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THE RESEARCH PERSPECTIVES PANEL (CONTINUED)

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IV. THE HIGH IMPACT ANTI-CRIME PROGRAM:
A PROCESS EVALUATION

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MR. GRANDY:

As you are aware, yesterday we fell behind in our schedule. I am not too concerned about that. I think the relaxed and candid interchange of ideas and information is worth it, and I admire your perseverance and stamina in sticking with us. We have two papers left from yesterday afternoon's session which we will begin with this morning. It is our plan to delay our luncheon one hour, so we will go to lunch at just after 12:45 instead of 11:45. This will somewhat shortchange our afternoon working panels. We will try to make up time there later in the afternoon.

This morning our program will start with a presentation of another research paper by Eleanor Chelimsky who is Head of Program Evaluation at the METREK Division of MITRE. Her paper concerns an evaluation conducted of LEAA's High Impact Anti-Crime program for the National Institute of Law Enforcement and Criminal Justice.

Ms. Chelimsky is an economist by training, served as a statistical analyst at the U.S. Mission to NATO, and, since 1970, has held a variety of research positions at the MITRE Corporation. Most recently, she has directed policy analysis and program assessment in the areas of health, welfare and criminal justice. She presently heads up our program evaluation department. Eleanor.

MS. CHELIMSKY:

Thank you. Well, as Cork has just said, I am going to talk to you today about the national evaluation of the High-Impact Anti-Crime program which MITRE performed between July of 1972 and December of 1975. There is a summary of this evaluation on the table outside, and it may be useful to look at it because I know that in the short time I have, I am not going to be able to do more than give you a very broad-brush and generalized account both of the evaluation and of the findings.

Before examining them though, I'd like to look just a little at the program itself and at the origins of the program, not because their bureaucratic and political aspects are especially unusual, but-- in the sense that Jim Stockdill was talking about yesterday²¹--because they help to explain the program and some of its peculiarities--its ambitiousness, for example, and its unusual complexities--and because they also say something about the agency needs which drove our evaluation.

When you go back to the crime control context of 1971, perhaps the first thing you need to remember is that the Nixon Administration had been in office for about three years, and that the 1968 campaign had focused very heavily on crime as a political issue. Although the Safe Streets Act had created LEAA in 1968, the crime problem had not abated by 1971, as many people pointed out yesterday. Another election was coming up in 1972 and it seemed to be a propitious time for a major new anti-crime initiative. Also, by 1971, LEAA seemed to be coming out of the turmoil which had marked it since its creation,

²¹See pages 129-132 above.

turmoil due, at least in part to the troika organization that Congress had imposed on it. So in 1971, there was not only an Administration need for a big, visible, ambitious anti-crime program, there also seemed to be an agency capability to mount such a program.

Another factor which explains the ambitiousness of the Impact program was the still optimistic, gung-ho climate of 1971. It seems a little strange to remember it now, but it was common then to hear people saying things like, "If we can send a man to the moon, we can... fix the economy, or cure cancer, or turn the corner on crime and drugs," or a hundred other good things.

It is true that researchers were not quite so optimistic at that time, after the poverty programs of the '60's, but their caution doesn't seem to have penetrated the upper reaches of administration where programs are born and made. At least, not then. In fact, there was real optimism about the potential of a concentrated thrust for "doing something" about crime.

As for the complexities of the program, some of these can, I think, be traced to policy issues that were confronting LEAA at that time. Many of them had to do with the fact, as Dick Linster said yesterday, that LEAA is basically a block grant program. LEAA is, and must be, concerned with the problem of working with states and localities. Some of the issues surfacing in 1971 concerned questions like: How can Federal leadership be made acceptable to states and localities in an area where they had had undisputed primacy three years earlier? How do you apply Federal resources to local crime problems so that local people have a dominant voice in deciding how the money gets spent and at the same time insure that the money is not misapplied or misappropriated? How do you go even further and insure not only

that it's not misapplied or misappropriated, but that it's effectively spent? What kinds of analytical capabilities do you need to bring in at the local level in order to do that?

