowledge. In short, the foundations of organization—i.e., norms and rules—and the foundations of production—i.e., principles of re-use and evaluation—are brought into question by the need to confront the compartmentalization of knowledge and guarantee the plurality of expression; by the need to confront change and assure responsiveness, to leave a place for incompleteness, considered as the driver of interaction; and the need to consider organization as implied by dynamic processes of regulation and learning.

In this context it seems necessary to revise the status ascribed to humans, non-humans and their environments, and more fundamentally to rethink the dynamics of their interrelations by considering the technical system, not in terms of a "simple" interface or "pure" tool of communication, but as a mediator of human activity in its biological, cognitive and social dimensions.

It is particularly worthwhile recalling the necessary anchoring of the technical in the human: as Laurent Gille [6] notes, "one cannot think of technological innovation without thinking of the human conditions for its appropriation and emergence." In a dual perspective, one cannot think of the evolution of human knowledge either without thinking about the role of technical mediations in the principles of it appropriation and evolution.

It is thus worth developing an integrated view of the relationships between humans and technical artefacts, which leads us to consider the interdependent chains that link the biological, cognitive and social dimensions of humans, that link the material and informational dimensions of the conception and use of technical artefacts, and, lastly, that link human acts, technical acts and the construction of knowledge.

Making progress in understanding these issues means developing a strong interdisciplinarity. Such interdisciplinarity cannot be spelled out in terms of specifications from computer science models, nor in terms of a human/non-human comparison: it puts into play new objects of study, new scientific ends, which shift disciplinary spaces.

New Scientific Ends

The Department of Information and Communication Science and Technology at CNRS (Centre National de la Recherche Scientifique) in France has created a new form of scientific networks: the "Thematic pluridisciplinary networks" (RTP) as a tool to investigate and monitor the new trends of research emerging in this field (the complete list of the RTP networks is available on the CNRS site). Some of these new trends are briefly evocated in what follows.

The development of virtual- or enhanced-reality interfaces and the appearance of so-called “immersive” environments require greater understanding of the sensorimotor bases of human cognition. Cognitive production itself is modified, as well as the modes of social interaction, and the questions of situated action, collective action and the construction of meaning present themselves in a new way. The possibilities of communication open up at the same time and become gestural and iconic. Understanding the phenomena of the construction of meaning and reference, of the effects of pragmatics, at the cognitive and social level becomes essential.

The digitalization of texts, images and sounds, their utilization and their technologies of production opens up new perspectives in terms of access to cultural and artistic works, and in terms of greater appreciation of the cultural heritage. The tools of knowledge engineering, of the knowledge domains’ terminology and linguistics, as well as intentional, collaborative and social models of information research, are increasingly being used. A new context opens up at the same time for the study of art and artistic activities. This is also a revolution of the technical infrastructure of the culture industries, a decisive element in economic development. The development of models, tools and infrastructures that help promote the spread and sharing of knowledge makes an essential contribution to the improvement of education and training programs and, more generally, to human learning.

Due to the increasing penetration of technology in all sectors of society and life, the questions of acceptability, ergonomics and use are given a central place. The inclusion of concerns over use in the process of the conception and development of information services and communication becomes crucial, and is tied with developing possibilities for cooperative, computer-aided work. Information and communication technologies play an important role in the economy and organization of work, and become a driving force for the evolution of organizations and greater economic competitiveness. This movement is accompanied by new modes of forming and regulating both human groups and the governance of these groups, which leads to new means of social control: questions of law, security, and the respect of freedoms must be considered with revived interest. Their formalization, their accessibility, the impact on the development of new forms of participation and decision-making and on the renewal of the democratic debate must be studied.

A Place for Learning Plurality

Just as the Internet induces us to think of information as a flow with a plurality of contexts, research on the network similarly forces us to think of science as a dynamic enterprise with a plurality of referents.

A pluralist view of the basic notions of information, cognition, action and interaction is part of this approach, which increases the dimensions and referents of its analysis.

Pluralist views are thus developed, which will gradually become fixed elements of the new trends of explanation and modeling brought about by the information processing sciences.

Practicing interdisciplinarity means taking advantage of this plurality; it means guaranteeing the diffusion of ideas and the mobility of concepts. This can't mean assimilating the other to another like oneself, or relying upon an assumed correspondence of concepts and theories [3]; nor can it mean limiting the other to the "supposed" levels of discourse of their intervention, or refusing that they enter our own fields out of fear of the questions that necessarily arise. Instead, practicing interdisciplinarity implies "a desire for an alliance that leads to the explication of positions and models, and that promotes the recognition and development of multiple and complementary referents according to which the positions and models will in turn be clarified and critiqued in a new way: a desire for an alliance in which one uses the other to teach him or her about one's subject in order to better understand the meaning of what one's doing by recognizing the choice upon which one proceeds" [4].

At the center of this movement one finds those models that "have no meaning when considered in isolation, but only as part of the game of relationships within the quadruplet: {problematic, theory, model, phenomenal domain}" [1].

Interdisciplinary cooperation thus implies anchoring the necessary efforts of mutual explication within the dynamic caused by the interrelationships of these elements, as opposed to considering them in isolation, as disembodied.

In fact, according to Bruno Bachimont, "theoretic research cannot be interdisciplinary…The objects of study of one theoretic discipline are the objects constructed from an ideal and abstract viewpoint. This ideal and abstract viewpoint by definition abolishes the properties and traits of the reality that cannot be integrated as part of it in order to better isolate the relevant traits and theorize about them. Consequently, a discipline is constituted only by abolishing others…".

Moreover, models play an essential role in the process of constructing and formalizing interdisciplinary objects, serving as a kind of pivot point between theoretic reflections and experimental practices: "modeling is a particularly relevant and fruitful method in this process…due to the demand for rigor that it brings…It also makes it possible to simulate alternatives, to explore larger scales of time, to legitimize conditions of use by specifying the valid domain of the results" [9].

The information processing sciences have an essential role to play here, "which contributes to the conception and creation of artificial systems that allow users to represent themselves, to understand and intervene upon a certain reality…" [5]. They offer, by way of engineering, a pragmatic view on the theories by making it possible to evaluate their range of application as well as their dimensions of re-utilization and generality [10].

Concepts produced this way are characterized by their operative character, and it's the "object's finality, its internal characteristics and the environment in which it's placed" that are of interest [10]. Lastly, knowledge does not precede action here, "it is co-constructed in action by researchers interacting with other actors" [9].

Knowledge thus becomes “actionable”, legible by the domain’s actors, a source for understandable, appropriable, and shareable models [12]. This is the project of knowledge engineering, whose basic principles is an interventionist stance towards the practice of actors. This stance ultimately leads one to consider the organizational aspects of their activity, postulating that “the model to be constructed and the conception of the model are thrown into the organizational system and are an integral part of the organization” [12].

In this way encounters can be forged around projects being finalized, that are still open to experimentation, around “a kind of modeling that’s constructed and critiqued as an interpretation and representation of observed processes” [11], around models whose conception “postulates a certain form of indetermination and rejects all forms of reductionism, dogmatism and exclusive logic, models that reciprocally question one another in order to suggest new modeling efforts” [4].

Finally, we consider three levels on which interdisciplinary comparisons are made: the level of theories, models and experimental practice. The field in which they are carried out is made up by the pair: {end (or problematic), experimental domain}.

These entities are set in play in the very heart of a dynamic of complex interrelationships following the operative scientific practices, and their articulation within customary scientific practice already constitutes a challenge:

* because of the reductionism inherent to every scientific activity, whose effect is to broaden the gap between the hoped-for end and the actual result: this reductionism can be carried out in all stages of the analysis, from theoretic study to experimental implementation;

* because of the existence of implicit assumptions that are capable of introducing bias at all levels of the process;

* because of improper attributions of meaning (especially over-interpretation of experimental results) that cover up the reality of the work undertaken.

It is the task of interdisciplinary practice to mitigate these effects.

One frequently comes across a partial interdisciplinary approach, which restricts disciplines to certain areas or certain moments of intervention (providing problems, theories, data…). This most often leads to the development of “ancillary” or “instrumental” forms [8] of partnership, and gives rise to relationships of allegiance or dependence, restricting disciplines to a supposed domain of intervention or competence. It’s also worth developing its strong forms, that of “forging alliances in order to think jointly about technical artefacts, to think jointly about the architecture of networks that integrate humans and non-humans” [8].

It's thus a matter of opposing an approach that seeks to use other disciplines for producing data, models or theories, and to limit the time and place of their intervention. Instead, the aim of real interdisciplinary cooperation is bringing together different skills. The driving force of such a dynamic is the search for real multidisciplinarity. But this isn't decreed: it's instead established on the basis of ruptures and perceived imbalances [7]. If the proposed schema of interdisciplinary interactions makes it possible to organize the modes of the interdisciplinary encounter, it does not replace the basis of this dynamic, which would be the construction of shared scientific questioning. This is not reducible to the announcement of answers or solutions; to the contrary, it's more precisely translated as the search for ruptures and obstacles, as the awareness of a lack that in turn leads to the emergence of a common search. These ruptures, these obstacles, constitute the intermediate ends that cannot be established at the outset nor posited a priori, but which instead will be "discovered" and constructed during the process involving the actors and their interactions. A research field that's completely apart can at this point be put into play: the one concerning "collective distributed practices" and their new instruments [13], and which constitutes the last anchoring point of the systemic view proposed.

The only question now is whether the information processing sciences, with these new tools, are able to join this search for interdisciplinarity.

Acknowledgments

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Discussion

Une autre interdisciplinarité?
Dan Sperber
Sep 1, 2003 12:40 UT

La recherche scientifique en général (et de façon particulièrement appropriée la recherche interdisciplinaire) peut, en effet, être décrite comme une "pratique collective distribuée". Dans cette distribution, des artéfacts ordinaires (les écrits par exemple) et des artéfacts spécialisés (les instruments de mesure par exemple) ont de tout temps joué un rôle essentiel. Comme Catherine Garbay le souligne dans son premier point, nous avons maintenant affaire à de nouveaux artéfacts beaucoup plus actifs dans le traitement de l'information. Comme elle le souligne dans son troisième point, l'Internet – qui est lui-même un système de tels artéfacts actifs – permet de nouvelles pratiques collectives, en particulier dans la recherche interdisciplinaire.

Je voudrais inciter Catherine Garbay à mettre plus précisément en rapport ce premier point et ce troisième point en répondant (si possible avec des exemples) à la question suivante. L'Internet favorise-t-il la recherche interdisciplinaire seulement par la facilité avec laquelle il met en rapport les chercheurs entre eux et avec les bases de données, les textes, etc. (ce qui est déjà considérable), ou bien les nouveaux artéfacts infléchissent-ils la recherche, de part leur dynamisme propre, vers de nouvelles formes de recherche interdisciplinaire ? Et si oui, de quelle façon et avec quelles conséquences pour les contenus même de la science ?

> **Internet et l'interdisciplinarité**
> Catherine Garbay
> Sep 3, 2003 7:25 UT
>
> Mon propos n'était pas de placer internet au centre de la discussion, mais plutôt d'insister sur le rôle potentiel des STIC et sur les responsabilités des chercheurs dans ce domaine, qui à mon sens vont croissant.
>
> Dans ce sens, il ne s'agit pas bien sûr de s'arrêter au constat qu'"Internet favorise la recherche interdisciplinaire", constat auquel il me paraît bien facile d'adhérer, mais bien plutôt d'engager une réflexion plus ambitieuse, qui rejoint le deuxième terme de l'interrogation de Dan Sperber :"les nouveaux artefacts infléchissent-ils la recherche…".
>
> Il y a ensuite plusieurs angles de vue pour traiter cette question : je n'ai pas précisément choisi celui évoqué par Dan Sperber, qui est celui de la dynamique propre aux artefacts ; j'ai plutôt adopté (enfin tenté d'adopter) un point de vue épistémologique, celui de l'émergence d'objets de recherche communs, poussé par la pénétration dans les usages des technologies.
>
> La question posée par Dan Sperber est bien sûr cruciale, mais difficile, et je ne saurais y répondre dans l'intervalle de temps nécessaire à la poursuite de cette discussion.
>
> Un exemple néanmoins pour avancer un peu.
>
> Des expériences sont menées en archéologie sur le développement d'outils d'annotation de documents hypermédia. Le problème est de permettre le partage de points de vue différents sur le temps archéologique, la détection d'inconsistance entre ces différents points de vue et leur

révision éventuelle. Un "langage pivot" est adopté dans ce but, qui permet la construction d'un graphe associant et confrontant les différents points de vue.

Il me semble voir là les "nouvelles formes de recherche" qu'évoque Dan Sperber.

Quant aux conséquences de tout cela sur le contenu même des sciences… une vision idéaliste serait bien sûr de penser que ces outils permettront de retrouver une approche plus éclairée, moins cloisonnée des sciences, en permettant à de multiples passerelles de s'établir entre elles… mais quelle naïveté n'est-ce pas?

Vers une Société de la cognition ?
Mario Borillo
Sep 2, 2003 16:33 UT

La discussion que vous lancez est à la fois passionnante, par les problèmes scientifiques et philosophiques qu'elle soulève (" De nouvelles finalités scientifiques"), et extrêment grave par les enjeux sociétaux de toute nature ("Des confrontations croissantes entre humains et artefacts...") attachés à l'émergence de cette "interdisciplinarité" d'un nouveau type dont les STIC sont le ferment et le moteur.

Les refléxions qui suivent font partie de ce travail et elles font partie d'un chapitre de l'ouvrage:

M. Borillo et J.P. Goulette (Eds.), Cognition et Création, Explorations cognitives des processus de conception. Ed. Mardaga, Bruxelles, 2002

Vers une société de la cognition?

Peut-être est-il prématuré de parler d'une "Société de la cognition". Pourtant, plus que les logiciels dits "intelligents" pour la prise de décisions ou l'interprétation des images, d'emploi déjà courant dans la production flexible des biens, l'optimisation de la gestion ou la conduite tactique de la guerre, un ensemble impressionnant de recherches encore peu diffusées sur la cognition humaine mérite de retenir toute l'attention, dans la mesure où, associées aux développements matériels et logiciels de l'informatique, elles annoncent l'émergence d'une technologie de type nouveau, celle des "systèmes cognitifs", où les "machines" ne seront plus désormais cantonnées aux fonctions de support matériel de la communication, aux opérations préalablement programmées de traitement et de diffusion de l'information entre machines et opérateurs, mais où elles seront susceptibles d'intervenir de manière de plus en plus autonome sur les flux de représentations symboliques, selon des modalités qui tendront à se rapprocher toujours davantage de celles d'un nombre croissant de nos propres processus mentaux.

Quelles seront en effet les conséquences prévisibles d'une greffe des technologies cognitives sur les dispositifs techniques qui constituent les socles des sociétés de l'Information et de la Communication?

Tout d'abord, cette greffe repose sur une profonde compatibilité conceptuelle. En effet, par leur nature hybride, à la fois matérielle ET logicielle, ces technologies seront doublement compatibles avec tous les dispositifs actuels de représentation, de traitement et de diffusion de l'information, en d'autres termes avec les systèmes informatiques. Mais en les transformant en "machines sémantiques", les modèles cognitifs modifieront radicalement leur comportement, en ouvrant la possibilité de les doter d'aptitudes "intellectuelles" de niveau élevé, par exemple l'analyse et l'interprétation des signaux visuels et acoustiques, la compréhension et la génération de structures linguistiques, l'exploitation de ces compétences symboliques dans la conduite de raisonnements "de sens commun", etc… De ce fait, dans le triangle fonctionnel constitué par l'émetteur, le récepteur et le dispositif technique, le rôle de ce dernier sera vraisemblablement transformé du tout au tout dans la mesure où pourront lui être assignées des fonctions "sémantiques" (c'est à dire extraire et utiliser le sens des signaux et des structures formelles qui lui sont soumis, y compris pour en modifier le sens ou la destination originels), qu'il s'agisse de communiquer et de faire communiquer, de traiter l'information et de l'interpréter, de prendre des décisions, de contrôler les mécanismes de commutation qui sont à la base des échanges

sur les réseaux... Une sorte de "partenaire" d'un type complètement nouveau dans l'histoire des techniques.

Vers une société de la cognition
Catherine Garbay
Sep 3, 2003 7:32 UT

Mario Borillo aborde ici, me semble-t-il, un débat un peu différent, qui est celui de l'impact des technologies sur la société et sur l'humain.

Il s'agit d'un important et vaste débat, mais qui me paraît dépasser le cadre de celui que nous abordons ici.

Search engines as an example of integration of humans and artefacts
Gloria Origgi
Sep 9, 2003 10:12 UT

Catherine Garbay's text traces some lines of what should be a systemic approach to the role of information science in future interdisciplinary research. She insists on the emergence of new objects of study and new forms of interactions between artefacts and humans.

I think that some concrete examples may help in thinking about the new trends in interdisciplinary research that are evocated in her paper. She mentions one example about archaeology in her reply to Dan Sperber's question below. I will discuss another example, that of search engines.

Search engines are today probably the most astonishing change in our everyday research habits and in our way to approach information. In a sense, one might say that Google is the AI that we have been dreaming of for more than 40 years.

Second generation search engines like Google are self-organizing knowledge structures that exploit patterns of behaviour of web users in order to rank their results. This search strategy is very different from first generation search engines such as AltaVista or Infoseek, whose ranking of results was based on the number of occurrences of a query item in each page. Google exploits the collectively created knowledge implicit in the link structure between web pages. The link structure is a *knowledge structure*, a cultural product made by the agents' actions of establishing a hyperlink from one page towards another. Google's search algorithm extracts this cultural information.

Here we have an example of a hybrid knowledge structure that integrates humans and artefacts and that is profoundly changing our ways of doing research and teaching. Also, this knowledge structure can be influenced by commercial strategies or public policies: The algorithm calculates the authority of a site by weighing in a special way links that stem from a set of "authoritative hub pages". Of course, there are many situations in which these authoritative pages may be manipulated for commercial or political purposes. Technical, cultural and political questions are thus superposed in the production and management of these knowledge structures.

I think that a promising direction in assessing the impact of new technologies in interdisciplinary research in the future would be not only to think of new objects or fields of research made possible by the new technical means, but also to re-describe the integration between human activities and new technologies that are already influencing our everyday life in terms of the production of knowledge structures, neither artificial nor human, that require a multi-level analysis at a cognitive, computational, cultural and political, level.

Réponse à Gloria (et partiellement à Mario)
Catherine Garbay
Sep 24, 2003 7:07 UT

Gloria Origgi, en prenant l'exemple d'Internet et des moteurs de recherche, insiste sur deux caractéristiques majeures de ces nouveaux artefacts : la capacité à créer des structures de connaissances hybrides, reflétant une connaissance créée de manière collective, et la dimension non seulement culturelle mais aussi économique et politique de ces nouvelles " créatures ".

Gloria propose également que " Google is the AI that we have been dreaming of for more than 40 years ".

Je voudrai reprendre ce point important et le discuter. Pour moi en effet, nous sommes justement en train de dépasser ce vieux débat d'une intelligence artificielle susceptible de remplacer l'homme, voire de le supplanter. L'utopie de la " machine intelligente " ou de la " machine sémantique " comme la nomme Mario Borillo n'est bien sûr pas totalement abandonnée, disons qu'elle se retrouve déployée dans un contexte différent, dans le cadre d'une conception de l'artefact comme système d'inscription de la connaissance, comme le dirait B. Bachimont.

L'artefact se révèle ainsi plutôt dans son rôle de " passeur ", de médiateur, que dans un rôle objectif de contribution à la création du sens. C'est là que la dernière remarque de Gloria prend tout son sens : il s'agit de prendre comme " pivot " de l'interdisciplinarité non pas l'artefact et sa science, non pas l'humain et sa culture disciplinaire, mais les inter-relations qui s'établissent entre humains et artefacts et les structures de connaissances qui se tissent en leur sein.

cognition située, ou comment les moteurs de recherche participent à la création du sens
Christophe Heintz
Sep 29, 2003 17:36 UT

Comme Catherine Garbay, je ne crois pas que le vieil enthousiasme lié à l' intelligence artificielle puisse éclairer les phénomènes du STIC. Par contre, qualifier les moteurs de recherches de simple "passeur" ou "médiateur" ne permet pas, me semble-t-il, d'en saisir la complexité. Si les STIC requièrent une approche interdisciplinaire, c'est bien parce que les artefacts qu'elles produisent s'intègrent à la cognition humaine et **contribuent** ainsi " à la création du sens".

Exemple: Si je tape 'ornithorynque' dans la case 'recherche' de Google, j'obtiens une page particulière qui me donne des informations sur ce drôle d'animal. Cette page contribue bien évidemment au sens que prend le mot 'ornithorynque' pour les internautes... et ainsi toute action ayant contribué à mettre cette page en première place des résultats de google (algorithme google, créations de liens vers cet page) participe à la formation du sens du mot.

Le fait que les artefacts dépassent souvent le rôle de simples "médiateurs" s'explique par le fait que la cognition humaine est située: elle se déroule en interaction avec le milieu, et surtout, en utilisant pleinement les artefacts. Les artefacts contribuent à la constitution de représentations. Dans le cas du web, on peut même dire que la cognition est distribuée entre les artefacts STIC et leurs utilisateurs.

Nous pouvons donc affirmer que la cognition **transcende** l'individu de manière à **intégrer** les artefacts STIC. Peut-être peut-on voir une généralisation de cet observation dans l'article d'Helga Nowotny qui insiste sur le fait que les produits de la technologie s'integrent à la société qui les accueille. Cette intégration, nous dit Helga Nowotny, se fait non seulement au niveau morale, mais aussi dans la production de solutions aux problèmes auxquelles cette société est confronté. Il y a ainsi production de connaissance, et, pour reprendre (à contre-pied) les mots de Catherine Garbay, production de sens.

Un lieu d'apprentisage de l'interdisciplinarite
Mircea Bertea
Sep 15, 2003 11:38 UT

L'article de Catherine Garbay est également incitant et passionnant. Comme Mario Borillo l'a déjà mentionné, il est aussi extrêmement grave par les enjeux sociétaux de toute nature attachés à l'émergence d'une interdisciplinarité dont les STIC sont le ferment et le moteur. En quelques mots (hélas, l'espace limité!) je veux parler de „l'essence" pour ce „moteur", essayant de garder l'essentiel, d'un point de vue du chercheur dans les sciences de l'éducation qui considère le systhème technique non pas comme interface ou outil de communication, mais comme médiateur de l'activité humaine dans ses dimensions intégrantes, idéee fortement soulignée par Catherine Garbay.

Un article dense, bien documenté et très bien synthétisé. Quelques idées fortes: STIC comme liens sociaux autour des valeurs nouvelles, conduisant au développement des réalités hybrides qui entremêlent l'humain et l'artefact technique; la nécessité d'affronter la parcellisation des savoirs et de garantir la pluralité d'expression, d'affronter le changement et d'assurer la réactivité, de laisser place à une forme d'inachèvement et d'incomplétude, considérés comme moteurs de l'interaction; la nécessité de développer une vision intégrée des relations entre humains et artefacts techniques et de progresser dans la compréhension de ces enjeux en utilsant une interdisciplinarité forte; les RTP du CNRS (14, 32, 35, 36, 40); les enjeux d'exercer l'interdisciplinarité et la pluridisciplinarité, de co-construire la connaissance dans l'action par les chercheurs en interaction avec les autres acteurs etc., etc.

Un article qu'inspire… Par conséquent, je veux ajouter quelques suggestions et extensions possibles:

1. Les considérations et les références concernant l'interdisciplinarité sont justes, parfois mémorables. Mais, affirmer en titre (donc, l'idée forte) que les STIC sont (le) pivot de l'interdisciplinarité c'est trop, bien qu'il s'agisse d'une vision systémique! La thèse me semble contraire à l'une des idées vivantes de l'article: celle qui affirme très justement qu'une approche interdisciplinaire doit éviter „les formes ancillaires ou instrumentales de partenairiat, suscitant des rapports d'inféodation ou de dépendance". Le mot „pivot" dans tous les trois sens usuels (axe, racine principale ou principe fondamental) est trop dure, trop ferme pour les contextes coopératif, de partenairiat réel supposés par l'interdisciplinarité. C'est aussi la situation du mot „forger" dans le syntagme „forger des alliances", une jonction vraiment ancillaire (Voyez encore: „Trois niveaux d'exercice de la confrontation interdisciplinaire sont finalement considérés…", où l'utilisation du mot „collaboration" ou „construction" interdisciplinaire me semble plus suggestive).

2. Repenser l'interdisciplinarité (plus que du point de vue de la STIC) signifie accélérer le dynamisme de la recherche scientifique et renouveller les hypothèses et les routes, enllargissant la dimension coopérative de la recherche, la construction d'un questionnement scientifique commun. Dans cette demarche les recherches du CIRET doivent être considérées et valorisées (Basarab Nicolescu, René Barbier, La Recherche Action). Voir aussi les recherches du Laboratoire de communatique appliquée de L'UQAM et les recherches associées au DESS en communatique (v. Pierre-Léonard Harvey, Gilles Lemire, LA NOUVELLE ÉDUCATION. NTIC, transdisciplinarité et communatique, Les Presses de L'Université Laval L'Harmattan).

3. Une approche integrative de l'interdisciplinarité doit affirmer son esprit transdisciplinaire et établir la taxonomie de ces valeurs. Si ces valeurs seront bien „humanisées", on peut considérer que la réponse de la question finale de l'article de Catherine Garbay est entièrement positive.

Globalisation disciplinaire
Abdelkarim Fourati
Sep 23, 2003 11:43 UT

Les épistémologues ont emprunté quelques termes de leur jargon du vocabulaire des politiciens, comme les mots « révolution » (scientifique), « obstacle » (épistémologique), « débat démocratique »… Catherine Garbay a utilisé ce type de jargon et elle a même ajouté d'autres notions analogues, comme « frontières disciplinaires », « tensions » et « espaces disciplinaires »… Dans le même sens,

je veux bien pousser un peu plus loin ces métaphores pour essayer d'ébaucher une théorie de la globalisation disciplinaire.

Malgré les réticences, les oppositions, les ignorances tranquilles, la mise en place d'un « marché commun », d'une « union » s'esquisse entre les disciplines. Comme en politique, faut-il plaider pour une fédération ou pour une simple coopération entre disciplines souveraines ? C'est un choix épistémologique majeur. Pour Catherine Garbay, « une approche partielle de l'interdisciplinarité est fréquemment rencontrée, qui cantonne les disciplines à certains lieux ou certains moments d'intervention (fournir des problèmes, des théories, des données…). Ceci conduit le plus souvent au développement de formes "ancillaires" ou "instrumentales" de partenariat, suscitant des rapports d'inféodation ou de dépendance, et cantonnant les disciplines dans un lieu supposé d'intervention ou de compétence… Il s'agit donc de s'opposer à une approche visant à instrumenter les autres disciplines pour produire données, modèles ou théories, et à borner leur lieu d'intervention et leur moment d'exercice ; au contraire, l'enjeu d'une réelle coopération interdisciplinaire est de tirer parti de l'union des compétences…».

Je pense que c'est la fédération de certaines disciplines voisines, comme les sciences cognitives et/ou les sciences sociales, qui peut former des noyaux durs de l'organisation des disciplines. En effet, une notion importante, le concept de disciplines distribuées : ce sont les disciplines qui peuvent interagir avec le plus grand nombre d'autres disciplines. Actuellement les « sciences cognitives » et les technologies de l'information, par leur union (et non seulement les sciences du traitement de l'information), peuvent être considérées comme les disciplines les plus distribuées parmi les sciences-disciplines, et par là, comme pivot de l'interdisciplinarité. Peut-on former les « Sciences-Unis du Social » (par analogie avec les « Etats-Unis d'Amériques », en politique, qui sont actuellement la nation la plus distribuée qui peut entrer en interaction avec tout le reste des Etats-nations du monde entier) : cette union disciplinaire vaudrait la peine d'être tentée au cours des années à venir.

Encore faut-il lors de cette mise en commun de techniques et de connaissances, que chacune des disciplines participantes dans cette union ne reste pas enfoncée dans son travail particulier, aveugle ou sourd, comme la veille, à ce que disent, écrivent ou pensent les autres disciplines ! Encore faut-il que le rassemblement des sciences sociales, des sciences cognitives et des technologies de l'information soit complet et viable, que l'on ne néglige pas les plus anciennes au bénéfice des plus jeunes, capables de tant promettre, sinon de toujours tenir. Raison de plus pour signaler qu'au cours des trente dernières années, les sciences sociales sont assez mal informées des disciplines cognitives et technologies de l'information...

Réponse à Mircea et à Abdelkarim
Catherine Garbay
Sep 24, 2003 7:10 UT

Merci d'abord à tous deux pour votre remarque amicale concernant l'emploi du terme " pivot " (que je viens d'ailleurs de réemployer !). Effectivement il y a là comme un " dérapage ", et à vouloir démontrer avec enthousiasme combien les STIC peuvent servir une interdisciplinarité " idéale ", j'ai bien fini par leur conférer un rôle central en la matière, prenant alors le risque de ne transmettre qu'une vision " féodale " de la science organisée autour d'une seigneurie unique : les STIC.

La vision proposée par Abdelkarim est très stimulante, en particulier la tension proposée entre une vision " globalisante " de la science et une vision distribuée reposant sur des " noyaux " associant les disciplines les plus susceptibles d'interaction avec les autres, comme les sciences cognitives. On peut bien sûr imaginer non pas un noyau unique, constitué comme le propose Abdelkarim de l'association " sciences de l'humain " / " sciences de l'information " mais plusieurs noyaux constituant autant de germes potentiels d'interdisciplinarité.

La question qui demeure est celle de la vie des disciplines, car à prôner l'interdisciplinarité à tout va, on risque d'en oublier l'activité disciplinaire ! Ceci me fait penser à une réponse de Godard à un journaliste qui l'interrogeait sur sa propension à travailler " à la marge ". Il lui répondit à peu près ceci : " C'est la marge qui tient la page ".

Comment donc penser les relations entre interdisciplinarité et disciplinarité ? Peut-on comme Godard penser l'interdisciplinarité comme " soutien " à la disciplinarité ?

Globalisation disciplinaire ou interdisciplinarité globalisante?
Mircea Bertea
Sep 25, 2003 15:57 UT

J'ai l'impression que quelque chose nous échappe (peut-être parce qu'on pense encore sur la transdisciplinarité dans une manière disciplinaire !). On ne discute pas de cette partie commune des disciplines qui prise en conscience et activée peut générer un flux d'interdisciplinarité ?

En ce qui concerne l'article de Catherine Garbay, sur ce «quelque chose» qui peut nous garantir que STIC ne sont et ne deviendront seulement des instruments et aussi qu'ils peuvent activer les interactions dans une vision systémique ? Le résultat de cet accès pourrait être le remède d'une disfonctionnement signalé par Abdelkarim «qu'au cours des trente dernières années, les sciences sociales sont assez mal informées des disciplines cognitives et technologiques de l'information.

Si on parle des «noyaux durs» de l'organisation des disciplines et aussi des «Sciences-Unis de Social» (par analogie avec les Etats-Unis d'Amériques en politique) c'est selon moi, trahir l'interdisciplinarité. Noyaux durs ? Il me semble plus suggestif de parler d'un môele. La vision globalisante de la science est dangereuse.

On peut rencontrer la même situation comme dans la réalité socio- politique contemporaine- une globalisation qui met les Etats-Unis une position de hégémonie et les transforme en policier du monde. La globalisation de la science peut mener à une hégémonie spécifique ou, selon l'exemple de Abdelkarim, les sciences cognitives et le STIC peuvent s'emparer des autres sciences.

En reprenant l'une des idées de mon intervention antérieure, je veux souligner fort qu'une approche intégrative de l'interdisciplinarité doit affirmer son esprit transdisciplinaire et la taxonomie de ces valeurs. C'est-à-dire, une approche interdisciplinaire doit réunir, et non diviser. Son idéal c'est de redécouvrir l'unité initiale de la connaissance humaine. Sciences coopératives, bien équipées (bien sur en équipe), mais restant loin du modèle socio-politique. Les sciences subordonnées sont des sciences disciplinées et non disciplinaires, soldats d'un monde scientifique utilitariste, pas humaine.

En gardant la métaphore, l'interdisciplinarité c'est la part distribuée des sciences disciplinaires. Pas une nouvelle discipline. Les disciplines doivent rester des disciplines, parce que «C'est la marge qui tient la page». On doit changer la vision. Les disciplines doivent être abordées dans une vision interdisciplinaire suivant le chemin d'une valorisation des compétences.

Interdisciplinarity. The Loss of the Heroic Vision in the Marketplace of Ideas

Steve Fuller (University of Warwick)

(Date of publication: 1 October 2003)

Abstract: In this paper, I provide the background historical and philosophical assumptions that inform my rather 'heroic' interpretation of the value of interdisciplinary inquiry. Unlike most contemporary defenses of interdisciplinary research, mine does not presuppose that interdisciplinarity supplements, complements, or replaces discipline-based research. Rather, I see the matter the other way round, namely, that disciplines are artificial 'holding patterns' of inquiry whose metaphysical significance should not be overestimated

In this paper, I provide the background historical and philosophical assumptions that inform my rather 'heroic' interpretation of the value of interdisciplinary inquiry. Unlike most contemporary defenses of interdisciplinary research, mine does not presuppose that interdisciplinarity supplements, complements, or replaces discipline-based research. Rather, I see the matter the other way round, namely, that disciplines are artificial 'holding patterns' of inquiry whose metaphysical significance should not be overestimated. A key feature of my perspective is that inquiry needs a social space where it can roam freely. That space, the natural home of interdisciplinarity, is the university. Unfortunately, that institution is often deconstructed, if not completely under erasure, in contemporary discussions of interdisciplinarity (e.g. Lyotard 1983).

In a book first published ten years and soon to come out in a second edition, I called myself an 'ideologue of interdisciplinarity' (Fuller & Collier 2003: chap. 2). In other words, I do not see interdisciplinarity as simply a call for open borders between disciplines, so that cross-disciplinary borrowings are tolerated and even appreciated for the value they add to solving problems in one's home discipline. Rather, the persistent need for interdisciplinary solutions to disciplinary problems brings out the inherently conventional character of disciplines. Of course, these conventions can be socio-historically explained and epistemologically justified, but so could alternatives that perhaps already exist in neighbouring countries or had existed in earlier times. Could we dispense with disciplines entirely and simply follow the course of inquiry wherever it leads -- each of us, as it were, our own unique interdisciplinarian? That is not quite what I mean. Rather, disciplinarity should be treated as a necessary evil of knowledge production – the more necessary it is made to appear, the more evil it becomes. One important way in which disciplinarity can appear 'necessary' in this objectionable sense is by a historical perspective that cannot imagine alternatives to the current regime of disciplines.

Disciplinary success is largely a function of institutionalisation – matters relating to control over the flow of various kinds of resources. Basically any discipline can succeed if its members are provided with adequate resources to solve their own problems, which are in turn more generally recognized as problems worth solving. However, this commonplace continues to be shrouded in epistemological mystery because the ebb and flow of disciplines appears to happen without any central planning, let alone philosophical legislation. As a result, with a little help from secular theologies like 'scientific realism', a trivial sociological insight is transubstantiated into a version of the 'invisible hand' fashionable in the 18th century and vigorously pursued by the merchants of 'self-organisation' today – a very broad church that includes followers of Hayek, Luhmann, and Maturana, as well as a miscellany of postmodernists, evolutionary epistemologists and complexity theorists.

A very influential figure in this respect – though usually regarded as anti-realist – is Thomas Kuhn, whose account of paradigm formation in The Structure of Scientific Revolution leaves the impression that 'scientists' are the people who manage to wrest control of the means of knowledge production from the politicians, religious fanatics, and other folks who make it impossible to pursue The True without also pursuing The Good and The Just at the same time. This autonomization of inquiry – symbolized by the founding of the Royal Society and similar scientific societies in the 17th century – epitomizes all the perceived benefits of disciplinarity. Here are some of them: (1) secure borders for inquiry that keep larger societal demands at a distance; (2) common standards for incorporating new members and topics, as well as for evaluating their efforts; (3) discretion over the terms in which the concerns from the larger society are translated into 'new' problems.

