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AUTHOR   Milburn, Thomas
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ABSTRACT
        The problems mankind faces are of such overwhelming
importance that it is easy to see why we are interested in
integrating the academic disciplines to study war, violence, and
social change. Could not the behavioral sciences, properly mobilized,
enable us to reduce the probabilities of war and violence, and make
social change more tolerable? Toward these ends, the integration of
academic disciplines, and the cooperation of these disciplines with
practitioners (educators) should prove worthwhile. There are several
obstacles to interdisciplinary work: 1) the anxiety-provoking nature
of this area of research, which causes people to withdraw to safe
problems in their own disciplines; 2) the tendency to regard only the
things in one's own discipline as problematic, and those things
outside as givens; 3) the defensiveness and jealousy that often
exists between disciplines; and, 4) each discipline having its own
distinct methods. One way to overcome these obstacles is to be aware
of the conceptual convergences that exist across disciplines. These
convergences set the stage for cooperation. There are new methods and
concepts that make cooperation easier, e.g., the man-computer
simulation to study conflict. It is not obvious how such projects can
best be organized, and we need to collect critical incidents
concerning successful attempts at cooperation. (Author/JLB)

PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES

IN THE STUDY OF

WAR, VIOLENCE, AND SOCIAL CHANGE

THOMAS MILBURN

Department of Psychology
DePaul University
Chicago, Illinois

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PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES
IN THE STUDY OF
WAR, VIOLENCE, AND SOCIAL CHANGE

Thomas Milburn
Department of Psychology
DePaul University
Chicago, Illinois

It is not too difficult to see why we might be interested in integrating the academic disciplines in the study of such phenomena as war, violence, and social change. The topic is unbelievably pertinent to problems which confront and imperil civilization and humanity. These categories are not mutually esclusive; e.g., wars include violence although personal and small group violence do not imply war. Social change contributes to the frequency of war and non-war violence. Wars and violence may produce social change. Consequent changes in societal values may reduce inhibitions toward violence within or without the societies in which they occur.

Wars grow more expensive, especially major ones. The economic costs of World War II were an order of magnitude greater than those of World War I (Sprout, 1963). In a lucid, cogently reasoned essay, Harrison Brown (1966) has argued that because of ecological skimming of our planet's natural resources, the birth and rise of technological civilization after a World War III could not reoccur. It is conceivable his argument is optimistic: maybe not only technological civilization but also mankind would be too changed to rise again after another major war.

One small, undeclared war in southeast Asia costs well over thirty
billion dollars a year, has taken fifty thousand American lives alone,
and may be producing profound value changes at home. It is a principal
cause of inflation, and by the ecological and capital drain it has put
upon U. S. resources, it has contributed to urban blight and to reduced
rates of the accumulation of capital and reduced funding of education
and research. Some argue that it makes us accept violence more readily.

Violence within countries now appears as one indicator of the
prevalence of the social system's pathology, akin therefore to readings
of the pollution levels of air or water, of levels of radiation, of
levels of hunger, or of the frequency of various disease syndromes.
The Feierabends (1966) have discovered relations between reliable
estimates of need to achieve within a society and increased frequencies
of violence when achievement needs do not lead to increased rates of
modernization. Increased rates of violence within societies may reflect
the dissolution of emotional bonds between men and the social insti-
tutions - family, church, lodge - of society together with the bonds
to formerly valued norms - rules, laws, and moral values. Predatory
lethal violence to strangers, followed by neither strong remorse nor
suicide, is greater in the U. S. than in all other developed countries of
the world. We also excel substantially for deaths by violence to family
members and friends (West, 1966).

The problems represented by the phenomena under discussion are
thus substantial ones. We realize that physical science has been
remarkably successful in producing vast technological change. Could not
the social and behavioral sciences, properly mobilized, achieve equiva-
lent wonders in enabling us to reduce the probabilities of war and

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widespread violence? And make social change more tolerable? If the physical sciences can harness natural forces, cannot the social sciences help us control dangerous social pathology?

