
Interdisciplinary Research and Information Overload

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ABSTRACT

INFORMATION OVERLOAD IS A PROBLEM for all those involved in research but seems especially threatening to interdisciplinary research. Teamwork supplies the remedy, but most research in the social sciences and humanities is done by scholars working alone. That fact limits the scope for interdisciplinary work. In this article, we examine several ways in which actual and potential overload affects research choices for the solo researcher, paying special attention to the creation of ad hoc idiosyncratic specialties. As a matter of policy, should solo interdisciplinary work be encouraged? A strong social preference for interdisciplinarity might discourage solo practice as just another example of the huge disparity between individual and collective capacities.

TYPE OF OVERLOAD

Everyone engaged in research is aware of the problem of information overload. It is always a threat if not a reality. It is perhaps most familiar as a problem of maintaining currency. A basic requirement for the maintenance of expertise, and of a reputation for expertise, is that of staying current—i.e., keeping up with what other research workers are doing that is relevant to one's own work (Wilson, 1993). One wants to be able to claim intellectual command of a field, and this requires deep and wide knowledge of what has been done and is being done by others in the field. Just how wide and how deep one's knowledge must be is not something on which there are (or could be) any precise rules, and it is very clear that wide differences in the scope of current knowledge will be

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found among different people working in the same area. But the requirement is there and ordinarily means that one must devote time and effort to reading what others have published or are going to publish or have otherwise communicated. How much time is needed will vary with the size and level of activity of the field—a small field of slow producers will present no problem of keeping up; a large and very active field of fast producers may tax or overwhelm one's capacities. Specialization in research is partly a response to, and defense against, overload—i.e., one adjusts the size of the field over which one hopes to maintain expertise so that the burden of keeping up is manageable. The field cannot any longer be the size of a conventionally recognized discipline; even in philosophy, not an especially populous discipline, it has long been impossible for American philosophers to keep up with what their colleagues were writing, says Nicholas Rescher (1993), and philosophy “which ought by mission and is by tradition an integration of knowledge, has itself become increasingly disintegrated” (p. 730). As time goes on, one may discover the necessity of narrowing one's scope: “Every scientist who has been in business for a long period knows perfectly well that in order to remain an expert in some area he has to cut down the width of his interests more or less continuously” (Bar-Hillel, 1963, p. 96). This is by no means the only thing that limits the size of an individual researcher's area of expertise, but limiting width of interest is definitely unavoidable and increasingly important.

A different problem of overload arises in the context of particular inquiries or research projects. Here the problem is the overabundance of available data relevant to the particular inquiry—i.e., experimental results, field observations, historical records, statistical and survey data, and the like. Data may be scanty in one case but torrential in another to the point that no one could hope to analyze and evaluate them all or integrate them into a coherent picture, even supposing that there were no problems of locating and assembling them in the first place (Wilson, 1994). The kind of overload involved in maintaining currency we might call “upkeep” overload—the price of maintaining the intellectual capital that is the research worker's chief asset; the kind of overload presented by information relevant to a particular inquiry we might call “task” overload (the two kinds will frequently overlap).

In both cases there are a variety of ways of coping with overload. A certain amount of upkeep overload may be accepted as normal, though inevitably leading to nonuse of relevant, but less than top-priority, information. Task overload can be dealt with by the adoption of strategies of inquiry that allow the elimination or ignoring of huge categories of relevant information (Wilson, 1995). In both cases, one consequence of overload is that relevant information does not get used. Whether or not this is a problem, it does seem a clear failure to meet conventional stan-

dards of rationality, which call for the use of all available relevant information. The conventional understanding is reflected in statements like: "These estimates are rational, in the sense of taking account of all available information" (Elster, 1989, p. 109) or: "The common understanding [of the term 'rationality'] is ... the complete exploitation of information, sound reasoning, and so forth" (Arrow, 1987, p. 206). So overload is of theoretical, as well as of practical, interest; one cannot simply disregard the fact of large-scale ignoring of relevant information if that is what happens in research. Of course it is of both practical and theoretical interest to library and information studies, where the chief criterion of success in information retrieval has been the provision of all and only relevant information, a goal that loses some of its allure in the face of persistent problems of overload.