Even if you can get states and localities to accept Federal leadership, how do you make that leadership effective in terms of research, given that there is something of a gap between research capabilities at the Federal level and research capabilities at the state level, and an even greater gap between researchers in general and the criminal justice practitioners who need to use and apply their research? How do you insure that Federal research can be disseminated, made understood and used by criminal justice practitioners at the state and local level? That's a pretty tough question.

How do you overcome the reluctance of independent agencies to coordinate their efforts when very often it seems to them that they have, in Sam Seeman's terms yesterday,²² little practical incentive to coordinate, and a great many incentives to avoid coordination? How do you get them to include the public in their planning and program processes when again, there are real disincentives to do so, despite all the studies which have shown that it's important for the success of social programs to involve the public in their planning and execution?

All of these were major policy questions for LEAA in 1971 and all of them found their way into the Impact program.

Still another source of complexity in the program was the professional criticism that had been heaped on LEAA in 1971 in the 1971 report. There were four general areas of criticism raised

²² See page 27 above.

in the report. The first one was that state and local recipients of LEAA block grants were squandering a great deal of money, and that LEAA had failed to perform an adequate fiscal monitoring job.

The second area of criticism was that too much money was going into police hardware.

The third area was that not enough money was going to corrections and specifically, to rehabilitation programs.

Fourthly, the Congressional report said that evaluation standards hadn't been built into LEAA programs so that it was difficult to judge their effectiveness.

The final source of complexity in the Impact program which I want to mention here is just precisely this question of evaluation itself. It seemed to many people at LEAA that evaluation could be a very promising tool not only for discovering whether programs work or not, but also for doing what LEAA wanted to do in the area of upgrading state and local analytical capabilities. But the fact was that, in 1971, no one really knew how to do that. There were not many social program evaluators around in 1971; there was no great pool of expertise to draw on.

In sum, the context that I have been looking at here points to the emergence of a very special kind of anti-crime program: big, visible and ambitious; highly complex; focused on corrections rather than on the police; locally run but financially unassailable; and containing a major effort to upgrade system and research capabilities at the state and local levels.

Vice President Agnew launched the program in January of 1972, very very visibly. The program was to be sizable: \$160 million over two fiscal years to aid crime control in eight U.S. cities (Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, Portland and St. Louis). These cities were asked to have their programs operational within six months--that is, "on-the-street" and working by July of 1972.

To understand what this meant in terms of the enormity of the local implementation problem, you need to look a little bit at the criminal justice budgets of these cities before they go this \$20 million increment; it meant different things to different cities. For Baltimore, with an annual criminal justice expenditure of \$72 million, a \$10 million increase did not seem so very indigestible. For Atlanta, on the other hand, with a total expenditure of \$15 million, city efforts to absorb the Federal funds resembled those of a cobra trying to swallow a piano. But for all of the cities, the questions of how that \$10 million should be spent, and what mechanisms could be found by which to spend it, were major problems.

The modus operandi of the program was New Federalism. Briefly put, this is the idea that local priorities ought to be set by local people. The cities were told that they could develop their own programs, run them and evaluate them according to their own criteria. In this way it seems that LEAA was avoiding coming to grips with the Federal leadership question and was instead proposing an equal, Federal-local partnership. The local control that is implied by New Federalism, however, was going to be tempered and corrected by a very tight fiscal and program review that would be done by LEAA's state planning agencies and regional officers.

The most important means of upgrading system and research capabilities at the local level would be the crime analysis team, a group

of researchers and criminal justice practitioners who were to be established in each city. Their function was, first, to supervise the performance of the highly complicated Crime-Oriented Planning, Implementation and Evaluation process (a mouthful of jargon which we call the COPIE-cycle, to shorten it). Second, they were to do what they could to improve agency coordination; and finally, they were expected to involve the community, to the degree possible, in the workings of criminal justice plans and programs.

The COPIE-cycle was clearly a very complex operation. The cities were being asked to collect a great deal of data (much of which was not in existence) about their crime problems. They were supposed to look at local data on victims, offenders and crime settings to get some real sense, based on the data, of what their problems actually were. Then they were supposed to rank their problems, achieve some consensus on their priorities among the various agencies of the criminal justice system, develop programs to address their crime problems in some reasonable way, build evaluation components into their programs, and finally, evaluate them.