A striking feature of this account of disciplinarity is that it presupposes that the prior probability that disciplines exist at all is low. Certainly in Kuhn's case, but also in much of the 'self-organisation' literature, it is considered a minor miracle that institutions of inquiry have been maintained in the face of various internal and external conflicts over the course of history. Indicative of this perspective is the tendency to think that disciplined science had a rather specific origin – perhaps even traceable to a singular cultural moment like the founding of the Royal Society – and that its development cannot be, nor could have been, more perspicuous than it has been. Even contemporary philosophy of science, which has almost completely purged its old positivist fixation on the goal of unified science nevertheless refuses to consider that science (or a particular science), had it pursued a different course of inquiry earlier in its history, would have ended up in a better epistemic position than it is in today. It is simply taken for granted that it was better to dump Aristotle for Newton, Newton for Einstein, etc. – and at roughly the times and for the reasons they were dumped. The purgatorial status of the Popperian philosophers who last questioned these intuitions – Imre Lakatos and Paul Feyerabend – testifies to deeply held assumptions about the metaphysically special character of the history of science as it has actually occurred. Insofar as contemporary philosophers of science engage in criticism at all, it is with other philosophers or scientists who retain vestiges of the positivist world-view (and hence 'misunderstand' the nature of science). Yet, across the passing fashions in philosophy of science, the one constant has been a providential view of the history of science. In short, science normally is as it ought to be.

Everything I just said about philosophers of science could have been said about sociologists of science, who have equally sanguine views about the history of science. However, I stress philosophers because a traditional source of inspiration – and irritation! – in the philosophical enterprise is the postulation of norms that are so at odds with ordinary practice that philosophers are forced to wonder how people manage to make do with their suboptimal standards and what might be done to improve their performance. I mean to recall here the ultra-competences required to defeat the sceptic in epistemology and to satisfy either Kant or Bentham in ethics. One source of this hyper-normativity is the assumption that human beings are rather unique creatures – rational, to be sure, but perhaps even touched with the divine – who should always try to make good on their capacity to imagine having done better. However, if, in contrast, you view human beings as mere homo sapiens, one clever species among many, then our capacities for change are inscribed in the variation that our history has tolerated. When humans are seen in this ontologically diminished (a.k.a. 'naturalised') light, induction acquires a luminous significance. Institutions become entrenched lucky accidents that we radically change at our peril.

The origins of this 'naturalistic' mentality in the 150 years prior to the American and French Revolutions make a superstitious attitude toward history understandable. Accompanying a gradual

secularisation of humanity was a realization that governments of any longevity typically arose from the ashes of war and were maintained by hereditary succession. Succession by election was seen as an opportunity for renewed conflict -- witness the intrigues associated with eccelesiastical and academic appointments -- and constitutional conventions were little more than philosophical chimeras. That autonomous scientific societies managed to survive as well as they did in their self-selecting, self-organising fashion was thus a considerable political feat in its own right, not to be tampered with. The founders of the Royal Society and similar bodies must have therefore hit upon the via regia to reality! This, then, was the great miracle associated with the so-called Scientific Revolution. Interestingly, this miracle only gets canonized as such around the end of World War II, by Herbert Butterfield in Britain and Alexandre Koyre in France, each adding his own distinctive air of mystification to the episode.

It is interesting to look at the history of disciplinarity before the canonization of the Scientific Revolution. Of course, most of the same people, events, and institutions are discussed but their respective significance is 'spun' rather differently. In the first place, disciplines were portrayed as more loosely 'bounded' than they are today. From reading, say, Kuhn or Michael Polanyi, it is easy to get the impression that a discipline is akin to a monastic order in the stringency of its entry criteria, training procedures, evaluative standards, etc. However, until the late 19th century, with the introduction of nationwide textbooks for discipline-based instruction in universities, an academic discipline was really little more than a collection of certification boards announcing that a piece of research met the standards upheld by the boards. Here I mean to include what is common to doctoral examinations and peer review journals. The exact nature of the training, the source of funding, and the overarching programme of inquiry to which the research contributed were largely left open to discretion. Of course, some people aspired to stricter criteria – and the 20th century has been the story of their steady ascent – but these have been always difficult to enforce for any great length of time or expanse of space.

Despite the looseness of the concept of disciplinarity pre-1945, nevertheless a shape to its history can be discerned that is common to, say, the massive studies undertaken by the engineer-turned-historian John Merz and the Neo-Kantian philosopher Ernst Cassirer. It does not resemble the post-Kuhnian commonplaces of today, according to which disciplines are the natural products of the 'functional differentiation' of the cognitive superorganism. Rather, the sorts of things we call disciplines (or even sub-disciplines) today were originally world-views designed to explain everything. They flourished as social movements in several countries, where they campaigned against each other to acquire professorships, funding, influence, etc. 'Crucial experiments' and Methodenstreiten functioned as symbolic events in the ongoing struggle. Over time, these clashes were institutionally resolved, especially through the creation of academic departments that were entitled to self-reproduction. (The 'nebular hypothesis' proposed by Kant and Laplace for the origins of the universe may be the appropriate scientific metaphor here.) In a sufficiently wealthy academic environment, even the losers could console themselves with a department they could call their own. (Social scientists are very familiar with this scenario!) Moreover, the resolutions were themselves subject to significant cross-national differences, such that the losers in one country may turn out victorious in another. As for the apparent 'universalisation' of particular disciplines – the fact that, say, physics or economics may be taught the same everywhere – that tendency simply tracked the geopolitical interests of the nations whose universities housed the discipline.

I believe that we should return to this older historical sensibility toward disciplinarity, one that diminishes the phenomenon's significance in the ontology of knowledge production. Indeed, in the older story, 'disciplines' function as little more than the legitimating ideology of the makeshift solutions

that define the department structure of particular universities. Taken together across institutions and across nations, the history of disciplinarity constitutes a set of test cases on how to resolve deep differences in cognitive horizons. As for interdisciplinarity itself, the main benefit of this general approach would be to highlight its centrality as an internal motivator of sustained epistemic change. In effect, today's disciplines were born interdisciplinary, as social movements that aspired to address all manner of phenomena and registers of life, not simply the domain of reality over which they came to exercise custodianship (Fuller 2000: chap. 8). In this respect, positivism holds a special place as a metatheory of interdisciplinarity.

Common to the various projects that have travelled under the rubric of 'positivism' has been an interest in constructing a medium of epistemic exchange across disciplinary boundaries. Indeed, in the case of the logical positivists, it would not be far-fetched to regard their ill-fated attempts to 'unify' science as having taken seriously – much more so than Peter Galison's bland notion of 'trading zone' – that pidgins and Creoles may evolve from their origins as trade languages to become the official language of the trading partners (Fuller 2002). In their original Viennese phase, the logical positivists were keen to invent an interdisciplinary lingua franca from scratch, partly inspired by ongoing efforts in the 1920s to make Esperanto the official language of the League of Nations. However, once in exile, at least one positivist, the Harvard-based Philipp Frank, considered in some detail the strengths and weaknesses of two living examples of interdisciplinary social movements that at the time showed no signs of retreating behind disciplinary boundaries and containing themselves to specialist puzzles: Thomism and Dialectical Materialism (Frank 1949). Both movements, despite their obvious cognitive deficiencies and proneness to dogmatism, earned Frank's respect for keeping alive the ideal of inquiry that roams freely across domains of reality in the service of individual enlightenment and collective empowerment.

In recent years, Frank's curiously ineffectual career in the United States has been subject to serious historical investigation. Based on unpublished sources, including the archives of the Philosophy of Science Association, the Chicago-based independent scholar George Reisch has discovered that the FBI found Frank's interdisciplinary vision potentially dangerous in a political climate increasingly keen on 'containing' conflict. Even Frank's philosophical colleagues detected a 'totalitarian' mindset lurking behind his critical appreciation of Thomism and Marxism (Reisch 2004). I raise this lurid bit of Cold War history because if one is to take seriously the heroic ideal of interdisciplinarity as free-ranging critical inquiry, then one must find a place hospitable to its conduct. For Frank, the natural place was the university, especially its mission of liberal education, which continually forced academics – no matter how specialized their research – to return to the question of what citizens need to know to exercise their liberties most effectively (cf. Fuller 2003a).

To be sure, in one sense, Frank was simply restating the classical ideal of the university found in, say, the writings of Wilhelm von Humboldt, the famed first Rector of the University of Berlin. However, at age 25, long before he became the Prussian education minister and icon for its dedicated bureaucracy, Humboldt invested this ideal with radical political import, partly inspired by Kant. In the 1792 essay, The Limitations on State Action, Humboldt entrusted the university with making the state 'wither away' from a prescriptive agency to a service provider by enabling citizens to legislate for themselves. Humboldt's youthful vision deeply influenced John Stuart Mill, who dedicated On Liberty to him. The Mill-Humboldt connection, in turn, inspired Karl Popper to think about epistemological matters in terms of liberal political theory (Fuller 2003b: chap. 12). Frank also clearly drew on this history, and not surprisingly he was one of the few logical positivists with whom Popper remained on good terms throughout his life.

This genealogical excursus yields some interesting practical insights about the promise and perils of interdisciplinarity. While interdisciplinarity may not respect disciplinary boundaries, it needs boundaries of its own to protect its free-ranging activities, especially so that inquirers are not cut short as they attempt to challenge or bridge differences in existing bodies of knowledge. Historically, the institution that has most adequately addressed this need is tenure. However, tenure has tended to attach to membership in a specific department rather than the university housing the department. Moreover, tenure is typically treated as akin to a guild privilege that defines corresponding obligations solely in terms of what one must not do, rather than in terms of what one must do: As it were, the tenured are not obliged to cure, but they are obliged not to harm. These quasi-legal arrangements are insular and even self-protective: Undermining the credibility of your colleagues is always a greater sin than simply doing nothing. In such an academic environment, interdisciplinarity is a highly risky venture for which there is little clear reward.

Yet interdisciplinarity flourishes today – but typically at the expense of the university as a tenure-granting institution. Briefly recall the relevant history from the Cold War onward. In the Cold War era, as universities expanded to meet national defense needs, a variety of 'area studies' and 'systems theoretic' approaches were proposed as interdisciplinary fields. The founders of these fields typically had a good enough grasp of the history of academic disciplines to realize their status as glorified reifications that strategically downplayed or omitted certain cognitively and socially important problems. Nevertheless, the intellectual power of the founders' visions was no match for the existing department structure of universities, especially when it came to securing tenure for the would-be interdisciplinarians. Echoes of this old obstacle can still be heard in the final report of the recent Gulbenkian Commission on the future of the social sciences (Wallerstein et al. 1996). Although very much in favour of interdisciplinary research, the Commission could recommend nothing bolder than for academics to be granted tenure in two departments.

However, as universities restructure themselves to face an increasingly competitive market for both training and research services, tenure is seen as a luxury that few institutions can afford. Moreover, for the younger generation of researchers who have come of age in this new regime, the ideal represented by tenure is far from clear. In particular, the guarantee of permanent department employment seems to license – in many of the older generation – the mindless repetition of old lectures and the artificial extension of exhausted research programmes. In short, tenure and department affiliation are despised together as representing the most reactionary aspects of the university. Under the circumstances, the ability to undertake interdisciplinary research is seen as a mark of 'flexibility' and 'adaptiveness', highly valued qualities in today's 'knowledge economy'. However, arguably, these qualities are less profound than the 'critical reflexivity' promoted by interdisciplinarians of an earlier era. The goal of interdisciplinary collaboration today tends to be less the fundamental transformation of intellectual orientation – a realignment of disciplinary boundaries – than the fostering of good communication skills so that no vital information is lost in the pursuit of a common research project. Thus, obstacles to interdisciplinarity that in the past would have been interpreted as based in disciplinary considerations are now demoted to local problems of project management that need to be overcome as expediently as possible, for purposes of grant renewal and securing the employability of the project members – in whatever field their future ventures happens to take them (cf. Lazenby 2002).

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Discussion

marketplace: institutions and cognition
Tim Moore
Oct 2, 2003 12:09 UT

I find Steve Fuller's paper excellent.

I would add two considerations. First, his emphasis on 'universities' (as the space in which issues about interdisciplinarity should be played out) ought to be developed, if not corrected. Relevant institutions set up for education or research at different times and in different places are extremely diverse in all respects, as Fuller recognizes when he refers to the Royal Society. Consider the College de France, set up partly because Greek was still not being taught in the Universities in France, and to provide education or eye-opening to all-comers, without providing diplomas. Consider too the long history of individual mavericks who have gone beyond existing styles of disciplinarity without necessarily belonging to a 'university'. Thus I think that despite the importance of considerations concerning 'university tenure', this field needs further development.

Second, we should consider the underlying cognitive mechanisms involved in interdisciplinarity. Very briefly, I consider that the distinction between cognitive daring and cognitive caution goes deep in our make-up for reasons that may be modulated but are not determined by specific social configurations. Such mechanisms should be seen as fundamental to issues about interdisciplinarity, providing a psychological space which makes them possible.

I don't know that we can say in general where the heroes stand. It may sometimes be the mark of a hero "to boldly go where ...". Perhaps it may also sometimes be the mark of a hero to stand firmly within a handmade stockade in what others considered barren land.

One minute footnote: I feel a bit uneasy with the very idea of a "marketplace of ideas" in so far as this may suggest that there could be a practicable or sustainable method of computing the exchange- or use-value of ideas, or indeed their market-value.

reply part one: marketplace of ideas and intellectual heroism
Steve Fuller
Oct 3, 2003 19:40 UT

Thanks for this response. I will take the points in reverse order:

1. The Marketplace of ideas: I don't care much for the normative implications of the image either. However, the image reminds us that a default mechanism for value conversion (i.e. market value) already exists in contemporary society, absent any other overriding criteria. Thus, a sign of the decline of academic culture is that our own standards are positively correlated with capitalist values (i.e. bigger is always better): e.g. the best professors and universities make more money, have bigger grants, etc. To be sure, the correlations are not perfect yet, but they are heading that way. Even a slight shift in the economic frame of the marketplace of ideas – e.g. valorizing people and institutions who do the most with the least – would disrupt the hierarchies in contemporary academic culture. For example, the US would not always come out on top. This point is relevant to how the younger generation of researchers see matters because for them 'proper academics' are simply 'rent-seekers' who obstruct the free flow of intellectual trade. They know of no other way of judging academic work.

2. On the intellectual heroism-caution distinction: This is tricky because, in order to avoid retrospective biases and anachronism, it is important to circumscribe 'heroism' as a phenomenon in intellectual life. I see it as mainly a philosophical virtue – very much like being a 'revolutionary' in the modern sense of the term. It is intimately connected with seeing oneself in terms of a big picture or plan of action that is designed to change things. However, many of the scientists frequently called heroes – Einstein comes to mind here –were simply very good at the sort of thing popularly called 'lateral thinking', which he was able to exercise more freely by being outside the physics establishment. It was typically left for others to realize and extend the significance of his work, which in retrospect is made to appear heroic from the start. In contrast, the positivists definitely had the 'delusions of grandeur' associated with true heroes.

...continued in next message

reply part two: the universalisability of universities
Steve Fuller
Oct 3, 2003 19:42 UT

3. On universalising universities: If we think of knowledge-producing institutions as historical experiments, then I think the desirable qualities have been most often collocated in universities. I mean these: (1) an incentive to challenge taken-for-granted ideas in research; (2) a requirement to pass on the fruits of research to the next generation of citizens (and not simply keep them to one's colleagues); (3) a mechanism for smoothly reproducing the process constituted by (1) and (2) – namely, tenured academic appointments. In Fuller (2003), I speak of universities as expert in the 'creative destruction of social capital', i.e. the destruction of any knowledge-based advantage by its widespread distribution. Historically this has been more a by-product than a plan of universities, but once universities became engines of nation-building in the 19th century, it was inevitable that nations worried by external foes would take an active interest in ensuring that the citizens most vulnerable to those foes (e.g. the poor) are not excluded from higher education. Alvin Gouldner called this symbiotic tendency the 'welfare-warfare state'. My view is that main obstacle to this idealized view of the university is the intellectual drag created by those bureaucratic encumbrances known as academic departments.

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disciplines as world-views?
Christopher Green
Oct 6, 2003 2:15 UT

Dear Steve,

Could you please expand a little bit on your claim that "the sorts of things we call disciplines (or even sub-disciplines) today were originally world-views designed to explain everything"? Assume my knowledge of the history of disciplines is fairly limited. A couple of examples (and perhaps some good references I could follow-up with) would be greatly appreciated.

Disciplines as methods
Gloria Origgi
Oct 6, 2003 14:31 UT

I also have problems with this claim. Does Steve mean that originally disciplines were methods of inquiry? That is, Aristotelians have their method to inquire all branches of knowledge, Francis Bacon proposed a general experimental method to establish progressive stages of certainty from the evidence of the sense, and so on ? Would you accept the equation between disciplines as world-views and methods of inquiry?

Reply part one: disciplines as world views
Steve Fuller
Oct 6, 2003 18:54 UT

There are three responses, here: The first two to Christopher, the third to Gloria.

The idea that disciplines start life as free-ranging world-views is not a thing of the past but something in our very midst today. I refer you to the ongoing efforts to conceptualise everything as a computer (a.k.a. ‘informatization’). The movement begins outside of academia, typically as an innovation whose productive capacities are then rendered ideologically luminous. Academics then jump on board and regularize the ideology by turning it into a rigorous body of knowledge that can be pursued for its own sake, even outside of practical settings. Perhaps the clearest precedent for the current computer craze is the 19th-early 20th century fetishism of the electric motor, or dynamo, which eventually became ‘energeticism’ (Rabinbach 1990). In the first decade of the 20th century, it looked like everything could be explained as transfers of energy, just as in the first decade of this century, some people think everything can be explained as transfers of information. Indeed, the two movements even share many of the same thermodynamic equations!

A good historical presentation of disciplines as world-views is the 4-volume Merz (1965), a reprint of books that were published in the decade prior to World War I. His chapters have titles like ‘the mechanical view of nature’, ‘the morphological view of nature’, ‘the atomic view of nature’, ‘the genetic view of nature’, etc. The plot of each chapter is similar: A big metaphysical picture tries to conquer the intellectual world and runs into trouble with the other pictures, via debates, experiments, etc. Over the century, the dust begins to clear and they retreat to particular ‘methods’ that are said to provide unique access to distinctive domains of reality, which are in turn codified as ‘theories’. However, the big picture clashes continue much longer in the biological and especially social sciences, where, say, mechanical models of social reality (with analogues to concepts like inertia and gravity) are still periodically advanced. A good compendium of these is Sorokin (1952), originally published in the 1920s. Indeed, even as we speak, I am scheduled to have a debate on BBC Radio 4 with someone who has just published

a book called ‘Critical Mass: The Physics of Society’. My point here is that this is not just some misbegotten application of science but a rekindling of the original scientific spirit – like it or not!

Merz, John (1965). A History of European Thought in the 19th Century. 4 vols. New York: Dover.

Rabinbach, Anson (1990). The Human Motor: Energy, Fatigue and the Origins of Modernity. New York: Basic Books.

Sorokin, Pitirim (1952). Contemporary Sociological Theories. New York: Harper & Row.

Contiued in the next message....

Reply part two: positivism and disciplines as world views
Steve Fuller
Oct 6, 2003 18:59 UT

The view of disciplinarity I described in the last message is the one presupposed by the logical positivists. It explains their preoccupation with resolving differences of ‘metaphysics’ in some systematic fashion. At the time, the disciplinary world-views competed on all fronts at once, and success or failure was often determined by political popularity and other factors that the positivists did not believe was worthy of forms of knowledge that claimed universal assent.

In effect, they aspired to regulate a ‘free market’ that was leading to some suboptimal outcomes. The implicit conceptualisation of intellectual space was akin to international relations of the period – especially the problems raised by nationalism and imperialism. However, it is worth stressing that the positivists, at least in their Viennese phase (and Popper in his whole career), did not think of Kuhn-like functionally differentiated disciplines as ‘the final solution’ to interdisciplinary conflict. The idea that ‘good paradigms make good neighbours’ in intellectual life is a creature of the Cold War era’s paranoia, whereby any sort of conflict could lead to world war. (Witness my remarks in the text about the FBI spying on Philipp Frank.) But even if you resist this speculation, it is nevertheless striking that scientifically literate people like the positivists never placed the sort of philosophical weight on disciplinary differences that philosophers place on them today.

Reply part three: disciplines as world views vs. methods
Steve Fuller
Oct 6, 2003 19:02 UT

At the risk of being pedantic, ‘method’ is a rather modern word (i.e. post 17th century) that seems to have gained currency precisely because it suggested something much more procedural and hence less ideologically committed than a world-view. (Here I think of Descartes alongside Bacon.) Method’s neutrality to metaphysics and politics was supposedly its strong point as a route to reality. However, Aristotelianism and Baconianism were never simply methods in this stripped down sense but methods-plus-justification, which amounted to a world-view that could be used to understand everything.

I also find it artificial to equate disciplines with methods. Is there a sociological method? A psychological method? Even a physical method? Maybe particular academic departments are dominated by particular methodological schools, but if one looks across the different departments with similar names – e.g. all the 'sociology' departments – you will find an assortment of methods practiced within them. This is why I said in response to an earlier seminar here that disciplines were better seen as akin to ‘states’ than ‘cultures’.

A Natural Home for Interdisciplinarity?
Davydd Greenwood
Oct 7, 2003 2:46 UT

In this interesting and provocative paper, I find much to agree with but will leave that mainly aside to focus on the points that could lead to debate. Fuller's contention that universities are the natural home of interdisciplinarity mystifies me completely. If we look at the most securely interdisciplinary of the fields (the physical sciences and biology whose contours are constantly changing), we can see that it is not because of their university home but because they are obligated to study massively complex problems at the behest of large funders and have to get results, not just reinforce their academic bunkers. The social sciences, with their nearly severed connection to the extra-academic world have the most resilient disciplinary structures precisely because they are not accountable for getting much of anything done that matters to anyone else. Regarding the disciplines, I don't regard intellectual discipline as an evil. Disciplined thinking and the accumulation of knowing how is exceptionally important. What it is not is an end in itself. But in the social sciences, severed from the murky contexts of the real world, disciplines become an end in themselves, perhaps a protection against the sense that the world is too complex to be understood anyway and so we should just talk about whatever we can agree to talk about, even if no one else is interested. I do very much agree that "today's disciplines were born interdisciplinary, as social movements that aspired to address all manner of phenomena and registers of life, not simply the domain of reality over which they came to exercise custodianship" (Fuller 2000: chap. 8) and I think most of the histories of the social sciences I have seen support this point. Thus disciplinarization in the social sciences and the organization of the contemporary university are the phenomena to explain.

Regarding the value of interdisciplinarity, I have no particular interest in making academic life feel better to the inhabitants of the university. My own commitment is to studying the problems of the world at the level of the multiple causalities and historicities that characterize them. To do that, one has no choice but to be inter-disciplinary and, even then, to live with a sense of constant ignorance and frustration about what is not understood.

Finally, on tenure, I don't agree. I can only see it as being about the protection of professional privilege. I have seen too many tenured faculty members use tenure to purge unpopular ideas by peer review and to keep their juniors in place to have much respect for a device that grants so much while demanding so little.

> **Reply part one: On the university as the natural home of interdisciplinarity**
> Steve Fuller
> Oct 8, 2003 15:51 UT
>
> Perhaps the difference between Davydd and me is in terms of the ideals that drive the university. I have in mind two – I call them 'Masters' and 'Doctors' – in honour of the medieval precedents for them:
>
> The Masters ideal is to build the next generation of well-rounded free citizens able to cope with the future. Interdisciplinarity enters here at the level of the liberal arts curriculum (or 'general education'): What is it that everyone needs to know from a given field, however specialized? In what order should this stuff be taught? Do we need a metatheory that specifies the logic of the course arrangement? The value of research, one might say, is test-driven in the classroom. Research with no obvious classroom use is, in that sense, 'not ready for prime time' – regardless of how smart you need to be (or how much funding you get!) in order to do it. This is my benchmark of interdisciplinarity, and the one I attribute to the logical positivists – and even more obviously, the German idealists.
>
> In contrast, the Doctors ideal encompasses the more recent and perhaps more usual examples of interdisciplinarity – including the natural science examples Davydd raises. These have more to do with filling in research gaps that emerge between disciplines or, in extreme cases, remapping certain domains of reality in a so-called 'transdisciplinary' fashion. To be sure, these have historically flourished off campus. But the flipside of this anti-academic stance is that its educational implications are not immediately apparent. It all depends on how the

interdisciplinary projects make their way back – if at all – into the academic curriculum: Do they become bases for new disciplines, degree programmes? In short, how is this interdisciplinary research institutionalised so that it becomes a genuine public good, i.e. teachable, and not simply a piece of intellectual property?

Reply part two: On the value of tenure
Steve Fuller
Oct 8, 2003 15:53 UT

ympathize with Davydd's criticisms of tenure as it is actually practiced – or abused! But I believe that these abuses are not the product of tenure per se, but its institutionalisation: (1) It is tied to a specific department. (2) It carries no specific obligations. What would it mean to have an institution of tenure that does not have these features?

At the very least, it would mean that tenured faculty are obliged to submit to periodic reviews of their academic performance, as defined in terms of, say, 'five-year plans'. For certain controversial, competitive, or otherwise volatile fields of research, the university may insure continued financial support, even if the tenured academic fails to get a grant. These are just ideas – but the key point is that tenure should be accompanied by a support system that makes it reasonable to oblige – not simply allow -- academics to undertake long-term, risky research. In other words, it should encourage academics to think about their activities in markedly different terms from the usual hand-to-mouth grant-guzzling researcher who regards flexibility and adaptability as the cardinal virtues.

I happen to believe that if tenure is made to carry some fairly heavy intellectual obligations, most of today's academics would not choose it as a career goal. Most would still prefer the so-called freedom to move from topic to topic on short-term contracts as the market dictates. Here I am reminded of Popper's slogan (from Goethe, I believe) that humans are distinguished from other animals by the fact that when we're wrong, our ideas die in our stead. The market blurs this distinction, which tenure tries to make sharper. (Nevertheless, it must be said that the market is much less judgemental at the personal level than what, for better or worse, we still call 'peer review'.) As it stands, tenure as an alternative is simply evaporating as old professors retire and are replaced by short-termers who think in purely market terms.

An opportunity for tenure is imperative, in order to ensure that there will be a generation of academics interested in pushing the frontiers of research in directions that may occasionally cut against market forces in uncomfortable ways – and are committed to passing along their knowledge to the next generation in some systematic fashion.

Les cinq continents disciplinaires
Abdelkarim Fourati
Oct 13, 2003 11:30 UT

Stever Fuller, comme dans le texte précédent de Catherine Garbay et d'autres épistémologues, a emprunté quelques termes du jargon des socio-politiciens. Comme il le dit au début de son texte : « Une caractéristique fondamentale de son point de vue est que la recherche a besoin d'un espace social où elle peut vagabonder librement. Cet espace, qui est le domicile naturel de l'interdisciplinarité, est l'université ». De fait, du point de vue sociologique, nous vivons quotidiennement dans trois types d'espace social : l'un géographique purement physique, l'autre symbolique purement cognitif (celui des scribes, prêtres, savants, scientifiques, chercheurs…) et le troisième mixte géo-symbolique (celui des politiciens, citoyens…). L'université est un espace géo-symbolique.

D'autre part, pour Fuller, « la disciplinarité devrait être considérée comme un mal nécessaire de la production de la connaissance ». Cependant, la notion de discipline, telle qu'elle est discutée dans son texte, est très étroite ; bien qu'il considère « que les "scientifiques" sont des gens qui s'arrangent pour arracher le contrôle des moyens de production de la connaissance des mains des politiciens, des fanatiques religieux » ; et qu'il utilise des métaphores théologiques, comme "église" (de chercheurs), "ordre monastique" (d'une discipline)... Bref, il y a un conflit interdisciplinaire débordant l'espace symbolique de la science, alors que la notion de "discipline" dans le texte de Fuller se limite aux

sciences-disciplines physiques et biologiques (comme on les conçoit à partir du XVIIe siècle) et à l'université moderne qui essaient d'ignorer et d'éliminer les autres types de connaissance. En fait, pour pouvoir étudier l'évolution des disciplines et de l'interdisciplinarité, nous devons considérer l'ensemble des connaissances et savoir-faire, et leurs différenciations au cours de l'histoire, à partir des mythes, religions, philosophies…

Pour cela nous pouvons revenir à l'analogie entre l'épistémologie et la politique que nous avons déjà évoquée dans une intervention précédente (Globalisation disciplinaire : voir les discussions autour du texte de Catherine Garbay, septembre 2003), mais en passant de la pure métaphore à l'ébauche d'une théorie qui considère l' "espace symbolique" du savoir comme formé de cinq continents disciplinaires ayant une histoire épistémologique (comme les cinq continents géopolitiques de la planète ayant une histoire politique). Remarquons que le nombre cinq n'est pas important (cela peut être plus ou moins). Nous considérons donc les continents suivants : 1- le continent des disciplines mythologiques et religieuses ; 2- le continent des arts-disciplines ; 3- le continent des sciences-disciplines physiques ; 4- le continent des sciences-disciplines biologiques et médicales ; 5- le continent des sciences-disciplines humaines et sociales. Nous pouvons ainsi dessiner une carte disciplinaire similaire à la carte géopolitique avec ses continents, îles et des frontières disciplinaires délimitant diverses disciplines. Il y a même un nouveau monde disciplinaire (les sciences-disciplines sociales) et un ancien monde. Actuellement la philosophie n'est plus qu'une île au voisinage de l'ancien monde qui regarde avec envie le nouveau monde des sciences sociales. Stever Fuller nous parle à partir de cette île, alors que je vous parle à partir du nouveau monde, plus précisément des Sciences-Unis du Social…

Disciplines are not like geographical regions
Steve Fuller
Oct 14, 2003 21:07 UT

If I understand Fourati correctly, I think he has confused two different accounts of disciplinarity in my article. There is the usual view of disciplinarity, which is quite narrow and associated with the rise of the natural sciences as experimental disciplines in the 17th century. I oppose this view. My own view is almost the exact opposite, namely, that disciplines in this narrow sense are relatively late developments (beginning in the late 19th century). Before that point, so-called 'disciplines' were really interdisciplinary and wide-ranging. However, it's only once they are incorporated in academic structures as disciplines that they actually acquire a regular pattern of reproduction (or institutionalisation).

One consequence of my view is that I really don't believe that there are any intrinsically deep metaphysical differences between the sciences. (Consequently, I find geographical metaphors for disciplines very misleading – they basically reify academic department structures: Ontology recapitulates bureaucracy!) All disciplines basically aim to explain everything, but in the wider social world where they compete, their legitimacy is limited – in various ways, in various societies. The idea that sociologists can explain physics better than physicists (as in the 'Science Wars') or that physicists can explain sociology better than sociologists is not a product of social and natural scientists overstepping the bounds of their respective competences. Rather, such ideas tap into the inherent tendency of these fields – reflecting their roots as universal movements – that is normally contained and repressed by the 'discipline' imposed by department structures and other forms of academic etiquette.

Etroitesse disciplinaire?
Abdelkarim Fourati
Oct 17, 2003 9:23 UT

Pour commencer, je suis complètement d'accord sur la conclusion de Fuller quand il dit dans son texte initial : « Aujourd'hui le but de la collaboration interdisciplinaire tend moins à être la transformation fondamentale de l'orientation intellectuelle - un réalignement des frontières disciplinaires - que l'encouragement d'aptitudes à la bonne communication qui font qu'aucune information vitale ne se trouve perdue dans la poursuite d'un projet de recherche commun ». Mais pour lui, cette conclusion se limite aux sciences-disciplines telles qu'elles se pratiquent à l'Occidentale, d'où l'étroitesse de sa notion de discipline qui doit être étendue à d'autres types de connaissance comme je l'ai expliqué dans mon intervention précédente. Et c'est dans ce

sens que j'ai pu dire que « la notion de discipline, telle qu'elle est discutée dans le texte de Fuller, est très étroite… ».

Cependant, Fuller utilise le mot "étroit" (en réaction à mon intervention) dans un autre contexte. En effet, bien qu'il dise « Disciplines are not like geographical regions », il utilise l'expression « frontières disciplinaires » ; or une frontière (au sens scientifique du terme) délimite toujours un espace (dans notre cas l'espace symbolique). En effet, chaque discipline a un « espace disciplinaire » plus au moins grand. Et comme le constate Fuller : « Les disciplines sont les produits naturels de la "différenciation fonctionnelle" du super organisme cognitif…» ; de telle façon que le corps des connaissances s'est émietté. Ainsi, l'étroitesse de l'espace disciplinaire de chaque discipline a augmenté au cours de l'histoire, depuis l'essor des sciences de la nature comme disciplines expérimentales au XVIIe siècle, et elle s'est même accentué aujourd'hui avec la montée du spécialisme. Satiriquement on a pu dire : « les spécialistes connaissent tout sur rien » ; autrement dit l'espace disciplinaire de leurs disciplines respectives est tellement étroit qu'il tend vers zéro.

Pour remédier à cet état de chose, comme le constate Fuller : « Les disciplines d'aujourd'hui naquirent interdisciplinaires, en tant que mouvements sociaux aspirant à aborder toute sorte de phénomènes et de registres de la vie… Tandis que l'interdisciplinarité peut ne pas respecter les frontières disciplinaires, elle a besoin de ses propres frontières pour protéger ses activités de libre exploration… ». En gardant la théorie que j'ai expliquée dans mes interventions précédentes, il faut insister sur le fait que nous ne pouvons faire de l'interdisciplinarité sans « disciplines distribuées » qui peuvent enter en interaction avec les autres disciplines, comme nous ne pouvons faire de l'internationalisme sans Nations suffisamment distribuées qui peuvent agir à l'échelle de la totalité. On ne peut pas donc abolir les frontières disciplinaires, comme l'on ne peut pas abolir les frontières politiques entre les Etats-nations.

Comment donc (re)penser les relations entre interdisciplinarité et disciplinarité? Peut-on construire les « Disciplines-Unies »? Comme les Nations-Unies, le projet des « Disciplines-Unies » reste un projet d'avenir, difficile à atteindre, encore ouvert dans sa conception. Nous nous contentons pour le moment des disciplines interdisciplinaires : c'est le cas des « Sciences-Unis du social »…

A United Nations of Disciplines?
Steve Fuller
Oct 18, 2003 14:50 UT

Thanks to Fourati for the clarification and elaboration. I agree with some your assumptions. First, I think disciplines are akin to nation-states in the political sense, and moreover that interdisciplinary problems have much the same character as problems in international relations. However, I think there is one big difference between interdisciplinary relations and international relations, especially today. I don't believe that disciplines aim mainly to secure the territorial integrity of their borders, which is the context in which a world agency like the United Nations is so helpful.

Admittedly, if you look at disciplines solely through the lens of academic departments, they look like more-or-less peaceful states with sovereignty over discrete domains. But in fact, disciplines remain much more imperialistic in their ambitions – their opening up of new 'sub-disciplines' is often an attempt to capture areas that had been studied by other disciplines in the past. This point also applies to competition for resources, both human and material: The people and problems 'out there' do not come with specific disciplinary affiliations. The disciplines need to design strategies to capture those resources from potential competitors.

In this respect, universities try to contain the conflict so that it does not turn into a Darwinian struggle for survival. One handy budgetary strategy is cross-subsidization (i.e. overheads from the research income of wealthy departments help cover the cost of poorer departments). Without such constraints, medical and business schools could easily dominate many of the major universities throughout the world, monopolizing both research funding and student interest.

However, it is not clear how the containment of interdisciplinary conflict currently performed by universities could be scaled-up to something like a United Nations of Disciplines. The ultimate problem is that even though many historians, philosophers and sociologists of science like to speak of disciplines as corresponding to discrete domains of inquiry (a slice of reality, as it were), disciplinary practitioners still believe that their own discipline can potentially explain everything.

Reading Otherwise
James Collier
Oct 13, 2003 18:19 UT

One should not be surprised by the hallmark breadth, sophistication and critical engagement of Fuller's account of interdisciplinarity. But I wonder if Steve would be willing to reflexively address the place and function of "reading otherwise" as a discipline-based (in STS), or perhaps, metadisciplinary, assumption in his account.