OVERLOAD AND TEAMWORK

How does the matter of overload affect the possibility and the actuality of interdisciplinary research? Interdisciplinarity must, at the very least, involve the use of the knowledge and skill involved in two specialties from different disciplines, and for the moment we will assume that the interesting cases are those involving the application of expertise in the two specialties, not an insider's knowledge of one specialty and an outsider's knowledge of a second. Is there something about interdisciplinary work that raises especially troubling problems of overload? There is a quick answer to this question: it all depends on how narrowly the burden is concentrated. A group of workers can easily do what a single individual would find impossible. Think of the process of drawing up requirements for a research project—i.e., skills required, bodies of knowledge needed, as well as facilities and equipment needed. There is no theoretical limit to the number and variety of specialties that might be specified in the cognitive budget, and no problem of bringing them to bear on a single project if each different specialty can be contributed by a different individual. You can add an ethicist if you need one, an expert in witchcraft, a deconstructionist, and a risk assessor. You can add information specialists to search the literature, and literature specialists to serve as designated readers, reporting to others on what they need to know of the literature. Each specialist may continue to face the problem of upkeep overload, but the problem need not be exacerbated by the social situation of working on a team; indeed it may well be mitigated (if for instance there are others to serve as filters to screen out literature one need not bother to examine). And while the problem of task overload may be horrendous—if, for instance, the task is to explore real social problems and find plausible solutions—still it can be treated as a collective problem, not an individual one. So the conduct of research by teams or groups is a way of increasing the amount of expertise and information that can

be brought to bear on a problem without increasing the burden of overload on the participating individuals. This is not to avoid the problem of information overload entirely, but at least it makes it possible to do what overload would otherwise make impossible.

INTERDISCIPLINARY WORK AND THE SOLOIST

The place to look for the real trouble in interdisciplinary research is in the work of the lone researcher—the soloist. While research and development in natural science and technology is increasingly done by groups, solo research still predominates in the humanities and social sciences. Research in natural sciences and applied fields is increasingly collectivized or industrialized (Weinberg, 1972; Ziman, 1981, 1983, 1987), but humanities and social sciences are still predominantly areas for the cottage industry—i.e., inexpensive small-scale production involving little or no staff or equipment or logistical support. This may be the chief reason why interdisciplinary research often seems so problematic in the humanities and social sciences: it is problematic where the organization of research, the mode of production, makes it so.

The many specialties in the various social science disciplines are all trying to produce information relevant to the understanding of concrete social phenomena—and they do not always fail. But separately they at best elucidate a part or side or aspect or feature of some element of the social world, and it is not hard to see why a student of society would aspire to a better understanding of society than can be got by work within a single specialty. “There is scarcely an individual phenomenon or event in society with which we can deal adequately without knowing a great deal of several disciplines...” (Hayek, 1956, p. 464). Understanding social reality requires crossing or ignoring disciplinary boundaries. The problem is that the number of specialties contributing relevant information is likely to be very large, and the quantity of information provided far beyond the capacity of any individual to absorb and use—in a word, overload. Staying within disciplinary boundaries means giving up trying to understand concrete phenomena; not giving up means facing intractable overload. Compromise is unavoidable and may easily recede from consciousness; Hayek (1956) suggested that: “We are probably so used to this impossibility of knowing what we ideally ought to know that we are rarely fully aware of the magnitude of our shortcomings” (p. 464).