The program did not target law enforcement alone, but rather a comprehensive, across the board, anti-crime focus which addressed the Congressional criticism about police hardware. The program would specifically encourage and emphasize corrections programs through a fiscal incentive: cities only had to provide 10 percent matching funds for corrections projects (as opposed to a 25 percent local match for other kinds of efforts).

At least two conflicts in the program are immediately apparent. The first is that the cities were told that the program would be theirs to run, yet the emphasis on corrections which is a state function, signified that this could not really be the case.

The second conflict is that the programs were expected to be operational in six months; yet it is hard to see how the cities could get through the COPIE-cycle and also have their programs implemented in time, especially since most of the teams that were supposed to supervise the cycle weren't yet hired, or in residence in the city.

In practice, it turned out that for the cycle to be performed in a reasonable way, it took about sixteen months.

The major objectives of the program were six (see Figure 7 below), and they are typical of the objectives of most broad-aim, action programs. That is, they are not operationally defined, and they fit to a "T" Bob Hemmes' description yesterday of vague, virtuous and desirable goals like "support civilization."²³

The first objective was to reduce crime (that is, decrease stranger-to-stranger street crime and burglary) and the stranger-co-stranger street crimes targeted were murder, aggravated assault, robbery, and forcible rape. It was hoped that these crimes, as well as burglary, could be reduced by 5 percent in two years and 20 percent in five years. Now this objective may seem more specific than the others, more quantified, but basically it was meaningless because the cities were going to develop their own programs. They hadn't yet even started to think about them when the objectives were announced, and they had a choice of project options which could affect crime rates differentially, unmeasurably, or not at all, so that there was no way to determine in advance what crime decreases might logically be expected from a program still to take shape.

Planning for the Impact program was forcibly curtailed by the great rush to speedy implementation. There were, perhaps, three months of program planning performed in all, but almost no evaluation planning at the national level, except to decide that there would be three levels of program evaluation--city-level, national-level, and a macro or global level.

²³See page 71 above.

FIGURE 7

PROGRAM OBJECTIVES
The High-impact Anti-crime Program

- Reduce crime: Decrease stranger-to-stranger "street crime" and burglary by 5% in 2 years and 20% in 5 years
- Demonstrate the copie-cycle and test the crime analysis team
- Acquire new knowledge about crime
- Improve coordination among criminal justice agencies
- Increase community involvement
- Institutionalize innovative, effective projects

city-level evaluations were expected to produce findings of effectiveness for all the projects that would be implemented in the cities. (It was mandated at the start that every project would be evaluated.)

The national evaluation was supposed to look at program activities and processes within and across the eight cities, using data generated in the cities as building blocks.

The macroevaluation would examine the anti-crime effectiveness of the program using victimization surveys. This evaluation was intended to be performed by the Statistics Division of LEAA in combination with the Bureau of the Census.

We, MITRE, contracted to do the national evaluation in July, 1977--about six months after the program began, and worked closely with the National Institute to develop an evaluation plan. We knew we wouldn't be looking at overall anti-crime effectiveness because the global evaluation was going to do that. And we knew that we couldn't very well impose an experimental design on this free-form, New Federalist program that was going to be totally different in each city and didn't allow the possibility of special data collection. (All of our data was to come from the city-level evaluations.)

We felt there were a great many process questions to answer and we tried to identify, among the multitude of possible inquiries, what LEAA and the National Institute were really hoping to find out from the program. So, moving toward a process evaluation which would ask the question, "what happened?" rather than, "did it work?", we began to examine the researchability of questions like, How feasible, in fact, is the LEAA-level at the city level? If it is feasible, if the cities

arrive at performing it, does it allow, at the national level, some ability to determine whether city programs are effective or not? Does it improve research capabilities at the local level? How useful is the crime analysis team? Is it actually possible to do something about agency coordination? How likely is it that the team can be successful in getting people in high-crime, inner city communities involved and concerned with criminal justice? How viable is New Federalism as a program philosophy? What happens when the time comes to get city compliance with program requirements (when we, MITRE, need to ensure that city data has been collected and evaluations reported so we can do our own evaluation), and there are not teeth in the program with which to do so? How reasonable is it to expect objectivity in city evaluations of their own anti-crime projects?

