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DRUGS, SCIENCE AND POLICY: A VIEW FROM THE U.S.A.[1]

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Professor Edwards has offered us a series of small case-studies of the relation between science and policy in the areas of alcohol, tobacco and illicit drugs. Primarily his case-studies are success-stories for science, examples of where science has made what seems to us today a positive contribution to policymaking and for that matter to human progress. Others have also made collections of such case-studies, including cases where the knowledge transfer was not so successful (Gordis, 1991; Room, forthcoming), and indeed much useful can be learned from collecting and analyzing such case studies of the interaction between science and policymaking.

Using such material, issues in the relation between science and policy can be tackled from a number of perspectives. My comments are directed at just one of these perspectives: at the issue of how the scientific effort might best be organized and managed so that it is maximally useful. So my attention is directed at the science side of the science/policy interface. How can it be better organized to be of use to policy? I start from the premise that the primary justification for research in these areas is practical; that however much we as researchers find ourselves fascinated by knowledge and the pursuit of new knowledge for its own sake, our work is primarily justified by and supported for its potential to better the human condition.

My comments are based on and directed particularly at the U.S. experience, which is a particularly interesting case for study. The U.S. has a comparatively large investment in alcohol, tobacco and drug research, probably in absolute terms the largest in the world. It is an ambitious effort, which aims to cover the entire range of scientific work, and tends to assume that it is going it alone. In discussions of U.S. research priorities for alcohol and drugs, thus, I have never heard anyone say that the U.S. should give a lower priority to a particular line of study because it was already strongly covered in some other country.

It should be stated at the outset that there is clearly no pat answer to the question of how to organize science for the greatest payoff. The kinds of problems we are facing are multilevel, multidetermined and messy, and there is not going to be a quick technical fix that could be provided by a single-minded Manhattan Project or space-agency task force.

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It's also clear that any organization of the scientific effort has to allow for serendipity. Quite commonly, the practical relevance of research turns out in the long run to be quite different from its original justification. This is not only a matter of the proverbial happy accidents at the lab bench, but also of more farreaching advances. If we look at the success-stories Professor Edwards has offered us, the relation between science and the policy payoff is of two main sorts. One kind of relation is quite direct: an epidemiologist is commissioned to find an explanation of the increase in lung cancer mortality, or a new treatment is tried out in a controlled trial. The impetus for doing the research may come from the policy process, it may come from the researcher, or it may come from a clinician or some other third party. The research is a process of filling in a gap, of "normal science", using Kuhn's term (1962) -- although it should be kept in mind that the process looks much more "normal" and routine in retrospect than it did beforehand. The other kind of relation is much less obvious; this is where the science contributes to changing the whole paradigm by which we understand and attempt to do something about a problem. The reinterpretation of the cigarette habit as a form of drug dependence, or the shift from "alcoholism" to the broader policy frame of "alcohol problems", or even the realization that drunkenness was responsible for many road traffic deaths, are examples of this kind of shift in the whole governing image (Room, 1973) the society carries of a problem. Behind such shifts in perspective there is often a whole train of research developments which were not aimed at such a shift at all. These are cases where the research turns out to have a policy significance which may be invisible and indeed unimagined beforehand.

There is considerable variation in the U.S. in the extent to which there is slack for serendipity and for scientists' self-management of research priorities. The form of support for the scientific effort (and not the source of the funds) largely determines this. Thus researchers working in research positions with security of tenure probably have the greatest scope for serendipity and self-management, but there are relatively few alcohol, tobacco and drug research positions of this sort in the U.S. Only a few state positions (primarily in New York State), the intramural programs of NIAAA and NIDA, and some positions in the Veterans Administration, resemble the kind of "settled research institutions", as a Finnish report once put it, that are the backbone of the research effort in such countries as Finland, Norway and Canada. All other substantial research support specifically for alcohol, tobacco or drug studies is for specific projects funded for a fixed period, at most five years.

