Why Rethink Interdisciplinarity? Dan Sperber (CNRS, Institut Nicod)
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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.

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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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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.