What kinds of projects do cities generate when the Federal Government gives them \$20 million and tells them to do crime analysis? What happens in that process? If they are effective, those programs, do they get institutionalized? Or does the whole thing just fade away when the Federal money goes? What are the lessons we can learn in terms of future programs? Those were the kinds of questions we wanted to look at.

Together with the National Institute, we eventually developed an evaluation plan which contained eight tasks in four general areas. Our major process mechanism was a program history in each city which featured interviews with a great many people during and after their involvement with the program. In those histories we looked at program development; at key actors and their roles; at the ways in which the crime analysis teams functioned, what they were doing to attack some of the problems that they had, where they were succeeding (if they were) and what their techniques were; and we looked at various types of city-state power relationships (much like those John Harper described yesterday)²⁴ across the criminal justice agency spectrum.

²⁴See page 173 above.

To observe the COPIE-cycle, we did in-depth examinations of city-level planning, evaluation planning, implementation and evaluation reporting. We looked at what speeded up implementation, what slowed it, where the bottlenecks were, what the quality was of evaluation planning and evaluation reporting. We did get a great deal of data in those areas, data which now furnish an interesting baseline of what local capabilities were in 1972, in terms of planning and evaluation.

At the beginning, we were hoping to do cross-city studies of commonly-encountered strategies and problems. What we found was that these projects were simply not comparable. In nearly all our fields of effort, what we got basically were case studies. But we did look across the cities and compare these studies, examining areas like drug treatment strategies, police patrol efforts, intensive supervision for juvenile probationers. Again, across the cities, we looked at caseload and trial delay problems in felony courts, which gave us an unhappy familiarity with the recordkeeping systems of some of our courts.

We looked carefully, across the program, for signs of project innovation. Although innovation was not a major objective of the Impact program, everybody was hoping, nonetheless, that there might be some exciting new projects developed despite the difficult analytical constraints of the program. Finally, it turned out that there weren't many, but we did find some, mostly in the area of community-focused projects. We also looked at projects to see if they might be likely candidates for transfer to other places, and we tried to see what could be determined about the probability of institutionalization for many of these projects.

What happened, then, in the Impact program? In all, over three years, the Impact cities implemented 233 action projects, and those projects cost about \$140 million in Federal funds. Generally speaking, the program did focus on corrections--in particular, on the juvenile recidivist offender (see Figure 8)

If you look at the Impact program according to the objectives of each of the projects--and this is possible because, with evaluation planning built in, we had a fairly clear record of precisely what was being expected of each one of these projects--you can divide it into three thrusts or foci. There was a straightforward crime reduction focus which essentially involved police programs, street lighting programs, crime prevention programs. Some of these were community based, and some of them were police based, but all of them had as their intention to reduce crime in a particular area. About 31 percent of program funds went to that kind of effort.

Forty-two percent of program funds went to recidivism reduction, which was essentially an effort to treat, find jobs for, counsel, rehabilitate, individual offenders via various correctional or diversional alternatives.

Finally, what we characterized as a focus on improvement in system capability (that is, efforts which tried to increase capabilities through data systems, research, better management, that kind of thing) accounted for 27 percent of the Federal funds.

Thus the Impact program was not essentially a deterrence program (as it has sometimes mistakenly been called) but was rather a comprehensive criminal justice effort with its major emphasis on offender treatment.

FIGURE 8

**Dimensions of The High-impact
Anti-crime Program in Terms of Emphasis**

Project Focus	Percent of Impact Funding (%)	Funding
Crime Reduction	31	\$ 44.0 M
Recidivism Reduction	42	58.4
Improvement in System Capability	27	37.6
Total	100%	\$140.0 M

Let me just try to summarize now very briefly what our general findings were. Overall, they fall into two gross categories: findings on the objectives, and findings on program management.