Cultural studies are in a similar position, as David Damrosch (1995) illustrates:

I spent twenty years, beginning in college, trying to learn everything I needed to know to work on the things I wished to study. The problem was that I was loyal to too many interests, in several ancient and modern literatures, in literary theory, in biblical studies, in history, archaeology, anthropology, and art history. A reasonable enough

constellation of interests, but in the advanced state of modern scholarship it is inherently unmanageable, if one wants to be seriously engaged with scholarly work...I have learned that I do, sometimes, need to sleep. Worse, my memory simply isn't good enough to hold in mind everything that would be necessary for full-scale multidisciplinary work. (pp. 15-16)

Damrosch found that he had set himself too ambitious a task. Overload did turn out to be an insurmountable problem.

The obvious fact that there are limits to an individual's capacity to utilize information does not, however, imply that interdisciplinarity is ruled out for the solo practitioner. Even though individual capacity is limited, no scholar or scientist need stick to just one specialty, but rather they may simultaneously work in two or more (and of course may move from one to another—i.e., a serial specialist). There is no standard size of specialty (for that matter, there is no agreed way of identifying and distinguishing specialties), and a person may be perfectly capable of maintaining expertise in more than one though not in dozens. Is there reason to think that it is harder to maintain currency in two specialties if they are in different disciplines than if they are in the same discipline? Would it be harder to keep up with streams of literature, one in sociology and one in political science, than to keep up with two streams of literature within sociology? There is no obvious reason to think so. It may be harder to *attain* expertise in a new specialty if it is in a new discipline (new to the learner). However, given that one has somehow attained expertise in specialties in different disciplines, the fact that they are in different disciplines does not itself imply anything about how hard it will be to keep up. In any given case, overload may be a problem, but then the same may be true for maintaining currency in multiple specialties (or even a single specialty) within a single discipline.

So overload does not rule out the kind of solo interdisciplinary research that requires expertise in at least two specialties in different disciplines, if we consider only the case of the research worker who has somewhere and somehow already achieved expertise in the different specialties and consider only the problem of keeping up (task overload can arise anywhere). But it is different when we consider the worker who proposes to enter interdisciplinary work, say in mid-career, by acquiring expertise in a new specialty in another discipline and using it in conjunction with already acquired knowledge. People do change disciplines in mid-career and, if one can change disciplines, one can add them too. This is not a quick and easy task; entry into a specialty is harder than keeping up in it once one has entered, and entry into a specialty in a new discipline is likely to be very much harder than adding a specialty in one's home discipline. It will also take time—from months to several years (see Ziman, 1987, for relevant information on this and closely related matters). Still,

it is certainly not out of the question, and so it is worth considering what makes it more or less difficult to do.

ENTRY BARRIERS

There are some obvious features that affect ease of entry into a specialty (remember that we are still thinking of the attainment of expertise as it would be judged by already established practitioners). First is the extent of the prerequisites for gaining competence in the new specialty—the background knowledge needed by any competent worker in the new specialty and the tools to be acquired elsewhere and brought to work in the specialty. If one does not already satisfy the prerequisites, entry may be almost out of the question—e.g., if work in the specialty assumes scholarly knowledge of several ancient languages, those without prior knowledge will usually find entry too costly. (Mathematics can serve as a similar impassable barrier; this is one reason it is generally easier to move from a hard to a soft specialty than vice versa.) But not every specialty has demanding prerequisites.

Other factors influencing an individual's entry into a new specialty are the age and size of the literature of the field. The entrant has to catch up not only with the current practitioners of the specialty but also with the literature, and that will be easier if the specialty is new and has practically no literature yet. (The physicist Eugene Wigner [1950] wrote that: "Today, we are neglecting the theory of solids in which a student has to study perhaps six hundred papers before he reaches the frontiers and can do research on his own; we concentrate instead on quantum electrodynamics in which he has to study six papers" [p. 423].) Menard (1971) discusses at some length the barriers to entry into new and old fields in terms of the size of the literature to be worked through. Next is the sheer intellectual difficulty or complexity of the new specialty itself; it may be, as a critic said of research in diffusion of innovations, a mile wide and an inch deep, or it may be subtle and intellectually dense, requiring huge investments of time for mastery. (Ian Stewart [1992] is critical of applied mathematicians in perturbation theory for not adopting a technique developed in mathematical logic but then notes that the new technique requires "a distinctly different cast of mind, a new style of thinking that takes several years to get used to" [p. 114]. Little wonder they are not rushing to adopt it.) A further feature is the degree of codification of the field—i.e., whether text books and serious expositions of an agreed body of knowledge will bring one up reasonably close to the research front, or whether one has to organize knowledge for oneself on the basis of the original literature of the field. Given that work at the front is disorganized in all fields (Cole, 1992), the codified field (roughly, the hard as opposed to the soft field, often but not always the natural and formal science as opposed to the social science and humanities field) offers less of a burden to overcome.