Part of the disciplined way of thinking in STS, canonized Shapin and Shaffer's Leviathan and the Airpump, and illustrated in Fuller's Thomas Kuhn (2000) and Kuhn vs. Popper (2003) is that if we read history (usually specific historical debates) "otherwise" we can move away from Panglossian accounts of disciplinary and epistemic development. The reason for doing so, in this particular instance, is to recover a space where inquiry can "roam freely" (such phrasing signals a curious softening of Fuller's normative agenda). Still, I am unclear where the other shoe drops. Does Fuller believe that retelling the story of disciplinary formation (with a preferred ending) will, by its very doing, change the intellectual landscape? Will the new disciplinary story simply be appropriated to ensure better communication skills and a more adaptive disciplinary framework? Does Fuller's account simply trade in one set of disciplinary assumptions for another and, therefore, reify narrative elements used by Kuhn?

Reading otherwise as Re-legitimiation
Steve Fuller
Oct 14, 2003 20:40 UT

Collier raises an important challenge. Of course, I don't believe that telling the history of disciplinarity differently will by itself change the structure of the intellectual world. In this respect, I agree that science and technology studies (STS) remains too wedded to a postmodernist conflation of discourse and world, whereby an ability to irritate scientists with what one says is confused with a capacity to alter what scientists do. However, postmodernism does draw attention to a traditionally neglected feature of knowledge policy, namely, that the people who typically decide on the future of knowledge production are not the knowledge producers themselves. In that sense, science policy is always 'virtual science'. One needs to bring in the neglected non-scientific players, funders as well as 'users and beneficiaries'. In contrast, most philosophy of science – as well as the various ideologies of self-organisation – continue to operate under the illusion that scientists are the primary determiners of what science is, at least in its most interesting (i.e. general) intellectual contours. To be sure, scientists are necessary for setting out alternative futures for knowledge production, but the relative legitimacy of these alternatives will be ultimately decided by people not directly involved in their pursuit.

I say all this because these 'interested non-participants' (as opposed to the 'disinterested participants' who populate classical epistemology) take alternative historical arguments – like the one I present -- seriously. They realize that, given their lack of technical knowledge, their policy judgements must be predicated on certain causal assumptions about what has enabled the science to develop. For example, an important part of the contemporary legitimacy of Darwinism is the assumption that a believer in Divine Creation could never have come up with its characteristic theses of genetic variation and natural selection. Yet, no historically sophisticated person can be comfortable with this assumption. Indeed, if US Creationists were willing to play up the religious credentials of card-carrying evolutionists, and invoke the inspiration that the idea of a 'Divine Plan' has historically provided to respectable biologists (including Darwin), then they would make some serious inroads in the public school classrooms. Unfortunately, most Creationists accept the stereotyped position in which their opponents place

them, and consequently believe that their goal should be to destroy – rather than colonize – Darwinism.

I think something similar about interdisciplinarity. In the current science policy climate (thanks partly to the mythology of Mode 1 vs Mode 2), it is difficult to defend BOTH a robust interdisciplinarity and university autonomy. They are portrayed as pulling against each other, mainly because interdisciplinarity is seen as always generated from outside academia, or at least outside some given disciplinary structure that is supposedly essential to academia. But my point is that disciplines in today's strongly bounded ('paradigmatic') sense is relatively recent, and the university had survived perfectly well without them in the past. One thing I hope would follow from this alternative history is that academics would stop thinking that their integrity as inquirers is tied to a specific disciplinary affiliation. This only leads to a heads-down herd mentality (the kind of thing Kuhn valorized as 'normal science') that leads to unhelpful responses like "I can't help thinking this way because I was trained as a physicist," when faced with the need to justify what one is currently doing.

Tenure and Disciplines
William Lynch
Oct 17, 2003 20:38 UT

It is really interesting to see the shift in the way that tenure is conceived that Davydd Greenwood's comments bring out. I have no doubt that tenured professors can and do stifle inquiry through peer review. In part, this is the result of the fact that one must pass muster first through an extensive process controlled (usually) by one's home discipline and a discipline-dominated university structure. Yet, the crucial point is not whether tenure overall does more harm than good (one could argue that most institutions do more harm than good), but whether it will do better or worse than possible alternatives.

The assault on tenure seems to be related to a more general pattern in the global economy. With the limits of (geographical) expansion, capitalism must expand _intensively_, which it has done through health care, knowledge workers, and the service sector more broadly. What this means for professors is the proletarianization of their labor. Of course, one might continue to hope, in a Marxist fashion, that the final exhaustion of the commodification of the world will lead to that great dialectical reversal where production (of knowledge, as everything else) will be socialized, but this seems to be blind faith at this point. A more likely result is the further elimination of those sources of intellectual resistance (however much they are a feeble minority). If no one resists commodification, there definiely will be no reversal!

How do disciplines figure here? Well, disciplines are the stronger unit in most contexts (even if occasionally administratrors use interdisciplinary programs to trim liberal arts programs). At my own university, liberal arts and sciences are apparently quashing the implementation of a Duke style, interdisciplinary liberal arts general education curriculum. Perhaps Fuller's suggestion that the problem wih tenure is that it is awarded through disciplines is correct here. One utopian suggestion: once one was awarded tenure, professors would be free to choose how they associate themselves with other researchers institutionally. Perhaps one could also justify a non-disciplinary assessment regime for getting tenure in the first place. Then there would be a kind of market (or self-management?) among producers of knowledge distinct from the larger commercial market. And if disciplines ceased to serve the needs of a newly tenured faculty, they could bolt, while leaving those committed to disciplines with a vote of their own. There could, of course, be a collective deliberation of these moves--ahh, perhaps that is the danger--even more committee meetings!

Tenure: The disciplines v. the university
Steve Fuller
Oct 18, 2003 14:47 UT

Bill Lynch provides additional support for the idea that tenure is a reasonable institution that should be associated with the university rather than a specific department. He raises the interesting prospect of tenured academics 'self-organizing' into interdisciplinary research and teaching teams, perhaps eliminating the departmental structure of universities altogether. I believe such an arrangement already operates in some of the funkier US liberal arts colleges.

However, usually the only academics who survive in that setting are those who can find common cause with others on the campus. If you can't, you're basically invited to find employment elsewhere. I raise this point more as an observation than a criticism of the idea.

Here let me reiterate a point I made in response to Gloria Origgi, namely, that disciplines should not be confused with methods. I think that most of what Davydd Greenwood likes about the rigour of 'disciplines' is really captured by 'methods', which of course can be learned in a variety of disciplinary settings. Think, e.g., of all the disciplines from which you might learn to do ethnography, experimental design, survey design, statistical analysis, archival research, etc. You can learn these methods without undertaking unconditional commitment to a discipline. The 'surplus value' of the discipline mostly amounts to a legitimatory narrative that prescribes the contexts in which these methods are appropriately applied, based on precedents drawn from a self-serving mythical history. What often impresses me about the liberal arts curriculum is that novel interdisciplinary programmes can be generated from an assortment of disciplinary methods without everyone having to work under a common research programme or paradigm. They simply need the constraints provided by such low-tech items as the academic calendar, the course outline, and (to measure outcomes) student performance and evaluations.

Nevertheless, I believe that in the long-term autonomy of inquiry requires that academics identify more with their universities than their disciplines. I came to this conclusion when looking into the incursion of 'knowledge management' thinking into academic administration (see Fuller 2002). Knowledge managers basically want to run the university like a business firm. Moreover, they take comfort from academics who are so alienated from the corporate identity of the university that they blame the university for getting in the way of 'their work', which is usually defined in terms of a discipline-based research agenda. The knowledge manager's solution then is to turn the university bureaucracy into an efficient machine that administers and encourages grants that support such research. The campus thus becomes a glorified bazaar in which each discipline sets up a stall hoping to attract customers. In this regime, academic administrators are custodians who keep the market stalls tidy, and 'interdisciplinary' is little more than a euphemism for market-sensitivity. This is perhaps the worst legacy of postmodernism – the reduction of the university to a mere physical space, so that the 'agora' becomes a purely commercial space without any larger political import.

Fuller, Steve. (2002). Knowledge Management Foundations. Butterworth-Heinemann.

The Myth of "Interdisciplinarity Itself"
Julie Klein
Oct 19, 2003 20:35 UT

I regret entering the Fuller discussion late and missing discussions of Pierre Jacob's and Catherine Garbay's papers. I was on leave and only recently returned to work. I appreciated Steve Fuller's layout of historical and philosophical assumptions that inform his self-styled "heroic" interpretation of the value of interdisciplinary inquiry. There is much I agree with, but I balk at the term "interdisciplinarity itself." Interdisciplinarity is a generic term for a plurality of activities that perform a range of functions with regard to disciplines, new fields, and programs and projects. Today's disciplines, moreover, were not "born interdisciplinary" in the sense we understand the term today. Yes, they were "wide ranging," but, to be accurate to the history of interdisciplinarity, they were "pre-interdisciplinary." As interdisciplinarity became a major concept, it assumed many forms, with a range of commitments to disciplinary inquiry, problem solving, campaigns for unity, free-ranging inquiry and radical critique. More recently, disciplines have also been changing in ways that scramble tidy generalizations about disciplinarity, although I'll grant the terrain differs. Witness the difference in the trajectories of interdisciplinary developments within political science and literary studies, as opposed to philosophy and economics. On this point, see Thomas Bender and Carl Schroske's American Academic Culture in Transformation. Finally, while granting that interdisciplinary research is often seen today as a mark of "flexibility" and "adaptiveness" in the "knowledge economy" today, I would not use the term "knowledge" or "interdisciplinarity itself" within a particular range of examples, minimizing contradictory examples of 'critical reflexivity' and even "project management" (not in the name of creating the next new widget but transdisciplinary research on environmental sustainability). Our discussion sometimes rests upon a narrow range of examples when invoking such complex and even contradictory terms as "disciplinarity" and "interdisciplinarity."

What's in a word? 'Interdisciplinarity'
Steve Fuller
Oct 20, 2003 12:33 UT

This disagreement looks like a dispute over how one should use words. Of course, I do not deny that 'interdisciplinarity' has meant more things than I -- or for that matter, any of the other discussants -- have talked about. However, words can be used prescriptively as well as descriptively. And in fact, there seems to be a preferred, or at least default, understanding of interdisciplinarity in this discussion that began with Dan Sperber's original paper: namely, interdisciplinarity arises as an afterthought to fill in the gaps that are left over by the existing disciplines, which are taken to be the fundamental knowledge production units. This way of viewing things presupposes a certain view of the history of knowledge production, which I believe is false or at least profoundly misleading. My paper is an attempt to redress the balance by envisaging interdisciplinarity as a project in its own right, with disciplines simply as institutional fallback positions when the interdisciplinary project is stalled.

Against this backdrop, I interpret the current blurring of disciplinary boundaries somewhat differently from Julie. I see this phenomenon simply as disciplines recovering their old imperial, interdisciplinary ambitions in a university culture that has considerably weakened. Here I think it is important to distinguish between how interdisciplinarity looks to the open-minded people who engage in interdisciplinary collaboration and how it looks once it is presented in a monograph or textbook for classroom consumption. The former may appear very democratic, but the latter look very imperial. What one thinks of, say, evolutionary psychology, depends on which standpoint one looks at it from.

Finally, the fact that 'interdisciplinarity' and affiliated terms are used to mean contradictory things makes it no different from any other word. My hope is that we can go beyond doing a natural history of the word 'interdisciplinarity' and start talking about why this word means so much to those of us who champion it. I think I have made it clear that I do not wish to valorize everything travelling under the banner of 'interdisciplinarity'. So far most people who have participated in this discussion seem to have a rather instrumental view of interdisciplinarity's value. I disagree with that widespread view. I must confess I'm not sure what Julie ultimately likes about 'interdisciplinarity' other than the word itself.

So Much in a Word
Julie Klein
Oct 20, 2003 15:18 UT

This is not a mere dispute over how one should use words but touches upon some of the most fundamental questions before us. What is the purpose of research? Of education? Of disciplines? Of alternative constructs? Yes, certain meanings have arisen in this forum but I wouldn't say there is a default position, given that we have had challenges to every definition put forward. I join Steve in envisaging interdisciplinarity as a larger project, but see the current blurring of disciplines as something more than recovering old imperial, interdisciplinary ambitions. Some blurrings emanate from challenges to those ambitions. I also have a different position on institutionalization than Steve apparently does. We are not pitched between the heroic freedom of open-minded collaboration and codified monographs and textbooks for classroom consumption. Interdisciplinary fields and projects need to build literatures and communication forums to make their marks in cultural space. The danger of blunting innovation is real, but a new imperial regime is not the inevitable outcome. I also share Steve's desire to talk about why this word means so much to those of us who champion it, but do not valorize everything travelling under the banner of interdisciplinarity, either. Nor do I balk at instrumental views in service of worthy goals (ah .. "worthy .. another troublesome word that raises the issue of problem choice). Finally, I reject the notion that what I ultimately like about "interdisciplinarity" is the word itself. That trivializes the worthy projects of everyone I have helped to create a greater interdisciplinary presence in contexts as wide ranging – to name but a few -- as an approach to general and liberal education that recognizes the plurality of culture; an educational program aimed at helping students think critically about the centrality of new concepts of visuality, information, and communication in the media that pervade their lives; a health sciences program that incorporates knowledge from social sciences and humanities in training

future practitioners; and an environmental research and education project centered on a local pollution problem.

Yes...but are there bad interdisciplinary projects?
Steve Fuller
Oct 21, 2003 9:03 UT

Of course, I do not dispute that Julie has probably done more than anyone here to promote the gospel of interdisciplinarity. The question is whether you draw the line anywhere. Have you ever run across examples of bad or pseudo-interdisciplinary projects? Examples of THOSE might be helpful. And of course, it's hard to say anything bad about 'pluralism' – except that not everyone is part of the plurality, and so it becomes interesting how the costs of pluralism are borne. For example, one ongoing interdisciplinary project that I think has destructive consequences is the attempt – most closely associated with sociobiology and evolutionary psychology – to unify the humanities and natural sciences to the exclusion of the social sciences. (The pseudo-science of 'memetics' is a recent incarnation.) A lot of today's leading science popularizers fall into this category, and a major website has been devoted to promoting this movement: www.edge.org.

What makes this interdisciplinary project especially destructive is the implicit – and sometimes not so implicit – narrative that draws all the parties together in common cause, which makes the social sciences singularly responsible for retarding any systematic understanding of the human condition. Interdisciplinary projects are prone to such scapegoat narratives because the parties are often so disparate that all they share is a common foe that inhibits their progress: 'my enemy's enemy is my friend'. Consequently it becomes important to see whether what is enabled by common cause is worth what ends up being excluded in the process.

The Good, The Unacceptable, and The Shoddy
Julie Klein
Oct 21, 2003 13:58 UT

While disputing the characterization of me as a missionary spreading a gospel, I agree with Steve that bad or pseudo-interdisciplinary projects must be distinguished from good ones. Sociobiology would have been my first pick for a project with dubious, and sometimes destructive, intent, not only in the campaign to unify the humanities and natural sciences to the exclusion of the social sciences but all of the criticism that feminist scholars have leveled against it. So, too, E.O. Wilson's fabled doctrine of "consilience," which reduces humanities to a biological explanation. The second example raises another problem – reductive use of other disciplines and fields. There are numerous instances of erroneous or simplistic use of other disciplines and fields, as well as an impoverished understanding of the process of interdisciplinary work among individuals and on collaborative teams. Even the high-minded transformation of a formerly narrow, discipline-bound, elite notion of "culture" into a robust interdisciplinary construct that pervades so much of humanities scholarship and its current intersections with social sciences and sciences often results in disciplinary imperialism in a new and trendy guise. Great phrase, Steve – "scapegoat narratives."

Reductionism as symptom
Steve Fuller
Oct 22, 2003 20:40 UT

Thanks to Julie for the illuminating examples of bad interdisciplinarity. (I generally think you find out a lot more about a person's views when you ask them for the bad cases rather than the good cases.) I am 'sort of' sympathetic with what you say about these cases – I say 'sort of' because 'reductive' is not such a swear word in my lexicon, and I'm not sure it would capture my own problems with sociobiology and cultural studies. I'll elaborate briefly.

I rather expect – at least hope! -- that people who engage in interdisciplinary activity change their own disciplinary horizons somewhat after the experience. This may even include dropping some fundamental dogmas of one's home discipline and taking on board those of another. Some social scientists who read a little bit of sociobiology and evolutionary psychology seem to have such experiences. To the unsympathetic observer this may look like intellectual

colonization – i.e. the social scientists, or at least their minds, have been subsumed under biology: Reductionism in action! But disciplines are not religions, and the idea that a sociologist might come to believe that biologists could do a better job of sociology than sociologists is not cause for burning the sociologist at the stake. One may regret this conversion – such as it is – or try to argue him/her out of this fixation on biology, but ultimately I think it is just part of the normal dynamics of inquiry. People who are afraid of reductionism should perhaps try to defend their home disciplines better. Social scientists are notoriously bad in interdisciplinary public relations, especially with respect to the younger generation, who find all this renewed interest in 'human nature' very sexy.

But this is not to say that I give imperial rampages of the sociobiologists and evolutionary psychologists a clean bill of health. My problem with their version of interdisciplinarity (and this objection also applies to cultural studies) is that its success is parasitic on people not knowing much about the histories of the disciplines they are rejecting. Even the conversion to biology is often made by social scientists who do not know much social science – but perhaps don't care much about their social science colleagues! What I mean to say in all this is that while Wilson, Dawkins, Dennett, Pinker, etc. are rightly charged with various intellectual crimes, nobody is forcing people out there – including reasonably sophisticated academics – to read their stuff and give their views more credibility than we may think they deserve. You can't intellectually colonize anyone or any field unless there is already a receptiveness to the imperial call.

▽A Non-Reductive Reflexive Farewell
Julie Klein
Oct 24, 2003 14:04 UT

Steve is right to say that you find out a lot about a person's views when you ask for the bad cases rather than the good ones. So too, you find out a lot when you learn which words – in this instance "reductive" -- they are (un)comfortable with.

Reductivity is a familiar problem in all forms of interdisciplinary work, in the form of imposing one approach without the kind of engagement of assumptions and boundaries that Steve describes well in his other writings. The accompanying reflexive posture should result in scrutiny of knowledge claims and forms, as well as an understanding of both the histories of disciplines (the ones individuals practice and the ones they reject) and the interdisciplinary geneaologies embedded within and across them. On this latter point, I recommend Craig Calhoun's mammoth essay on "Sociology, Other Disciplines, and The Project of a General Understanding of Social Life," in the book Sociology and Its Publics: The Forms and Fates of Disciplinary Organization, ed. by Halliday and Janowitz (University of Chicago Press, 1992).

Great talking with you again, Steve. I'm off in a few hours to Uruguay, for a UNESCO/IDRC Summer School for Latin America and the Caribbean, on environmental sustainability in the region. (Off to preach the gospel of transdisciplinary problem solving, eh?) See everybody in November for the next paper of the seminar.

▽Bad interdisciplinarity?
Dan Sperber
Oct 29, 2003 16:33 UT

I found plenty of food for thought in Steve Fuller's paper and in the discussion, and the only reason I didn't join in is that I have been absurdly busy. However, I feel I must, if only for the record, stand in defense of what Steve (and Julie?) see as the parangon of bad interdisciplinarity, namely (quoting Steve):

"the attempt – most closely associated with sociobiology and evolutionary psychology – to unify the humanities and natural sciences to the exclusion of the social sciences. (The pseudo-science of 'memetics' is a recent incarnation.) … What makes this interdisciplinary project especially destructive is the implicit – and sometimes not so implicit – narrative that draws all the parties together in common cause, which makes the social sciences singularly responsible for retarding any systematic understanding of the human condition."

Sociobiology, evolutionary psychology, and memetics are different projects that share with yet other approaches (that of Boyd and Richerson or mine for instance) the goal of integrating biological, psychological and socio-cultural approaches to human affairs. There is much to criticize in the various attempts so far, and in particular in the pop versions that makes it sound so easy when in fact, the task is formidable and no one has anything like a "roadmap". The most serious criticisms of what has been done so far have been made by people who share the general goal or at least are sympathetic to it. On the other hand, what is mostly criticized from within the social sciences is the goal itself, as if it had no scientific merit, as if it had inspired no valuable work at all, and as if it could only be motivated by dubious ideology. This is mostly ignorant self-righteousness and it does stand in the way of a better *"systematic understanding of the human condition,"* just as do occasional ignorant and arrogant dismissals of the contribution of the social sciences by natural scientists.

By the way, I am not sure What Steve has in mind when he describe the project as that of *"unify[ing] the humanities and natural sciences to the exclusion of the social sciences"* (unless this is a typo and he meant unifying the natural and the social sciences to the exclusion of the humanities, which, although still unfair, would be closer to the truth). What is happening rather is that it is the social scientists that tend to exclude themselves (and their students) from this particular project instead of contributing to it, if only with constructive criticisms.

> Anyhow, a question: Is Steve objecting to the goal itself, or to all the various ways in which it is being pursued?
>
> **Do sociobiology etc. exclude the social sciences?**
> Steve Fuller
> Oct 31, 2003 17:59 UT
>
> My own apologies for not responding sooner – given the lateness of the submission – but I've been out of town for a couple of days. Dan raises two questions that deserve longer answers and discussions but here are some opening moves: (1) Do the various programmes to unify our understanding of the human condition under a broadly biologized framework exclude the social sciences? (2) Am I against the unification of the sciences?
>
> In answer to (1): I actually do think that the answer is yes. While social scientists are welcomed as data gatherers, they are not welcomed as explainers (unless they use a biologically inspired explanation). Having read the popular and technical literature in this area, and having attended seminars devoted to the topic, I am struck by how the social sciences are stereotypically reduced to the Cosmides-Tooby SSSM: 'Standard Social Science Model'. I am not sure what is more objectionable about the stereotype – that it is wrong or that it is ridiculed. (You see, I believe that broadly speaking SSSM is right.) In any case, the bare invocation of this model provides an excuse to ignore the social science literature that has been devoted to a given topic.
>
> It is easy to see why younger people would be attracted to the biological turn, since large numbers of cross-disciplinary social scientists have failed to reach agreement on any common theoretical framework – at least since the breakdown of structural-functionalism and Marxism. Moreover, it is often forgotten that before postmodernism and the dominance of the humanities by cultural studies, humanists tended to believe in a robust sense of human nature – one that was often used to justify the greatness of, say, Shakespeare as someone who said something profound about our 'common humanity'. Not surprisingly, both E.O. Wilson and Steven Pinker try to appeal to this older humanist sensibility – and Wilson, in particular, has been rewarded for it. The book Consilience is an outgrowth of Phi Beta Kappa Lectures, which are awarded for the author's relevance to US liberal arts colleges.
>
> The hostility between the humanities and the social sciences is perhaps older than that between the natural and social sciences. And I think sociobiology et al. has helped to rekindle the older hostility. Perhaps I should say, in conclusion, how should say how I distinguish the social sciences from the humanities and natural sciences. Historically, what has distinguished the social sciences from these other two great bodies of knowledge is that, on the one hand, the social sciences (unlike the natural sciences) privilege human beings above other creatures. At

the very least, this means that some special explanatory principles are needed. On the other hand, the social sciences (unlike the humanities) treat all human beings – and their activities – equally, not least because what any individual is is, in some significant sense, the product of what many have done, are doing, and can do.

To be continued...

Is the search for unity good or bad?
Steve Fuller
Oct 31, 2003 18:05 UT

In answer to (2) from previous message: I am by no means against unifying the sciences. Otherwise, I would not be so well-disposed to the spirit (if not the letter) of Positivism. (For the record, I am the author of the article on 'Positivism' in the new Elsevier International Encyclopedia of Social and Behavioral Sciences – so I am not afraid of unity!) However, there is a question about the terms on which the unification occurs. Whatever else one wants to say about the logical positivists, they were certainly alive to this issue. If we assume that all the sciences must ultimately obey the same laws of logic and evidence (even if these are not entirely known), then we cannot assume that there is some 'natural hierarchy' of sciences. I realize that the positivists were not always consistent on this point because, like today's social constructivists, they perhaps shifted between epistemological and ontological matters too easily. I certainly plead guilty to that charge!

I stand – perhaps alone today – in believing that Auguste Comte basically had the right idea, namely, that sociology (as general social science) is the unifying science. Moreover, as the history of science progresses, as Comte suggested, we need to reconceptualize and reorganize what our earlier scientific conceptions were about. I mean here not merely to correct past empirical errors, but more fundamentally to correct past oversimplifications in our understanding of the world – which may include thinking that the human realm is simply a special case of the natural realm. Thus, I disagree with vulgar forms of positivism that basically apply natural scientific models to social life 'off the shelf' as it were. Of course, no one ever really does this, but it's interesting that when we talk about 'unifying' our understanding of the human condition, we imagine that the unifying science will be, broadly speaking, natural scientific. My own interest in unification goes the other way around – namely, to treat the natural sciences as part of the social sciences. In my own work on 'social epistemology', I have not hid this desire, which I think is more in the spirit of Comte's very interesting project, which would have given us a 'scientific socialism' of sorts.

My objection to sociobiology and kindred unifying efforts at the end of the day is that they take advantage of the disorganized and even dispirited state of the social sciences by capitalizing on the prestige – to be sure, based on genuine insights – that the natural sciences enjoy today. I understand why biologists are attracted to this interdisciplinary imperialism, but social scientists should put up a bit more resistance and become more sensitive to the social contexts of knowledge production that make sociobiology appear so seductive today.

La naissance des choses
Martine GROULT
Oct 29, 2003 18:01 UT

Votre texte m'a beaucoup intéressé. Je partage votre affirmation comme quoi les disciplines d'aujourd'hui naquirent de l'interdisciplinarité. Dans un récent volume paru aux Editions du CNRS sur "L'Encyclopédie ou la création des disciplines" nous avons tenté de réfléchir à plusieurs sur cette naissance à partir de l'interdisciplinarité. Nous avons peut-être surestimé la signification métaphysique et, en restant dans le 18e siècle, nous n'avons pas abordé les agencements de ressources. J'ai toutefois employé beaucoup d'énergie pour obtenir des ressources afin que cette recherche ait lieu, et j'ai compris le rapport entre argent et idée ! Mais l'interdisciplinarité a plu et l'argent est venu ... pour le temps d'un colloque. Je voudrais toutefois essayer de penser une lueur d'espoir dans cette fin de l'image héroïque que vous présentez et que je subis également en voyant la fin du CNRS. Si discipline veut aussi dire méthode, c'est-à-dire règles nées du collectif et conduisant à un même but,

on peut penser qu'il y aura toujours une méthode. Alors, dans les changements - même les plus noirs - il y a toujours le mouvement de la naissance des choses et ça, c'est philosophique. C'est aussi ce qui fait que les biologistes sont si proches des philosophes lorsqu'ils parlent de la pratique de leur science. Et puisqu'on est dans la pratique : avant les idées, on pourrait prendre la biologie comme porte d'entrée dans la philosophie et désormais ce n'est plus l'argent qui dirigerait totalement la réforme mais l'éthique. C'est inverser un ordre du savoir qui commencerait par la représentation de la naissance pratique (la biologie) pour conduire une interdisciplinarité entre les disciplines à partir de l'éthique. Ne pourrait-on pas penser que même avec l'argent qui dirige le marché des idées, l'éthique représente de nos jours ce point de vue de la naissance des choses à même de conserver le vrai sens de l'interdisciplinarité? Ou alors est-ce que l'éthique vous paraît constituer un obstacle à la liberté que vous voyez, avec juste raison, comme une nécessité à l'interdisciplinarité? Penser un ordre du savoir en commençant par la pratique de la naissance, ne me paraît pas trahir la métaphysique, bien au contraire, mais je lance ici des idées bien personnelles et je vous remercie avant tout pour votre texte.

Ethics as a heroic interdisciplinary strategy
Steve Fuller
Oct 31, 2003 19:07 UT

My apologies for responding so late. Nevertheless, you certainly raise a very valid point. In recent years, 'ethics' has increasingly served as a basis for launching the heroic interdisciplinary vision – mainly, as you suggest, because it can serve as a counterweight to the financial interests that normally control the direction of interdisciplinary work. The sense of 'ethics' in these cases is always 'holistic'. To be sure, there are many different – and often competing – ethical movements that recreate the heroic vision. The ecology movement is a good example, especially its care for the land and animals. A more traditional example is provided by Catholic approaches to the biomedical sciences. An interesting feature of these ethically inspired interdisciplinary movements is that they often draw attention to relevant phenomena that might otherwise go unrepresented in the research. Consequently, they provide 'added value' epistemologically as well. However, I am not sure how generalizable is the ethical strategy to interdisciplinarity – but it is a good one.

The Evolution of Knowledge Domains. Interdisciplinarity and Core Knowledge.

Dominique Pestre (EHESS, Centre Koyré)

(Date of publication: 1 November 2003)

Abstract: This article is a general reflection and a proposal for action on the research policies of the French CNRS. It reflects in an historical perspective upon the question of the evolution of knowledge domains in the medium term and proposes, as far as possible, bold and nonconformist readings that can help in thinking about the current situation.

(Translated from French by Marcel Lieberman)

Introductory Note

This article was written for the Scientific Council of the CNRS (Centre National de la Recherche Scientifique) where it was presented on 14 January 2002. It was used as the introduction to a full-day working session titled "The evolution of knowledge domains, interdisciplinarity and core knowledge". It was intended as both a general reflection and a proposal for action.

1.

It will certainly be understood that my remarks that follow can by no means cover all the knowledge domains within the CNRS. This might be a task worth pursuing, though it could only be done by a group of researchers with the qualifications and time that are not at my disposal. I have thus chosen the more limited goal of reflecting upon the question of the evolution of knowledge domains in the medium term and of proposing, as far as possible, bold and nonconformist readings that can help in thinking about the current situation. More precisely, I'll begin by putting the question in an historical perspective, hoping in this way to draw out the blind spots or the perfunctory, and perhaps simplifying, frameworks of analysis.

I'll begin with a few general remarks resulting from a reading of some of the reports written a year ago by various divisions of the National Committee of the CNRS[1], reports by the heads and directors of interdisciplinary departments and programs. The first remark concerns the very strong tension I feel between three persistent points. One has to do with the defense of what constitutes the primary objective of the CNRS, basic research: a ubiquitous notion found in nearly all texts, but difficult to define today. The second point has to do with the imperious duty of trans- or interdisciplinarity, affirmed as the primary value by all the authors and directors of the institution; yet, the notion is ambiguous, especially as interdisciplinarity is not a good in itself but an approach which, only in certain contexts, can turn out to be of central importance. The last notion concerns the need to take into account the economic valorization of research carried out by the CNRS. These three points often give rise to feelings of incompatibility, to orientations that are experienced as being irreconcilable, as leaving little room for mutual accommodation. The argument I'd like to make is that these three points constitute intellectual and practical goals that must be "held together", even if they are in part antinomic, and that one ought to perhaps reformulate the problem in order to better situate the stakes and the contradictions. These terms are not, in fact, neutral; they often carry with them strong values (or a certain tendency to euphemism, as with the expression of valorization)—and they perhaps mask certain realities that would be interesting to uncover.

My second remark concerns the topic I was asked to address: the evolution of knowledge domains. My problem is that the idea of a knowledge domain is perhaps too restrictive for thinking about the construction of the sciences today. The expression tends to refer to systems of utterances detached from the know-how they convey and without which they have little impact; to knowledge separated from instruments, material and technical devices that alone make its existence possible; to systems of

(pure) knowledge with no connection to the concrete and differentiated spaces of production. The notion also leads one to believe that bodies of knowledge are "naturally" developed for themselves (knowledge for the sake of knowledge), that they are not linked to interests and goals, that they are not formed by the social, economic and political contexts in which they're found. To the contrary, it seems to me that over the past 150 years, and even more so in the last decades, we have been dealing with forms of techno-knowledge that vary according to their place of production (the academic world, the firm, start-ups, experts), and that adequately grasping them requires that one not forget the complex structure linking utterances of knowledge, material devices, the space in which they're developed and economic and political aims.

The dynamic of the growth of the sciences deserves attention as well, since it consists not just of one dynamic, but of several: there are multiple dynamics that do not necessarily pass from the basic to the applied, or from the scientific to the industrial. These dynamics certainly follow theoretical or experimental inquiry, but they also follow other logics, instrumental, institutional, political—or in feed-back. I therefore suggest that it is better to think of the question in terms of institutionally and economically situated practice domains, of mixed regimes of production that define the forms of knowledge whose logic varies according to the institutions in which they're inscribed.

2.

Let me clarify these ideas and their importance in developing a strategy, by way of a rapid historical review of the knowledge production regimes beginning in the second half of the nineteenth century. What characterized the period from 1860-1900 was, first of all, the expansion of research and teaching laboratories in universities in the areas of physics, physiology, and psychology—that is to say, a multiplication of places where learning was by doing, for both researchers and students. A second novel element: new industrial sectors (underwater telegraphy, electrotechnology, organic chemistry, the radio) established laboratories of "basic" research. A third element: institutes for standards and testing were created by national governments, industries, the military and universities. The best example of this is the PTR of Berlin, the first of its kind, where technical norms were developed on the basis of the most recent knowledge (the Cavendish laboratory occupied this role at Cambridge, under the leadership of the great Maxwell himself). The last element: the efficiency of these institutional creations strictly depended upon the politico-economic context, especially the patent policies in force in different countries (in France, for example, patents are more often issued for products, while in Germany they are issued for processes, which has led to enormous differences in the dynamics of innovation).

From 1900 to 1940 the industrial laboratory became the norm in many industrial sectors (but to a much lesser extent in France). It was in this way that the new physics of electrons, begun by J.J. Thomson, was introduced to the labs of GE and ATT (making up the Bell labs), initially to optimize electric lighting and intercontinental communications. Interdisciplinarity came upon the scene at that time as a management tool: as a concerted demand and deliberate action, this interdisciplinarity was not in fact born within the university, but in industrial research laboratories. Because there were concrete problems to solve, because it was necessary to reduce the bottlenecks in technical development, and such reduction could only result from a state-of-the-art science that was still under way, the heads of these laboratories called on and recruited individuals from complimentary professions and with complimentary skills. Bell's research program on materials during the 1930s is the perfect example of this: since it had wagered on new types of objects (the transistor would be the best known product), chemists, physicists specialized in metals, magnetism or electronics, experts in quantum mechanics (including many Europeans), crystallographers—and engineers from many other specialties—were brought together within a common space. Each one had a wide margin of latitude within their own program, but was required to participate in formally organized exchanges—the company's patent specialists, for example, circulated among the various groups in order to identify opportunities for future meetings.

I'd like to note here four things: (1) it was these programs that demonstrated at that time the interest of interdisciplinarity (there was no reason for it to be so intensely expressed within the university); (2) it concerned more than a simple mix of academic disciplines, rather, different professions were put into action; (3) what was basic was not considered as a separate category giving rise to the applied, but as a working logic that would bear no results unless enriched and guided by what was happening elsewhere; (4) this in turn led to a recomposition of the disciplines: if it was in the industrial space that solid-state physics was concretely established, it was only after its assimilation by the university that it became a discipline.

In the years of the Second World War and the Cold War there emerged other types of professions and combinations of disciplines (around nuclear energy, electronics and quantum optics, around the molecular approach in biology, etc.). More importantly, mathematics, along with the new computer, entered into the heart of scientific and engineering practices. It was the creation of the H-bomb that brought forth the idea of simulation, as developed in Monte Carlo methods; it was at the RAND Corporation— an organization of several hundred researchers created by the American Air Force, bringing together mathematicians, logicians, physicists, engineers, economists as well as experts in organization and the social sciences—that game theory and systems analysis gained greater currency. The places in which science was constructed were diversifying. Areas such as Silicon Valley or Route 128 emerged and military and industrial think tanks placed scientists at the center of their activities. (One mustn't forget that at the time it was a matter of winning the Cold War—a war that was carried out by the deployment of techno-scientific tools). Scientists were thus called on to think about submarine warfare or the organization of R&D—and this put them in a position to change their practices. Academics and basic researchers were both consultants to and the beneficiaries of various structures, which led them to meet regularly with their colleagues, compare their approaches and tools, to become inter-disciplinary and inter-professional.