The findings on the objectives deal essentially with the COPIE-cycle (that is, the Crime-Oriented Planning, Implementation, and Evaluation cycle which I discussed earlier), with the crime analysis team, with project effectiveness at the city level, and with project institutionalization.

After looking at the various segments of the COPIE-cycle in depth, looking at planning, implementation and the rest in each of the eight cities and across all of them, we found that despite the newness of the concept and the difficulties of implementation, and despite the lack of enforcement mechanisms, all of the eight cities actually did perform this complicated thing. Some of them performed it well (there were four cities that did very creditable jobs) and some of them performed it less well. But we found evidence of quite notable increases in analytical capabilities (new efforts undertaken, new approaches, new products generated), and in research capability, generally, wherever it was performed.

We found evidence that the crime analysis team was effective, but effective under certain circumstances only, quite outside the question of the professional and personal characteristics of the people who were in the teams. It seems that organizational locus was extremely important. When the crime analysis team was in the Mayor's office, or was closely affiliated with it, benefiting from the support and power of the office, it could barter effectively with the various criminal justice agencies; and that was really the essential point in

its ability either to supervise the complicated COPIE-cycle process or to do anything about coordinating agencies. When the team was located elsewhere, it tended to be ignored and to founder.

We also saw that when the team was deprived of the evaluation function, which happened in two cities, it was considerably weakened. Apparently the ability to work closely with agency managers which accompanied the evaluation function, was very important in getting the agencies to accept them. Technical assistance in evaluation was a quid pro quo which could be offered in return for cooperation or coordination. When the teams didn't have that possibility, again they were much less effective.

Four of the crime analysis teams improved agency coordination in their cities. Part of this was due simply to the inauguration of a process whereby staff people from different agencies were obliged to talk to each other on a regular basis. In Cleveland, for example, probation and parole people began working closely together in ways which they had not done before. Eventually, both groups were housed together in the same building. Before Impact, those people didn't speak to each other. There were all kinds of things of that sort that occurred, that were made to happen.

In Denver, a community mechanism was developed which they called the Neighborhoods Task Force. This task force, recruited in the community on a volunteer basis, worked regularly with agencies and the public throughout the whole program. People went out into the community, and it was a little like getting out the vote. They actually got community members involved and to meetings; every month during the program, there were real interchanges among judges, police, people in

all areas of the criminal justice system and the communities they were serving. Some of the meetings were quite heated at times because many people were disturbed by some of the programs which they felt were being foisted on them. These were real interchanges, not lip-service; by the end of the program, new procedures for consulting involved communities before program implementation had developed in Denver.

The COPIE-cycle did permit us to examine project-level effectiveness. We performed secondary analysis, and were able to reinforce city claims of success in reducing crime or recidivism in quite a few instances, accounting for about \$35 million of Federal funds. This doesn't mean that the projects we looked at were the only ones which may have been successful. They were, however, the only ones that had evaluations rigorous enough so that we could attempt to validate their claims.

Our inquiries showed that about 43 percent of the projects funded were set to be institutionalized in one form or another. We believe this to be unlikely, based on past performance in similar programs. Obviously, you would have to return to the cities a year or two from now and see what really comes to pass. The final number will probably be closer to 25 percent, or something like that. (Even that would be very good, however, compared to many other Federal programs.) We did find that institutionalization appeared to depend much more on the support of key personnel than it did on whether the project was good or not, which rather threatens the conventional wisdom.

In terms of program management, we found that New Federalism was much more of a hindrance than a help. It isn't even clear that it elicited the local priorities it was supposed to elicit. I think the data analysis did more for developing priorities than did New Federalism because what really happened was that when you could show, in Baltimore, for example, via data, that you had a tremendous

aggravated assault problem, or in Portland, that the problems were really robbery and burglary, it then became difficult for people to take projects off the shelf and say, "we need to do this or that," when there was no data there to support it. Further, New Federalism was a great hindrance in getting cities to do what they had contracted to do, because the philosophy precluded enforcement mechanisms in the program.

We found evidence that the fiscal review was successful. There seem to have been very few dollars that weren't accounted for in the Impact program.