All of these are features of the new specialty that affect the likelihood that overload will prevent entry. This is task overload; performing the task is, of course, at the same time investing in new intellectual capital, and the size of the task is a measure of the size of the investment. If one will have to spend a great deal of time acquiring prerequisite competencies—i.e., acquiring a new style of thought, organizing the content of the specialty for oneself, and/or catching up with a huge literature—the chances are good that one will consider alternatives to the acquisition of a new area of expertise. Uphill work like this would require special inducements; the natural gradient or direction of movement will be toward new fields without heavy prerequisites (or with prerequisites already met) that are relatively easily detached from other specialties in their discipline. But if what one wants is to work in a specialty that unfortunately has a big disorganized literature, heavy prerequisites, and so on, all is not lost, for there are alternatives—many of them—to an attempt to gain full expertise. One alternative is collaboration. Damrosch (1995) devotes a volume to arguing the merits of collaboration in the humanities and social sciences in the face of an “archaic hyperindividualism” (p. 7), an entrenched prejudice against anything except lonely research. Collaboration produces a small team and has the expected effect of reducing the pressure of overload and increasing the size of the field that can be worked. But there are still numerous alternatives for the soloist. One of these is ad hoc interdisciplinary specialties.

AD HOC INTERDISCIPLINARY SPECIALTIES

Rather than attempt to become an expert in an established specialty in another discipline (as expertise would be judged by the practitioners already in the field), one can try something else. One can design a new ad hoc idiosyncratic interdisciplinary specialty, with a specially delimited subject matter and specially formulated conceptual and theoretical basis, research questions to be pursued, techniques and methods to be employed, type and style of result to be aimed at. In practice, one is perhaps more likely to develop such a specialty piecemeal and instinctively rather than as a result of deliberate planning, but it is the sort of thing that could be planned. The new specialty may incorporate fragments or major fractions of existing specialties but need not correspond to anybody's understanding of a pre-existing specialty. So, for example, one could be “drawing on work in psychology, cognitive science and history of science as well as epistemology and philosophy of science” (Solomon, 1992, p. 453, describing her own work). Some of the prerequisites associated with preexisting “outside” specialties may be skirted and simply worked around by deliberately setting out to do what can be done with big gaps in knowledge. (One of Bazerman's subjects [Bazerman, 1988, p. 244] says that his field is so interdisciplinary that he inevitably must live with

vast areas of "relevant ignorance," and this may be accepted as the price one pays for easier and cheaper employment of a specialty.)

The full design specifications of the new specialty would include a policy on information use covering types of data to be used as evidence and bodies of already existing literature to be relied upon. The policy might direct one to discard or ignore much of what has already been done in one or more of the fields from which the new specialty is constructed. Large literatures may be cut down drastically: one may ignore the past, ignore "foreign" contributions, ignore contributions from identifiable schools and traditions of thought (e.g., no Marxists, no deconstructionists, no positivists, etc.), ignore work done with certain techniques or in particular styles or with particular approaches. Creation of a new ad hoc specialty may be the occasion for an idiosyncratic definition of "what is living and what is dead" in the specialties involved.