This explains a second characteristic element of techno-scientific practices during those years: the preoccupation, if not the obsession, with gadgets (to use the term of American physicists of that time), with deriving from all their research the instruments, technical objects, the "black boxes" later reappropriated by industry or other disciplines. The war-era work on radar gave rise to resonance techniques (this work was carried out by Félix Bloch and many others at Stanford and elsewhere); the work on molecular jets and optical pumping led to masers and lasers, especially as a result of Isidor Rabi and Townes at Columbia; and war electronics made possible various types of detectors used in the physics of cosmic rays and later in industry. This work, stimulated from the outside, became an integral part of scientific work—often resulting in the creation of new companies. In short, instrumentation and instrumental and technical developments became a constituent part of the work of physicists. It's worth noting, once again, that it was a new form of life that emerged, another way of being in the world that took shape, another definition of self and legitimate practices that came to light—and that this emerged because the social context changed and otherwise stimulated scholars. Lastly, these new practices came to define the norms that were established internationally since they were the bearers of a new industrial, economic and political efficiency.

The landscape has changed once again over the past two or three decades. The greater performative capacity of techno-science—especially by way of molecular biology, biotechnologies, the physical technologies of communication and information—and the change in the main political and economic regulations (what's grouped under the heading of globalization, the withdrawal of States, the rise of financial capital, etc.), along with the concomitant change in the modes of knowledge production, have led to the academic world's loss of its central role, reduced investment in basic research on the part of large companies, drastic changes in rules concerning intellectual property, evidence of having to create start-ups, changing values in scientific communities, etc. One could say that there is a tendency to pass from a mode of production capable of balancing together two systems, one being an open and public science and the other a private science, to a mode in which the role of the former is reduced—at least in certain regions of the world such as the United States and the United Kingdom. Under the

influence of the liberal economic revolution and the change in social values, the knowledge production regime inherited from the Cold War and centered on the university has found itself placed in doubt, much to the benefit of a more private production regime of techno-scientific goods. The change in patent policies and the tendency to broadly extend the patentable areas of application have been decisive here: they made up the privileged means by which the commercial university has been able to alter the previous balances. These changes bring forward important questions, questions that are decisive for all research and for the CNRS in particular.

3.

Having completed this rapid overview, I want to return to the questions that currently concern us. I'll address them by considering the notions of the basic, of valorization and interdisciplinarity.

Regarding the basic, I'll say two things that are partially contradictory—but this tension is a fact that must be addressed. What the brief historical overview showed above is, first, that "the basic" must not be conceived of as something given or evident, something that might serve as the origin of technologies. In a knowledge-based liberal economy, which is one way of defining our societies, "the basic" doesn't have (anymore?) its own being and place. The world of innovation and development, in order to succeed, requires the conjunction of different logics, their spatial integration, their temporal phasing. The basic is intimately mixed with the technical, the instrumental and industrial, with management, capital risk, appropriation policies and patents, with engineering sciences—and the mentalities of the various actors.

This first remark does not imply, however, that one needn't defend "basic" work's right to autonomy, but just the opposite, whether it concerns physicists, biologists or philosophers. Even if it doesn't have an intrinsic definition, it's a way of being in the world, a way of asking questions that are worth defending. The CNRS, because of its size, can fulfill here an important function. If one considers that the liberal logic (and especially that of companies) can lead to the neglect of the long-term in favor of the short-term—like the neglect of certain global and collective interests expressed by society—the CNRS and it researchers must identify the research domains that will be abandoned by the market economy, and can decide to make them "exist". A classic example is the study of transgene flow, regularly ignored by agricultural biotechnology companies whose goal, understandably, is to put to market as quickly as possible the GMOs they developed, but a study which many "basic" researchers have taken up again. The CNRS already does this type of thing, especially in its multidisciplinary programs, but it must make this matter one of its raisons d'être. I would say that, in the new socio-economic context, one of its basic missions must be, quite precisely, to promote a type of research of the long-term that's critical, but that risks being neglected by economic actors who are pressed for time.

The question of valorization, which is symmetric to the preceding one, is subject to the same remarks. Placing valorization at the center of the problem is of capital importance for the institution today. The CNRS does considerable work in this area, but it could perhaps still do more. The question of the frame of mind of researchers (their desire for involvement in development, if you will) is clearly a delicate one: it is political and goes well beyond the CNRS's sphere of influence. Yet, considering the case of the Bell labs cited above, the CNRS could put into place a policy of systematically surveying the work carried out in its research units, a policy led by professionals whose task would be to actively identify possible fertilizations, and initiate exchanges. The economic potential of their work often remains invisible to researchers, immersed in their own logic, and on-going investigations aimed at valorization, investigations entrusted to specialized personnel or to important figures responsible for bringing together the work taking place in different areas, could provide significant benefits.

However, we must also calmly consider our relationships to industry and the business world since the logic of the latter can be short-sighted, as I just noted, but especially since it's essential for society that other points of view are regularly considered. Being aware of the importance of valorization does not

imply giving without expecting anything in return, it does not imply not being respected as a full partner, it does not imply not asserting one's rights (on the timescale of research, for example) and one's values. In short, it is up to an active CNRS, managing its own discoveries and cross-fertilizations to decide the social and economic uses of these, and to put them to public debate if it judges it useful for the general community.

Interdisciplinarity, as such, can be approached through two questions: What are its ends, and What are its tools? Interdisciplinarity, at a first level, is inherent to all scientific work. By definition, students are trained through more or less interdisciplinary groups, and researchers are socialized in the paradigms and working methods of diverse communities. With the intervention of creativity, these frameworks regularly find themselves to be in misalignment—a new interdisciplinarity appears, seeds for new ideas pass between neighboring fields, and novel frameworks spread and take root. And so it goes repeatedly, as seen in the regular overhauls of the National Committee's divisions. At this level of interdisciplinarity, only the professionals of the domains concerned can judge what will be the best possible arrangements. The idea of creating transversal programs (and not necessarily reorganizing the committees) is another solution, a solution that takes into account both the fact that there is not just one possible arrangement and the need to maintain several parallel structures. The same "object" can be approached from many different angles, and care must be taken to maintain this multiplicity (I could develop this in much greater detail in the case of history as a discipline, with its different temporal registers, its various scales of analysis, its different connections with other approaches, the anthropological or economic, for example; but the point is rather banal and not worth developing among this group of readers).

However, the question of interdisciplinarity as it's addressed by different segments of the CNRS today goes beyond the knowledge industries' "customary" reconstructions. It covers—as is the case with most interdisciplinary programs—other issues and aims. Although it's certainly a matter of helping us do our work well, it's also a matter of making us more sensitive to what many call the social demand. If we admit that the institution's inclusion among the concerns of the society that maintains and finances it is both important and legitimate, then two aspects need to be considered. The first was just mentioned, that is, the active and original contribution that the CNRS can bring to economic innovation and development. Such action implies a policy of valorization and vigilance, a policy that must be actively updated regarding what's happening within and outside its walls. The issue here is much more than a kind of interdisciplinarity as understood in the earlier sense; indeed, the notions of profession, on the one hand, and of interests on the other, lead us to frame the question in a completely different way.

The other aspect is that of public debate (on GMOs or the greenhouse effect, for example), and the various positions it can assume—which leads to a third level of social interactions involving the sciences, a level that goes well beyond the notions of interdisciplines, inter-professions and the defense of interests, whether for single participants or research companies. I'd like to note three things. First, it's worth repeating that all studies have shown that the growing fears in society with respect to the practices of industrial technoscience are due less to a mistrust of "science" than to a worry about, or refusal of, the means of social and political regulation, on the one hand, and the exclusively reductionist, technological and technocratic manner of addressing problems on the other—with the corresponding neglect of a variety of necessary approaches. The response to these concerns is not, therefore, to find new pedagogical methods or means of popularization (it would be necessary, above all, to instruct an uninformed public); it is not a matter of bad communication.

The second point: research domains having to do with studies on the environment and its management—the introduction of genetically modified organisms, laboratory- assisted human reproduction, impact measures of technology, nuclear energy and waste, planning for health crises, planetary equilibria (ozone holes, climatic changes, etc)—imply radically new forms of exchange that must be invented. It is not, in fact, a matter of making those who produce knowledge, techniques or industrial devices "collaborate" around the same question (in order to reconfigure a domain or to

innovate, for example); rather, it's a matter of intervening in debates that potentially involve the society as a whole, that are distinguished by an infinite variety of sensibilities and values, and whose outcome is, for each society, the choice of its future and the implementation of preventive actions.

In this task of defining the social issues for itself, all the facets of scientific work can be put to action. The sciences are among the primary actors (they set things in motion by contributing to the deployment of technical systems), and also among those actors located at the end of the cycle (science is asked to help regulate those things to whose change it had previously contributed). Yet, unlike what one sees in questions of interdisciplinarity, the problems here are completely framed from outside (for example, due to an unexpected health crisis), they unfold in timeframes defined by social and political needs (and not by those of research or innovation)—and they take on greater importance according to radically diverse criteria and must be accepted as such (conflict is inherent to politics). The intrinsic complexity of questions, like the inability to control work schedules, presents the sciences here with problems of which they have only imperfect knowledge. Transdisciplinarity is thus no more than one set of questions, and not necessarily the most critical, that are broader than the ones concerning the choices that society wants to make for its future (does it want a productivist agriculture or not?), such as the forms that the political or social debate takes or must take on. Experts and scientists certainly play an important role in these debates (think of the question of climate controls, unimaginable without modeling), but the stakes are such that they cannot be the main contributors nor the only judges. It is even likely that their research programs, including the basic questions that they define as those of their disciplines, will be redefined due to their involvement in these dialogical exchanges. And I think it is good to be aware of this, to expect it, and to want it.

The tools of interdisciplinarity will now, finally, be noted. They'll obviously vary according to the meaning one gives to the expression "interdisciplinarity", and each person can easily modify them. I will only draw attention to two general aspects, since the first is already suggested by the post-war situation, but also because it can be seen today and read in division and department reports — namely, that the development and transfer of instruments, techniques, and materials is the decisive tool of interdisciplinarity. By extension, one must also include collection techniques, the management and processing of data, software, calculation and modeling techniques, information sharing—all of them objects and practices that ought to be valorized. Perhaps certain departments and divisions that still live in relative isolation, due to their objects of study, could be decisive here (Nuclear and Particle Physics, for example, might come to mind). As the researchers from this department note, their strength is in the tools they develop.

The second remark concerns the place of the human sciences in the definition of the CNRS's programs. If one agrees that the public debate is a space to take seriously, as much in order to analyze it as to assure that it unfolds in the best possible manner, one will note, first of all, that the human sciences are not without importance (this concerns life, ethics, law, the body, the economy, agriculture, the food industry, sustainable development, the precautionary approach, "governmentality", etc.) I note that they are present in all of the interdisciplinary programs of the CNRS, yet I wonder if everyone really believes in their importance. One often has the impression that it's still an "expensive hobby", so to speak, something that cannot be central since there is nothing to be learned from this exchange. I therefore stress that if one has understood that these questions are by now, and for a long time to come, our horizon of thought and work, and that social issues can no longer be dealt with only in a "top down" fashion, then it's better that the ear they lend to one another be profoundly attentive. It must be so between the scientists and "laymen" who reconstruct the social bond on a daily basis; it must be so as well—and wouldn't it be a good start?—between the hard sciences, the human and the social sciences.

[1] The National Committee of the CNRS, an authority that is in part elected and in part nominated by the government, is divided into more or less disciplinary divisions. The National Committee plays an essential role in recruiting researchers and it periodically publishes situation reports.

Discussion

Teaching, Research and Learning
Tim Moore
Nov 1, 2003 12:46 UT

I found this text very interesting in all respects. Though consciously addressed to the evolving or possible roles of the CNRS, it can also, I think, be profitably considered by those in different institutional environments. Given its stated brief, its focus is on research. The point I should like to make is that the notion of 'research' is itself an evolving and varied construct (especially when it is, for instance, contrasted with teaching). I recall being suprised as a young university teacher in the 1960s that a lot of the work I had been doing in philosophy for some years was called "research", and might even be eligible for grants (previously, I thought of it as 'work', and had made no distinction between teaching and research, or between being a university student doing various tasks, and doing research). I see numerous tendencies in different contexts to go against the research/teaching dichtomy, and replace it with learning (or in Boyer's terms, different types of scholarship, including the scholarship of teaching and learning). This too has considerable implications for disciplinarity, interdisciplinarity, cross-disciplinarity. If discipline is a mode of learning, it may be quite plastic for individuals or groups, whatever psychological, sociological or economic pressures may be present.

> **Tim Moore is right about research and learning**
> pestre dominique
> Nov 3, 2003 14:26 UT
>
> I do not think that an author has necessarily to comment on any reaction to his work. I will nevertheless do it now to help the debate start. My essential remark is that I fully agree with Tim Moore and that a shift in vocabulary would likely lead to intellectual changes. My not so well-thought usage of 'research' in my text has to do with a French habitus in academia (do you know that in CNRS parlance we talk of 'laboratory' for groups in the humanities ?), an habitus derived from the fact that the CNRS was initially imagined for, and built by, physicists. Its role was first to staff university (hard) science laboratories with 'full time researchers', people with no teachnig duties. And of course, my paper mainly adresses the 'hard sciences' -- which dominate CNRS.
>
> **defending the distinction between teaching and research**
> Dan Sperber
> Nov 18, 2003 10:58 UT
>
> There are few things I like better than questioning accepted dichotomies but I must confess that I had unreflectively accepted the research/teaching distinction. Let me—reflectively this time—say why I think I will go on doing so. Various forms of teaching are found in all the cultures where at least some aspects of the transmission of knowledge are institutionalised. In most of these cultures, the knowledge to be taught was accepted as such because of its anchoring in tradition, and was not meant to be revised or even incremented by means of research in whatever form. In past cultures with a distinct research activity, still much or most of the teaching was about topics where knowledge was seen as already well established: think of the teaching of rhetoric with no new research by any definition for millennia and only changes in pedagogy and the curriculum (in particular with Ramus' reform). Even when the outcomes of research are rapidly integrated in the teaching curriculum, as is the case today, the activity of teaching and that of researching—literally, how you spend your time—are extremely different

and involve quite different skills (except, of course, when you train future researchers, but this is, what, 0,01% of all teaching?). Teaching is a very widespread professional activity, with an almost universally recognised usefulness. Research is a much more localised professional activity, with its usefulness recognised only in some cultural contexts. Most teachers in primary and secondary schools have no research goals at all. The oddity rather—and I would like to know more about its history—is the notion that any university teacher should be a researcher (*un enseignant-chercheur* to use the French idiom), with a clear implication that this is better than being a mere teacher. Why so? And how realistic is it, in particular in the Humanities where many excellent teachers waste much of their energy writing so-so dissertations that no one will ever read?

In the present context of a seminar on interdisciplinary, the distinction between teaching and research is not to be too easily washed away, since the notion plays quite different roles in the two cases. The motivation for interdisciplinarity in teaching has to do pedagogic efficiency and the kind of general and scientific culture we want to impart to our children. The appropriateness and feasibility of interdisciplinarity is much more obvious for children in primary school than at the university (even if it is highly desirable also at higher levels). The motivation for interdisciplinarity in research are more complex and have to do with the scientific and technological goals well evoked in Pestre's paper.

laboratories
Tim Moore
Nov 5, 2003 12:56 UT

I like the usage of "laboratory" mentioned by Dominique Pestre. In fact, when I was at the University of Hong Kong, I had a work-room used for various purposes named the "Philosophy Laboratory" (without consciously modelling this on a French precedent).

But the research/teaching dichotomy obviously goes much wider than the French model, though I remember reading that those who go in for "research assessment" of institutions said that in France 80% of publications that made the grade were down to the CNRS, implying a low research profile for French universities.

If this is so, my perception (not based on adequate historical knowledge or research) is that this has been due to a tendency in France, when Universities were perceived by the powers-that-be not to be doing a satisfactory job, to create new institutions, starting with the Collège de France, and going on to the Grandes Ecoles, etc., and including especially the CNRS.

Measuring interdisciplinarity -- an empirical question
Roberto Casati
Nov 6, 2003 10:13 UT

Measuring interdisciplinarity

Pestre's article prompts me to suggest a research programme, concerned with the study of actual interdisciplinary practices within a large research body such as CNRS. The programme would be oriented around research questions that lead to operazionalizable variables (which would have to be defined), partly based on single researchers' self-assessment of involvement in interdisciplinary work, partly based on some objective standards (again, to be defined). Historians of science and philosophers would be involved in the research. Appropriate temporal and geographical cross-sections, samples of the research activity would also have to be defined. The reason beyond this proposal is that it looks as if many discussions about interdisciplinarity are not very keen on distinguishing between programmatic (and normative) claims about interdisciplinarity an descriptive

claims about actual practices. (Labeling a research activity 'interdisciplinary', putting together people from different disciplines, etc. does not necessarily make it interdisciplinary.) Although Pestre's contribution is not oblivious to the distinction, my feeling is that the descriptive part of the study is still missing on a sufficiently large scale.

Descriptive, normative and the assessment of interdisciplinarity
Gloria Origgi
Nov 12, 2003 11:11 UT

The descriptive study of interdisciplinarity will be the focus of the next paper published on interdisciplines, by the psychologists Howard Gardner and Veronica Boix-Mansilla. It will deal precisely with the problem of assessing interdisciplinary research and will present results of an empirical research made within some of the leading interdisciplinary institutions (Santa Fe Institute, MIT Medialab, etc.).

Although I understand the need of a more descriptive analysis of interdisciplinary research, I think that what this seminar shows is that the very notion of interdisciplinarity needs to be clarified. Sometimes discussions revolve around the need of interdisciplinary research for scientific innovation (most of Dominique Pestre's historical examples show the role of interdisciplinary équipes in technical and scientific innovation). Sometimes interdisciplinarity is presented as a new method of research that can stabilize and replace the traditional institutional framework of disciplines. This second aspect is particularly confusing, because the involvement in inter-, trans-, or multi-disciplinary projects is very often a way to reinforce – instead of challenge - disciplines as combinations of sets of knowledge that can be pieced together to create new research programmes.

My feeling is that the normative inquiry of interdisciplinarity may help to better understand whether and how interdisciplinary research is challenging our scientific image and the way in which we represent our everyday activity.

As a personal example, I have some difficulty is assessing some part of my work in terms of interdisciplinarity. Is the www.interdisciplinary.org project a piece of interdisciplinary work? Or is it just a happy encounter of people who work in very different fields (academy, start up) and have shared their knowledge to create an innovative software? Or is it an interdisciplinary project because it has selected interdisciplinary topics for the virtual conferences? What is interdisciplinary, the code or the content? Here I am employing at least two different senses of the concept of "interdisciplinarity", that refer to two very different areas of activity, that is, making new things and conceiving new frameworks in which old disciplinary ideas can be reinterpreted.

Interdisciplinarité par la science du social
Abdelkarim Fourati
Nov 17, 2003 10:53 UT

Dominique Pestre nous décrit, à partir de ses expériences professionnelles concrètes, et en utilisant la méthode historico-critique, les étapes évolutives de la naissance de l'interdisciplinarité moderne. Mais ce texte a été écrit particulièrement pour le Conseil Scientifique du CNRS ; et on a l'impression que ses observations et ses conclusions ne sont valables que pour cette institution de recherche. En fait, il distingue de façon générale dans l'évolution des savoirs au cours des XIX-XXe siècles, trois étapes clairement définies, et une quatrième (l'étape actuelle) qui reste encore floue. Cette ultime étape doit se dégager en repensant trois notions : science fondamentale, valorisation de la recherche et interdisciplinarité.

Nous pouvons intituler ses trois étapes, à partir des analyses de Pestre, de la façon suivante : 1°- Première période (1860-1900) : L'institutionnalisation disciplinaire. 2°- Deuxième période (1900-1940) : Naissance de l'interdisciplinarité par le management. 3°- Troisième période (1940-1990) : Interdisciplinarité par la science fondamentale. Dans cette dernière période, les sciences fondamentales physico-biologiques dominent, alors que les sciences humaines et sociales ne sont que des outils au service des premières. Pour repenser l'interdisciplinarité, je suis de l'avis de Pestre qui dit que le "fondamental", coupé du social et de son contexte, n'a plus d'être et de lieu propre. Autrement dit, il faut un "renversement épistémologique" entre les rôles des sciences du social et les sciences de la nature : ces dernières ne seront que des outils au service des premières. En effet, la troisième période de l'interdisciplinarité a préparé le champ et les instruments de travail pour les "Sciences du social" (voir mon intervention : Globalisation disciplinaire, dans la discussion du texte de Catherine Garbay, le 23 septembre 2003). De fait, comme le suggère Pestre dans ses remarques préliminaires les connaissances ne doivent pas être séparées des instruments, or les seuls instruments des sciences du social sont les moyens et les technologies de cognition/communication. Ainsi, nous sommes déjà entrés dans la quatrième période de l'interdisciplinarité depuis le début des années 1990 que nous pouvons intituler : Interdisciplinarité par la science du social.

D'autre part, Pestre distingue dans sa conclusion trois niveaux d'interdisciplinarité d'espace de plus en plus larges : 1°- Interdisciplinarité par fécondation des champs de savoir voisins. 2°- Interdisciplinarité par des programmes de recherche transversaux. 3°- Interdisciplinarité par l'inscription dans les préoccupations de la société. Cependant dans ce dernier niveau qui est le plus large, il n'évoque que la valorisation économique de la recherche, alors que nous devons considérer une valorisation plus générale, à la fois sociologique, culturelle, artistique, économique… De fait, le débat public et les interactions sociales impliquant la science nécessite une vision au-delà de l'économique. Mais Pestre se rattrape à la fin de son texte lorsqu'il discute de la place des sciences humaines et sociales. Finalement, comme il le dit : « le social ne peut plus être traité seulement sur le mode top-down », d'où l'intérêt des "Conférences de consensus" (voir mon intervention dans la discussion du texte de Helga Nowotny, le 6 mai 2003)

learning, teaching and research
Tim Moore
Nov 20, 2003 2:33 UT

While I consider that Dan's defence of the teaching/research distinction is well-taken at institutional levels, there are other things which can be said, and which motivated my challenge. In particular, teaching is nothing unless it leads to learning. On the one hand, we are familiar with the model of teaching as the transmission of knowledge and skills; but on the other hand it may be conceived as the creation of an environment in which people can learn for themselves. If learning is the key concept, then it can be maintained that any process of learning structured as trying to find the answer to some question or problem has the general form of a research project.

> **Learning, research, and interdisciplinarity**
> Dan Sperber
> Nov 21, 2003 9:43 UT
>
> Tim (replying to a message of mine under the earlier discussion thread "Teaching, Research and Learning") is making an interesting point. I fully agree that learning is more fundamental than teaching. In fact, most human learning is done without the help of any teaching, and this has been particularly the case in non-literate societies (see, if I may, Scott Atran & Dan Sperber (1991) Learning without teaching: its place in culture. In L. Landsmann (ed) *Culture, schooling and psychological development*, Ablex) I am less in agreement with the view that the learning process is akin to a research process. The extreme version of this approach (cf, for instance, *The Scientist in the Crib : What Early Learning Tells Us About the Mind* by Alison Gopnik

Andrew N. Meltzoff, & Patricia K. Kuhl, Perrenial 2000) sees the baby as a "little scientist" and the older child (see Susan Carey's work) as going through Kuhnian paradigm shifts. There is much of value and relevance in this approach. Still, my reservations come from the fact that I believe that most learning is based on domain-specific learning mechanisms (Marler's "learning instincts"). If so, it has a lot to do with maturational processes, parameter fixing, slot filling, weight settings in dedicated neural networks, and so on, and little with the highly metarepresentational and virtually domain general character of scientific research.

This is highly relevant to the issue of **interdisciplinarity** in teaching/learning and in research. In research, it is beneficial to be able to freely redelimit or open domains on the basis of theoretical advances. Also, borrowing of models, or at least metaphors from one domain to the next is an obvious source of insight. In much of learning, it might be a mistake to ignore, or try to by-pass inbuilt domain-specific learning readiness, or to assume that a cross-disciplinary approach will systematically be a source of insight rather than confusion. Of course, I don't want to be dogmatic about this. I do believe that, especially with older children, interdisciplinary inputs present a challenge that often does stimulate learners. However, much more research need to be done on domain-specific and domain-general acquisition mechanisms to better ground our pedagogy. Untill then, the endearing metaphor of the learner as a quasi-scientist may often do more harm than good.

Evolution du savoir
Clotilde Lampignano
Nov 20, 2003 9:48 UT

Mes commentaires se concentrent sur l'idée évoquée dans l'article de Dominique Pestre, d'évolution des champs du savoir. L'auteur affirme que l'idée de champs du savoir aujourd'hui ne peu plus être conçue comme un savoir abstrait indépendant de la pratique, du savoir faire. Puis il présente des dichotomies : connaissances-instrumentations/dispositifs matériels et techniques (la connaissance existe parce qu'elle est appliquée). Donc le savoir se transforme en techno-savoir, ou mieun les savoirs en techno-savoirs. L'auteur fait des exemples concrets, de comment ces techno-savoirs sont devenus des disciplines dans les universités ayant vu le jour dans les laboratoires industriels, à travers la collaboration de plusieurs disciplines orientées à la création d'un objectif/objet déterminé. Même si l'auteur avait dit au début de son article que sa recherche ne pouvait être que partielle, il me semble qu'il traite ici seulement de l'interdisciplinarité entre connaissances scientifiques et leurs applications. Comment pense-t-il d'expliquer la relation entre connaissances humanistes ou humaines et leurs applications techniques ? Naissent-elles aussi d'abord dans les laboratoires ? Et si oui, quelle est leur évolution ? Il me semble que ici aussi l'interdisciplinarité est vue en blocs, le savoir scientifique d'un coté, et celui humaniste, ou littéraire de l'autre.

des sens multiples du mot interdisciplinaire (1)
dominique pestre
Nov 21, 2003 11:38 UT

Si mes amis me le permettent, j'aimerais répondre d'un seul trait aux quatre dernières interventions. Mon texte étant trop long pour la machine, je me permets de le couper en deux morceaux.

D'abord, je dois préciser que mon texte ne traite que des sciences dites dures travaillant en laboratoire, et Clotilde Lampignano a raison. Je n'ai donc pas considéré le lien aux humanités et aux sciences sociales, ni le lien aux demandes sociales en général, ni le lien aux régulations politiques de ces savoirs.

Dan Sperber pense que l'enseignement est une activité professionalisée, ancienne et à utilité reconnue alors que la recherche est plus locale. Ceci est peut-être très vrai dans une perspective historique longue mais ne s'applique pas très bien aux sciences dures du dernier siècle et demi. La

recherche y est ici une activité très reconnue, très professionnelle, et qui est considérée dans ces milieux comme plus fondamentale et utile que l'enseignement. On peut certes débattre de la 'vérité' de cette assertion 'en général', mais elle dit quelque chose d'intéressant sur les réalités sociales de cette époque. C'est par ailleurs depuis le dernier tiers du XIXe siècle qu'est reconnu, dans les milieux scientifiques universitaires, que le bon professeur doit être celui qui a une oeuvre originale 'en recherche', 'au laboratoire'. Ce sont d'ailleurs ces critères qui, depuis cette date, conduisent partout à son recrutement.

Sur l'interdisciplinarité moderne autour et dans les sciences dures, je voudrais préciser ce que je souhaitais dire. Premier niveau, l'université, car elle enseigne, constitue des corpus de savoirs qu'on nomme disciplines. Dans le travail d'innovation (de recherche), ces cadres sont régulièrement dépassés et le travail intéressant opère souvent entre disciplines. Apparaît ainsi la physico-chime à la fin du siècle dernier qui, via l'enseignement, se tranforme alors en 'discpline'. Ce premier processus d'hybridation disciplinaire est banal, courant, ancien et sans fin dans le travail intellectuel. En bref, il est l'essence du travail des sciences.

les différents sens d'interdisciplinaité (2)
dominique pestre
Nov 21, 2003 12:23 UT

Second niveau : depuis le dernier tiers du XIXe siècle, ces sciences de laboratoire sont devenues technosciences, je veux dire qu'elles sont alors produites aussi dans des espaces non académiques et qu'elles sont actives et souvent au centre des dispositifs techno-industriels. Ici s'ajoute donc la logique d'autres institutions, celle des entreprises par exemple. La science industrielle est réglée par d'autres principes que la science universitaire (il faut gagner de l'argent par exemple, et produire techniques et brevets) et les managers (ou les militaires) organisent un autre quadrillage des travaux et créations ; ce faisant, ils créent de l'interdisciplinarité mélangée à de l'inter-métiers (avec des ingénieurs, des professionnels des brevets, etc.) Cela conduit aussi à une autre dynamique d'élaboration des savoirs. A nouveau toutefois, ces nouvelles spécialités hybrides (la physique des solides dans les années 1930 à la Bell, la science des matériaux en 1960 autour des programmes du DoD) ont besoin d'être enseignées et elles sont alors constituées en corpus canoniques de savoirs dénommés disciplines. Elles incluent toutefois maintenant des enseignements dans les facultés de 'génie' (electrical engineering, etc.) Précisons qu'après 1945, une fondamentalisation accrue en physique et bilogie déplace les modes de relations aux techniques (j'explore cela plus longuement dans un livre livré hier par l'imprimeur et intitulé Science, argent et politique, édité par l'INRA).

Troisième niveau, plus sensible aujourd'hui : le crops social manifeste des défiances accrues vis-à-vis d'une tecnoscience impérieuse et de plus en plus capable de transformer nos vies et de redéfinir 'notre commune humanité'. Il demande débats, révision des principes d'expertise, retour à des réflexions plus générales (éthiques par exemple) -- et le vocable d'interdiscipli-narité est invoqué : ces questions relève de divers champs de savoir (le changement climatique mobilise mathématiciens, physiciens, chimistes, modélisateurs, biologistes, économistes, politistes, etc.) et divers champs de pratiques sociales (industriels, universitaires, politiques sont concernés). Le sens du terme est toutefois encore très différent et ce qu'il implique en termes de pratiques sociales est autre chose. Son lien à la décision politique et à la mise en place de régulations me semble notamment devoir conduire à l'invention d'un autre vocabulaire.

The Responsibility of Agencies
Julie Klein
Nov 28, 2003 20:49 UT

Dominique Pestre's paper is an excellent contribution to this conference. I especially appreciated it since I traced a parallel history in a chapter on "National Competitiveness," in the book CROSSING

BOUNDARIES. I also appreciated having an update on CNRS reports, because I have been reading the new generation of counterpart reports in the U.S. lately, in anticipation of a forthcoming convocation in Washington, D.C. on facilitating interdisciplinary research. The new heightened rhetoric of inter/transdisciplinarity in Europe and the U.S. puts greater onus on not just disseminating the wisdom of practice from the history of interdisciplinary initiatives but the thematics of valorization and vigilance Pestre articulated so well. The targeting of particular interests and goals has reshaped the timetable and the logics of investigation. “Basic” research as Pestre also wrote, has been realigned in a new political economy of knowledge that has refigured older boundaries of academic and proprietary inquiry. That is all the more reason, then, key agencies such as CNRS and its counterparts in other countries must recognize not only the new complexity of interdisciplinarity but also its entanglements in social, economic, and political priorities that leave aside the moral and ethical dimensions and obligations of research.

Assessing Interdisciplinary Work at the Frontier. An empirical exploration of 'symptoms of quality'

Veronica Boix Mansilla (Harvard University)
Howard Gardner (Harvard University)
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Abstract: How does one ascertain the quality of interdisciplinary work when criteria from the individual disciplines do not suffice? Assessment is one of the most important and least understood aspects of interdisciplinary research. Building on an empirical study of interdisciplinary work in exemplary institutions, we describe common challenges and propose epistemic criteria by which interdisciplinary work can by evaluated.

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Introduction

Arguably, the most dynamic research at disciplinary frontiers and in novel terrains is interdisciplinary [1]. Yet a re-emerging awareness of interdisciplinarity as a pervasive form of knowledge production is accompanied by an increasing unease about what is often viewed as "the dubious quality" of interdisciplinary work [2]. Central to the controversy is the lingering challenge of assessing interdisciplinary work [3]. Addressing the lack of available criteria to assess interdisciplinary work on its own terms, as Don Kennedy put it: "It is a terribly difficult issue….Interdisciplinary research institutes have the challenging task of producing as much good research as the departmental silos, judged however in a somewhat different [disciplinary] universe." [4] "Criteria for judgment constitute the least understood aspect of interdisciplinarity," adds Julie T. Klein, "in part because the issue has been the least studied and in part because the multiplicity of tasks seems to militate against a single standard." [5] Against this background how does one determine what constitutes quality interdisciplinary work?

In this paper, we present the initial results of an empirical study of experts' views of interdisciplinary research. Specifically, we address the ways in which individuals in established and well regarded interdisciplinary research institutions assess the quality of their work and describe the dilemmas they confront. Our findings reveal that researchers systematically rely on indirect quality indicators (e.g., number of patents and publications-or type of journals and funding agencies associated to the work). Measures that directly address epistemic dimensions of interdisciplinary work (e.g., explanatory power, aesthetic appeal, comprehensiveness) proved rarer and less well articulated. In what follows, we introduce the study and summarize our findings. We delineate three core epistemic "symptoms" of quality interdisciplinary work emerging from our analysis: consistency, balance, and effectiveness.

Study overview

In the last two years, we conducted an exploratory study of research and teaching practices at exemplary interdisciplinary institutes and programs. Our goal was to understand qualities of expert interdisciplinary work in order to inform educational practice that fosters interdisciplinary understanding. In this study we defined "interdisciplinary work" as work that integrates knowledge and modes of thinking from two or more disciplines. Such work embraces the goal of advancing understanding (e.g., explain phenomena, craft solutions, raise new questions) in ways that would have not been possible through single disciplinary means.

In our formulation, disciplines comprise rich collections of theories, accounts, and findings believed to be acceptable within specifiable scholarly communities at a particular time. Such bodies of knowledge cannot be detached from the dynamic repertoire of methodological choices and forms of communication that give birth to them. We view interdisciplinary work as a purposeful means to reach

a cognitive or practical goal (e.g., understanding, solving a problem) as opposed to as an end in itself. Our definition stipulates that disciplinary lenses be integrated in mutually informative networks of relationships rather than simply juxtaposed. By focusing on disciplinary integration –as opposed to the integration of multiple perspectives, disciplinary or not - our focus is more stringent than the "transdisciplinary" one presented earlier in this forum and in the literature.[6]

The insights that we report stem from interviews with 60 researchers working in interdisciplinary institutes- specifically: the MIT Media Lab (ML), the Santa Fe Institute (SFI), the Center for the Integration of Medicine and Innovative Technologies (CIMIT), the Center for Bioethics at the University of Pennsylvania's (CB-UP), The Art-Science Laboratory (ASL), and the Research in Experimental Design group at XEROX-PARC (RED). We selected these institutions for their accumulated experience exploring novel disciplinary combinations (e.g., nonlinear dynamics and history; technology and music) and their good reputation. We expected that the difficulties associated with developing such novel integrations would have engendered a certain epistemological awareness among these researchers—a trait we were interested in capturing. Our data consisted of in-depth, semi-structured interviews (including questions on how to assess interdisciplinary work), selected samples of work, and institutional documents.

Assessing interdisciplinary work – Challenges and measures

Most of the researchers in our study referred to the validation of interdisciplinary work as an obscure and challenging topic. They identified three sources of difficulty. First, they noted that disciplines themselves bring a variety of, often conflicting, standards of validation to the interdisciplinary meeting ground. Second, our subjects pointed to a lack of conceptual clarity about the nature of interdisciplinary work and its assessment, recognizing the need for a more systematic reflection in this regard. Third, they emphasized that in highly innovative work where novel territories are charted and few precedents are available, developing validation criteria is part of the inquiry process itself.