Toward those ends, the integration of academic disciplines might prove most worthshile; and maybe the time for more interdisciplinary cooperation is not far away. There already appear to be conceptual convergences across the disciplines: e.g., although the hypothesis that frustration produces aggression has recently suffered some truncation by psychologists, it also has been utilized by political scientists with some success (Feierabends, 1966). And there are many others: the generality of social scientific hypotheses and concepts advances.

However, it is from the differences among the academic disciplines, through their diversity and heterogeneity that mankind may produce the most gain. Each discipline represents a separate pattern and range of human resources - separate diversities of skills, knowledge, talents, and methods. It is true, moreover, that complex, many faceted problems yield more readily to a variety of abilities than to a single kind. Some kinds of problems are solved more readily by heterogeneous groups than by individuals.

The utilization of several research strategies in concert, e.g., surveys, content analysis, field work, the clinic, computer simulation, and laboratory experimentation, promises to reduce the error or un-reliability of measurement associated with any particular method. Technically put, the use of several methods to test a set of hypotheses reduces method error variance (Campbell & Fiske, 1959). What new knowledge emerges is therefore likely to prove both more certain and more general than what might have been produced by a single discipline.

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Aside from the fact that convergent findings indicate a stronger, because diverse, evidential base, several methods together offer a strong basis for convincing policy makers of the truth of empirical findings. Policy makers and other practitioners tend to have been educated in only one of several disciplines and to respect some forms of inquiry and evidence far more than others. The physical scientist respects mathematics and systems approaches; historians appreciate historical data and analogs. In general, each man respects forms of inquiry and evidence which he best understands and with which he is most familiar. Multiple research strategies are likely to yield evidence to satisfy men with very different training and backgrounds.

What I have said thus far may imply a belief that the academic disciplines could "go it alone," could, on their own, contribute materially not only to the understanding of more and less legitimated forms of violence and social change, but also to their resolution or successful management. I do not hold any such belief. Inevitably practitioners must act to utilize whatever new findings are generated, or those findings do not constitute effective knowledge possessing social relevance. (Any new knowledge concerning violence and social change has the quality of social relevance.) What is a prime essential is that members of various academic disciplines concerned with these matters meet together with practitioners, as we are doing now, to review what we feel we know, but also to question premises, look for interrelations, to decide anew what appears most problematic, and what queries most deserve attention. Practitioners or policy makers can serve as catalysts to increase the probability that academicians will work well together, especially if they help jointly to pose queries relevant to making

decisions and taking actions. Moreover, research which involves some
practitioners from the initial stage of formulating questions stands a
better chance of being utilized sooner than later. And at the risk of
sounding a prophet of doom, I must say that it looks very clear that
mankind's days may be numbered unless we can solve man's over-determined
penchant for self-destruction.

If cooperation is so desirable, what then are the major obstacles
to its attainment; and how shall we deal with them? First, there is
a general one related to the phenomena which interest us: like research
on death, suicide, racial prejudice, religion, or sex, war and violence
are subjects hard to be dispassionate and objective about (Farberow,
1963). They arouse anxiety in many researchers who, gazing at change
and violence, fear both their own feelings and the untractability of
the subject matter - as contrasted with their reactions to inquiries
into, say, resource allocation, problem solving, or verbal learning.

What is more important is the fact that the various academic
disciplines are often jealous of and defensive toward one another.
Scholarly territoriality can look very real as psychologists or sociol-
ogists protect their domains. The existence of clear status differences
across the disciplines unfortunately does not help. Members of one
discipline can resent the very possibility that they may "consult" for
members of another or that their findings will appear in appendices.
The most interesting tasks with the highest status involve formulating
the questions.