The major U.S. source of support for "investigator-initiated research" is the federal research grants program, which includes a variety of support mechanisms, but primarily supports specific time-limited research projects or groups of projects. Proposed projects are subjected to a rigorous peer review, but investigators have considerable discretion in how the research is carried out once they are funded. The funding agency maintains somewhat more control over the research in cooperative agreements, a related federal support mechanism. My further comments mostly revolve around these federal grant programs, since they are the primary visible research support mechanism.

There are two other major organizational arrangements for research in the U.S. One of these is contractual studies, where a federal, state or local government agency specifies a study it wants done. The contracts for such research are modeled on the mechanisms for buying paper-clips or an army tank. Typically, such a study is performed by the winner of a competitive

bidding process, and has a relatively short and often extremely compressed timetable. The research product is defined in terms of prespecified "deliverables", typically a series of progress reports and a final report.

A great deal of work on alcohol, tobacco and drug topics is done under such arrangements in the U.S. In the short term, the government involved presumably gets something related to what it wanted. But from the point of view of building cumulative traditions of research findings, the arrangement is highly problematic. Those working in the contract economy often have to zig-zag between topics as the exigencies of the market require. Often the contracting agency has the right to edit out potentially embarrassing or politically problematic findings. There is typically no time and little incentive to get study results into the journal literature, so the work remains buried in contract reports and other fugitive documents. Clearinghouses and abstracting services typically resist systematically collecting and indexing such reports, finding that the process is expensive and does not enhance their prestige.

The other organizational arrangement for research in the U.S. is research performed without specific support by faculty members and advanced graduate students in the University system. Perhaps a majority of doctoral dissertations on alcohol, tobacco and drug matters in the U.S. are done without drug-specific funding. The substantial upsurge of historical studies on alcohol and other drugs has had very little in the way of such funding. While qualitative social science studies of illicit drug use have received some grant support, most of the qualitative studies of drinking have been produced by scholars whose research is an adjunct to a university teaching career. Studies produced under these conditions are often relatively isolated efforts not firmly rooted in a cumulative literature. Rather little effort has been made by government alcohol and drug agencies to explore the relevance of these studies to their policy missions.

I have left out of this accounting in-house and contract research financed by the tobacco, alcohol and pharmaceutical industries. Most such research which would be of policy relevance is not publicly available. Even excluding this, it is apparent that the U.S. research scene for the drug field is extraordinarily polymorphous and diverse. It includes a variety of studies potentially relevant to policymaking, such as local needs assessments or program evaluations, that may not even be thought of as "research" at all. A great deal of the research is pursued outside the frame of any planned or coordinated research program.

The primary place we can find such planning and coordination is in the programs of the main federal agencies for research support in the field. For alcohol, this primarily means the National Institute on Alcohol Abuse and Alcoholism (NIAAA), and for illicit drugs and psychopharmaceuticals, it primarily means the National Institute on Drug Abuse (NIDA), although other agencies in the Alcohol, Drug and Mental Health Administration (ADAMHA) also have some involvement. For tobacco, the responsibility is presently split between several agencies, including the Centers for Disease Control, NIDA, and such NIH agencies as the National Cancer Institute. That it is federal agencies that have the primary responsibility for a planned and coordinated research program reflects a general consensus in the U.S. polity about the societal location of responsibility for scientific research. Even those who are most skeptical about government are usually willing to give it some responsibility for supporting scientific research, and particularly medical research; and even those most committed to the "federalist" policy of keeping central government small tend to see scientific research as appropriately more a federal

than a state or local government responsibility.

There are a number of ways in which the organization and content of the major U.S. research programs in the drug field seem to me fall short of being of optimum usefulness from the perspective of providing results useful in policymaking.