Whether creation of such an ad hoc idiosyncratic specialty is likely to be acceptable to others or even possible at all clearly depends on the intellectual environment. If it requires resources under others' control (e.g., money, research facilities), it will not be possible at all unless those others are persuaded that it is more desirable than alternative uses of the resources. This is one basis of social control on knowledge production. Even where resources are no impediment, intellectual acceptance may be; editors must be persuaded to publish, reviewers must not denounce the results. Acceptance may be denied to work perceived as heterodox or nonstandard—not just of poor quality, but of the wrong sort entirely—a second basis of social control. Both bases of control are generally stronger in the natural sciences than in the social sciences and humanities: research in the former is more dependent on expensive facilities and equipment, and agreement about what counts as acceptable work is generally greater. Stephen Fuchs (1992, 1993) argues plausibly that, in weakly controlled fields of inquiry, it is particularly easy to create new specialties; such fields tend to be fragmented, and further fragmentation is not resisted. As it happens, the weak fields he is thinking of are the social science and humanities fields where soloists predominate. If this is right, creation of ad hoc interdisciplinary fields is easiest exactly where it is most wanted to satisfy the soloist.

Even if social controls permit creation of such new idiosyncratic specialties, work in any particular new specialty may be rejected by others as a caricature or desecration, as involving distortions or misunderstandings of ill-assimilated specialized work, and so on (compare Klein, 1990, p. 88 on standard criticisms of borrowing), or the new specialty may turn out to be widely appreciated but essentially inimitable, remaining a unique soloist style, or it may actually attract imitators and grow into a new establishment.

Such specialty creation is not as radical as it may sound. Indeed, the world of research may actually be filled with unrecognized or unacknowledged idiosyncratic specialties, developed quite unintentionally in the

course of accumulating whatever special knowledge and skills seem to be needed to do the job one has undertaken. And an ad hoc interdisciplinary specialty will differ only in degree from a single-discipline specialty modified by the import of concepts, tools, methods from outside, or partly transformed by outside influence. Modification of research practice by import and under outside influence goes on all the time in research; it might even seem that every specialist would constantly be open to influence from outside and eager to import useful tools. But specialists differ enormously in their interest in, and openness to, influences from other specialties in the same or in different disciplines, and their practices will reflect such differences. In fact, we can imagine—and find—specialists who practice what we can call informationally closed specialization—i.e., ignoring everything done outside the specialty itself, confining one's information intake to that produced by fellow specialists. The very idea of such an informationally closed practice may seem perverse, but we have no reason to think that it cannot exist and yield valuable results (for a different view, see Wilson, 1996). Microanalysis of the information use component of research practices would reveal a continuum of types of solo practice ranging from the narrowest informationally closed specialization to the full interdisciplinary practice based on expert knowledge of specialties in two or more disciplines, with a huge range of intermediate types representing greater or lesser isolation from, or involvement with, other specialties and other disciplines. This may look like a source of wild variation in an otherwise standardized and stabilized world of specialties, but the standardization and stability are mirages. While specialties no doubt differ in this regard, any specialist is likely to be more concerned with some parts of his specialty than others, more interested in some techniques and concepts than others, more convinced about some outcomes of research than colleagues are, and so on. And each specialist will bring to the work a unique repertory of intellectual resources (Ghiselin, 1989). We should expect to find that the practitioners of a single specialty all define their specialty somewhat differently and practice it somewhat differently. Just as each individual's language constitutes an idiolect slightly different from everyone else's, so each specialist's expertise constitutes a research idiolect slightly different from everyone else's. As for stability, John Ziman (1985) has emphasized that "at the subfield level, there are no really stable specialties at all...all is in flux" (p. 12).

THE SOLOIST AND KNOWLEDGE POLICY

With all the variety of practice, the crucial fact remains that the soloist is limited—whether practicing within the boundaries of established specialties or working across boundaries—by what single individuals can manage. The simple desire to do interdisciplinary research does nothing to increase one's capacity to utilize information or to lessen the burden

of overload. Research whose success requires the application of multiple specialized bodies of knowledge and skill and the utilization of vast quantities of information simply has to be done by teams, not by soloists. Serious large-scale interdisciplinary work is not for the soloist.