Each discipline tends to have its own distinct methods of study,
tailored to fit its self-defined objects of inquiry. Methods are norm-
ative, however, and like other normative objects, can lead to some very

strong emotional attachments. Psychologists mostly believe that evidence
from the experimental laboratory is the very best and may disparage
the existence of phenomena which are not readily amenable to laboratory
investigation. The clinical concept of repression, i.e., anxiety-
escape motivated forgetting, comes readily to mind. Sociologists
cherish and admire field observation as the first method. They prize
understanding processes. Political scientists manipulate archival
documents or utilize content analysis. Members of one discipline
do not often study nor readily appreciate the methods of another.
This "fact" too hinders mutual respect and cooperation.

Each discipline tends to regard the phenomena it studies - at
the level of analysis it employs - as problematic and all else as
given. So each discipline looks at endogenous factors and ignores
exogenous ones. Economists consider economic factors as endogenous
and problematic and non-economic ones as exogenous ones, givens which are
not to be investigated. Psychologists look at intra-organismic or (at
largest) at interpersonal phenomena as problematic and worth investigating:
economic, cultural, political, or historical factors are for them exo-
genous givens, outside the purview of their disciplinary charter. It
is always easy to forget that the nature of each discipline has an
arbitrary quality about it, that it exists in the shape it does, with
the subject matter upon which it concentrates, with its distinctive
premises - as the result of certain historical accidents, i.e., the
interests of its founders and developers.

Most of the above factors which I suggest are obstacles to inter-
disciplinary research because they make mutual respect more difficult
and mutual defensiveness easier are sociological ones. They are also

more purely philosophical ones. As economist Mancur Olson has put it:

> The work of economists in international studies often
> involves the assumption that a nation's behavior is
> capable of being described in terms of the rational
> pursuit of determining interests. The object of the
> analysis is typically to determine the most effective
> or efficient methods of attaining goals. By contrast,
> the sociological mode of analysis tends to concentrate
> on the influence of socialization on the behavior of
> nations and political leaders, and the problem of deter-
> mining a rational strategy for attaining determinant
> goals tends to receive little attention. Where the
> causes of peace or war are at issue, the economist
> tends to think primarily of the mutual advantages at-
> tained from avoiding war (e.g., the theories of deterrence
> and balance of power and from international trade). The
> sociologist, by contrast, often tends to emphasize the
> fact that common ideologies or religions reach across
> many national borders, the negotiating styles of dip-
> lomats, and international laws and normative precepts.

While presumably equally scientific, the disciplines may differ remarkably

in terms of their premises and approaches. At least at present a

psychologist interested in the experimental analysis of human behavior

would refuse to have any traffic with non-observalbes as are reflected

in the (also non-minipulable) cognitive categories of the sociologist

such as perceived or understood consensus. A term like legitimate or

legitimacy is rarely found in the indexes of psychology texts.

The obstacles I have described are formidable ones. Not only

are members of different disciplines suspicious of strangers to them,

protective of their own domains, and occasionally defensive about their

possible weaknesses. They also so differ with respect to methods,

premises, concepts (and the philosophical roots of their concepts) that

they have difficulty in understanding one another quite as much as

they have trouble trusting one another. Their differences in ways of

looking at problems, in the units they employ for observation and analysis,

and so on, are their strengths, cumulatively built to enable them to deal

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with their favorite dependent variables. These very strengths are also
obstacles in the way of interdisciplinary cooperation even for those
disciplines which concentrate their energies upon the study of man.
Their obstacle quality is reinforced by the organization of universities
into academic departments which themselves structure academic careers.
It is also reinforced by the various professional societies which
strive to emphasize separate and unique professional and scholarly
identities.