(1) The problems are not only health problems. The programs are lodged in agencies operating under a health research rubric, while health problems are only part of the whole spectrum of drug-related problems. The American taxpayer is clearly much more willing to pay for research on medical than on social problems, and the relatively large research commitment in alcohol and drug research is clearly linked in the public mind to a definition of the problems as medical in their nature or their consequences.

Despite their health rubric, NIAAA and NIDA accept responsibility for research on the whole range of alcohol- and drug-related problems. But their portfolio of research grants is heavily tilted towards the health side of the problems. An example of a research area that is largely neglected because of this is the area of alcohol and crime. In the U.S., which has a larger proportion of its population in penal institutions than any other industrial country, alcohol is heavily implicated in violent crimes, and a large proportion of all arrests are for alcohol-specific crimes. Yet the federal research portfolio typically includes only one or two research projects in this area.

(2) Alcohol, tobacco and drugs should often be studied within in a common frame. The research programs for tobacco, for alcohol, and for other drugs are separately organized, by and large in separate agencies. Populations of users and of dependent persons, on the other hand, overlap considerably, and drugs are often used conjointly or in sequence rather than in isolation. The separation of research programs facilitates funding priorities for research being decided in terms of the cultural politics of drugs rather than in terms of the relative need for and utility of research. It is also an impediment to research on conjoint use of drugs, and to some extent to comparative studies across drugs.

(3) Alcohol and drug problems are largely local issues. The location of research as a federal responsibility tends to point research away from the local level. So does the prestige structure of science, which favors knowledge which has the widest degree of generality, and thus tends to regard local particularities as "noise" rather than as part of the data. But, as the temperance movement and the alcoholic beverage industries well know, alcohol and drug problems are in the end local problems, and much of the burden of handling and of preventing these problems is inevitably local. Research which would inform policymaking and practice in this area includes not only a series of case-studies and evaluations of the effects of particular interventions at the local level, but also meta-analyses across these case-studies of community and situational factors which influence the outcome. In the field of research on the prevention of alcohol problems, the process of accumulating such case-studies has begun (Giesbrecht et al., 1990), but research traditions in this area at present seem stronger in such countries as Canada, Britain and New Zealand than in the U.S.

(4) Policy-oriented research needs quick-response mechanisms. The research grant review mechanism is ill adapted to studies of "natural experiments", that is, studies of what happens when a new law, regulation or procedure goes into effect. Since policymaking is about the planning of change, and yet polities are understandably reluctant to experiment with actual changes at the

direction and convenience of scientists, studies of natural experiments in change have emerged as a major field of policy-relevant science. Such studies require data from before the change has occurred, and where possible data also from control sites where no change is planned. Typically, however, changes in laws or regulations happen without much notice, while in the grant review cycle about a year elapses between the decision to write a proposal and receiving funding. In the past, federal agencies have sometimes used ad-hoc solutions within the grants structure, such as supplementing existing grants to collect the "before" data, but this solution seems now to have been dropped.

What is needed is a quick-response capability, which could take the form of a service or centers specifically dedicated to studies of natural experiments as they arise. An alternative which has been fruitfully used in studies of the effects of alcohol supply strikes (e.g., Mäkelä, 1980) is the mobilization of tenured research staffs to carry out such studies as a short-term diversion. The paucity of such tenured research positions makes this alternative unlikely for the U.S.

(5) The research agenda should not be distorted by prestige considerations. The prestige structure of science tends in a number of ways to disfavor research which is potentially directly useful in policymaking. The prestige structure has its effects not only in what individual investigators propose to investigate, but also in the behavior of the granting agencies. Agencies in ADAMHA tend to define the path to greater prestige for the field in terms of how much their research program resembles that of an idealized National Institute of Health (NIH) -- a cancer institute say, or one on heart disease. Indeed, a proposed reorganization may very soon split NIDA and NIAAA from ADAMHA's prevention and treatment service and support functions and turn them into NIH institutes (ADAW, June 19, 1991). Prestige for the field and for its institutions is seen as coming primarily from biological research, and, within biological research, from research conducted at the most microscopic levels. Thus more prestige attaches to a new finding at the molecular or the gene level than to one at the level of a body organ.