Faced with the task of making their assessment criteria explicit, researchers typically referred to indirect or field-based measures [7] of quality. They pointed to indicators such as the number of accepted patents, publications, devices, and citations stemming from the work; the prestige of the universities, funding agencies, and journals in which it is placed; and the approval of peers and a broader community. "Simply counting things are easy answers as far as I'm concerned," claimed Jonathan Rosen, Director of the Office of Technology Implementation at CIMIT. "How many patents have you filed? How many patents have been licensed? How many new companies have been started? How many Science papers? How many Nature papers?" Field-based measures of this kind sidestepped the question of what constitutes warranted interdisciplinary knowledge by relying on social procedures of peer review, inter-subjective agreement, and ultimately consensus as generators of acceptable insight. Our subjects were often critical of these "proxy" criteria because they saw them as ultimately representing a disciplinary assessment of their interdisciplinary work. Yet they described these criteria as the standard way - however flawed - in which the quality of interdisciplinary work is determined at the forefront of knowledge production today.

When probed, most individuals also referred to more primary or epistemic measures of acceptability - i.e., epistemological indicators directly addressing the substance and constitution of the work. Researchers referred to a broad range of epistemological criteria (e.g., experimental rigor, aesthetic quality, fit between framework and data, power to address previously unsolved questions in a discipline). When considered collectively, these criteria shed light on three realms that demarcate symptoms of quality interdisciplinary work.

Toward an epistemic framework for assessing ID work

Our interviewees highlighted the complexity of knowledge validation at disciplinary borders. In their view, interdisciplinary findings, theories, or exhibitions were not assessed as a sum of independent claims to be tested against equally independent disciplinary bars. Rather, researchers tended to provide a dynamic picture of knowledge validation in which the work as a whole can be assessed on three fundamental grounds:

1. the way in which the work stands vis à vis what researchers know and find tenable in the disciplines involved (consistency with multiple separate disciplinary antecedents)

2. the way in which the work stands together as a generative and coherent whole (balance in weaving together perspectives)

3. the way in which the integration advances the goals that researchers set for their pursuits and the methods they use (effectiveness in advancing understanding)

1.*Consistency with multiple separate disciplinary antecedents*

While the impetus of their interdisciplinary work was to move beyond established disciplinary boundaries, researchers often evaluated the degree to which their work was reasonably consistent with antecedent disciplinary knowledge (i.e., accepted methods, preferred conceptualizations, and epistemic values). They referred extensively to the act of satisfying multiple -sometimes conflicting- disciplinary standards at once.

In their view, borrowed disciplinary theories, methods, and communicative genres embodied epistemic values, which collectively informed the acceptability of interdisciplinary outcomes. For example, seeking to satisfy "two masters," SFI researcher John Padgett expected that his computer models of political life in Renaissance Florence would meet standards of scientific elegance and historical significance. Padgett valued work that could "explain coherently, highly heterogeneous phenomena... explaining heterogeneity with simple principles," while revealing important qualities of the period under study: "in a hundred years, will anyone read it? Historians care a lot about that."

The disciplinary canon was often a basic parameter against which researchers assessed their work. If a new finding was consistent with the "the laws of physics" or "current predictions in biology" it gained credibility. "There is a tremendous sense of freedom associated to breaking [disciplinary] rules" commented Mark Chow from RED, as he described his group's search for new ideas for an exhibit on "Experiments in the Future of Reading (EFR)" at the San Jose Technological Museum. At the same time, he added "you can do a lot of wild things, [but you need] somebody down the hall….who adheres to the scientific method [and is] squarely involved in the disciplines ….to say, well, this is against the laws of physics." If indeed interdisciplinary findings violated fundamental disciplinary tenets or revealed their limitations, additional justification was often seen as required. "The burden is on me to get a deeper understanding of their [disciplinary] methods and show them how their methods do and don't relate to our interdisciplinary methods" noticed Rosalind Picard, Media Lab Director of Affective Computing Research.

Ensuring appropriate fit between interdisciplinary products and findings and their antecedent disciplinary counterparts was not without challenges. Disciplines often conflicted vis à vis what they considered worth studying and what they viewed as warranted understanding. "What is this physicist doing writing a sociology proposal?" asked SFI researcher Mark Newman, as he imagined how colleagues in physics would critique his work on social networks. Illustrating differences in validation standards, SFI's Doyne Farmer commented: "Computer models are looked down on much more in economics than they are in physics. Mathematical proofs are regarded as much more important in economics than they are in physics. Physicists are more comfortable with approximations."

Occasionally, standards stemming from different disciplines appeared as openly incompatible. For instance, the Experiments in the Future of Reading exhibit and the San Jose Technological Museum invited visitors to experience new forms of reading (e.g., interactive books, image and sound enhanced texts) while an explanatory voice guided them at each stop. XEROX-PAIR artist Paul DeMarinis spoke about what he and his colleagues perceived as the conflict between aesthetic and scientific dimensions of the exhibit. He claimed, “In art contexts you don't want a lot of text. You don't want to be told what it [the exhibit piece] is. You want it to come through and allow your mind to make other associations. In a science context you want to be sure that the person isn't misunderstanding what they are seeing.” DeMarinis perceived the exhibit as embodying a central tension between science and postmodern theory in which “the [explanatory] text had the upper hand in formulating the theory [more] than anything you might experience yourself.”

In sum, while a reasonable fit with antecedent knowledge in multiple disciplines strengthened the credibility of interdisciplinary outcomes, it clearly did not suffice as the sole source of rigor in deeming outcomes acceptable. Quality interdisciplinary understanding did not rest on a sum of established disciplinary rules, but rather on a unique coordination of disciplinary insights where disciplines played particular roles in the overall composition of the work. It is not surprising then, that our interviewees viewed reflective “balance” as a second symptom of quality interdisciplinary work.

2. Balance in weaving together perspectives

Assessing interdisciplinary work involves an appreciation of how disciplinary insights are intertwined and the relatively different roles that they play in yielding an overall composition. When disciplinary values conflict, compromises and negotiations are in order. Our interviewees valued work that exhibited a thoughtful balance of perspectives. Reflective balance did not imply an equal representation of disciplines in a piece of work, but a sensible one. For example, Arthur Caplan, Director of the Center for Bioethics at the University of Pennsylvania, described the relative contribution of law and philosophy in his work. He illustrated how inquiry goals largely determined what counted as a workable balance.

“There is some tension between law and philosophy,” he claimed, “as to what is the best way to talk, literally [about matters such as organ donation or human cloning]. Should we talk like lawyers and use case precedents and analogical reasoning? Do we use principles? That battle goes on. I think each [view] has a case and I think healthy tension is ok.” Caplan crafted a pragmatic balance. “For certain issues you do want to know what really is the legal framework that you're operating in. And for some other issues like, ‘Should we ban cloning?’ — starting with the law is really not a good idea. For those, you really need to think philosophically about what cloning is and why it would be bad. You can make a law later.” Caplan critiqued work that made legal recommendations “prematurely, before there is consensus about the values” as well as other cases where “there's a lot of consensus about the values and you don't need to dig in the same old ethical holes again”.

Relatedly, our subjects referred to finding an appropriate balance vis à vis the levels of depth at which various disciplines were engaged. Again in this case, specific inquiry purposes seemed to inform the weighing of options against each other toward a sensible balance overall. In assessing the Experiments in the Future of Reading exhibit, Mark Chow noticed how the inclusion of an animal character in his piece made the content of the exhibit more accessible and interesting to his audience. He established that the exhibit succeeded “at being a dog that could read aloud, rather than a computer exhibit,” adding that such success “allows you to relax some of the more stringent requirements of technology. For example, in terms of engineering, the performance of the reading was not 100% accurate, but it was, after all, only a dog.”

Researchers found isolated disciplinary assessments of interdisciplinary work dissatisfactory because they failed to capture the knowledge composition as a whole— a critique often applied to peer review panels composed of specialists working in isolation: “Sometimes very good interdisciplinary papers

may be viewed in a very negative light simply because narrow disciplinary criteria are used to assess them. If you have a paper that is interdisciplinary and you think that it really does require a broad-gauged person, then I assume you try to find that kind of person. I think for reviewers it is hard not to make mistakes [when it comes to interdisciplinary pieces]."

As our interviewees described it, the interdisciplinary "balancing act" seemed to involve maintaining generative tensions and reaching legitimate compromises in the selection and combination of disciplinary insights and standards. Such a delicately balanced whole gained credibility if it did not violate central tenets of the disciplines involved. It gained relevance and acceptability if it afforded new understandings, solutions, products, and questions—including proposed transformations in established disciplinary practices. Determining the effectiveness of the leverage afforded by interdisciplinary integration was a most informative criterion to ascertain the success of interdisciplinary enterprises - the third symptom in our categorization.

3. Effectiveness in advancing understanding

Not surprisingly, researchers overwhelmingly tended to assess the success of their work in light of the aims of their inquiry. Interdisciplinary inquiries varied broadly in their specific aims and their favored validation criteria varied accordingly. When SFI physicists James Crutchfield and Mark Newman assessed their mathematical theories of innovation and network behavior respectively, they favored qualities such as their theories' ability to "predict" unstudied social and biological phenomena and their "tangible success in explaining something that wasn't explained by somebody else before." At CIMIT, the combination of physiology, molecular biology, nano-physics, and material sciences brought scientists like Joseph Vacanti closer to the creation of an unprecedented entity-- a vascularized artificial human liver that "works" and whose creation could have a "transforming effect" on organ transplantation surgical practice.

No single set of assessment criteria can do justice to the enormous variation in inquiry aims. Still it is worth noticing that, among our interviewees, contributions oriented toward pragmatic problem solving and product development seemed to place a premium on standards of "viability," "workability" and "impact". Contributions that seek formal algorithmic models of complex phenomena seemed associated to measures of "simplicity", "predictive power", and "parsimony".[8] Contributions aiming at a more grounded understanding of multidimensional phenomena (e.g. lactose intolerance or organ donation viewed in their intertwined biological, cultural, and psychological dimensions) tended to favor work that reached new levels of "comprehensiveness," "careful description," and "empirical grounding."[9]

In addition to assessing the substantive leverage afforded by interdisciplinary work, researchers highlighted their methodological contributions. For example, Media Lab ethologist and artificial intelligence expert Bruce Blumberg claimed that his computer models of animal behavior provided novel method for cognitive scientists to "test out" their hypotheses. "Increasingly, computation is going to be a very valuable way to test out models [in psychology and cognitive science]," he claimed. "Because it is one thing to write a book and say this is how it [animal intelligence] must be organized. The proof is, could you take those ideas and actually implement them?" Enhanced methodological options, in turn, raised the standards for the interdisciplinary inquiry that followed. "What we used to do in the past are now things that 16 year-olds on the Internet can do," explained MIT's Rosalind Picard. "We were the only ones doing that 10 years ago."

Because of their non-paradigmatic approaches to knowledge production, our interviewees often confronted the challenge of a lack of precedents or viable contenders against which to compare their achievements. "We don't know if we are doing better than people working by themselves…we don't have these kinds of measures," noted Jonathan Rosen from CIMIT. Working in uncharted terrain implied that "there is no higher authority to appeal to adjudicate what's relevant knowledge and what's not or what is useful and not," claimed Anne Balsamo, Principal Scientist at RED, Xerox-PARC.

“[When] you are at the cutting edge of anything, by definition you're taking risks that most do not take,” added Joseph Vacanti. “So having somebody who can, in an expert way, help you is problematic because there's this vested interest problem, where the status quo and building on the status quo is most of what goes on.” For these researchers, the effective advancement of interdisciplinary understanding involved not only developing new insights and methods, but also fashioning criteria with which to gauge their progress.

To Conclude

Under close scrutiny, researchers’ views about the epistemic evaluation of their work revealed three realms in which to discern the acceptability of interdisciplinary work: (1) the degree to which new insights related to antecedent disciplinary knowledge, (2) the sensible balance reached in weaving perspectives together, and (3) the effectiveness with which a particular piece of work advances understanding and inquiry. As these researchers portray it, quality interdisciplinary understanding does not rest on an accumulated set of established disciplinary rules. Instead, each piece of interdisciplinary work revealed an idiosyncratic coordination of disciplinary insights geared to accomplish researchers’ cognitive and practical goals.

Distilling workable criteria to assess the epistemic dimensions of interdisciplinary work requires that we tackle the problem at a productive level of analysis. Criteria too local (e.g., innovative experimental methods, accurate protocols, or rich original sources) will fail to account for the formidable diversity of aims and approaches legitimately characterized as “interdisciplinary.” Categories too generic (e.g., coherence, accuracy, parsimony) will be ill-fit to capture the particular challenges associated with interdisciplinary integration. Categories with the greatest potential for the assessment of interdisciplinary work, our analysis suggests, will capture (1) the relationship between interdisciplinary outcomes and their multiple disciplinary antecedents, (2) the delicate adjustment that takes place as disciplines are intertwined toward a well-balanced whole, and (3) the leverage provided by the newly created hybrid insights.

In the end, while the assessment categories we propose might contribute to the cause of a more reasoned and reasonable consideration of interdisciplinary work, they will not render interdisciplinary work immune from “the unfortunate propensity for error” that characterizes human knowledge construction.[10] Indeed, interdisciplinary work gains its strength from its keen awareness of the provisional epistemic status of its findings. In our view, a serious assessment of interdisciplinary work should not seek to establish “warranted truths” nor, on the contrary, to let “all interdisciplinary flowers bloom.” Such assessment should instead yield illuminating evidence to grant provisional credibility to the work in question. Thus the acceptance of an interdisciplinary insight (much like that of the framework here proposed) rests on the assumption of the inherent provisionality of understanding and the endless human capacity to “retrench, retool, and try again”. [11]

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[4] Interview excerpt November, 2002.

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[7] Mihaly Csikszentmihalyi, “Society, Culture and Person: A systems view of creativity”, in The Nature of Creativity: Contemporary psychological perspectives, Robert Sternberg (ed). New York: Cambridge University Press, pp.325-339, 1988. In this chapter Csikszentmihalyi defines field as the social organization of a domain—i.e., as a network of interlocking roles (peers, reviewers, gatekeepers) in charge of selecting among proposed variations in a domain.

[8] See examples of this kind of work in James P. Crutchfield, Peter Schuster (eds), Evolutionary dynamics : exploring the interplay of selection, accident, neutrality, and function, New York: Oxford University Press, 2003.

[9] For examples, see William Durham, Co-evolution: Genes, Culture and Human Diversity, Stanford: Stanford University Press, 1991; and Rene Fox & Judith P. Swazey, The Courage to Fail: A social view of organ transplants and dialysis, Brunswick, NJ: Transaction Publishers, 2002.

[10] Catherine Z. Elgin, Considered Judgement, New Jersey: Princeton University Press, p. 12, 1996.

[11] Catherine Z. Elgin, ibid p. 12.

Discussion

Core "Symptoms"
Julie Klein
Dec 2, 2003 17:39 UT

I am especially pleased to see the Boix Mansilla and Gardner paper in this seminar, since I have been working on the question of evaluation recently. I hope a number of discussion threads will unfold over the course of this month. In addition to this opening comment, I am also starting another thread called “Bibliography” because I would welcome additional references I listed some of the key works I have been using and look forward to learning about others.

As the authors point out, evaluation of interdisciplinary research is an area needing greater attention, for all the reasons they identify. Having read earlier reports on this particular set of centers, I am also pleased to see them moving to this new phase. I recommend that everyone participating in this conversation read more detailed accounts of their research. A good place to start is “Building Bridges Across Disciplines,” by Boix Mansilla, Dillon, and Middlebrooks. It’s available from the GoodWork Project at the Harvard Graduate School of Education (http://pzweb.harvard.edu/ebookstore/).

This project joins a number of other recent empirical studies that give us a firmer foundation for talking about evaluation. The three core epistemic “symptoms” identified here (consistency, balance, and effectiveness) make sense, and their explanation is convincing. I would urge a fourth, however, or at least an extension of the third to include criteria that emerge within interdisciplinary epistemic communities that are drawing on not only pertinent disciplines but new interdisciplinary fields as well. To their credit, the authors push beyond disciplinary primacy, rendering it a necessary but not sufficient condition. Interdisciplinary fields also generate their own assumptions about “appropriate fit.”

The particularities of centers is an added factor. This particular pool of centers, the earlier account acknowledges, is in some respects a set of rarified atmospheres with unusual freedom, though not without counter pressures. It would be useful to compare the kinds of examples in the Rhoten report

well as Stokols and colleagues' synthesis of early findings from the NIH program of Transdisciplinary Tobacco Use Research Centers.

In addition, the kinds of problems and research questions being addressed, as Boix Mansilla and Gardner indicate, make a difference. Rhoten pays some attention to that factor and Stokols, et al. to the nature of the center as well. While heeding the wise caveat the authors sound – about not succumbing to the fallacy of a single set of criteria – comparison across these studies, I believe, has great potential for generating generic guidelines that can be used in conjunction with center-specific and problem/question-specific conditions of evaluation.

Obviously, then, this welcome study invites conversation. I would like to add, as an aside, that I hope that another word than "symptom" will be used as the study develops. The etymology of "symptom" does not carry the pathological connotation of disease we associate with the word today, but I would be more comfortable with a less negative term.

other interdisciplinary fields and symptoms
Veronica Boix Mansilla
Dec 3, 2003 20:16 UT

Julie is right in calling our attention to the potential lessons to be learned from unearthing the accumulated expertise about interdisciplinary knowledge validation embedded in interdisciplinary fields such as European or American studies, Women studies, and the like. In our study we did not focus on these and I am intrigued by the possibility of additional criteria stemming from their approaches to interdisciplinarity. Our closest experience with established interdisciplinary fields was our examination of the Center for Bioethics at U Penn. Researchers there exhibited a militant capacity to collaborate—e.g., they were vocal advocates of group-authorship. Curiously, unlike their counterparts in other centers for whom the integration of disciplines happens more seamlessly, researchers at CB were crisply aware of the disciplinary lenses that they brought to their shared commitment to Bioethics. In their discourse, images of "generative tension", "checks and balances", "keeping each other in check" abounded, as qualities that they viewed as assets not liabilities. In this sense CB is probably not representative of more seamlessly integrated established interdisciplinary centers and fields. What additional assessment criteria might then stem from such fluidly integrated traditions?

On Julie's footnote: I see what Julie means by the negative connotation of "symptoms" In opting for "symptoms" as our descriptor for the qualities that we have identified, we have shown our true Goodmanian colors. Nelson Goodman, a philosopher of great influence in our thinking and the founder of Project Zero (the research institute from where we work) set to the task of identifying qualities of the aesthetic that would advance our understanding of artistic cognition—a drastically ill-studied problem at the time. He proposed "symptoms" of the aesthetic as qualities that were not all necessary, nor individually sufficient, to distinguish an aesthetic from a non-aesthetic experience. Symptoms, he claimed, tended to be present in aesthetic ones in varying degrees. I find his choice of words felicitous because it allows for a more complex consideration and weighing of qualities of a piece of –in our case-interdisciplinary work. It invites us to avoid neat formulas or algorithmic criteria. Of course, this raises a more substantive question for our discussion: Is the definition of crisp (quantifiable?) criteria a worthwhile pursuit when it comes to the epistemic evaluation of interdisciplinary work? And if not, how can we best capture quality in a way that is informative but does not undermine the dynamic complexity of this kind of work?

Nelson Goodman (1976), Languages of Art, Hackett Publishing (Indianapolis).

Core principles
Gloria Origgi
Dec 11, 2003 11:14 UT

The reference to Nelson Goodman's symptoms of the aesthetic makes me think of an interdisciplinary workshop on Art and Cognition that Noga Arikha and myself have organized on this web site at http://www.interdisciplines.org/artcog

The workshop has been very stimulating and also gave us an example of the difficulty to find a dialogue between art criticism, aesthetic and cognitive science: people coming from the world of art do not accept a discussion about art that doesn't primarily focus on the notion of "artistic value", whereas most of the works in cognitive science suggests a "continuity hypothesis" between non artistic and artistic experience (which makes the task of finding some cognitive basis of artistic perception more realistic).

A workshop is just an exchange of ideas and not a research program: but still, one needs to find some common epistemic criteria in order to share ideas and points of view. On that occasion, I had the feeling of an irreducible divergence of objectives between the two positions that made the discussion quite tensed sometimes. As if we touched to some "core principles" that a discipline cannot put into question without losing its identity.

Have you perceived the same attachment to some "core principles" in your investigation or in your personal interdisciplinary research?

▽A good example
Veronica Boix Mansilla
Dec 12, 2003 5:27 UT

Goodman's "symptoms" of the aesthetic are interesting examples to explore, Gloria. They illustrate two of the assessment criteria we examined— i.e., coherence with multiple disciplinary antecedents and effective advancement of understanding.

In the budding years of the "cognitive revolution," the explanation of the aesthetic experience had arguably been co-opted by psychodynamic analyses focused on emotion which contributed to a broadly shared sense of incommensurability between the arts and other "ways of knowing". A cognitive analysis of the arts, many feared, would violate the central intangible "magic" of the artistic experience—a belief often also held about cognitive studies of creativity. Similarly, in defending "artistic value" as a non-negotiable part-taker in an acceptable integration of art and cognition, the workshop participants you describe argue for the uniqueness of the artistic experience, thus imposing important (yet productive I would argue) constraints on the attempted integration.

In contrast, embracing a "continuity hypothesis", if I understand your use of the expression correctly, contemporary studies that apply information processing and thinking skills categories to the arts, see no fundamental difference associated to intellectual experiences across domains. Their bridging cognitive categories may lead them to view the arts as "one more case" of cognitive activity (e.g., analysis, synthesis, hypothesis testing) roughly following patterns that are present across domains. At the same time, theese individuals may (correctly I believe) seek empirically testable explanations of the mind at work.

Standing at the intersection of the arts and epistemology, Goodman's "symptoms" represent a productive interdisciplinary integration. For instance, exemplification, addresses how a work of art symbolizes by serving as an example of certain properties -- e.g., a poem works aesthetically not simply by "referring to" a mood but by "showing" it, so to speak. Multiple and complex reference, another symptom, captures how symbols operate with several referential functions—another prominent trait of the aesthetic experience. With a focus on how symbol systems work in the arts, Goodman's work articulates what were viewed as disparate irreconcilable realms, embracing both "aesthetic value" as an art specific core category and the explanatory aims of epistemology. This line of work invites a reframing of the question "What is art?" Toward a more functional explanation of "how do symbols work aesthetically?" in other words, "When is art?"

Criteria of Learning Assessment in Interdisciplinary Fields
Julie Klein
Dec 20, 2003 15:53 UT

I'd like to pick up on Veronica's comment about potential lessons from the expertise embedded in interdisciplinary fields, while inserting the topic of education more into our discussions. Despite the lack of extensive work on interdisciplinary learning assessment, the topic has been addressed in women's studies.

Students at the Center: Feminist Assessment* contains results of a national study, with sample instruments and design models. Assessment in women's studies is rooted a dialogue of content knowledge and learning process. Content is comprised of both pertinent disciplinary knowledge and feminist concepts, methods, and theory. Feminist assessment is also student-centered, participatory, and sensitive to dynamics of collaborative learning in local contexts and cultures. Because feminist pedagogy is shaped by feminist philosophy and informed by relationships between teachers and students, a variety of quantitative and qualitative approaches are used. Quantitative measures include analysis of enrollment figures and statistical data, plus questionnaires. Qualitative methods include participant observation and textual analysis. Feminist ethnography entails close examination of data such as transcripts, observations of class discussions, and interviews with teachers and students. Illuminative evaluation, which encompasses ethnographic and phenomenological modes, measures the success or failure of innovative projects. Portfolios evaluate progress over time.

The Human Development and Social Relations Program (HDSR) at Earlham College is based in a different field -- human development and sociocultural systems. Yet, there are parallels. Faculty from sociology/anthropology, psychology, philosophy, and education cooperate in offering a core sequence of psychology and social anthropology courses that introduce theories, methods, and empirical data in disciplines related to specific problem areas. Students deepen their understanding of a particular discipline or content area at the upper-division, and they can align courses with personal interests, career goals, or graduate study. The program culminates in a senior seminar, and students do field study in a domestic or international setting. The designated learning outcomes in HDSR resemble many general education programs, including development of sensitivity to values and ethical judgments, and the ability to manage ambiguities, contradictions, and congruencies. Multiple instruments are used to assess learning, including comprehensive exams, field-study reports, a problem analysis, and a reflective essay. The program also conducts an alumni survey and does longitudinal tracking of employment and graduate-school admissions. Information from these tools supplies feedback for improving the curriculum. Added information comes from course evaluations, curriculum development/assessment workshops, steering committee meetings, brainstorming, and reflections in Senior Seminar and anecdotal information from current students and graduates.

"Consistency "with antecedent disciplinary knowledge and "Balance" in weaving together perspectives are important. So is "Effectiveness" in the epistemological advance of understanding and pragmatic changes in the lives of people in concrete settings. In both cases, though, an added dynamic is present. The measure of quality also lies in how well strategic knowledge and learning process are combined with antecedent domain knowledge that includes both disciplines and field-specific body of concepts, theories, methods, and tools.

* C. M. Musil (Ed). Students at the Center. Washington, D.C.: Association of American Colleges and National Women's Studies Association, 1992

The role of "strategy"
Veronica Boix Mansilla
Dec 31, 2003 4:43 UT

I thank Julie for suggesting a possible expansion of the criteria proposed in our paper. She claims that in assessing " 'effectiveness' in the epistemological advancement of understanding and pragmatic changes in the lives of people in concrete settings" one might want to consider how well " 'strategic knowledge' and 'learning process' are combined with antecedent domain

knowledge [disciplinary and field-based]". While a focus on "learning processes" seems more uniquely relevant to educational practice and only tangentially related to the evaluation of expert research outcomes, "strategic knowledge" could add an important dimension to our understanding of expert interdisciplinary work and its validation at the frontier.

Our interviewees illustrated various approaches to interdisciplinary work – e.g., some geared to producing explanatory theories and descriptive accounts (typically expressed in academic publications), others seeking to contribute to the practical solution of pressing medical and social problems (embodied in products and recommendations for action, in addition to publications). Distinct research purposes demanded tailored research designs and, not surprisingly, distinct characterizations of "effectiveness" (e.g., comparable explanatory advantage in the first case, and "viability", "usefulness" and "potential impact" in the second). Transdisciplinary research -- a brand of work that stands beyond the scope of our study- takes the pragmatic approach further. It seeks to reach practical solutions to social, medical, and environmental problems, integrating multiple disciplinary views as well as other non-disciplinary perspectives and interests. Action-research projects as well as Habermas' notion of transformative knowledge interest sometimes illustrate this approach.

Given the centrality of practical action in transdisciplinary work, a criterion such as "strategic strength" (e.g., the ability to work with multiple actors, organize practice, distinguish more and less favorable paths for collaborative pursuits) may be viewed as "epistemically relevant" to projects of this kind, in part because the "knowledge" being evaluated is enacted in practical transformation----(as opposed to in an explanatory Theory).

"Transdisciplinary" initiatives take place in what Joseph Schwab called "eclectic domains," where disciplinary knowledge only partially satisfies the knowledge requirement for effective action.

on assessment of student work
Veronica Boix Mansilla
Dec 31, 2003 4:53 UT

Assessment of student interdisciplinary work is another only partially understood challenge in interdisciplinary theory and practice. In our project we have focused on the assessment of students' interdisciplinary work by interviewing close to 70 faculty in interdisciplinary programs at Stanford, Swarthmore, San Francisco State University, and the University of Pennsylvania.

When asked about the assessment of their students' interdisciplinary work, faculty systematically referred to the means by which they gathered information — their claims often echoed Julie's account of how area studies and centers assess. The most innovative faculty used multiple sources of evidence tailored to the courses at hand. They used student portfolios, journals, and performances in which students are invited to use knowledge in various disciplines to create something new (as opposed to reproducing information as a sign of their cultural literacy). The practice of these forms of assessment is not new and its effectiveness has been well documented in pre-collegiate education.

Faculty expressed a great deal of uncertainty, however, when it came to describing the "what" of interdisciplinary understanding. "What are the qualities of student interdisciplinary work that you value when you assess these pieces of work?" we probed. Their responses (e.g., well written, timely, effortful) tended to focus on generic qualities of work. Only after considerable probing did some faculty engage in more epistemic depictions of the pieces under examination.

While no faculty revealed a systematic epistemic framework to assess interdisciplinary work, three core criteria seemed to capture the qualities of integration that they valued. They emphasized "multiple disciplinary grounding"-- often referring to the selection and application of disciplinary knowledge from multiple sources. They highlighted "reflectiveness"— i.e. students' clarity about the meaning, purposes and limitations of their integrative work. They addressed what we came to refer to as "integration leverage" -- the degree to which a particular piece of

work has contributed to the advancement of student understanding in a way that a single disciplinary approach could not.

Perhaps not surprisingly, the epistemic criteria used by faculty to examine interdisciplinary student work echo criteria used by experts in their research at the frontiers. While we have not yet conducted a systematic comparative analysis of these two data sets (teaching faculty-expert researchers), we have observed important differences between them. For example, faculty were more inclined to emphasize disciplinary accuracy and personal meaning than their expert counterparts. This was not surprising since student "disciplinary" understanding and the development of richer worldviews were often important learning goals in their courses.

Education Versus Research
Julie Klein
Jan 3, 2004 14:38 UT

I agree with Veronica that a forthright focus on learning processes would seem more uniquely relevant to education practice, though several studies of inter- and transdisciplinary research underscore the importance of a reflexive posture that is attentive to learning process, for individuals and groups alike. (Call it cognitive growth, if you like.)

Jack Spaapen and his colleagues at Wageninen, cited in the Bibliography thread, also stress the importance of generative process, underscoring emergent criteria of quality and value that are intrinsic to the character of the work.

As for transdisciplinary research, I don't consider it entirely outside the scope of studies of interdisciplinary research, because a cross-disciplinary engagement of separate research traditions is still occurring. The added engagement of stakeholders complexifies the question of evaluation. Yet, the integrity and quality of interdisciplinary exchange and integration are still part of the mix. The criterion of "strategic strength" that Veronica suggests makes good sense here in both respects, inter- and transdisciplinary.

I'd add a final analogy to the discussion of appropriate criteria in education. Until recently, there were no clear guidelines for assessing interdisciplinary learning and program review. The logic that is emerging from recent work, though, underscores the gap between traditional assumptions about quality and the demands of interdisciplinary learning. The cornerstone of traditional thinking is measurement of progress toward clear and operationally-defined learning outcomes. Knowledge of content and concepts is theoretically easy to evaluate if there is wide agreement. Interdisciplinary curricula, however, tend to be unique (just as interdisciplinary research projects are often sui generis). No standard model supplies a universal index. More than one discipline is involved, with sometimes conflicting assumptions about criteria. Contexts of practice differ, complex learning outcomes in interdisciplinary classrooms do not match up with standardized tests (or research outcomes with proxy criteria), and goals are sometimes combined in a way that makes analytic and reductive measures difficult to implement. A narrow scientific, experimental mode of evaluation fails to capture the intellectual multiplicity, discovery-orientation, diffuse skills, multiple outcomes, and complex goals that are typical in both interdisciplinary education and research.

Education versus Research, Round 2
Julie Klein
Jan 3, 2004 15:02 UT

Veronica's report on assessment of students' interdisciplinary work was quite interesting, because it parallels my own findings in looking at the question across K-12 (primary and secondary) and post-secondary education.

In earlier studies, unfortunately, the greater weight has fallen on how students work with disciplines without enough attention to interdisciplinary process as more than just mixing together existing disciplinary knowledge. The "discipline" in interdisciplinarity is important. Yet, more is involved.

I was struck by how far ahead of their college counterparts the K-12 teachers were in articulating appropriate criteria, though I found the same tendency to speak of generic qualities that Veronica found (and in K-12 of school management and pupil development issues that don't loom large for their college counterparts). When pushed to consider epistemic issues, teachers in the studies I examined did have disciplinary grounding on their mental lists, as well as reflectiveness. I did not find talk of "integration leverage," but the concept is useful, because it pushes the discussion of appropriate criteria beyond disciplinarity to the production of something novel – returning us to parallels in research, as Veronica suggests. I'm eager to see systematic comparative analysis of the two data sets (teaching faculty-expert researchers), to see both differences and the similarities in contexts.

As the number of studies grow in both education and research, I think it important to be asking as well how much new knowledge domains are factoring in, not just traditional disciplines.

Looking at expert work to inform student learning and assessment.
Veronica Boix Mansilla
Jan 4, 2004 19:42 UT

It was a pleasure to learn about Julie's convergent observations at the K-12 level. Not surprisingly, while teachers at the pre-collegiate level often face the challenge of enhancing their own understanding of the content and modes of thinking central to the disciplines and areas that they teach, the do tend to be quite attentive to matters of pedagogy. At this time, using an action research approach, we are working closely with twelve exemplary high school teachers in Massachusetts in the development of a series of experimental interdisciplinary units of instruction. Our teachers were selected for their uniquely strong training in at least one discipline and their extensive experience teaching in interdisciplinary ways. Collaborating in the development of interdisciplinary approaches to the teaching of "globalization" for students in the late-high school and early-college years, is allowing us to see (and experience) their challenges and the strategies that teachers devise to address them.

An interesting common quality across these teachers is relevant to our discussion of assessment of student work. Our teachers seem to share a disposition toward close analysis of expert work. For example, to conclude a course on world history focused on Genocide, a teacher at Boston Latin High School invites her students to create a memorial for one of the Genocides studied during the year. Their memorial project begins with a close analysis of current memorials around the world, and the critiques that they have received. In addition, students examine the criteria set forth in memorial competitions –e.g., Ground Zero in New York City. Close analysis of multiple cases of expert work allows students to develop a sense of parameters for excellence associated to this particular genre of work that brings together history, memory, art, and urban planning.

Looking together at models of expert integration in a particular genre or emerging domain is a productive rule of thumb that educators can use to guide the development of local criteria at multiple levels of instruction. This approach may inform assessment of student work that integrates traditional disciplines as well as emerging domains of study.

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Julie Klein
Dec 2, 2003 17:41 UT

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A few more resources
Veronica Boix Mansilla
Dec 10, 2003 2:50 UT

Thanks to Julie for beginning this relevant thread. Given the limited empirical work in the area of interdisciplinary research evaluation we too look forward to learning about other initiatives.

Some additional interesting pieces might include:

1. Joshua Guetzcow, Michele Lamont, and Gregoire Mallard. (2003). What is Originality in the Humanities and the Social Sciences? Paper presented at the American Sociological Association 2003 Annual Meeting. An examination of peer review in multi disciplinary contexts.

2. Knorr-Cetina, Karin. (1999). Epistemic Cultures How The Sciences Make Knowledge Cambridge. MA: Harvard University Press.

3. Braxton, John M. and Lowell L Hargens. (1996). Variation Among Academic Disciplines: Analytical Frameworks and Research. In Higher Education Handbook of Theory and Research. J. C. Smart Ed.

The “Art” of Interdisciplinarity and Another Resource
Julie Klein
Dec 20, 2003 14:32 UT

Thanks, Veronica, for the additional resources. I want to add another that calls to mind your earlier remarks about Goodman. It’s Danielle Boutet’s “Interdisciplinarity in the Arts,” Harbour, (1993): 66-72. I came across this reference in Jill Vickers’s (“'[U]framed in open, unmapped fields”: Teaching and the Practice of Interdisciplinarity.” Arachne: An Interdisciplinary Journal of the Humanities [1997] 4, 2: 11-42.) Vickers stressed the importance of disciplinary rigor in the past but expanded her criteria of assessing quality here.

When interdisciplinarity in Canadian studies was simpler, Vickers noted, a "respectable" account of a borrowing meant understanding it well enough in its original context that a disciplinary practitioner would be satisfied. Currently, more students are entering interdisciplinary programs with no training in a disciplinary method, and some contend knowledge of a "donor discipline" is unnecessary when its cognitive map is being read "against the grain." In open cross-roads fields, two forces may be at work: an "integrative" tendency, evident in Canadian studies as area studies, and a self-asserting "disintegrating tendency" that draws the focus away from the center of existing knowledge systems, evident in critical, oppositional or self-studies.