The program review was much less successful, on the other hand. The state planning agencies and the regional offices didn't have the personnel to do the program monitoring that had to be done, and they didn't have the expertise to review the evaluation plans and reports which needed careful review. The program review was also excessively slow and caused a lot of irritation in the cities. We found that technical assistance to the cities, especially in evaluation, was generally lacking, and I guess this was part of the overall evaluation problem of the period. People didn't realize how much technical assistance was needed. We also found that the absence of national evaluation planning was a serious loss to the program because, of course, a great deal more and better information could have been collected if program development had been accompanied by evaluation planning.

Finally, we found significant data problems. These are much too long and complicated to go into here, but I really would like to quickly mention four of them. First, inadequate agency record-keeping, especially in courts and corrections--there were major gaps and inconsistencies in the records which caused serious problems to city evaluators.

Second, difficulty in using UCR data. I guess everybody knows about that, the discretionary problems, the difficulties that are involved there.

Third, there is a lack of standardized data for measuring recidivism. You can say all you want about how terrible the UCR's are, but they exist. They are there. You can, if you want, go to look at your crime-reduction program, see what you are getting and measure your results against the UCR's. There is nothing to measure recidivism outcomes against. This is an important gap; there is a great need for a standardized data base in this area.

Finally, there was the crucial inability in any Impact city (or elsewhere to my knowledge) to trace an offender from his point of entry into the criminal justice system until his return to society. This meant you couldn't really look at what was happening in your programs and at their impacts, except in little segments. This was again a major problem for evaluation.

All of these data problems again reduced the amount of technical information which evaluation at any level could produce in Impact.

We think LEAA has made considerable use of our findings and recommendations. They have been explicitly examined and incorporated into planning for new programs. One of these programs now specifically implements our recommendations for greatly increased technical assistance to localities; for phased program approval which could put teeth in a program in the sense that we didn't have them in Impact; for management information systems to flag operational problems; for increased program monitoring; and finally, for a much amended and improved COPIE-cycle.

I think there are several reasons why our findings were used by LEAA. First, there was great continuity in the person of the program manager, Dick Barnes, Head of the National Evaluation Program at the National Institute, who endured the stresses and strains of Impact from beginning to end. Despite profound and frequent changes in philosophical approach and in personnel at LEAA and at the National Institute, the program was never "lost from view" or "lacking an organizational home" (in John Evans' terms²⁵), thanks to Dick. Another important factor was that we were able to have a great deal of interaction with both Institute and LEAA decision-makers, to feel very clear in our minds about what kinds of information they needed to get from us and to be able to make changes in our plans in time to be responsive to those needs.

Our major frustrations came from impediments we had to face in the development of relevant information: restrictions on our travel and our presence in the cities, inability to collect our own data (i.e., reliance on the cities to provide us with data), and finally, problems of design arising from not having been involved early on in developing an evaluation plan for the program.

There are thus two areas where we'd still like to see LEAA move in terms of our findings and recommendations. The first one is a much more generalized application of evaluation planning at the national level; this still does not take place routinely at LEAA.

The second is the development of a more effective data policy. We feel, after the Impact program experience, that these two efforts taken together--better evaluation planning and better data--could significantly increase the payoff to evaluation in the criminal justice area.

²⁵ See page 111 above.

THE RESEARCH PERSPECTIVES PANEL (CONTINUED)

V. DISCUSSION (SPEAKERS AND PARTICIPANTS)

MR. GRANDY:

Shall we take some questions?

PARTICIPANT:

I'm Tom White, the Urban Institute. What happened to the other \$20 million?

MS. CHELIMSKY:

That went for planning and evaluation. You mean the discrepancy between \$160 and \$140 million? The \$140 million represents exactly what was spent on the action programs. The other \$20 million were spread across the cities in increments of about \$500,000 for planning and evaluation by the crime analysis teams.

PARTICIPANT:

Do you believe that those programs had an effect, or do you think it's just the luck of the draw?

MS. CHELIMSKY:

Are you asking whether I think they had an effect on crime?

PARTICIPANT:

Or any of those output measures where you believe there is a positive result?