There are ways for us to overcome obstacles to interdisciplinary
integration vis-a-vis conflict. Translations of similar meaning terms
across disciplines do seem feasible though terminological meanings only
partially overlap. There is, for example, the non-zero sum game which
can result in win-win or lose-lose relationships in which both parties
to a relation come out ahead or behind depending upon whether the coop-
erative or the conflictual elements of a relation come to be most
emphasized. Win-win games look analogous to the principle of comparative
advantage in international trade, to the concept of role complementarity
in sociology, the creative relationship in psychology, to constructive
symbiosis in biology. Lose-lose games look like neurotic or psychotic
relationships in psychology, to role conflict in sociology, to mutual
parasitism in biology. It seems plausible that explorations of the
similarities and differences in the meanings of these terms for example
could suggest worthwhile research to contribute to our understanding of
conflict. Certainly the exercise could contribute to communication among
the disciplines, assuming that men across disciplines interested in
conflict would be motivated to attempt the initial exercise. These term-
inological difficulties do not look impossible to overcome, and confronting
them would seem potentially very fruitful.

Except for man-computer simulations, phenomena like war, have not been encapsulated in the laboratory. They are emergent phenomena which appear at only certain levels of societal complexity. Still, phenomena which lead to wars, e.g., crises, may be studied at various levels of social complexity, with many units of analysis and through the utilization of a wide variety of methods. A better understanding of the distortions of perception, decision making, and action, which occur in real or artificial crises can be expected to benefit the advance of knowledge across several disciplines and perhaps be of worth to practitioners through reducing vulnerability to crises. Note, however, that mutual respect and some ability to communicate across disciplines is a prerequisite for easy collaboration at empirical or conceptual levels on related problems, especially when we cannot tell where major gains will appear — if, indeed, they show at all.

It is still not altogether obvious how projects on the investigation of phenomena such as those we concern ourselves with here can best be organized. We need to collect critical incidents concerning notably successful or unsuccessful attempts at cooperation. I imply that we can gather evidence of the success or failure of collaboration. How shall we measure success - no more wars, less violence? Those are all right, but others are necessary too; I suggest several possible indicators of success or failure of efforts.

(1) Scientific: is new theory developed - or concepts, methods; is new research inspired as a result?

(2) Pragmatic: are new problems solved or old ones solved more readily?

(3) Public Policy: what evidence exists that these efforts can make or have made a difference on the actions or decisions of policy makers? Have such impacts been at all institutionalized (have they resulted in organizational learning), or must other projects start from scratch?

The obstacles to interdisciplinary cooperation and to men of action and men of knowledge cooperation are formidable ones, psychological, sociological, and conceptual. Except for some of the inhibitions I have suggested above, it is surprising that there have not been more studies of change, conflict and cooperation among the disciplines. Certainly we have much to learn, and such investigations might suggest hypotheses toward the general understanding of processes of conflict together with means of resolution. The problems we and the world face are of such overwhelming importance that we must not regard the obstacles of interdisciplinary and practitioner-academician cooperation as insurmountable. If we succeed, we all gain. If we fall to work together, mankind may go down the drain.

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THOMAS MILBURN: "PROBLEMS OF INTEGRATING ACADEMIC
DISCIPLINES IN THE STUDY OF WAR, VIOLENCE AND SOCIAL CHANGE"*

The problems we face here are quite big - so big they really
are overwhelming. Moreover, the problems are multi-faceted, and I don't
see the possibility within the intellectual capabilities of any one
discipline to attempt to solve them. One of the interesting things
about war, violence and social change is that they all still contain so
many aspects that are problematic - that is, whatever we may say that we
know about them, there is still a lot that we don't know. And this,
of course, is one of the reasons for us to be concerned with working to-
gether. The problems may be immense, but it is this multi-faceted nature
of the problems that seems to me really calls for interdisciplinary re-
search. Problems that are complex often yield more readily to heter-
ogeneous groups; if you have a group of people, all of whom are bright
but with overlapping knowledge and diverse skills, you do much better at
solving problems than otherwise. It takes more than diversity that way.
It also takes cooperation, which is hard to generate.