A further tilt of the scientific playing-field comes from commitments to including as much as possible of the phenomena of alcohol and drug problems within a disease framework. Apart from the health rubric under which the research is funded, in the case of alcohol this tilt reflects a continuing societal disposition to keep presumptively unproblematic "normal drinking" by you and me separate from somebody else's "alcoholic drinking", defined if possible in terms of biological vulnerabilities in the drinker. This disposition helps fuel the continuing quest for biological markers and causes of addiction. It is also part of the explanation of prestige differentiations within biological research, by which research on the nature and causes of addiction is favored over research on the pathways connecting alcohol and drug use to biological harm (see Room, 1990). Yet understanding these connective pathways may offer much more practical leverage for prevention of harm than finding a pattern of genes that predispose an individual to addiction. As Gordis (1991) has pointed out, discoveries of ways to identify genetic vulnerability raise thorny problems of public policy around insurance-company adverse-risk selection; they may be a case of "good science, but policy enactment not desirable".

Across all scientific disciplines, more prestige is attached to "basic" than to "applied" science. This is as much true in social as in biological or physical sciences -- for instance, within sociology, the study of social problems traditionally has a lower prestige ranking than, say, the study of social theory or social structure. Alcohol and drug research starts, then, with the prestige

handicap that it is tied to concrete everyday problems, and is supported by the society not out of a commitment to prestigious science but on the premise that it will come up with practical answers. There is a tendency for alcohol and drug research institutions to try to counter this handicap by identifying with and buying into whatever is perceived as the "cutting edges" of basic science. In medically-oriented research, following the pattern of the last 200 years, this still tends to mean tying into new machines for measurement at ever more microscopic levels -- at the moment, such techniques as magnetic resonance imaging and positron emission tomography. In scientists' discussions of research priorities for alcohol and drug agencies, this pull away from applied research and towards the lure of new measurement techniques is expressed in terms of a counterposition of "research needs and opportunities". There are thousands of research needs, the argument goes, and such needs we always have with us. But what are the research opportunities, where the really exciting science can be done? Time after time, the answer tends to be in terms of the exploitation of new measurement technologies -- which, of course, is a rather odd way to define what is basic about "basic science".

In the limited time available, I have concentrated on identifying some ways in which the U.S. research effort is less than optimally attuned to providing findings useful in policymaking and practice, and have left any suggestions for reform implicit. I have entirely omitted discussing the other side of the science/policy interface, where the disjunctions are often severe. The policy process in the U.S., it seems to me, too often turns a deaf ear to inconvenient science, and on occasion even tries to suppress it.

Today the U.S. stands at a turning-point in the relation between science and policy and practice. As I have mentioned, it seems likely that NIAAA and NIDA will be split off from the service-oriented offices, the Office of Substance Abuse Prevention (OSAP) and the Office of Treatment Improvement (OTI), which will remain in a renamed Alcohol, Drug and Mental Health Services Adinistration. This provides an opportunity for rethinking the organizational relationship between science and policy. But unless the opportunity is seized creatively, the result of the split is likely to be a further attenuation of the relation between the scientific effort and practical affairs. A summary of the legislation introduced to formalize the change concludes that "researchers and service providers share a common goal, but they speak a different language and thrive in different professional cultures. The climate at ADAMHA has been competitive rather than collaborative" (ADAW, June 19, 1991). The director of a Massachusetts treatment service agency put it less diplomatically. "Sometimes, researchers and clinicians act and talk like they are from other planets. Clinicians feel that researchers engage in a veritable vocabulary of denigration and researchers perceive clinicians as being resistant to research and change. In the proposed new structure, issues of turf, control and disruption of clinical programs will be minimized" (ADAW, June 26, 1991).

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