Students and faculty choosing projects with conflicting understandings of evidence encounter several problems. They may prioritize one discipline or field. They may have difficulty integrating knowledge from an epistemologically "softer" field into a "harder" one. In transdisciplinary or anti-disciplinary movements, they may reject epistemological claims of disciplines altogether, preferring alternative understandings of "knowledge" and "evidence." Yet, they may be uncertain how to make knowledge claims, other than the ground of life experience. Complications arise even in recognized disciplines. Literature and history have undergone so much change that characterizing them as "stable" disciplinary matrices is problematic. Bridging certain practices of a discipline or even two disciplines with compatible epistemologies can be as difficult as bridging disparate fields. In some disciplines, evidentiary protocols are also in dispute.

Boutet's notion of artistry, Vickers suggests, provides a model for teaching interdisciplinarity in an open field. Boutet conceives of interdisciplinarity as a process that begins with knowledgeable borrowing from different disciplines. During the generative process, an artist unbinds tools, techniques, methods, generative theories, and materials from disciplinary packages. The working context is not supplied by the disciplines, rather the goals and frameworks an artist creates to mediate the interaction of components. Students in an open field that is not dictated by disciplines are in a similar position. Their research should be evaluated in its own right, on the ground of generative process and explanation/legitimation processes for the new conceptual frame that mediates interaction of elements. Artists are not usually required to weigh evidence and proof for a piece of creative work. In academic work, though, it is still necessary to become familiar with the languages of the disciplines and fields in question. The crucial skills, Vickers emphasizes, are knowing how to select among pertinent tools, mediums, and theories within disciplinary packages, and knowing how to design one's own goals and the tools needed to communicate in their working contexts.

Reservations about 'consistency' and 'balance'
Dan Sperber
Dec 2, 2003 18:38 UT

Some interdisciplinary work results in challenges—-sometimes radical ones-—to one or several of the disciplines involved, and it may do so in order to effectively advance understanding. For instance the approach to culture drawing heavily on developmental and evolutionary cognitive psychology pursued by Scott Atran, Pascal Boyer, Lawrence Hirschfeld and myself challenges standard tenets of anthropology (What Leda Cosmides and John Tooby described as the "Standard Social Science Model") and is seen as misguided and untenable by most mainstream anthropologists. We would defend it by arguing that it is quite effective in advancing our understanding of cultural phenomena in a way that is well understood by cognitive psychologists and should be understood by anthropologists. To this, it could be answered that we cannot be both judges and parties—-fair enough—-, and that the judges have to be recognized authorities in the disciplines from which we draw and to which we claim to contribute—-less obvious. Would this mean that, if we fail to convince most anthropologists of the value of our work, then it does not meet sensible criteria of quality? Or should we distinguish 'effectiveness in advancing understanding,' which is what defines good research, from 'consistency with multiple separate disciplinary antecedents' and 'balance in weaving together perspectives,' which although *ceteris paribus* desirable properties are indeed mere symptoms of quality, the presence or the absence of which may, on occasion, be misleading. In fact, high-quality ground-breaking interdisciplinary research (of the kind that may lead to the redrawing of disciplinary boundaries) is unlikely to exhibit these symptoms of consistency and balance.

Back to Definition
Julie Klein
Dec 2, 2003 18:55 UT

One of the complications that Dan rightly adds to the mix is that novelty, especially of the boundary-crossing kind, often has the intent of "advancing understanding" by undermining current understanding with its attendant paradigms and assumptions about criteria of quality. To be "consistent" with antecedents, then, can be to betray the very character of interdisciplinary work. Other examples, from molecular biology to feminist theory, come to mind. All the more

reason, as Gloria was urging the end of last month, in the forum on Dominique Pestre's paper, that we be clear about the definition we are assuming.

Interdisciplinarity and innovation
Gloria Origgi
Dec 2, 2003 22:25 UT

Although interdisciplinarity interests us because most of the time it is the only way to innovate research and knowledge, I think that we should keep separate the two ideas of interdisciplinary research and scientific innovation.

Dan raises a point that is not specific of interdisciplinary research. Any research program that determines a "paradigm shift" even inside a discipline has problems in being acknowledged and assessed. I don't know if Galileo was interdisciplinary. Surely he was a great innovator in physics, and the lack of any possible consistency between his views and the previous state of astronomy caused him a lot of trouble.

More generally, for at least two of the three criteria, that is "consistency" and "effectiveness", I have a problem in viewing them as belonging to a specific epistemology of interdisciplinarity. It seems to me that every discipline counts among its criteria a reasonable fit with antecedent knowledge and success in explaining some new phenomena.

"Balance" seems more specific of interdisciplinary research. In Dan's example of anthropology and psychology, a lack of balance among disciplines may cause a "migration" of a new research project (for example cognitive anthropology) from the Antropology Departments to the Psychology or Cognitive Science Departments. And if no methods, insights and presuppositions are shared by the new research group and the old disciplines, may be it is time to create a new, autonomous discipline.

Consistency with multiple disciplinary antecedents
Veronica Boix Mansilla
Dec 3, 2003 20:12 UT

The challenge that Dan describes—advancing understanding of culture (in his case) in ways that go against standard tenets in our disciplines of origin (e.g., S.S.S. model in Anthropology) -- was not uncommon among the researchers we interviewed. Indeed, a great number of our interviewees expressed frustration with the skeptic responses that they received from colleagues in their disciplines of origin. In a way, as Gloria points out, this seems a matter of the challenges of innovation of the paradigmatic kind -disciplinary or not. What made our interviewee's challenges unique to interdisciplinary research was the fact that they had opted for various disciplinary frames of reference selecting in each case methodological tools and insights that allowed them to advance understanding. In "borrowing" such epistemic tools from disciplines that were not their own, they typically insisted on the need to "get them right" and they relied on their colleagues to help them do so. When referring to the radical challenges that their findings and approaches posed to their disciplines of origin, their arguments emphasized, as Dan's does, the "advancing understanding" criterion. In other words, "all things considered" they felt justified (and inspired by insights validated elsewhere) to challenge disciplinary dogma.

Is Dan's complaint really about disciplinary resistance?
Steve Fuller
Dec 4, 2003 14:52 UT

To return to Dan's example of his fellow anthropologists not respecting evolutionary cognitive psychology: There is a specific and more general point to be made about this example.

First, I think you don't do justice to the nature of the objections you're facing. Like a lot of other beleaguered groups, you detach the cognitive from the social side from the establishment's complaints and focus purely on the social. This suggests that the complaints are more

unreasonable than perhaps they really are. In particular, you make it look like it's simply a matter of cross-disciplinary barriers: anthropologists can't understand what you're doing. I really think the objection is a bit deeper and can be recognized even by people who are not trained as anthropologists. The problem is less with the explanations you provide than with the way you gather the data to constitute phenomena worth explaining. For example, evolutionary cognitive psychology accounts of religion tend to lump together rather disparate phenomena that bear a superficial resemblance to each other (especially if one looks at the social conditions that generate and maintain them) and then offer a rather general broadly adaptationist explanation that is several levels removed from the phenomena. On ordinary grounds of scientific method, there are reasons for doubts here. The example of religion is especially pertinent because no one has ever seriously proposed that this concept could be the basis for anything approaching a 'social kind'. In fact, if you look at the sorts of things that are typically called religions, they are more defined by what they are NOT than what they share in common. So at best, it looks like you're trying to explain certain carefully selected aspects of religion – ones to which your explanatory model is well suited – not the phenomena as they are normally understood.

Second, the more general point starts from here: If you're really trying to change the subject and not address the subject as it is normally understood, then why should you care that anthropologists don't like your work? I suppose a good answer might be that they control your research budget. But isn't the grand strategy of evolutionary cognitive psychology to break away from disciplinary constraints and criteria – by developing your own autonomous funding bases (public and private) and recruiting the next generation of biosocial scientists who find this stuff exciting and have no particular commitment – or even knowledge – of traditional social science? Here I would have expected some sociological thinking, rather than appeals to nebulous second-order cognitive criteria of 'effectiveness in advancing understanding'.

Reply to Steve Fuller
Dan Sperber
Dec 4, 2003 16:49 UT

Steve is suggesting that the kind of interdisciplinary approach to culture I was giving as an example might be rejected by mainstream social scientists for good scientific reasons. This is indeed a possibility worth considering. However the good reasons could not be those sketched by Steve: neither Boyer nor Atran in their recent books on religion, nor I in my earlier more programmatic writings on the topic take religion to be a 'social kind'. On the contrary, we have all three argued against such a view, and we use 'religion', as do most serious social scientists, as a way to point at a variety of related phenomena exhibiting great intra and cross-cultural variety. Moreover we have all three argued at length against adaptationist explanations of religion (and, by the way, standard social science functionalist explanation of religion are, unlike ours, 'broadly adaptationist').

Steve's more general point ("If you're really trying to change the subject and not address the subject as it is normally understood, then why should you care that anthropologists don't like your work?") and suggestion that we are trying to recruit people who don't care or even know much about the social science agenda migh be a caricature (with the element of truth involved in caricature) of standard evolutionary psychologists (those who publish, for instance in *Evolution and Human Behavior*), but does not apply, not even as a caricature, to the people I was mentionning: Atran, Boyer, Hirschfeld and myself are all professional anthropologists with traditional fieldwork experience, and our aim is to address in a novel and insightful manner some of the central problems of the social sciences. As for the suggestion that we are building our "own autonomous funding bases (public and private)," I wish it were tue. Any tip on to how to proceed welcome!

Anyhow, this is not the place and time to discuss seriously this particular approach to culture in general and religion in particular. I was mentioning this example not on the assumption that its scientific merit are uncontroversial, but to illustrate the following question: should interdisciplinary researchers doubt the quality of their work just on the ground that it is widely rejected in one or another of the disciplines concerned? Julie's, Gloria's, and Veronica's replies to my posting confort me in thinking that the answer is 'no'. Which—need I say it?—does not mean that disciplinary rejection is desirable or a symptom of quality either.

Reply to Dan -- where's the news?
Steve Fuller
Dec 6, 2003 15:56 UT

Dan's response is interesting, not least because he deftly manages to avoid all the tricky questions. In fact, someone might even be left with the impression that his approach to religion doesn't differ very much from a standard social science approach at all, except for some stray metaphorical associations with Darwinism and epidemiology. But of course, this is not the place to argue about this matter.

And of course, I was not accusing your senior collaborators of not knowing social science. I was just talking about the younger generation.

Finally, it's hard to believe – though nothing much hangs on this – that it took an article from someone in the Harvard School of Education to persuade you that your interdisciplinary work might have value even if fellow disciplinarians don't like it. I would have thought that this is pretty obvious. The real issue is whether it's worth persisting with research that is unlikely to get appropriately recognized for a long time. Here the career structure of the people involved plays a big role: e.g. do they have the freedom to move between fields, do they have access to the relevant material resources, etc.? And as far as quality control is concerned, once you have established peer review mechanisms – via journals, etc. – for your own work that non-peers trust as authoritative in that area, isn't that legitimacy enough?

Où est la maladie sociale?
Abdelkarim Fourati
Dec 9, 2003 9:44 UT

Durkheim disait, dans son 'livre Les règles de la méthode sociologique': "Le sociologue doit porter sur la société le regard d'un médecin capable d'en discerner les maladies ou les dysfonctionnements éventuels: le devoir du sociologue n'est plus de pousser violemment les sociétés vers un idéal qui lui paraît séduisant, mais son rôle est celui du médecin: il prévient l'éclosion des maladies par une bonne hygiène et, quand elles sont déclarées, il cherche à les guérir".

Nos deux auteurs de ce texte nous parlent d'une recherche empirique des 'symptômes de qualité' pour évaluer le travail interdisciplinaire aux frontières. Or, en médecine le mot "symptôme" désigne un signe qui révèle un trouble fonctionnel ou une lésion; et par similarité, on parle en sciences sociales, par exemple, des symptômes d'une crise économique… En fait, nos deux auteurs ont fait une recherche empirique sur la recherche (une méta-recherche): une enquête épistémologique à la recherche d'une "maladie sociale" et d'une thérapie adéquates. Mais d'après la lecture de leur texte, il me paraît qu'ils ont confondu entre symptômes (éléments de diagnostic) et éléments de thérapeutique ou de prévention. En effet, ce qu'ils appellent « les trois principaux "symptômes" épistémiques de qualité du travail interdisciplinaire qui émergent de notre analyse: cohérence, équilibre, et efficacité » au début du texte, ils les présentent (après une rectification minime) à la fin du texte comme remède.

D'après ce que j'ai compris du texte présenté, la maladie des organisations de recherche est "le manque de critères épistémiques disponibles pour évaluer le travail interdisciplinaire". Alors que pour faire le diagnostic de cette maladie, on a ses symptômes (révélant un trouble fonctionnel sociologique): 1- l'utilisation des indicateurs de qualité indirects; 2- les mesures concernant directement les dimensions épistémiques du travail interdisciplinaire s'avèrent plus rares et moins bien articulées. Enfin, on cherche l'étiologie (la cause dans le jargon médical) de cette maladie sociale qui est, d'après l'enquête faite, l'excès d'autorité de la personne du chercheur qui a trouvé quelque chose de valable. En effet, les chercheurs interviewés ont désigné des indicateurs tels que le nombre de brevets validés, de publications, de systèmes et de mentions issus de leur travail; le prestige des universités, des agences de financement et des revues dans lesquels il est placé; et l'approbation de leurs pairs et d'une communauté plus large… On a souvent critiqué ces manières d'évaluation "par procuration"...

Comme en médecine, une fois posé le diagnostic et trouvée la cause il reste le traitement qui est ici la libération de l'autorité absolue de la personnalité du chercheur et/ou du prestige des institutions de

recherche. Autrement dit, c'est la démocratisation de la recherche (nous revenons ainsi au jargon des politiciens de démocratie; voir aussi mes interventions dans les discussions des textes précédents, en particulier, en ce qui concerne la notion de "frontières disciplinaires"…). Ici vient l'importance des trois critères épistémiques dégagés par le travail des deux auteurs. Ces critères me paraissent valables parce qu'ils se basent sur des principes généraux, en dehors de tout pouvoir autoritaire extra épistémologique…

La maladie
Veronica Boix Mansilla
Dec 15, 2003 3:11 UT

Abdelkarim Fourati's comment on the democratizing power of epistemological criteria to assess interdisciplinary work is illuminating. Yes, there are multiple challenges associated with the selection of interdisciplinary work for publication--lack of clearly defined venues, lack of adequately prepared peers to review the work, over-reliance on personal or institutional prestige-- all related to ill-defined assessment criteria.

An ill-prepared review system raises larger ethical questions: Are we leaving important insights unpublished due to insufficient understanding of their interdisciplinary nature on the part of reviewers? What are the consequences of doing so for individuals and their fields of study? Confronted with a system of peer review that is trying to come to grips with this mode of knowledge production, researchers in our study seemed to follow courses of action involving

(a) selecting less prestigious publication channels --which are often more willing to take risks; (b) communicating their findings through non-peer review venues –e.g., edited books. (c) educating their editors and reviewers about the particular merits of the findings proposed—which often included describing the disciplinary sources of the claims made.

Items (a) and (b) above are detrimental to interdisciplinary researchers' careers—and sometimes to the problems they study. As a matter of fact, several of our interviewees described interdisciplinary research as a luxury of seniority—i.e., once individuals are established in one discipline they can "afford to" take risks across domains. Arguably more constructive, item (c) above, still puts the burden of proof (and peer education) on researchers' shoulders.

Against this background, greater clarity about the qualities that render a piece of interdisciplinary work acceptable might not only contribute to a reasoned dialogue among reviewers and researchers but also enhance the likelihood that, as Abdelkarim Fourati suggests, prestige (and disciplinary traditions) will not be the de facto compass for assessment.

Are there special criteria for assessing interdisciplinary work?
Grit Laudel
Dec 15, 2003 4:16 UT

I enjoyed reading the Mansilla/Gardner paper because the authors raised a clear and focused question and answered it with a systematic empirical study. After having read the paper, I would agree with Gloria Origgi that possibly there are no specific criteria for assessing interdisciplinary work, i.e. criteria that would not be used to assess disciplinary work. Any attempt to produce new knowledge requires some consistency with regard to the knowledge that has been used in the production, and any research project will be assessed as to whether it has achieved its aims. According to my own empirical studies, the main problem seems to be who is able to assess interdisciplinary research according to these criteria. No single scientist has enough knowledge to assess an interdisciplinary project. That is why the interviewees of Mansilla and Gardner spontaneously referred to 'second order' indicators of quality such as "number of accepted patents, publications, devices, and citations stemming from the work; the prestige of the universities, funding agencies, and journals in which it is placed". I observed a similar behaviour from reviewers when they had to judge proposals and admittedly lacked the necessary knowledge because it was work outside of their own specialty. This is in fact not peer review any more (anybody could conduct such an assessment), and the validity of this procedure is questionable. The interdisciplinary funding program I analysed applied a sophisticated

assessment procedure: 1. An assessment panel is formed that contains reviewers from all fields that are included (plus two 'remote observers' from other fields who watch out for compliance with general standards); 2. The peer review process is not anonymous; the applicants present their work and proposals to the assessors, and a scientific discussion takes place; 3. The reviewers take part in scientific meetings of the assessed scientists; 4. The same reviewers accompany the interdisciplinary work for a long time (more than 10 years). Thus, the reviewers attain the necessary competence and a communication base for interdisciplinary assessments. This procedure has been regarded as beneficial by the assessed scientists. There were very few decisions by the assessors that the affected scientists regarded as "disciplinary blunders". My point is: assessments of interdisciplinary work need special institutional rules of assessment rather than special criteria. They can be revealed by fine-grained studies of the peer review process itself, i.e. the (inter)actions of the assessors and the assessed. (Such rules won't work for all cases of interdisciplinary work; there will always be work where nobody will believe you other than yourself. Than there is only the 'Fuller solution': create your own specialty which will probably fail in most cases).

Evaluating Interdisciplinary Research Proposals
Julie Klein
Dec 20, 2003 16:43 UT

The discussion between Grit and Christophe raised a number of issues surrounding who is qualified to judge interdisciplinary research proposals. In the absence of clearly-defined criteria for (and even definitions of) interdisciplinary work at public and private funding agencies, peer review is often a matter of defaulting to discipline-based criteria. Grit expressed the problem well: "This is in fact not peer review any more."

Longitudinal involvement of the kind Grit described is ideal, generating not only competence but also a communication base that increases the number of people capable of conducting interdisciplinary evaluation with interdisciplinary rigor. The term "rigor" is usually associated with disciplinary criteria, though it is often a code word for enforcing boundaries. In the current heightened climate of support for interdisciplinary research, it is incumbent upon individuals working on issues of evaluation and assessment, as well as the parties applying for funding support for project, to inform the process.

In the latter case, a parallel exists in education. To borrow a phrase Dan used earlier, people seeking legitimation of interdisciplinary initiatives have had to be both "parties" and "judges" by educating their judges in the process of doing and presenting their work. The same must happen in research proposals. A number of recommendations for reforming the peer review system arose from reports on interdisciplinary research in the 1990s. Those recommendations need to be collected, updated, and disseminated more widely.

Another Model for Evaluation
Julie Klein
Dec 28, 2003 15:56 UT

In the absence of a recognized social system, local criteria often taken on greater importance. The arena of judgment also widens to include emergent traditions of interdisciplinary work and the quality of collaboration and of integration.

The work of Dan Stokols, and colleagues evaluating Transdisciplinary Tobacco Use Research Centers (TTURCs) is instructive for our discussion of both social and cognitive factors. They took a macro-level approach to evaluating processes and outcomes in TTURCs, with the aim of providing a comprehensive assessment of the initiative's functioning and outcomes. Their outcome map and corresponding logic model provide an empirically and collaborative derived conceptual framework for evaluation.

Stokols, et al. considered the information needs of a range of stakeholder groups, including institutes of cancer and drug abuse research, a private funder, host universities, public health researchers and practitioners, and TTURCs. Not surprisingly, they found differences among centers. Broadly speaking, though, concept mapping yielded an overview of outcome domains

that need to be addressed in the evaluation system. The map of relevant outcomes was then translated into a logic model that depicts the sequence and causal relationships of outcome constructs. Together the map and the model guided development of approaches to measurement.

Brainstorming among multiple players, including scientific consultants and representatives of funding agencies, generated 262 potential outcomes that were edited and condensed into 97 final outcome statements that were then sorted for similarity and rated for relative importance. Analyses of sorted data yielded an outcome map showing 13 clusters of the 97 statements. The map also revealed five general regions of clusters: Scientific Integration, Collaboration, Professional Validation, Communication, and Health Impacts. Temporality was suggested as well, with outcomes arranged along short-term immediate, intermediate, and long-term time frames.

The logic model derived from the cluster map of outcomes was also time-sensitive, depicting immediate, intermediate, and long-term markers along causal pathways from initial outcomes proximate to the TTURC initiative to more long-term, distal outcomes. The logic model begins with basic activities of centers (training, collaboration, and transdisciplinary integration) that represent core activities of the initiative and the earliest outcomes that may be expected. Basic activities lead to development of new and improved Methods, Science, and Models. Resulting Improved Interventions are tested and lead to Publications at several phases. Publications lead to Recognition and Transdisciplinary Research Institutionalization, which feeds back on the overall infrastructure and capacity of centers, resulting in increased support for Training, Collaboration, and Transdisciplinary Integration. Publications also provide a content base for Communication of Scientific Results to a broader community. Recognition, through public relations it engenders, provides a secondary impetus from communications and publications. Policy Implications result from Communications and Publications, while Translation to Practice is influenced by Improved Interventions, though there is a dynamic relationship between Translation to Practice and Policy Implications. Health Outcomes, in turn, are influenced by treatments and health practices that developed and by related policy changes, with positive or negative Health Outcomes feeding back into new polices and practices.

The citation appears in the Bibliography thread.

Objects and Domains of Assessment (part 1)
Rainer Kamber
Dec 29, 2003 9:22 UT

Grit has pointed out that any kind of (research) assessment is to be related to goals (even if interdisciplinarity is viewed as being more about processes: processes have properties that can be assessed). The traditional criteria fulfill this condition in that they are to further the epistemic qualities of the research results (wether they effectively do this is a different matter). The study of Boix-Mansilla/ Gardner (BM/G) sheds much light on the self-image of researchers in interdisciplinary fields and one is immediately stricken by the strong adherence to traditional patterns of evaluation that is manifested by most researchers surveyed. I suggest we look at this outcome in terms of goals of research.

In addition to what we can learn from Grit's case study, I believe that procedural rules aiming at organizing interdisciplinary research should not be confined to social, but be extended to cognitive rules/ instruments. Generally speaking, what we strive for in interdisciplinary work are processes of integration aiming at products of synthesis. But while all of the examples in the study by BM/G illustrate this, "integration" and "synthesis" can mean different things in different settings, set for different goals. Accordingly, we need to make clear what it is we are trying to assess in interdisciplinary research.

First, we may discriminate between (A) social and (B) cognitive integration performances and products of synthesis - and treat the two theoretically independent (e.g., methodological integration between historiography and physics may or may not presuppose certain forms of research cooperation; social cooperation between researchers of law and ethics may or may

not result in products of cognitive synthesis). While these basic discriminations already point to further research desiderata they also highlight the peculiarity and the potential of genuine cognitive and social cooperation in research, i.e., its often cited learning potentials. But this in turn implies that interdisciplinary research may indeed be promoted "for the sake of itself" in one sense. Universities, or national founding bodies etc. may genuinely be interested in furthering the according experience base among its researchers since such experience may contribute positively to the innovation potential of a national research base. Most programs mentioned in the study (and in this online conference) seem to illustrate such a strategic interest. My point is that these are obviously considerations that lie well beyond pure methodological/ epistemic, and second-order criteria for assessing research.

Put differently, interdisciplinary research will, on the one hand, always have to be exposed to the latter kinds of criteria. To my mind, the study by BM/G even furthers this point by showing that interdisciplinary researchers are relatively well-prepared to be thus assessed. But on the other hand, genuine interdisciplinary research (that is, research that actually features cognitive and/or social integration processes and/or products of synthesis) can contribute strategically to an academic or national research base. Regarding Julie's concerns about the role of interdisciplinarity in education it can be added that such a strategic role obviously needs to cover educational concerns. I believe that these are all areas of future research in science studies, studies in higher education, the history and the philosphy of science etc. (Iskender has discussed the importance of latter in his thread).

Objects and Domains of Assessment (part 2)
Rainer Kamber
Dec 29, 2003 9:42 UT

In the preceding part of my contribution I have first argued for discriminating between social and cognitive integration processes and products of synthesis. If we accept the function of both kinds of integration and synthesis this can be seen to imply that inter- and transdisciplinary research can have, at least in one sense, an important structural role in the development of science. This is, of course, an interesting research question in itself. If it is an accurate perception (it is certainly widespread among interdisciplinarians), then it points to considerations that will not be assessable via epistemic/ methodological criteria.

The problem of interdisciplinarity is thus also one about usefully organizing research (i.e. setting research goals that can be assessed) in a context of (1) relatively well-definded (and only partially specialty-related) epistemic/ methodological criteria (plus criteria directly derivative upon them) and (2) basic desiderata about the furthering of innovation bases in research as well as in education. This is, of course, no news. But taken seriously it can lead to a further, tripartite discrimination that may serve in contributing to define future goals of research on inter- and transdisciplinarity. Research can be organized with three basic goals in mind and the organizing principles to be researched or assessed will obviously differ according to such goals.

First, there can be norms and policies regarding the activity of individual researchers regarding purely epistemic desiderata, the norms of knowledge production in very narrow sense (the domain of philosophy of science and the sociology of scientific knowledge); second, groups of individuals (e.g. research teams, or project management entities), need norms and policies in order for them to efficiently organize knowledge production - it used to be viewed as being derivative upon epistemic and methodological norms before the dawn od STS); finally, a third area of norms and policies concerns the ways society wants to organize science in a more general sense (e.g. regarding the innovation base in research, or problem choice, risk assessment, etc.).

Any assessment could thus be focussed on a type of integration process or product of synthesis in several domains of norms and policies. Further, the application of these discriminations between social and cognitive integration or synthesis and between different domains of norms and policies regarding research could help sharpen the formulation of further research questions regarding inter- (and trans-) disciplinarity and especially the ones regarding its assessment.

Proxy criteria
Christophe Heintz
Dec 16, 2003 11:42 UT

In this paper, the authors recount the skepticism of interdisciplinary researchers with regard to the capacity of "proxy criteria" to assess their research. The proxy criteria referred to are: "number of accepted patents, publications, devices, and citations stemming from the work; the prestige of the universities, funding agencies, and journals in which it is placed; and the approval of peers and a broader community".

I wonder how to interpret and explain this fact. The proxy criteria are common and essential tools in the current practice of science. They allow research managers, but also researchers, to pick up the relevant and authoritative work without having to check everything by themselves. Indeed, checking everything by oneself - i.e. applying what the authors called "more primary or epistemic measures of acceptability" - is not possible in current science. This fact has been pointed out by social epistemologists, who conclude that trusting is essential to scientific knowledge production. Yet, scientists do NOT trust anybody: they use the aforementioned "proxy criteria" to orient themselves in the vast amount of research done. The "proxy criteria" determine authoritative sources of scientific knowledge and, consequently, the paper that are worth reading, the people that are worth employing, etc.

This raises two questions:
1. Do interdisciplinary researchers rely on different trusting practices than the usual one above mentioned (i.e. trusting only authoritative sources, determined through proxy criteria)?
Not surprisingly, the study shows that they do use the proxy criteria. How can they do so if they really doubt of their validity?
2. Maybe they rely on proxy criteria, but think these cannot apply to them. But that doesn't seem really fair! How do they solve this dilemma (either don't use proxy criteria, and loose one of the most fruitful scientific practice, or use it, but then acknowledge its reliability)?
The authors hint that the proxy criteria are disciplinary biased. Why is that so? Since when coming back to the more "primary or epistemic measures", one can notice that they reflect the general 'folk epistemology of scientists'. Would interdisciplinary researchers accuse the usual referee of not considering or appreciating epistemological values such as "consistency" and "effectiveness in advancing understanding" or even "sensible balance"?
If interdisciplinary researchers are uneasy with proxy criteria, it is not because one should return to the "primary measures" that, as the authors seemed to suggest, would be lost by the "social procedures of peer review, inter-subjective agreement, and [...] consensus". It is not either because those who attribute patents and grants, publish articles, cite or employ (i.e. positive reputation) are blind to the epistemological values brought up by V. Boix Mansilla & H. Gardner. Rather, it is because those who attribute positive reputation apply "field based measure[s] of quality".

The problem arises because scientific criteria of quality are local, developing with each field. In this situation, interdisciplinary researchers need to develop and impose their own local criteria. It is not surprising that in the process of doing so, interdisciplinary researchers appeal to some general intuitions about what is good scientific knowledge.

> **Do proxy criteria replace the classic peer review?**
> Grit Laudel
> Dec 17, 2003 5:34 UT
>
> Christophe has made two statements that really surprised me, and I would like to know the empirical data from which they are derived. The first statement is that proxy criteria are used by scientists because “checking everything by oneself is not possible in current science.” I think this dichotomy is wrong because there is a wide range of possible validation strategies between proxy criteria and “checking everything by oneself”. In my empirical study of interdisciplinary collaboration, proxy criteria were used when scientists had to judge work from different fields, e.g. when a chemist had to form an opinion on which medical scientist should join the research network. Situations of this type occurred relatively seldom. In most cases, peer judgement dominated – both in everyday work and in explicit peer review situations. While scientists were never able to check everything by themselves, they were able to judge if the methods applied

were state of the art, if a sufficient number of experiments were conducted to back up the claims, if all important conditions were controlled in the experiments etc. This is exactly the type of criterion Mansilla and Gardner referred to – epistemic criteria of consistency and relations between existing knowledge and new knowledge. More generally, peer review of projects and publications proves that this type of judgement is possible. The second surprising statement was that trust between scientists is achieved by reliance on proxy criteria. In all the collaborations I investigated, personal contacts between prospective collaborators were of prime importance because without personal contacts, scientists didn't trust each other. Trust was a very concrete thing under these circumstances. One had to judge if one's collaborator is actually able to fulfil his or her part of the common work. That is why in theoretical physics, face to face contacts are still the way in which collaborations are initiated, even when they are later conducted mainly via email (Merz 1997: 323).

Merz, Martina, 1997. "Nobody Can Force You When You Are Across the Ocean" - Face to Face and E-Mail Exchanges Between Theoretical Physicists. Ion Agar and Crosbie Smith (eds.), Making Space for Science. London: St. Martins Press, 313-329.

Reply to Grit Laudel
Christophe Heintz
Dec 17, 2003 16:34 UT

I thank Grit Laudel for her reply. In fact, I read Grit's previous message: 'Are there special criteria for assessing interdisciplinary work?' after posting my own message. I was about to regret my posting because I thought that I had said more or less the same thing as Grit, except that her message was based on her own empirical investigation, while mine was based on Boix Mansilla and Gardner's text. It seems it is not the case.

The first statement that puzzles Grit is: "proxy criteria are used by scientists because checking everything by oneself is not possible in current science."

I do not understand why it is a puzzle for her, since she asserts a quite similar thing in the above mentioned message:

"No single scientist has enough knowledge to assess an interdisciplinary project. That is why the interviewees of Mansilla and Gardner spontaneously referred to 'second order' indicators of quality such as "number of accepted patents, publications, devices, and citations stemming from the work; the prestige of the universities, funding agencies, and journals in which it is placed"."

I think Grit disagrement stems from the fact that she thinks I said more than I did. This is apparent in the title of her reply: "Do proxy criteria replace the classic peer review?" and in her assertion "peer review of projects and publications proves that this type of judgement is possible".
I have no answer to the title question. The fact is that **proxy criteria *rely* on peer review** . Also, I did not say that assessing scientific work is not possible. On the contrary, I said that this pervasive assessment is essential for the practice of science since the resulting 'proxy criteria' are further used by scientists for determining the literature, people, groups and institutions which worth cognitive and financial investment.

The second controversial statement is, as stated by Grit, "trust between scientists is achieved by reliance on proxy criteria". But again, my assertion did not go that far. What I said, rather, is that scientists used proxy criteria in order to determine trustworthy sources of information – including people. This is compatible with Merz's observation that scientists want face to face interaction before collaborating with someone. My point is that they will not pick possible collaborators at random. They will use proxy criteria to select a small set of people and then they may put in place some other way to assess their 'collaborative potential', e.g. face to face interaction.

Last, there is a large literature about trust in science. The investigations are mostly philosophical and could certainly be enriched with more empirical studies (e.g. *to which extent*

do scientists use proxy criteria in order to decide whom to trust). An important paper, however, is Hardwig "The Role of Trust in Knowledge" (1991) *The Journal of Philosophy* Vol. 88, Issue 12, 693-708.

Is Trust Really an Epistemic Notion?
Steve Fuller
Dec 21, 2003 17:06 UT

In the context of research evaluation, 'trust', like 'reliabilty', is more a moral judgement than an epistemic one. It means something like, 'I would work with this person' or 'I would recommend others working with this person' or 'When they make mistakes, the mistakes are honest and do not alter my fundamental view of the person'. The epistemic basis of trust is little more than the absence of evidence of inexcusable error. I am always surprised how seriously BOTH philosophers and sociologists take the concept of trust in research matters. It is really little more than wishful thinking – or, more precisely, the conversion of ignorance into virtue. In 1996, I published an article in American Philosophical Quarterly ('Recent work in social epistemology') where I coined the term 'phlogistemic' (after the spurious element phlogiston) to describe concepts such as 'trust', which are increasingly popular to capture the social dimension of inquiry. I would argue that 'trust' seems so luminous simply because it's hard (or expensive) to find forms of research evaluation that do not rely on taking people's word for granted. For example, when a lot of taxpayers' money is involved in research, trust is not the default position. Thus, evaluators are motivated to find error as evidence of wasted funds, and not surprisingly cases of error and even 'fraud' get multiplied without too much difficulty.

Trust in science - reply to Christophe
Grit Laudel
Dec 22, 2003 5:50 UT

My studies about scientists' collaboration showed that the scientists started a collaboration after having had personal contacts with the other scientist (at conferences, at meetings of the research network etc.), usually after having found out that both are working at similar problems. They didn't first use proxy criteria to select collaborators and to check their trustworthiness. Trust (in this case) could be described as the assumption that a potential collaborator could conduct their part in the collaboration as required.

Epistemic dependence
Gloria Origgi
Dec 22, 2003 20:33 UT

Steve's position on "trust" as a conversion of ignorance into virtue seems to me a bit too extreme: I think that there is a possibility to argue for an epistemic notion of "trust": after all, being able to judge that another person is in a better epistemic position than mine is a perfect rational move that can be defended on epistemic, and not simply moral, grounds.

More on trust
Steve Fuller
Dec 23, 2003 15:23 UT

I must confess that I find Gloria's response a little surprising, especially coming from a philosopher. Of course, we make judgements all the time about someone being in a better epistemic position than oneself. Is this rational? Well, it depends how low you set the threshold for 'rationality'. Yes, most of us manage to get along perfectly well by engaging in this practice, but then there isn't much else to compare it to – and certainly little way of published track records of 'reliability' and related concepts that are normally presupposed in strictly epistemic conceptions of trust. What is clear is that when people – such as scientific competitors or cognitive psychologists – are motivated to find error, there is usually error to be found. This is why I call trust 'whistling in the dark'. Of course, this practice serves an important moral and even bonding function, but if you really want to stick to a strict moral/epistemic distinction, then trust is, at bottom, moral. (Back in the old days, of course, we'd fudge this whole issue by calling trust a 'pragmatic' virtue.) The reason why I push this issue is because complex social forms of

inquiry, like most science today, is mostly governed by norms that are non-epistemic, which is why so-called 'proxies' play such a large role. And from the discussion of this paper, it appears that 'epistemic' really means satisfying the people whom you think ought to be satisfied by a particular piece of research. Of course, if this how one wishes the word, then fine.

reply to Steve Fuller
Christophe Heintz
Dec 25, 2003 15:34 UT

I believe it is usefull to distinguish the act of trusting from the reasons or the judgements that lead someone to trust. Trusting may be defined as a cognitive act, viz. believing a communicated content without checking the truth of what is communicated with one's own means (i.e. on non-testimonial ground). More arguably, trusting someone can refer to the 'disposition' to trust what this person will comunicate (with often restriction on the domain of the content of what is communicated). This eventually leads to Grit's definition: "Trust (in this case) could be described as the assumption that a potential collaborator could conduct their part in the collaboration as required."