None of this is to imply that the disciplines should merely
work together even in an interdisciplinary approach. The disciplines
should have to cooperate with practitioners as well. In this case it
seems to me the practitioners clearly are educators and the educators,
among others, very much need to be invited in and involved in formulating
the research problems.

One obstacle to interdisciplinary work is that the particular
area of research we are concerned with produces anxiety in many people,
and with anxiety they retreat to the security of their own fields. Thus
conflict and violence are like other areas such as religion or sex, each
of which is also hard to study because it arouses anxiety resulting in
withdrawal to safe problems in one's own discipline. Note, by the way,
that in order to cooperate it is necessary to be daring, to take a chance,
to respect others even when you see that when they talk about your area
they do not know as much as you do. You have to find some criteria by
which you can decide to trust them when they are talking in areas about
which you don't know so much and about which it can be harder to judge
them. Each research area has its own methods and some of the strongest
emotional commitments I know are to methods.

Another problem, it seems, is the tendency to regard the things
that we work on in our own discipline as problematic: i.e., 'these are
the problems and everything else is a given.' For example, the economist

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who studies developing countries usually regards sociological factors as exogenous; he doesn't even look at them.

One way to overcome these obstacles is to be aware of the fact that there are conceptual convergences that exist across disciplines. One in particular seems quite relevant to conflict, the notion of the non-zero-sum game which can result in win-win or lose-lose relationships in which both parties come out ahead or behind. Win-win games look analogous to the principle of comparative advantage in international trade, to that of role complementary in sociology, creative relations in psychology, constructive symbiosis in biology. And lose-lose games look like neurotic or psychotic relations in psychology, role conflict in sociology or a kind of mutual parasitism in biology; they may also look like wars.

These conceptual convergences exist independently of cooperation. But they set the stage for cooperation across disciplines, and they suggest that we really do have something to communicate about if we can get into it, if we are willing to tolerate the fact that we have somewhat different languages. It takes a while to be aware that we are closer than we knew. We can abet cooperation if we concentrate first on similarities across disciplines (for example, the concepts) before emphasizing differences.

There remain many different kinds of things to be done, and there are new methods and new concepts that make cooperation easier. For example, the man-computer simulation can prove a good way to study conflict. Another is this whole notion of system - putting together hypotheses that have some meaning at different levels of societal complexity.

These are ways to talk and work together. We still, then, are faced with the problem of sitting down and realizing it is going to be tough and it is going to be abrasive and we are going to have a hard time tolerating one another. But this game is worth the candle.

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DAVID HAYS: COMMENTS ON THOMAS MILBURN'S PRESENTATION,
"PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES
IN THE STUDY OF WAR, VIOLENCE AND SOCIAL CHANGE"*

I don't know how many of you have had the experience of inserting
a hand between a knife and its target, but that happened to me a couple
of weeks ago. The young man who was wielding the knife, a straight bladed
knife, a knife at least six inches long, was trying to use it to pick
the lock on the door to the President's office at my University, and I
had to put my hand in between even though he said then that he does be-
lieve in the use of violence. But it turned out that he didn't believe
in it enough to do it when the case came. I'm going to take a minute
to tell you how I got into that situation and then say a few things about
what I think this interdisciplinary research might bring to a related
problem.

I started getting into that situation by going to Harvard when
I was 19 years old; I got a Ph.D. in Sociology there. Since then I have
been with the Center for Advanced Study; with the RAND Corporation work-
ing on studies of radar networks and then, for ten or twelve years on
computational linguistics; and about a year-and-a-half ago I went to
New York University at Buffalo to organize a Linguistics Department. I
had, in the meantime, invented the ultra-microfilm library and decided
I was neither a scholar nor a researcher but an inventor by trade. I
don't belong to a discipline anymore; I had belonged to the psychological,
sociological and statistical associations for a long time; I've done
mathematical modeling and computer work; and I've worked in the field of
education for Encyclopedia Britannica. So I find now that I am really a
generalist.