Still one could ask whether, as a matter of social policy on knowledge production, a soloist's effort to do interdisciplinary work is always to be encouraged over work within the limits of a single specialty. Perhaps single specialists should always join teams, and solo practice be reserved for interdisciplinary workers. (Granted that there is no such thing as an articulated social policy on knowledge production, there certainly could be; science policy is not an unfamiliar idea, and the social policy in question would be a generalization beyond science narrowly construed. See Kitcher, 1993, especially chapter 8, "The Organization of Cognitive Labor," and Fuller, 1993.) Is small-scale interdisciplinary work, of the size of which the soloist is capable, to be socially encouraged? Is full expertise-based interdisciplinary work to be considered more valuable than informationally closed work? And is this work more valuable than the other varieties of practice we have roughly sketched? Should solo interdisciplinary work be encouraged as a matter of policy by those in a position to affect the career choices of students and beginning researchers? Should educational institutions, foundations, and professional societies do what they can to encourage interdisciplinary work and discourage informationally closed solo practice? It is clear, for instance, that if such work is to be encouraged, students must be urged to start early, for we have seen how difficult it can be to add a new specialty in mid-career. A real social preference for interdisciplinary work could lead to a real policy with clear consequences for action.

But a real social preference for interdisciplinary work might instead lead to the end of private practice in research and the institution of teamwork everywhere. If one takes seriously that putative requirement of rationality for the use of all available relevant information, teamwork becomes unavoidable, for individuals cannot meet the requirement. (As far as the individual is concerned, it cannot actually be a requirement—one cannot require the impossible—but at most an unrealizable ideal, a "regulative ideal" of the sort proposed by Immanuel Kant that, though unrealizable, still provides an orientation for practice [Emmet, 1994, pp. 16-17].) Rather than encouraging soloists to do interdisciplinary work, we would urge them to join teams, contributing whatever knowledge and skill they happen to have to joint projects.

As we cannot realistically expect such a drastic reorganization of research in the social sciences and humanities, might we not still argue in favor of a general preference for interdisciplinary work by soloists on the grounds that it goes in the right direction—i.e., toward increasing utilization of relevant information and other cognitive resources—even

when it fails to go all the way, as it always must if done by a soloist? But "going in the right direction" may not be the best plan; success may call for indirection. The great successes of the natural sciences have been based not on scrupulous utilization of all available relevant information about natural phenomena but on systematic simplification, idealization, abstraction, approximation, and the concomitant ignoring of very large quantities of admittedly relevant information (Wilson, 1995 and works cited there). One may well want to reply that what counts as success in the humanities and in at least parts of the social sciences is so different from what counts as success in natural science and engineering that strategies of inquiry successful in the latter cannot guide the former. This might not be enough to warrant making the encouragement of maximum solo interdisciplinary work a social policy. Even if we praised solo interdisciplinary work for having its heart in the right place, we might hope for greater success from a system of inquiry in which individual workers narrowed their scope while contributing to a collaborative result beyond the capacity of any of them singly.

That sort of system is, in any case, the actual, the unavoidable one; we have been arguing at the margin over a little more or a little less. The gross disparity between individual and collective scope in research is nothing special but simply another manifestation of the general situation of the individual in the face of the collective cultural product, much emphasized long ago by the sociologist-philosopher Georg Simmel. Simmel noted that: "No cultural policy can eliminate the tragic discrepancy between objective culture, with its unlimited capacity for growth, and subjective culture, which can grow only slowly" (Simmel, 1976, p. 251) and described the "typical problematic situation of modern man" as that of "the feeling of being overwhelmed by this immense quantity of culture, which he can neither inwardly assimilate nor simply reject, since it all belongs potentially to his cultural sphere" (Simmel, 1976, p. 254). That is essentially the situation of the individual research worker in the world of research.

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