With this cognitive definition, I do not see how one can assert that 'trust' does not refer (phlogistemise (?)) without some revolutionary theory in cognitive psychology at hand. Trust is not an epistemic notion, it is a cognitive notion with some epistemological implications. This is at least what Hardwig argues, after showing that trusting, as a cognitive act, is pervasive in scientific practice.

I am sorry I do not have access to Steve Fuller's article. What I undertand from his message, however, is that he asserts that, most of the time, there is no reason that epistemologically justifies the act of trusting. Thus, Steve characterises trust by: "It is really little more than wishful thinking – or, more precisely, the conversion of ignorance into virtue" . The absence of reasons or fixed criteria that could provide some epistemological ground to trust, would be, if true, an epistemologically relevant fact (cf. anti-reductionist positions regarding trust).

I think, however, that trust is not the default position in science. This is why it is so difficult to do interdisciplinary research: if interdisciplinary researchers were trusted because, anyway, nobody wants to bother to assess their research, life would be easy for interdisciplinarity.

I can now go back to my previous messages: Most of the time, scientists trust other scientists because some previous assessment procedures took place, which positively labelled with 'proxy labels' (patents, publications, etc.) the researchers to be trusted. I would also like to go further: relying on proxy criteria for trusting is a reliable procedure that leads to knowledge. With this assertion, I assume that proxy criteria rely on valid (through local, I would add) epistemic judgements.

Reply to Grit (2)
Christophe Heintz
Dec 25, 2003 15:41 UT

I find Grit's finding interesting and, I must admit, suprisingI want to point out, however, that being present "at conferences, at meetings of the research network etc" is already passing the test of some proxy criteria.

Two senses of 'Trust'
Gloria Origgi
Dec 27, 2003 15:57 UT

I think that Steve conflates two very different senses of "trust" that one finds in the epistemological literature today, that is, "trust" as *assessing competence*, which is a perfectly rational procedure that has been analyzed in detail (see for example ch. 8 of Philip Kitcher : "The Advancement of Science") and "trust" as *evaluating honesty*, which surely is a moral

notion (whose history has been so well presented by Steven Shapin in his “Social History of Truth”).

Anyway, I do not see any interest in trying to understand whether a single act of trust is a matter of epistemology or moral: it is not the single act of trusting that is epistemologically or morally justified: rather, it is the design of the social system in which a belief spreads that should guarantee the epistemic validity of the distribution of cognitive labour. The particular reasons why a researcher may decide to trust to one of her colleagues in order to collaborate with him are not relevant for assessing the validity of the overall practice of generating consensus. Science presents itself as an epistemically sound social system (experiments, testing, peer review, etc.). We can argue that it is not the case, but this doesn’t depend on our practice of trusting our collaborators.

Finally, I am not sure that the amount of taxpayers’ money involved in research changes the threshold of credulity, especially when science and media interact. Take the recent case of the whale Keiko (the hero of the movie: “Free Willy”), released into the wild near Iceland in July 2002, after a $20m programme for teaching him how to catch his own fish. He died few days ago on the Norway coast, incapable of living in the wild again. The public perception of “animal captivity” has played a role in funding this research program, which has turned into a complete failure.

Response to Christophe
Steve Fuller
Dec 30, 2003 12:17 UT

My apologies for being late in replying to Christophe's message. I'm now on another computer. Let me quote the conclusion of his last message, and indicate where I disagree -- or at least see no reason to agree.

"I think, however, that trust is not the default position in science. This is why it is so difficult to do interdisciplinary research: if interdisciplinary researchers were trusted because, anyway, nobody wants to bother to assess their research, life would be easy for interdisciplinarity. I can now go back to my previous messages: Most of the time, scientists trust other scientists because some previous assessment procedures took place, which positively labelled with 'proxy labels' (patents, publications, etc.) the researchers to be trusted. I would also like to go further: relying on proxy criteria for trusting is a reliable procedure that leads to knowledge. With this assertion, I assume that proxy criteria rely on valid (through local, I would add) epistemic judgements."

Actually, the peer review process -- in the natural sciences especially -- is set up to make trust the default position. This is why it is very difficult to get falsifications published in major scientific journals. This provides a strong disincentive to distrust. People distrust only if there is a lot on the line -- such as money, prestige, etc. The problem with interdisciplinary work is that norms of trust have not yet evolved, though perhaps the proxy criteria are the beginnings of such thing. In other words, I think scientists are normally hoping they can find reasons to trust each other because it makes life easier on all concerned. I'm sorry if this sounds cynical, but I fail to see what else the evidence supports. As for 'previous assessment procedures' that become the basis for reliability measures, I think this is largely a philosophical hallucination built up from anecdotes and the incomplete records kept by funding agencies. No current vetting procedure in science operates in a way that would satisfy any rigorous philosophical definition of reliability. The open question is whether that fact matters morally, epistemologically, etc. What is clear, however, is that it amounts to wishful thinking.

Response to Gloria
Steve Fuller
Dec 30, 2003 12:33 UT

Yes, I am well aware of the philosophical literature on the subject of trust, and I think it makes artificial distinctions that at best dignify prejudice and wishful thinking. For example, the idea of 'trusting competence', which Kitcher gets from neo-classical economics, is primarily a labour-

saving device that enables you sort out the good from the bad, essentially by going on 'brand-name'. Again, we rarely have anything like clear track records for measuring degrees of reliability. All we have are prejudices built up from anecdotes that decision-makers trade amongst themselves. In other words, once you look at the microsocial processes involved in 'trusting competence', you're quickly back on the moral ground that Shapin is talking about.

In practice, the distinction you're drawing does not make practice a difference. If it did, we would be able to track with two different evidence bases. Instead, it's the same spotty evidence base interpreted in two ways.

And as for getting taxpayers involved, my point is not that their involvement always leads to correct judgements of who to trust or not to trust. Rather, their involvement motivates the questioning of trust, which is ALWAYS a good thing -- especially in science, where the default position (contra Christophe -- see my previous response) IS trust.

This whole discussion of trust would be much less mystified if it did not start from the assumption that whatever 'successful' science is doing must be good, and therefore our task as epistemologists is to justify whatever they happen to be doing. A little criticism toward science from people claim to be practicing social epistemology might be in order.

A question to Steve Fuller
Gloria Origgi
Dec 30, 2003 13:57 UT

Thanks to Steve for his reply. I am learning a lot from this discussion. But don't you think that there may be domains in which people develop reliable heuristics for assessing competence or authority? I don't see why these cognitive heuristics should generate "moral judgements", but may be I am confused about what a moral judgement is.

If I have developed a heuristics to extract social information about a person's social class by looking at the way he or she is dressed, is the judgement that I form in this way a moral judgement?

Reply to Gloria
Steve Fuller
Jan 1, 2004 0:52 UT

Here's how I'd answer your question: 'How do cognitive heuristics yield moral judgements?' (Your example is the use of clothing as a heuristic for social class.) First of all, the categories that frame heuristics i.e. the fact that you have a heuristic for recognizing class rather than for some other attribute pertains to their utility for the environments in which you normally act. The heuristic works because it gives you enough information to discriminate between various courses of action. This determines how much evidence you take in before forming a judgement. The only real validation of the heuristics is that it continues to enable you to make similar judgements in the future. Nothing is implied about why the heuristic works (e.g. the background social conditions that entrench class as a salient category) or even how it works (e.g. are you relying on the same or the same kind or same amount of evidence for each judgement made according to this heuristic?).

Without these action-relevant conditions, the heuristic would have no epistemic standing whatsoever, since all the epistemic indicators are at best indirect. The point of heuristics, after all, is that they don't involve deliberation (hence they save mental effort) and so there is no formal record of which inputs mattered in producing the output. In fact, people who use the same heuristics may explain why they work (and the range of cases for which they work) quite differently. Ethnomethodologists have become quite good at getting people to reveal these differences when they are forced to engage in so-called 'reparatory' work i.e. explaining, or excusing, why heuristics don't work on certain occasions (e.g. a wealthy person dressed like a tramp). At the very least, this research suggests that the moment-to-moment reproduction of

social life is epistemically shallow, i.e. inter-subjective consistency in judgements does not appear to be necessary.

There's an interesting book by James Franklin called 'The Science of Conjecture' (Johns Hopkins, 2000), which charts the history of reasoning under uncertainty before the formalization of probability in the 17th century. Of special relevance is the significance that the Jesuits attached to the idea of 'moral certainty', which is what I believe heuristics trade on. For the Jesuits, this idea implied that the exigencies of our decision-making environment mean that where we lack definitive evidence, we must supply a commitment to live with the consequences of our decisions. The Jesuits meant this originally in the context of trial juries, but it could equally apply to the heuristics people use to act as 'normal' members of society. In either case, the epistemic criteria used to determine whether the consequences vindicate our judgements remain quite loose e.g. we continue to function in society, we can sleep soundly at night, etc. We rarely find out later that we made the 'right' judgement in some epistemically approved sense.

Questions of research design
Steve Fuller
Dec 16, 2003 18:30 UT

Grit Lauder raised the interesting point that in both Mansilla/Gardner and her own research, second-order criteria -- like number of patents, citations, etc. -- tend to be used to evaluate interdisciplinary work. But is this the correct interpretation of what a research administrator means when he says, "Simply counting things are easy answers as far as I'm concerned"? When the administrator says this, isn't he just telling you what he uses to persuade others that the interdisciplinary unit should remain in existence? Why do you suppose that he regards these 'proxies' as quality measures in any strict sense? I would have thought that the situation is rather like a board of directors trying to persuade shareholders that the company is doing well, even though it produces weird things. They basically try to find the figures that will make themselves look good in the shareholders' eyes, regardless of whatever private views they might have about research quality.

Again, I do not mean to be cynical. However, since often a lot of material resources -- in terms of salaries, equipment, office space -- are at stake in the evaluation of interdisciplinary work, it might have paid to compare the evaluation of these situations with how innovative firms in an unconventional market niche justify their existence.

Another concern I have is whether the flight to second-order evaluative criteria is unique to interdisciplinary research. I would have thought that even within the same discipline, a peer's expertise is usually so limited that she will need to appeal to such criteria simply to evaluate most of the research in her own discipline.

The use of second-order criteria
Grit Laudel
Dec 17, 2003 5:49 UT

Yes, I agree, these cases of administrators using proxy criteria for persuading others of the usefulness of a research organisation but without believing in it as quality measure exist. However, in Australia, the universities' research funding is based on a formula that includes the components graduate student numbers or completion rates, research income, and publications. Similar formulas are used in some uiversities to allocate money to departments. Obviously, in this case the administrators and politicians believe they measure quality. I agree that even in the same discipline, a peer's expertise is limited. Thus, the word 'peer' refers not to a whole discipline but to a specialty which is a much smaller unit. I couldn't find the use of second-order criteria in the peer review of publications. If a scientist was asked to review an article he or she would either review it or send it back when he or she felt lacking expertise. In the case of grant reviews, second-order criteria seem at least to be more often included. But then it is again not a peer review because the panel who makes the grant decision might not cover all disciplines.

Community procedures and epistemic clarity
Veronica Boix Mansilla
Dec 17, 2003 21:33 UT

Grit Lauder, Steve Fuller, and Christophe Heintz focus on a central finding: Interdisciplinary researchers use second-order criteria to asses interdisciplinary work but express dissatisfaction with such criteria. Rather than a search for double standards or an irresolvable contradiction, their views embody a pragmatic compromise: “Proxi” measures of quality are used “for lack of better indicators”. Their conflict is central to peer review.

The posted comments highlight the social dimension of inquiry, offering social-procedural solutions. Grit Laudel describes a richly enhanced version of the peer review process. Christophe Heintz calls for an reliance on “proxi” indicators as part of our scientific ethos of “trust”. The question to pose is whether community consensus alone enables us to accept the insights we should accept and reject those we should not. Are solutions solely relying on social procedures -i.e. fully delegating epistemological judgment to the actors involved—sufficient? In my view such solutions seem to fail to open the “black box” of peer review to examine more closely how decisions are made. They are puzzling in principle and practice.

Social-procedural solutions are puzzling in principle because they defer to community consensus as the strongest (and only?) indicator of knowledge acceptability. If review procedures are followed by a given expert community and consensus is reached, the position suggests, a theory is rightfully accepted. Does the fact that centuries ago the world agreed on a geo-centric cosmology make such world view right? Conversely, should Galileo’s view be discredited given his contemporary’s inabilities to see the value of his argument? Community consensus alone can not ipso facto confer acceptability. Additional criteria of an epistemic kind seem necessary.

In practice, a reliance on “proxis” and consensus alone is equally problematic. As many suggested inter- and disciplinary knowledge must cohere with prior knowledge and advance understanding. The problem for interdisciplinary research is that it must reasonably do so “in multiple, often conflicting, fields” So who is to evaluate interdisciplinary work and on what grounds? Our study points to the epistemic inadequacy of current social-procedural peer evaluation practice.

Current social arrangements for peer review (which set the conditions for a careful evaluation) might be productively complemented with epistemic principles (that shed light on the standards to be observed). Such epistemic principles ought not to be tied by default to single disciplines. Rather they ought to recognize the central role of multiple disciplinary antecedents, capture and capitalize on the complexity of integrative efforts, and call for a compelling articulation of their added value.

Arguably, Grit’s enhanced review procedure works not because people meet over the years and reach consensus but because they have a chance to learn about the disciplinary antecedents to the work proposed, negotiate channels of integration, and assess potential research paths and findings against relevant contenders. It seems to me, that our “trust” is strengthened by our keen recognition of the provisionality of our knowledge and methods, and our active pursuit of new and better ways to go about doing things in the face of conflict.

> **Galileo is not a good example...**
> Steve Fuller
> Dec 21, 2003 17:10 UT
>
> I am somewhat bewildered by Boix Mansilla's attempt to distinguish strictly epistemic criteria from community procedures. Her appeal to Galileo epitomizes the problem here. Galileo did not triumph because somehow community procedures were overturned – or perhaps should have been overturned during his trial. Rather, the Roman Catholic Church did not have a monopoly on epistemic authority at the time of the trial. (In fact, even within the Church, Galileo was being taught by missionaries in China.) Galileo may be condemned in Rome but nonconformist Christians picked up his work across Europe, and eventually as their numbers and resources grew, they came to dominate science. To deny the thoroughly social character of this entire process – from Galileo’s trial to the ascendancy of his views – is simply to mystify the issue. If

you want to make an interesting case for how ‘strictly epistemic’ criteria of research evaluation would overturn ‘community procedures’, you should come up with a CURRENT example in which you think a kind of ‘groupthink’ among elite peers is preventing some worthwhile lines of research from being developed. Then I would have a clearer sense of the distinction you’re trying to draw between ‘communal’ and ‘epistemic’. Otherwise, it looks like you're promoting the old philosopher’s canard of blaming the past for not catching up with the present fast enough.

Social procedures
Grit Laudel
Dec 22, 2003 5:54 UT

Veronica wrote "Are solutions solely relying on social procedures -i.e. fully delegating epistemological judgment to the actors involved—sufficient?" But how else can epistemological judgements be reached if not through social procedures of peers. Scientists can be wrong (like in the example of geo-centric cosmology) but will be corrected in the long run through other scientists, and yes this happens for epistemic reasons (e.g. new observations that contradict the old judgements).

Further: "Community consensus alone can not ipso facto confer acceptability." What is acceptance if not (individual or communal judgement) and therefore a result of a social process?

Reviewing interdisciplinary work: The peer review process I described works because the reviewers themselves have the chance to conduct a minimum of interdisciplinary work in that they learn the basics of the other specialties’ languages, and because peers of all relevant specialties are in the reviewer panel where they talk to each other.

The social and the epistemic revisited
Veronica Boix Mansilla
Dec 23, 2003 4:15 UT

It seems to me that our underlying agreements are greater than our discourse reveals. First, (and this may serve as a point of clarification on my previous posting) we seem to view social and epistemological dimensions of interdisciplinary knowledge validation as central and intertwined. Typically, judges are selected as representatives of areas of expertise – i.e., “embodying” epistemic criteria so to speak. In turn, new epistemic criteria are advanced as expert communities come to view them as more fit to the problems and forms of knowledge production of their times. Neither social arrangements nor epistemic criteria exist in isolation.

We also seem to converge in noticing that, in interdisciplinary contexts, current peer review practices demand innovative thinking – since it is not fully clear who adequate “peers” are, not what epistemic criteria will do the job, when multiple disciplines and perspectives are brought together.

Our individual responses to this issue may say more about what we consider relevant areas of focus than about how we conceive the phenomenon of interdisciplinary validation as a whole. Grit’s enhanced review procedure illustrates the point. The board members she describes capitalize on the institutional conditions that set the stage for more reflective judgments of interdisciplinary work. They learn each other’s languages, they come to understand how perspectives are articulated, and reflect about why a particular approach is preferable over its contenders. In analyzing this case, Grit focuses on institutional conditions and I do on what individuals learn. In ideal cases of this kind, institutional norms and sophisticated criteria stand in synergistic interaction.

I would agree that better institutional conditions can increase the likelihood that more appropriate approaches to the evaluation of interdisciplinary work are developed. Howerver, I also believe, based on the interviews that we have carried out, that conditions alone cannot ensure that assessment decisions are productively informed. In our study, members of the coordination committee of the Center for Integration of Medicine and Innovative Technologies

(CIMIT) take part in multi-year interactions with researchers in various fields supporting collaborations on significant innovations—another exemplary case of institutional fertile ground. And yet, these experts continue to inquire about how exactly quality standards can be discerned when no clear precedents exist for particular disciplinary integrations, and when conflicting bodies of knowledge are merged. Against this background, it seems to me that an epistemic compass to orient those making validation decisions might inform peer review practices taking place in better and worse institutional contexts.

Théories vs. pratiques de l'interdisciplinarité
Iskender Gökalp
Dec 25, 2003 20:42 UT

L'étude de Boix Mansilla et Gardner est symptomatique d'une grande partie des travaux portant sur l'interdisciplinarité et qui se concentrent pour beaucoup sur « l'évaluation » du travail interdisciplinaire plutôt que sur l'identification et l'analyse des mécanismes cognitifs de ce travail de création. Autrement dit, la grande majorité des études sur l'interdisciplinarité, sont, me semble-t-il, des commentaires sur l'interdisciplinarité par des observateurs extérieurs. Bien sûr qu'il faut de la distance cognitive pour commenter un acte, développement, réalisation, événement, etc., fut-ce en recherche. Bien sûr aussi, il faut éviter que cette distance cognitive soit trop importante et que le commentaire devienne un discours sans intérêt, cognitif justement, pour l'objet commenté et pour les acteurs-praticiens de l'objet commenté. D'autant plus que ces acteurs là sont quand même un peu particuliers puisqu'ils montrent, par leur enclin au travail interdisciplinaire, leur propre capacité à prendre de la distance cognitive par rapport à leur discipline d'origine.

Le travail interdisciplinaire n'est pas nouveau. De nouveaux domaines de recherche se sont constitués, dans les premières décennies du 20ème siècle, à la frontière ou plutôt à l'interface de disciplines bien établies. Il existe d'excellentes études historiques sur la constitution de tels domaines de recherche et sur les différents mécanismes de rencontres interdisciplinaires, par exemple en cytologie, ou dans la découverte des relations entre les vitamines et les coenzymes ou encore en psycholinguistique (voir Darden and Maull, 1977 ; Bechtel, 1986).

Je pense qu'il faudrait mieux utiliser ces études et surtout continuer à les enrichir. Je les ai moi-même utilisées pour essayer de comprendre comment quelques acteurs très créatifs avaient réussi, dans les années 1930 et 40, à mettre en relation des concepts totalement autonomes de la chimie de la combustion et de la dynamique de la turbulence pour constituer le domaine d'interface aujourd'hui appelé la combustion turbulente ou les écoulements turbulents réactifs (Gökalp, 1989 ; 1990 ; 1994). Ils ont en effet réussi cet acte fondateur en imaginant divers mécanismes de mise en contact interdisciplinaires : utilisation des universaux comme les échelles caractéristiques de temps et d'espace pour relier les phénomènes de cinétique chimique de la combustion et ceux relevant de la dynamique de la turbulence, analogie cognitive entre certains concepts des deux domaines en interaction, commensurabilité additive de certains effets produits isolément par chaque domaine sur le même phénomène d'ensemble, et enfin la production de concepts hybrides comme celui extraordinairement créatif de « turbulence générée par la flamme ».

Il est bien entendu évident qu'il s'agit dans cet exemple d'un domaine interdisciplinaire constitué à l'interface de deux disciplines présentant une distance cognitive faible, la cinétique chimique de la combustion et la dynamique de la turbulence. Il est certainement autrement plus difficile de réussir la mise en contact de disciplines ou de domaines présentant des distances cognitives plus importantes.

Je pense qu'il faudrait que les acteurs-pratiquants et les historiens du travail interdisciplinaire s'expriment dans ce forum pour que nous avancions ensemble dans la pratique et l'analyse des mécanismes d'interactions interdisciplinaires, d'associations d'idées et de créativité.

Une précisation
Gloria Origgi
Dec 27, 2003 18:14 UT

Je voulais juste préciser que ce séminaire a hébergé aussi des textes de chercheurs engagés dans la recherche interdisciplinaire, surtout au début : le texte de Dan Sperber, mis en ligne le 1er Avril 2002 (et archivé sur ce site à : http://www.interdisciplines.org/interdisciplinarity/papers/1) est un récit de l'expérience personnelle d'un chercheur engagé dans une recherche de frontière entre les sciences cognitives et les sciences sociales. Le texte de Pierre Jacob, mis en ligne le 1er juin 2003 (et disponible à http://www.interdisciplines.org/interdisciplinarity/papers/4) est une réflexion d'un philosophe sur son interaction avec un neuroscientifique dans l'étude de la perception visuelle.

L'équilibre entre expériences et réflexions théoriques n'a peut-être pas été celui qu'on souhaitait, mais nous étions motivés à organiser ce séminaire par nos propres recherches interdisciplinaires (d'ailleurs tout le site est conçu pour faciliter la recherche interdisciplinaire).

Mecahnisms and assessment criteria
Veronica Boix Mansilla
Dec 29, 2003 18:17 UT

Iskender Gokalp is right in highlighting historians' potential contributions to our understanding of interdisciplinary knowledge validation. His own analysis of historical cases reveals cognitive "mechanisms" for disciplinary integration that are also present in our data. Examples include the re-framing of particular constructs and phenomena along encompassing scales or theories (e.g "time", "space" in his example, "complexity" in ours); the use of analogy to link concepts across disciplines; and the examination of forces typically studies by different disciplines interacting in the emergence of a phenomenon under study. Understanding mechanisms of this kind is central to our collective understanding of interdisciplinary work because they represent the very point of articulation between two or more disciplines.

Interestingly, mechanisms of this kind ensure articulation but may vary in the degree to which they yield successful insights. In successful articulations like the ones Gokalp describes, insights are measured by the ways in which they leverage understanding. Murray Gellman and George Cowan (two of our interviewees at SFI) illustrate this point. Needless to say, they value the role of complexity theory as a conceptual tool to bridge analogous phenomena across distant disciplines. Yet they also alert researchers about the risks of what Cowan calls the "reminiscence syndrome" in complexity work –the temptation to limit its accomplishments to the identification of novel analogies. At a "mechanism" level such analogies may be novel and well constructed. The question then remains… "How are they advancing our understanding --e.g., of the phenomena under study or of non linear dynamics itself?

From an epistemic perspective, historical cases of successful (and unsuccessful) interdisciplinary work can shed light on interdisciplinary knowledge validation by revealing the qualities and criteria that rendered particular interdisciplinary insights acceptable to scientific or intellectual communities at a particular time. Historians can capitalize on hindsight to identify more and less successful research outcomes and on their close analysis of the written record to explore the arguments used in their defense and critique. Historical analysis in turn can be combined with the cross-case comparisons that are more typical of social scientific research--shedding light on principles underlying "in vivo" practices across a great variety of disciplinary combinations. In such interdisciplinary approach, as Gokalp suggests, practitioners will also be a most valuable testing board for the validity and relevance of findings.

A final note
Veronica Boix Mansilla
Jan 4, 2004 19:46 UT

As we conclude our discussions on assessment of interdisciplinary work today, I would like to thank you for your many contributions to our conversations. We have learnt a great deal from your observations and analyses and we will keep them in mind as we advance in our attempts

at shedding some empirical light on this fascinatingly unruly territory. Wishing you a very good 2004, I look forward to “seeing” you again on this forum in the discussions to come.

The Complacent Disciplinarian

Ian Hacking (Collège de France)

(Date of publication: 5 January 2004)

Abstract: Collaborations between disciplines as well as openness between fields of expertise don't necessarily mean breaking down disciplinary boundaries. Ian Hacking draws on his personal experience to reflect on the very sense of interdisciplinary research, without describing himself as an "interdisciplinary" researcher, rather as someone who applies his discipline in different directions.

I am not a good person to discuss interdisciplinary studies because they have never been a problem for me. My undergraduate education in philosophy was more narrow than anyone today can imagine, and I loved it. Ever since then, I have dabbled in, and sometimes contributed to, more fields of thought than most people can shake a stick at. Analytic philosophers are not expected to write a book about experimental physics and another about multiple personality (etc.) but for me it has been the most natural, if not the easiest thing in the world, partly because I do not think of myself as 'interdisciplinary' but as applying my discipline in different directions.

Even my doctoral dissertation had two unconnected 'parts' that the examiners graciously accepted. One proved some new results in modal logic, while the other was infatuated with Wittgenstein's reflections on mathematics.

My role model has tended to be a predisciplinary man, namely Leibniz. I once had the project to write a paper every year, about a topic that exercised him when he was x years old, when I myself was that age. I kept it up for a while, but flagged, which he would not have done. He is usually catalogued as a philosopher, but what is the field of knowledge, wisdom or practice in which he did not engage his energies? Peace studies. Mining engineering. Comparative linguistics. He spent more of his days on those fields (which had not yet been invented) than on developing the calculus or the nascent physics. Above all, he was curious about everything. That is surely one way to be interdisciplinary. By the way, the late Pierre Bourdieu, quite an interdisciplinary figure, also cut his teeth on Leibniz.

Curiosity: that is my role model, with the Leibnizian imperative, namely discipline. Work hard and get it as right as you can. Maybe a newish sub-discipline will emerge. But do not try to create disciples. Just respect your juniors, and tell them when you see that they, too, are trying to get it as right as they can. And say when by your lights they are not trying hard enough. Yes, that is discipline for sure. Not comfortable, either to administer or to receive.

How strange that word is, 'discipline'. An old word, or words, as old as European vernaculars, and traipsing behind them not so much Roman Latin as the learning of Mediaeval times. In both French and English, there is both verb and noun. The noun that makes for interdisciplinarity implies fields of study defined by content and institution. But the verb implies chastising and punishment.

The root idea is that of a disciple. You can see how the idea forks. On the one hand, religious teachers, and modern scholars, engineers or artists who have disciples, create fields of knowledge, understanding and activity. Thus the noun. But then there is the verb, to discipline: the master chastises to ensure that the disciples toe the line. I say 'chastise', for I find that word in old French and English, and flogging is mentioned as a mode of chastising, of disciplining. How strange it is that ancient meanings are continued below the level of conscious awareness. Many who object to disciplines do so because they sense that they have been flogged by the institutional structures that determine the disciplines. There is no freedom to live other lives, or to create other kinds of knowledge.

I know many people who have been disciplined by disciplines. I mean, bullied by bosses who sternly strive to maintain pre-established institutional structures of inquiry. One need go no further afield than what I think of as my own discipline, analytic philosophy. Many students have felt oppressed by it. This is especially so because some of its practitioners have a remarkably narrow conception of what philosophy is. The student cannot get a qualification without fitting into the norms, and cannot get a job without continuing to do so. I respect these victims of the system, regret their plight, and hope that sometimes I have been able to help them.

Nevertheless, I would like to tell another story, of collaborations between disciplines, of the openness that has long existed between fields of expertise. Not a tale of breaking down of disciplinary boundaries, but of mutual respect, which, as a new group of issues arises, may create a new discipline. In my opinion what matters is that honest and diligent thinkers and activists respect each other's learned skills and innate talents. Who else to go to but someone who knows more than you do, or can do something better than you can? Not because you are inexpert in your domain, but because you need help from another one. I never seek help from an 'interdisciplinary' person, but only from a 'disciplined' one. Never? Well, hardly ever.

Why am I so complacent? Because I lucked out. In 1965 I published a book about the logic of statistical inference. It had a good publisher, in those days (Cambridge). That surely helped. But the book said on the front page that I worked at a nothing university, in those days (British Columbia). This book had been written entirely in isolation. I had not talked to a single statistician. It was my own introverted thing. Within weeks of publication I got long, critical and helpful letters from the leading statisticians who were preoccupied by the foundations of their subject. People whom I had only imagined, since I was an introverted nerd. They wanted to talk, and I wanted to talk, and it took a few letters, in the old days, to talk. So I learned, if you spend your energies thinking about what they are doing, within the domain of what you know how to do, then concerned others will want to learn about what you are doing, within the domain of what they know how to do.

I have always lucked out. Of course, as I have carried more cultural and academic capital with me, it has been easier and easier to consult people from different disciplines. Yet I have regularly found that people who have skills want to tell you what they do and how to do it. Are you scared of someone because they know more than you? Forget it. They will love to tell you. My only 'interdisciplinary' experience, so designated, was at the Centre for Interdisciplinary Research (the ZiF: Zentrum für interdisciplinäre Forschung) in Bielefeld. In 1982 the late Lorenz Krüger organized – and found the funding for – a year-long research group dedicated primarily to questions in the history of probability. It included many young scholars, plus a few established ones, from a great many countries in Europe, plus people from the USA and Canada. There were historians of science, philosophers, statisticians with an historical bent, economists, mathematicians, experimental psychologists. Yes, a lot of disciplines were represented.

The year was an amazing success, largely owing to the gentle and sensitive leadership of Lorenz himself who encouraged us to do what we were good at, and to listen to others doing what they were good at. I often come across references, in the general literature, to 'the Bielefeld group'. The productivity was amazing. In the first instance, two volumes of collected papers, and a volume, a sort of overview, written by a collective that was a subset of the group. In the following few years a number of truly excellent books were published by individual members of the group. Some of these are absolutely permanent contributions to the field – they have become 'classics'. Plus many more specialist papers.

One might say, twenty years later, that the Bielefeld group created a sub-discipline in the history of science, for there continue to be published important new papers and books. I suppose that the history of probability and statistics should not count as a fully fledged sub-discipline. Nowadays a good sociological criterion for the existence of a sub-discipline is the existence of at least one journal explicitly dedicated to the topic. There is no such journal for the history of probability! Nevertheless, a

great deal of first-rate research continues. I suppose that would have happened anyway, but Bielefeld provided a benchmark in the development of the field.

Is the Bielefeld group a model for 'interdisciplinarity'? Yes and no. Yes, of course, the participants were drawn from a number of disciplines, and worked in an institution dedicated by name and practice to interdisciplinary research. But in a sense, the answer is 'no'. Here I have to speak for myself. I never thought in those terms, and never once heard one of my colleagues use the word 'interdisciplinary'. Of course it was there, in the name of our host institution, but since we always called it simply the ZiF, we never heard the word. We thought of ourselves as individuals from different disciplines with some overlapping interests.

Allow me another example. A couple of years ago I was privileged to attend the eightieth birthday celebration of Mary Douglas, the anthropologist. It was quite rightly held in the rather grand premises of the British Academy. Seven people spoke about aspects of her work. (I did Risk and Culture.) Aside from her biographer, exactly one speaker was an anthropologist, who discussed Mary's early research in the Congo. And then: A famous urban sociologist-cum-politician. A biblical scholar - Douglas became fascinated by the pollution rules of the Pentateuch, and something of a Biblical scholar herself. An art historian. An expert on Hindu mythology. And no one spoke about her books on food and its meanings, or on styles of thought ... She is one interdisciplinarian! Except that is not how I think of her. Rather she applies her keen and totally unconventional mind and skills where she is interested. I shall have to ask her next time I see her, does she think of herself as anything other than a (non-conformist) anthropologist of a particular kind, education and tradition? I doubt it.

I conclude with a current hobby of mine. There is today an increasing awareness that diagrams can play a fundamental role in the communication of ideas. In modern physics, the Feynman diagram is ubiquitous, and an indispensable tool of thought. I have become interested in tree-diagrams. There are cognitive scientists who argue strongly that arranging hierarchies, taxonomies, temporal processes and the like, in the form of tree-diagrams, may be in effect innate, perhaps there is even a tree-diagramming module in the brain.

Certainly tree imagery is very deep in human culture: the Tree of Life goes back to Babylon and Assyria, long before the Hebrew Bible, and the Tree of Knowledge of Good and Evil is ancient too. The candelabra in the Temple, the Menorah, are branching trees. The Cross is a tree, so shaped, and made of wood indeed. Tree images are present in most civilizations, even if they are most prominent in regions where trees are not so easy to come by. Yet the use of tree-diagrams seems very recent. How recent? I had carelessly said in seminars that the Linnaean hierarchy in which classes are defined by division (species, genus, order, class) obviously produced a tree-structure. A student kept on protesting that before evolutionary theory, systematic taxonomists did not think in terms of trees or draw trees. Well, she was right. I have adopted what I call 'Scharf's maxim': it is not a tree unless it is drawn as a tree and called a tree.

I became curious about tree-diagrams in general. For example in traditional logic there is what is called the 'Tree of Porphyry', but which is not found in or mentioned in Porphyry's Isagoge (his introduction to the Categories of Aristotle) written about the year 300. Also genealogical trees, the tree of Jesse, trees of consanguinity, which determine the impediments to marriage (viz. incest). Tables of kinship relationships, which have played such a role in twentieth century ethnography. Trees are important in logic and essential in many aspects of computer science, though the first theorem about trees appears to have been published only in 1857. I have, over the past two or three months, consulted, usually by e-mail, a vast range of experts, most of whom have been extraordinarily generous. I do not read Latin properly, let alone Greek, but now I think the first logic trees were drawn and described in Syriac, perhaps about 500. The very symbols of that alphabet are unintelligible to me. So I have been consulting masters of this or that language – and biologists, anthropologists, Byzantine scholars, Renaissance scholars, computer scientists. I am still trying to find historians of

Islamic logic who can help me. All this in order to understand what we might call the cultures and uses of tree-diagrams.

If anyone should be curious: At present I believe that in the East, better named West Asia, tree diagrams start early, say 500. In Western Europe, and in particular Spain, there are trees of consanguinity from 600, but these are generalized to genealogical trees, the Tree of Porphyry, the Tree of Jesse only around 1100, always in Spain, so focal for joining Western Christian, Jewish (especially Cabala) and Islamic civilizations. The greatest tree-diagram fetishist of all time was a Catalan, Raymond Lull, who was at the focal point, around 1300. And who happens to have been one of Leibniz's heroes. Paolo Rossi wrote a great book about the history of combinatorial logic and the universal language – 'from Lull to Leibniz'.

Of course the fact that tree-diagrams are so recent in human history does not imply that they are not grounded in a universal mental module. It would imply that human beings learned to use and represent this faculty only in historical time. There is a further question for an evolving discipline, namely cognitive science. We have not yet sorted out how to run together questions of culture and cognition.

Is this an interdisciplinary quest? In one sense, 'yes': I am consulting experts from disciplines that are mutually unintelligible to each other. I have yet to meet a person truly knowledgeable about Byzantine civilization who understands evolutionary theory – or vice versa. Neither is likely to comprehend the present ambitions of the cognitive sciences. But in a more important sense the answer is 'no': highly disciplined scholars help out in a project that is quite easy to explain and to become captivated by.

I apologize for an all too complacent contribution to this ongoing series of discussions. I respect the questions raised by colleagues, but thought it worth while to put in a word for collaborating disciplines that do not need to be, in any important sense of the word 'interdisciplinary'.