I do not believe that you can train interdisciplinary specialists
from the beginning. I think that everybody's got to have one, two -
some disciplines and then he can say he is a generalist. I also feel that
if I hadn't bought my new position - my new position is that I'm not going
to do any work any longer - and if I hadn't paid for that with twelve or
so years of hard detailed effort, then I think maybe I wouldn't deserve it.

I came to the University, then, for the first time when I was
about forty years old to take my first full-time university job. And I
took it with the certainty that every professor has a paternal obligation
to his students. So that in the first year I stood between a column of
police and a line of strikers, and when the strike came up this year I
knew I had to get into it somehow. And inevitably I'm involved. I feel

that we are all inevitably involved in the things around us. And to walk away and close your eyes and say you won't worry about it is just impossible.

Now the most important thing to remember about interdisciplinary work, it seems to me, is that whenever you tackle a problem and say you will put an interdisciplinary effort into it, you are by that act creating a new discipline which will have its view of what reality is - which will have its theory that draws on the theories of all the disciplines put together. Its specialty will be the examination of interactions among variables that are the properties of the several disciplines which you draw on to create it. None of the disciplines is interested in the interactions of variables - some inside and some outside their fields - and so the new field has this specialty of its own, these interactions that nobody previously had thought about working on.

For example, as you think about applying solid science or basic knowledge to practical problems it seems to me that you have to forgive the solid scientists for disclaiming the responsibility for making the transfer to the practitioners themselves. The scientists are not special- ists in the transfer of knowledge from their level of abstraction to that of the practitioner. There is a new specialty: the specialty of the people who write review articles. Some articles are by scientists for scientists, but some are by specialists in gathering up knowledge and rewriting it in forms that make sense to practitioners. Now, that's not an art that a lot of people know, but it is one which could prove invaluable to our work here.

Finally, I would like to propose a kind of experimentation which it seems to me the new discipline of conflict studies might induce. There are beautiful examples of violent conflict and semi-violent conflict on our campuses. The story I began with here is a perfect example of this. And the thing is that the protagonists on both sides are us and our friends. And therefore, it's possible to be in touch with the leaders in the adminis- tration and the faculty, and among the striking, rioting students. Now, the anti-rationalist bias of the students, particularly the ones who are likely to be on strike or building riots, is fairly strong. But I think it may be possible to get through to them and convince them to study themselves as they carry on the riots so that society can understand them and they can understand themselves better. And they can then see more clearly what their goals really are and what mechanisms for reaching their goals are most efficacious.

So, to summarize, we need to recognize that interdisciplinary work, if it is to be successful, creates a new discipline out of the two or four or however many inputs there are. And one purpose for one such new discipline could be the study of conflict, with an eye to non- violent solutions of situations like the one I was involved in as opposed to choosing sides in, or polarizing, the problem.

KATHLEEN ARCHIBALD: COMMENTS ON THOMAS MILBURN'S PRESENTATION,
"PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES IN
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I think the study of conflict has developed as an interdisci-
plinary area, at least relative to other issues. But if you ask how do we
take it to the next steps, I think it is an institutional problem. You
have to face the fact that it is very hard to do interdisciplinary work
in the University, even in the so-called interdisciplinary institutes.
This is very noticeable when one moves, as I have done, from a university
to a place like RAND: RAND doesn't do its interdisciplinary work well,
but it is certainly way ahead of most Universities. I think it is easy
to say what we need to do; it's much harder to start the change process
that allows one to make institutional arrangements for the interdisci-
plinary work.

It seems to me that beyond doing good interdisciplinary research,
there is another important step: that is moving work in areas like conflict
and change into a more useful applied direction. I'm partly saying that to
have a good applied discipline it has to be interdisciplinary. And I
think in the field of conflict we have gone some distance toward solving
that. What we haven't come very far in solving, however, is how do you
do good applied work in the field.

There are alternatives for the researcher or expert working in
the field. One alternative is the one generally followed by social
scientists in universities who are concerned about war/peace issues. They
want to work in the area; but essentially they stay oriented to their
discipline, hoping that what they do can be made relevant by a middle-
man who will "translate" their findings so they will be useful for the
decision-maker. The research, therefore, is not designed to meet the
needs of the practitioner, or decision-maker, because the researcher isn't
sure himself how that translation should be done. I call this approach
"discipline oriented" for although the academic may wish to make a
practical contribution as well as a disciplinary one, in fact, the
thing that gives primary shape to the research he does is his discipline
and not the demands of the practical problem.

A second kind of orientation takes the boundaries of the
problem being looked at as the boundaries of the client. I call this
"client oriented," and it is represented by the human relations approach,
the organizational development approach, the client change approach. In
this orientation the consultant attempts to make an organization better

able to cope with its problems. So that, for example, with this orientation
if you're working for OEO, you don't worry about the task in the real
world that OEO is struggling with. You are trying, instead, to make OEO
good enough so that it can solve its own problems. In other words, you
don't try to solve its external problems; you try to solve its internal
problems. The underlying model is borrowed from psychoanalysis, only now
applied to an organization.

A third kind of model is what I call "decision oriented." This
seems to me closer to the RAND way of operating, to the way operations
researchers and economists tend to consult. They are looking at the real
problem in the real world. But they don't look at the total problem and
all that leads into it because they are, in fact, working for a decision-
maker, let's say OEO. I call this approach "decision oriented" as distinct
from "problem oriented," because the work is undertaken at the intersect
of (1) the real problem in the real world and (2) the capability of the
client to deal with the problem. With this orientation you don't tackle
the whole problem but only those aspects of it which are under your clients
jurisdiction. So when you are working with OEO on reading improvement,
the problem is defined in terms of the variables that OEO controls or
can affect. The difficulty here is that, while your research may have
some influence in improving the situation, its utility is limited by
the capabilities of your client. If other decision-makers control
most of the important levers that could ameliorate the problem, your
research will have little impact.

I think the best we can do at the moment is to combine the decision
oriented approach with the client oriented approach. The decision oriented
approach is pretty good at turning out a nice blueprint for the client
specifying the alternative he should prefer. But what is not considered
is whether the client's organization, at the operational level, is
capable of doing the job. So what comes out of the operational end of
the hopper often looks very different from the original blueprint. The
client oriented approach has a lot to offer on this kind of problem, so
I expect a lot of progress could be made by combining the decision oriented
and client oriented approaches. This is assuming, of course, that we
can solve the interdisciplinary problems. Part of the hang-up here is
disciplinary; it's the psychologists and social psychologists who tend
to move to the client oriented approach and the economists who almost
automatically move the other way. The resulting communications problems
are partly disciplinary.

I'd like to end by suggesting a further refinement; can't we
think about an orientation that is really "problem oriented?" In this
approach, the problem would be analyzed first and the key levers of
action identified, then the researchers would approach and work with
those who control these levers; that is, those decision-makers who have
what it takes (authority, resources, influence, etc.) to make significant
progress towards solving the problem. I can think of few cases where any
thing close to this has been done. And, of course, one of the reasons is--
who supports the researchers while they do it? It's not the kind of work
that, currently, can be done within a university. It's expensive and you'd
have to spend a lot of time looking at levers of action with a very

applied orientation. So what you need to do this kind of work is an applied research institute that is not dependent on client contracts for its support.

Perhaps we can think about ways to accomplish this. Ways to move in on a problem and say, "Okay, who are the clients we work with on this?" "What are the levers of action we move on that?" This means thinking of large institutional questions - because, unfortunately, you can't get ten good-hearted bright people together for a few weeks or months and solve the problems. You have to have some sort of an organization that permits good people to work on important practical problems on a long term basis.

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