Discussion

Interdisciplines or interdisciplnary topics?
Christopher Green
Jan 5, 2004 23:16 UT

Ian's recounting of how some of his many projects have come about implicitly raises an interesting point that may run, at least superficially, contrary to his claim to being a "complacent disciplinarian." His initial training may have been in analytic philosophy, but anyone who has read his work knows that it has depended as much on the approaches and methods traditionally associated with (to mention only two) the historian and the continental philosopher as with the analytic philosopher. (Remember, Ian once famously wrote that his work "reeked" of Michel Foucault, hardly a hero among analytic philosophers. Then again, Foucault once described himself as a "happy positivist." I suspect he would be fairly indifferent to what discipline others attempted to assign him.)

Now comes the tricky matter of trying to generalize from the individual experiences to the general case. Perhaps scholars and scientists are not (or at least need not be), *intellectually* speaking, denizens of any particular discipline. It is not so much that we are (or can be) "interdisciplinary." It is, by contrast, that we are typically focused on much *smaller* units than the discipline; we are engaged, instead, with particular *topics*.

For instance, Ian wanted to know about the history of probability. So he joined a group of other people who were interested in the same topic (ZiF). Their backgrounds were different -- they had been trained as historians, statisticians, philosophers, etc. -- but their main interest (and their common interest) was the topic at hand. For another instance, Ian is now interested in the (topic of) tree diagrams. He

consults with and learns from historians, philosophers, linguists, anthropologists, biologists, and even computer scientists. They are, he says, all traditional disciplinarians. What they share, however, is interest in a common topic.

Ian calls himself a "complacent disciplinarian" who just applies his discipline in different directions. At one point, however, in reference to Leibniz, he slips in the interesting term "pre-disicplinarian." This may be the better designation, or perhaps just "non-disciplinarian." Ian is not going to fight the strongly-entrenched system of dividing universities into disciplines. Perhaps he believes, as I think I do, that do so would simply take too much time and effort away from the things he's interested in. He is, as he says, just going to "work hard and get it as right as he can" using whatever methods, tools, approaches, and consultants he needs to do so.

This strikes me as a reasonable approach for the would-be interdisciplinarian. If one is going to get on with one's work, one must treat institutional structures with a certain respect, in more or less the same way that one respects the traffic code -- you may get to where you're going more slowly, but at least you'll get there in one piece. Both are a combination of convention and historical contingency. Another configuration might serve us better. On the other hand, a lot of other configurations would be worse and it isn't always easy to tell which are which.

Lucky you!
Dan Sperber
Jan 6, 2004 18:02 UT

Wonderful paper, thanks! Now, doesn't the kind of philosophy you practice make it part of its job to pay attention to other disciplines, talk to its practitioners, and occasionally get involved in collaborative enterprises? This kind of philosophy is not paradigmatic of disciplinary work in general. Is it, for that matter, of philosophy itself? Isn't it the case that many, possibly most philosophers talk only to other philosophers? Are they missing something important, essential even, or not? To what extent should the study of philosophy itself be monodisciplinarian? As you know, science is much more part of the philosophy curriculum in North America than in Europe (Britain included). Is this just a local tradition to be respected but not necessarily emulated, or is there something to be said for generalizing this? Philosophy, at least, gives you the opportunity to get seriously involved with other disciplines if you care to, and to do so without too much compromising your acceptance and recognition among philosophers. Not so in many other disciplines where "this is not really X!" (replace X with 'social anthropology' or 'experimental psychology' for instance) is a serious indictment: no publication in our journals, no job in our departments! The complacency of a complacent disciplinarian in those fields may be much less engaging than yours.

Hiding interdisciplinarity 1: X of Y versus XY
Christophe Heintz
Jan 7, 2004 16:24 UT

I would like emphasis the distinction between philosophy or sociology or whatever **of** some discipline and genuine interdisciplinary studies. Doing the history or philosophy or sociology of one discipline is not necessarily doing interdisciplinary studies. For instance, one does not need to be a chemist or to contribute to chemistry when doing the history of chemistry. Thus Hacking as a philosopher **of** X and Y, is, as he says, not 'interdisciplinary' but applying his discipline in different directions (viz. X and Y). At this point, I am really happy with Hacking disciplinarian perspective: it is a flawed argument (that can be found in the science war, I think) to say that sociologists of, say, physics should also be physicists – and thus interdisciplinary. The truth is that sociologists of X make a contribution to sociology, not to X.

Now, consider the difference between (1) and (2):
(1) You don't have inderdisciplinarity with **X of Y**
(2) You have interdisciplinarity with **XY**, for instance Philosophical Logic, Cognitive Anthropology, Bio-chemistry.

Doesn't Hacking, in this text, unduly slip from (1) to some polite, implicit, negation of (2)??

Hiding interdisciplinarity 2: Complacency and interdisciplinarity
Christophe Heintz
Jan 7, 2004 16:31 UT

Hacking's conclusion puzzles me. He asserts that his contribution is about "collaborating disciplines that do not need to be, in any important sense of the word, 'interdisciplinary'".

I am not sure what important sense of 'interdisciplinary' Hacking is referring to. I thought 'interdisciplinary' was exactly about collaborating disciplines.

Maybe Hacking's previous sentence provides some cues. He apologizes for being all too complacent. Why does Hacking need to apologize? Because interdisciplinarity is often associated with institutional difficulties and uneasiness. I think it is fair enough, but difficulties and uneasiness are a contingent facts that does not belong to the meaning of 'interdisciplinary'.

In Hacking's text, 'complacency' and easy collaboration sems to sends to non-interdisciplinarity. I don't see why it should be so.

The complacent interdisciplinarian
Christophe Heintz
Jan 7, 2004 16:41 UT

Hacking recounting of his academic experience seems to me to be the one of a complacent interdisciplinarian rather than a complacent disciplinarian.

Admitted: As a philosopher of X, Hacking remains disciplinarian (see my message 'X of Y vs. XY'). But what Hacking actually did does qualify as being interdisciplinary – which is independent of whether he thought in those terms or not.

In particular, Hacking contributed to Philosophy (his declared disciplines), Mathematics (his doctoral dissertation included results in modal logic), and History (e.g. his current work on tree diagrams).

Many discussions have been about the difficulties of doing interdisciplinary research and Hacking shows that it is not necessarily difficult to be interdisciplinary. Maybe it is because interdisciplinarity is so often associated with institutional difficulties that Hacking withheld the 'inter' in his title.

There may be another reason: Hacking's discipline – philosophy - has institutionally been interdisciplinary in the Anglo-saxon world.

First, Empirical logicism has put philosophical logic in fashion. Then the naturalistic turn has definitely consecrated the interdisciplinary status of philosophy. Kuhn's work especially motivated philosophers to do history. And there has been the social turn, and the cognitive turn.

As a consequence, Hacking has been a complacent disciplinarian of an interdisciplinary discipline. He has lucked out … or rather, he did the right (interdisciplinary) work at the right time and place (e.g. he did not to submit his dissertation at the Sorbonne).

I would like to point out, however, that Anglo-saxon philosophy is more an exception concerning its openness to other disciplines than the rule. Look at 'European Philosophy' for instance. Nicely collaborating disciplines is not usually the current state of affair.

Hacking needs not apologize for his complacency: it provides data for answering a key question in the study of interdisciplinarity: "how can one manage to be a complacent interdisciplinarian?"

The complacency of a priori being relevant
Christophe Heintz
Jan 8, 2004 19:27 UT

Let me try a first answer to the question I raised above: 'how [or why] can one be a complacent interdisciplinarian?'.

Here is my own story:

I've just been moving to a new research lab. While introducing myself and talking about my research project to my new colleagues I met the following reaction: "You really don't fit in!". Now, Let us suppose it is not because I am actually out of place (after all, I was given the position). Let us suppose the reaction is a consequence of the interdisciplinarity of my research project. What happens, then, is not that I actually do not fit in, but that **I have to show** that I do. There is an element in my research project that comes from another discipline than the one of my current research lab. What can we do about this element? I have to show that this element can bring something to the research of my colleagues. I have to show that it fits in, and how.

When you are a disciplinarian, your research is **most of the time** straightforwardly relevant to your colleagues. What you need to do is just to show that your work is of good quality – e.g. that it is true, not trivial and that it has numerous implications. When you are an interdisciplinarian, on the other hand, your research is **most of the time** NOT straightforwardly relevant to your colleagues. So even before showing that your work is of good quality, you need to show that it is relevant, i.e. that it has useful implications for your collegues' research.

Why does Hacking say he has lucked out? Not just by rhetorical modesty. He knows, as we do, that his work is of extraordinary quality and that is why it met its success. Hacking feels he has lucked out because he did not have to show that his work and approach was relevant to his colleagues. He did not need to show, in the department of Philosophy of the Cambridge University of the 60s', that proving mathematical results was relevant to philosophy. That was taken for granted.

Why is Sperber more aware of the difficulties that interdisciplinarians usually meet? As a cognitive anthropologist, he actually needs to prove that his work is relevant to his anthropologist colleagues. That's not taken for granted! When talking about Sperber's work with anthropologists, I have never met someone who told me that it was false, or trivial. What anthropologists told me, rather, is that they didn't know what to do with it. They did not know how to use his work for their own research. The relevance of cognitive approaches is not taken for granted in anthropology.

Les méta-disciplines?… Et les autres!
Abdelkarim Fourati
Jan 13, 2004 14:22 UT

Je félicite Ian Hacking de son exposé clair, avec ses exemples éloquents. Il veut nous dire que tout en appliquant sa discipline et être bien discipliné, il fait de l'"interdisciplinarité" à sa manière. Pour lui cela a été la chose la plus naturelle, si ce n'est la plus facile au monde, en partie parce qu'il ne se considère pas comme "interdisciplinaire" mais comme appliquant sa discipline dans différentes directions. Pour pouvoir faire comme lui, il nous prescrit une règle tirée de sa pratique: "si vous consacrez votre énergie à penser à ce que font les autres, dans les limites du domaine de ce que vous savez comment faire [la Philosophie], alors ces mêmes autres voudront apprendre ce que vous faites, dans les limites du domaine ce qu'ils savent comment faire [la Statistique]". Cependant, cette formulation, trouvée par introversion, n'est pas généralisable à toutes les autres disciplines. En effet, si nous inversons les rôles, en mettant la discipline "Statistique" à la place de la discipline "Philosophie" et vice versa. Notre formule magique citée ci-dessus n'est plus valable, parce que nous pouvons faire de la "Philosophie de la Statistique" mais pas de la "Statistique de la Philosophie".

Donc, il existe des disciplines, parmi lesquelles la philosophie, la sociologie, l'histoire, l'anthropologie et d'autres, qui peuvent fonctionner comme méta-discipline. Le préfixe "méta" qui a plusieurs

significations (changé d'état, au-delà de, supérieur, transcendant, qui vient après...) indique un niveau supérieur ou plus global de référence. Ici, il désigne généralement un ensemble d'information sur l'information ou de connaissances sur d'autres ensembles de connaissances. Une méta-connaissance est une connaissance qui décrit d'autres connaissances ou qui concerne ces connaissances; une activité méta-cognitive est une activité cognitive concernant des activités cognitives, etc. Ainsi, par définition une méta-X est une X qui "parle" d'autres X, et peut-être de lui-même; par exemple: une méta-discipline (exemple la sociologie, l'histoire, etc.) est une discipline qui peut étudier d'autres disciplines (exemple les sciences médicales). Autres exemples: on définit l'Internet comme un réseau des réseaux, c'est un méta-réseau; une recherche sur les recherches scientifiques est une méta-recherche... Le préfixe "méta" est apparu, au sens utilisé ici, avec des mots comme méta-langage, méta-logique, méta-mathématique, dans le contexte des recherches sur la formalisation des raisonnements mathématiques.

Cependant, d'autres disciplines, par exemple la Physique, la Biologie, la Statistique, etc. ne peuvent généralement pas fonctionner comme méta-disciplines. En effet, on ne peut pas faire de la "Physique de l'Histoire", si l'on veut être sérieux (mais l'inverse l'"Histoire de la Physique" est faisable). De même, on ne peut pas faire de la "Statistique de la Sociologie" (qui est différente de l'utilisation de la Statistique en Sociologie), sauf si l'on fait n'importe quoi. Par contre, il existe des disciplines qui peuvent s'appliquer à elles-mêmes. Par exemple, on peut faire une sociologie de la sociologie, une histoire de l'histoire, une philosophie de la philosophie, une recherche de la recherche ou méta-recherche (voir mon intervention: "Où est la maladie sociale?", discussion du texte de Boix Mansilla, décembre 2003) ou même une méta-médecine…

Disciplining practices.
Alice ter Meulen
Jan 14, 2004 16:53 UT

Noun and verb meanings are often correlated (program, concert, act(ion), race), as Ian Hacking is pointing out here for the expression 'discipline' (true for Dutch too, though we need an extra bit of verbal morphology). So let's take it to heart that disciplining is a goal directed action scientists are subjected to, intended to result in classifying us into an often rather chaotic academic environment of different disciplines, distributed in space, identified by all kinds of scientific practices, rituals, programs and cultures, as well as in academic budgets. Who are the actors doing this disciplining? Not just we ourselves (if we want to), I'd like to suggest, but more importantly our ancestors, elected politicians, teachers, parents, perhaps also our dependents, our students and our deans, for sure. So what if this disciplinary action results in someone being disciplined into two or more slots, rather than one? Perhaps she is being paid by more employers, teaching to students in different programs of study, lecturing at conferences with disjoint audiences, or writing books attracting a wide readership, including supposedly laymen and the 'general' public. The real issue is: can she personally and professionally survive in our current academic climate? Who is footing her bill in other words? The answer depends on too many individual and often quite contingent circumstances to be of any interest to our current debate. A more useful question to address is how our current institutional practices may be adjusted or improved to facilitate and support disciplinary actions applied to scientists that results in their multidisciplinary classification. Do grantmaking agencies realize that proposals may not necessarily belong to merely one domain of research and hence require different kind of referees who may not even agree? Are criteria for excellence characterized sufficiently flexibly that different traditions of academic inquiry may merge into a consistent and even coherent set of standards? Do scientists who enjoy such multidisciplinary classification feel sufficiently free to do what they feel they need to, as their role model Ian Hacking apparently does? But also, where can they learn to speak a multitude of tongues to address different audiences, that may not necessarily manage to cross disciplinary boundaries with such ease, having been mono-disciplined into a single field of expertise. Obviously, these questions are but a rhethoric way of pleading for programs of study and research that encourage and even exhort their students to acquire a unified perspective on insights from different disciplines and learn to use with equal ease the tools of different academic trades. Cognitive science is one such development that explicity invites different answers to questions of new, substantial research such as the reason why my mom keeps forgetting to set her alarm, or why this kid does not get my frustration in telling him he is STILL eating his porridge. Merely answering that Alzheimer's disease causes neural damage or autism affects those who have no 'theory of mind' will not do, if our ambitions are ever to alleviate these human problems and provide real help to future generations in ways ours is not yet capable of. Getting such disciplinary answers to merge into a

more, why not call it 'holistic' explanatory theory takes a new kind of training, that supports different kinds of understanding by one and the same individual. It takes lots of discipline, in the sense of hard labor, but a much softer disciplining than most of our current budgetarily constrained academic rituals allow for.

Loaded Terms
Julie Klein
Jan 15, 2004 14:08 UT

I agree with Dan Sperber. Thanks to Ian Hacking for a pleasurable set of recollections, all the more enjoyable because I'm working now with several institutions in several countries that do not enjoy complacency in their struggles to promote and legitimate interdisciplinary work. They do not find it "easy" in their local environments or in the dynamics of collaboration. They have not been "lucky." At the same time, when pressed, they could point to precedents in their fields that had elements of the "natural" in the sense Ian is using the term. They would also admit that while obstacles impede boundary crossing, science and research in general advance daily because of conversation and migration across boundaries to address problems and topics of mutual interest. "Discipline" in the institutional sense is not irrelevant but it is not necessarily the space of knowledge production. I would add that many of the descriptions of Ian's work are consistent with definitions of interdisciplinarity. Differences in definition can be traced to not only the nature of the work – a thread of discussion that has come up in previous months – but also the status of institutional structures – whether, to echo Christopher Green, the traffic code were respected or were irrelevant. Answers to the questions of "fit – to echo Christophe Heintz – follow in turn. "Fit," like "natural," is a loaded term.

Some additional définitions and models
Iskender Gökalp
Jan 18, 2004 17:12 UT

Ian Hacking's contribution incites me to cite some additional definitions and models of disciplines and interdisciplinarity:

discipline (lat. disciplina)

"La discipline est la dernière chose qui s'y (Rome) est perdue" Bossuet, Histoire III, 6, *Oeuvres Complètes*, 43 vol. 1815-1819

"Instrument de flagellation, fouet fait de cordelettes ou de petites chaînes dont les religieux et aussi les personnes laïques se servent pour se mortifier ou pour châtier ceux qui sont sous leur conduite" Littré, *Dictionnaire de la langue française*, tome 2, p. 1748

"Disciplines, like nations, are a necessary evil that enable human beings of bounded rationality to simplify their goals and reduce their choices to calculable limits. But parochialism is everywhere, and the world badly needs international and interdisciplinary travelers to carry new knowledge from one enclave to another. Having spent much of my scientific life in such travel, I can offer one piece of advice to others who wish to try an itinerant existence: It is fatal to be regarded as a good economist by psychologists, and a good psychologist by political scientists. "Immediately upon landing on alien shores, you must begin to acquire the local culture, not to deny your origins but to gain the full respect of the natives (...) The task is not onerous; after all, we acculturize new graduate students in a couple of years (...) Learning a new language every decade or so is a great immunizer against incipient boredom".

Herbert A. Simon, *Models of my life*, The MIT Press, 1996, p. 367

A Berkeleyan critique of interdisciplinarity
Marta Spranzi
Jan 21, 2004 17:05 UT

I agree with Ian Hacking that the emergence and stabilisation (with the creation of university departments for example) of a new discipline seems to be the hallmark of real interdisciplinary work. In

this sense examining the same object from different perspectives does not constitute interdisciplinary work, but only multidisciplinary work. Interdisciplinarity is thus a very rare event and, in some sense, can only be defined retrospectively. Interdisciplinarity both denotes the origins of a new discipline, and defines its nature and boundaries. It is also elusive. As Berkeley wrote in criticising Newton's "ultimate ratios of vanishing quantities": what else are these quantities but "ghost of departed quantities"? We may paraphrase Berkeley and say that before the emergence of a new discipline all we have are different disciplines working together, and afterwards all we have are a "ghost", and.. a new discipline. So, there is no getting away from disciplines, it would seem!

But isn't Ian Hacking working (albeit with his very personal style, and probably at the very far edge of this discipline in crisis) within one of such "interdisciplinary disciplines", namely "history and philosophy of science" (HPS) as it emerged in the '70s in the United States?

This rather improbable union of "history" and "philosophy" (with their respective and opposing ends, methodologies, "styles"..) was based 1) on a particular and often implicit theory concerning their common object, the sciences, and 2) the necessary link (in whichever way it may be, and has been characterised), between the mother disciplines, in this case the history and the philosophy, based upon a theory of science. Among the theoretical presuppositions of HPS is the view that science as an empirical practice cannot be analysed without unearthing the rational presuppositions (whatever they may be) underlying its history and development. Another interdisciplinary discipline, "the social studies of science", is based on a different theory of what drives science, namely social interests and conditions, and thus presupposes that the link between history, sociology and anthropology is a necessary and defining link, and not a contingent and discretional way of looking at the same object. It is an open question whether SSS has supplanted HPS, but it is certain that their struggle is justified, insofar as it concerns the definition of the nature of science. "Medical ethics" is not such an "interdisciplinary discipline", insofar as no necessary theoretically justifies link exists between medicine and ethics. It is either a sub-discipline of philosophical ethics or a sub-discipline of medicine.

(Is the emerging "Cognitive science" another such "interdisciplinary discipline"?)

The interesting question on this view is the relationship between the mother disciplines and the new "interdisciplinary discipline".

Rethinking Interdisciplinarity. Emergent Issues

Christophe Heintz (EHESS, Institut Nicod)
Gloria Origgi (CNRS, Institut Nicod)
(*Date of publication: 9 February 2004)*

Abstract: The moderators round up the virtual conference, offering a preliminary assessment of the main themes that have been raised by the papers and the discussions, and open a general debate with the speakers, participants and organizers.

The seminar « Rethinking Interdisciplinarity » takes a break. Starting from April 2003 we have been debating around a variety of issues related to interdisciplinary research, its definition, organisation, assessment and future. Eight texts coming from very different perspectives have been open to discussion in English and French, 281 commentaries have been posted on the site by 65 different discussants. As moderators, we have filtered part of the messages coming from the general public, edited or rejected a number of commentaries that we found not relevant for the debate. Now we pause for the month of February and take the opportunity to sum up some of the themes that have emerged since now, thus hoping to give a chance to everybody to comment on this experience of a virtual forum, and give us a feed-back about possible improvements for the future.

The overall aim of the www.interdisciplines.org project of which our seminar is a part, is to develop specific tools to investigate and promote interdisciplinary research. It creates a virtual locus where researchers from different fields and disciplines can meet. It thus allows discussions that usually take place within the boundary of one's department or discipline to emancipate from such boundaries. Our feeling is that the Web, with its "public face", is an appropriate environment to understand how interdisciplinary projects grow, which interactions characterize these projects, how they acquire authority and have impact on mainstream research. It is so in particular because interactions of the Web leave tracks that can be analysed in order to understand researchers' interests and behaviours. The eight papers that have been put on line during these months have given us the chance to tackle the issue of interdisciplinarity from a variety of perspectives. Nevertheless, browsing the archived discussions and texts, we had the feeling that some main themes have crossed most of the debates:

Definitions of interdisciplinarity

A recurring theme in the discussion has revolved around the meanings of a family of related concepts such as interdisciplinarity, transdisciplinarity, multidisciplinarity, etc. Maybe the most controversial notion has been the one of trandisciplinarity, which has been defined in different ways: Helga Nowotny relates it to the Mode-2 of scientific knowledge production and the transgression of disciplinary boundaries; Basarab Nicolescu and Edgar Morin attach a more specific meaning to transdisciplinarity as related to a more comprehensive framework that transcends the narrow scope of disciplinary research. Julie Klein tracks the history of this concept in her intervention The Transition to Transdisciplinarity. Are these differences just a matter of traditions of thought whose interest is more historical than substantial or do they really define alternative approaches to the problem? Also illuminating has been the history of the notion of 'discipline', which Ian Hacking tracked down through its positive and negative meanings, while Fuller described its socio-political role in the structuring of research institutions.

Interdisciplinarity: science and society

It has been argued that most interdisciplinary research at least in the past 150 years has been carried out in applied contexts (Dominique Pestre; Helga Nowotny). Aims and objects of the most innovative research projects within this period would have resulted from negotiation among a large number of different stakeholders. This means that the language game of science may be too narrow to account for the complex negotiations that different actors such as the private investors, the state or the local communities undertake in order to bring about technological innovation in our societies. The tension

between the need of autonomous scientific standards and the involvement of science in society has been raised at various points in discussion. How can science be “democratically” assessed by citizens? How can we maintain independent criteria of accountability and quality control while “immerging” science in the global functioning of a democratic society? Fuller has pointed out the impediments of the short terms and very local objectives of a research that directly depends on social demands. Nowotny, on the contrary, has argued that laypeople’s participation to research can lead to fruitful research.

Interdisciplinarity and innovation

As Dan Sperber points out in his comment Reservations about 'consistency' and 'balance' some interdisciplinary work results in challenges - sometimes radical ones - to one or several of the disciplines involved. But that is how innovation in science emerges. What are then the relationships between interdisciplinarity and innovation? Is every innovative program in science necessarily interdisciplinary? Is innovation a fundamental criterion to understand and to assess interdisciplinary research? The innovative aspect of interdisciplinary research is made apparent by its evanescent character since successful interdisciplinary research may lead to the constitution of its own discipline.

Practical difficulties of doing interdisciplinary research

While the extent of the specific difficulties of interdisciplinary research can be questioned, it has been nonetheless possible to point out some major problems:

* Language: Each discipline evolves its own jargon. Interdisciplinarity thus requires the appropriation and accommodation of different languages. Communication of interdisciplinary research results may also prove to be difficult since it requires the use of technical terms borrowed to one discipline but that are not well understood by the audience coming from the other relevant disciplines.

* Methods: disciplines are often devoted to their own methods of investigation. This may lead to misunderstanding and opposition as the one illustrated by Bill Benzon about the controversy among anthropologists and psychologists over the book The Geography of Thought

* Institutional constraints: While institutions, often disciplinarily organised, may appear as the first impediment to interdisciplinary research, most authors and discussants have been careful to show the necessity and importance of institutions. Some have pointed out the ability of some institutions to foster interdisciplinary research.

* Cognitive constraints: it is obviously hard to become an expert in two or more disciplines. Yet, deep knowledge of different disciplines is needed for doing genuine interdisciplinary research. Is it possible to develop a truly interdisciplinary methodology? What is the impact of these difficulties on education and, more specifically, on the institutionalisation of interdisciplinary training programs?

Assessment of interdisciplinary research

One of the key issues that have emerged through the seminar, and in particular around Howard Gardner and Veronica Boix-Mansilla’s text, is that of the assessment of interdisciplinary research. To put in Grit Laudel’s words, who has proposed a session on this theme to the EASST conference, “inspired” by our web discussion: Interdisciplinary research is always a new synthesis of expertise. How can the necessary expertise be mounted to evaluate research results? The notion of peer review entails the idea that you are evaluated by someone who works on similar topics. But what if peers in that sense don’t exist? Must evaluators resort to second-order criteria such as counting of articles in high-impact journals, because nobody can judge the content of research? Are there procedures of synthesising peer competence that can overcome the problem? What are the norms that govern the

complex social form of inquiries of contemporary science? Are they inevitably non-epistemic, as Steve Fuller suggests in his intervention: More on Trust? Is it possible a genuine interdisciplinary epistemology?

Autobiographical experience as "practical epistemology"

One of the goal of the seminar was to allow theoretical thinking to be informed by actual experience of interdisciplinary research. Thus Pierre Jacob, Dan Sperber and Ian Hacking have recounted their own experience with interdisciplinary research. The empirical data provided has largely been enriched with discussants relating their own experience and the communication of the results of social investigations from Gardner and Boix-Mansilla, Julie Klein and Grit Laudel. We hope that the gathering of these empirical data as well as the narratives of personal experiences can contribute to further thinking on interdisciplinarity. Do you think that some key phenomena have been left out?

Interdisciplinarity in the information society

One of our objectives in organizing this seminar was to understand how Internet is changing interdisciplinary research. We would therefore like to conclude this text by raising an issue that has been, up to now, not so much considered. Internet has introduced so-to-speak "soft-assembled" online research communities through lists, forums and web sites such as interdisciplines.org, that reduce the cost of organizing interdisciplinary research and the institutional load of "locating" these research networks or groups of in more stable structures. What is the impact of these techniques on the quality of research? The introduction of search engines might also bring changes in the disciplinary structure of science. Indeed, keywords search allow new clustering of documents through criteria – such as occurrence of the given keyword in the documents and its evaluated appreciation by agents of the Web – which ignore disciplines. Keywords thus create a common intellectual arena in which the pooling, combination, selection and recombination of ideas is realized by breaking the standard disciplinary boundaries. What are the consequences of the massive use of search engines on the organization of research? Are the content-driven information assemblies generated by search engines the beginning of a entirely new method of classifying knowledge domains?

These are just some of the issues that have been raised through the seminar. We hope that they can a starting point for a general discussion about the contents, the format and the possible developments of our seminar. In the meantime, thank you for the lively discussions until now. We have learned a lot and really hope that you have enjoyed to share all this together.

Gloria and Christophe

Discussion

Interdisciplinary "Information"
Julie Klein
Feb 10, 2004 17:22 UT

Thanks, first, to Gloria and Christophe for summarizing some key threads in our conversation to date. Each item merits conversation, though I'd like to comment on one in particular – how electronic access and codification is changing the search for interdisciplinary knowledge and information.

Gloria and Christophe mentioned the role that Keyword Searching can play. In my online course on "Interdisciplinary Problem Solving," I developed a two-week Interdisciplinary Database Searching Module for my students. It employs not only Keyword Search but other strategies too. I have used this method to teach not only students but researchers as well (in the past face to face and now online).

The first week I teach them how to “navigate” electronic resources with a self-guided e-tour of the University library and electronic resources linked through the library. After a general tour of the website, we concentrate on “Frequently Used Databases,” identifying the most relevant resources for their topics (which range from social and environmental problems to workplace issues of a social or technical nature). They learn how to use new resources that take them as close as possible to full text that can be printed or downloaded. I also guide them to new clustered subject resources and guides the library staff has established in particular knowledge domains (and counterpart library staffs at other universities).

The second week we refine interdisciplinary database searching strategy with three strategies: keyword searching, Boolean logic, and federated searching. They read my online lecture on “Interdisciplinary Database Searching” then practice those three strategies. I contextualize the challenge by synthesizing new thinking in the literature of Library and Information Sciences about identifying and locating appropriate resources. We “walk” through sample databases together, practicing how to use Keyword Searches and refine their search strings with Boolean logic. We then practice “federated searching” by working with information tools that allow simultaneous integrated searching of a variety of databases. I highlight the growing number of such tools, targeting the most pertinent ones for their topics.

Libraries and their tools are changing quickly, as librarians grapple with serving the increasing interdisciplinary information needs of their clients. Federated searching is becoming a new basic literacy.

Is federated search fostering interdisciplinarity?
Christophe Heintz
Feb 11, 2004 16:02 UT

It’s good news to know that Julie Klein takes Information Retrieval Technology seriously.

Interdisciplinary research is certainly giving a hard life to librarians. There is a kind of paradox in this activity of classifying documents in the best satisfactory way, while interdisciplinary research always renders the classification obsolete.

Federated searching seems to provide librarians with new means for satisfying the interdisciplinarians.
I say ‘seems to’ because search engines allow new clustering of information. They display information in a way that is un-biased by disciplinary boundaries. However, it remains an empirical question to know whether these new ways to display information do *actually* foster interdisciplinary research:
I would love to hear about someone being thrown into interdisciplinary research after discovering, via federated searching, a highly relevant document coming from some previously unknown discipline.

Definitions of interdisciplinarity
Grit Laudel
Feb 11, 2004 6:00 UT

Like Julie I admire the work of Christophe Heintz and Gloria Origgi in summarising the "Rethinking Interdisciplinarity" discussion. I would also like to concentrate on only one point of the summary, the definitions of interdisciplinarity.

For me it is quite frustrating to have the discussions about different definitions of interdisciplinarity going on for more than thirty years now without any progress. A definition is a convention about the meaning of a specific term. Some conventions seem to work better than others because they relate better to the existing body of knowledge. But finally it is the members of the scientific community who agree to define and to use a term in this or another way. Why can’t we as members of the community that investigates interdisciplinarity attain such an agreement in our scientific discussions?

In our previous discussions, the terms seemed to be understood in the following way: "Interdisciplinarity" characterises research actions that combine knowledge from more than one research field. "Multidisciplinarity" characterises research fields, journals, groups of researchers, organisations etc. that comprise different research fields. Multidisciplinary fields or organisations can comprise purely disciplinary research processes. The terms are problematic because in fact we don't refer to scientific disciplines, but to the much smaller units of specialties (see Chubin 1976 who referred to disciplines as teaching units and specialties as research units). But I will not suggest a new term because we already have enough of them to confuse each other. As Christophe und Gloria summarised it, there was no agreement about the third term "transdisciplinarity". I would prefer to use "transdisciplinarity" for interactions that transgress the boundaries of the science system (as it was used in the Nowotny paper).

However we define them, most of all I would like to have an agreement about terms instead of the present postmodern arbitrariness.

What is at stake when we define?
Christophe Heintz
Feb 11, 2004 15:51 UT

I suppose indeed that many of us are being frustrated by terminological quibbles. And yet, a lot is at stake when we define terms such as 'interdisciplinarity'.

Funding, for instance. Or research opportunities.

If research managers say: "Let's privilege interdisciplinary research", then no doubt that everybody will want to claim that his work is out-and-out interdisciplinary. Grit points out that there is some space for interpreting 'discipline' as either referring to broad fields of knowledge with established institutions, such as Physics, or as referring to any relatively bounded set of knowledge, such as model theory, a sub-branch of mathematical logic. At each extreme, the word looses its meaning, evaporating in some 'unity of science' ideals or made so subtle that any work elaborating on more than 2 ideas would qualify as interdisciplinary. Finding the right middle, here, seemed to be a purely political decision.

On the other hand, scientific definitions imply ontological commitments. Thus, defining 'interdisciplinarity' is already making statements about the organisation of science. Fuller's text, I think, is a case in point. He says:

I do not see interdisciplinarity as simply a call for open borders between disciplines […] rather, the persistent need for interdisciplinary solutions to disciplinary problems brings out the inherently conventional character of disciplines. […] Disciplinarity should be treated as a necessary evil of knowledge production.

It may also be reassuring to remind that even so serious a discipline as Mathematics spends some time arguing about definitions. Imre Lakatos gives a nice illustration of this fact in his 'history' of the term 'polyhedra' (*Proofs and Refutations*).

The Sociology of Definition
Julie Klein
Feb 12, 2004 17:46 UT

I share Grit's frustration about needing to keep clarifying definitions of interdisciplinarity. I wouldn't characterize the present situation as "postmodern arbitrariness," though (in no small part because I don't view postmodernism and arbitrariness as synonymous).

Definition is, as Grit rightly notes, "a convention about the meaning of a specific term." Conventions differ across communities of practice, but a degree of consensus has emerged in

and across particular domains around four primary terms (disciplinarity, multidisciplinary, interdisciplinarity, and transdisciplinarity).

Disagreements arise for several reasons. Certain connotations serve the interests of a particular research or education community. Many people using the keywords are ignorant of the literature on interdisciplinarity that registers common assumptions comprising a consensus. The needs that interdisciplinarity serves are widespread. And, the community that investigates interdisciplinarity is not one community but several.

Graduate Students Teaching Interdisciplinary Courses
Maricarmen Martinez
Feb 17, 2004 22:41 UT

Dear Colleagues, The Interdisciplinary Program in the Humanities at Florida State University offers over 60 undergradute sections of several interdisciplinary courses. Besides sharing the administration of this Program, my job is to train our grdaute students to teach in an interdisciplinary fashion. Are there any recommendations on how to teach graduate students to become better interdisciplinarian teachers?
Thank you very much indeed! Sincererly, Maricarmen Martinez, Ph.D Assistant Director The Interdisciplinary Program in the Humanities FSU

Resources for Teachers
Julie Klein
Feb 18, 2004 13:29 UT

In answer to Maricarmen, the Association for Integrative Studies has produced resources for interdisciplinary teachers. You can find them listed under Publications on their website (http://www.units.muohio.edu/aisorg/).

Ricerca
Clotilde Lampignano
Feb 18, 2004 14:55 UT

Vorrei fare un commento al commento su interdisciplinary information di J. Klein.. L'A. afferma come l'elettronico accesso e la codifica stia cambiando la ricerca nei confronti della conoscenza e informazione interdisciplinare. Noi pratichiamo una ricerca su mezzi che ci sono dati dalla macchina e su database indagati dai motori di ricerca, su una quantita di informazioni già classificate in questo piuttosto che in un altro modo, che poi noi possiamo ricercare in questo o quel modo.. I o penso che la novità sia data soprattutto dallo strumento elettronico, cioè dalla grande quantità di informazione velocemente reperibile ed anche mirata alla determinata ricerca, dagli elementi quantità e pertinenza, che pur presentati sul mezzo in maniera ordinata verticalmente, sono tuttavia possibili alla ricerca orizzontalmente. Penso che qualsiasi persona abbia fatto uno studio, ricerca, tesi, abbia comunque usato interdisciplanarietà sia nel consultare più libri di discipline diverse, o di campi diversi, ad es. nelle letterature, si sono affrontati gli studi degli scrittori da più discipline, la storia, la filosofia ecc.