MS. CHELIMSKY:

In those areas where we could get a really close enough look at the phenomenon, where we knew the process in detail and can explain

what we found in terms of the process, we feel we can talk about inference, not effects. You have to remember, our evaluation was essentially a set of case studies. Where we were able to follow crime analysis team operations closely, for example, we do know what happened and we think we know why. Obviously we can't tell you that if we established that team again somewhere else, with different people and a different set of agencies, that the same outputs would be seen. But we do have the sense that we know pretty well "what happened," what the criminal justice problems were, and we have the evidence that the same techniques worked (or failed to work) in several places for reasons that we could document through close attention to the process.

PARTICIPANT:

I am Charlotte Moore with the Congressional Research Service. I just wondered whether you thought the criminal justice evaluation state-of-the-art is at the point now where it can be depended upon for making Congressional policy decisions? You may or may not know that your study was used by the House Subcommittee in its consideration of high-impact funds for cities.

MS. CHELIMSKY:

In comparison to what? Evaluation can certainly make as good a contribution to policy as other types of analysis presently in use. I think there is no doubt about that. But in terms of definitive inputs, in terms of "truth," I have to join some of yesterday's speakers who made the point that evaluation was just one part--a rational part but still only a part--of decision-making. Rather, we are developing evidence which should some day cumulate in better knowledge.

One thing I would like to reiterate about knowledge in the area of crime, and which doesn't appear to be adequately understood,

there are very serious data problems in terms of being able to say something about what has happened in terms of crime, whether, in fact, crime rates have risen or declined. We can't look across cities, that is, compare crime rates from one city to another city, because of differences in police tactics (involving more or less enforcement of the laws, for example) and because of differences in police and in victim reporting. What we have now is different people measuring rates of crime and recidivism in different ways, so we can't really say what they are or compare them across jurisdictions. In some cases you can't get the data. In others, it may be inaccurate. From the viewpoint of Congress, this is a major problem for judging the effectiveness of anti-crime programs.

PARTICIPANT:

I am Daniel Wilner from UCLA. Eleanor Chelimsky, I wonder if you have given thought to the generic problem of data? Yesterday I think we heard from someone from the National Institute of Mental Health²⁶ that there was reliance on the information gathered by the local community mental health centers. You are saying now and bemoaning the fact that, I think, there is a lot of variation in how information is gathered across the cities in the crime and recidivism field. I guess we can multiply the same problem for every area of inquiry in the evaluation field. Have you given thought to the generic issue then of local data and how it's to be used and demanded?

MS. CHELIMSKY:

I have given thought to it, but I don't know the answer.

I think what you can do in local areas is to require a lot more rigor in the record-keeping that people do, develop a lot more

²⁶See pages 144 and 145 above.

understanding of how the data are going to be used (for administrative, management or evaluative purposes). I think all of those capabilities can be vastly improved from what they are now. But there are always going to be problems looking across cities, looking across projects. Even if you have similar projects, you are going to have tremendous variations in the way administrators administer projects. All of those things are going to mean that what is true in one place may not be true in another, and that we really need to know what the data signify in each instance before we can put instances together and examine a strategy--even with much better data than we now have.

I think the aggregation of data is the major problem we face because of local variation. I think it's extremely hard to say that aggregated data means something.

MR. WILNER:

Isn't there a need for some kind of national data policy or strategy in this?

MS. CHELIMSKY:

I think there is. We need to work on that.

PARTICIPANT:

John Greacen from the Police Foundation. I'd like to make an observation and ask a question. In terms of our discussion yesterday about the usefulness of evaluation, it seems to me this evaluation experience is very much in line with the kind of conclusions that I drew from yesterday's discussion. The Impact program as such was terminated by agency action long before the results or even preliminary

results of the national-level evaluation were available at all.²⁷
At the time, I thought that was sad. It seemed to me that evaluation should shed some light on that decision. I now see that that sadness was not necessary at all. Of course those decisions have to be made, and the challenge is to use the evaluation and its result in additional planning, which LEAA has been doing.

The question has to do with another issue. That is one that I find very troubling in the LEAA program, and I thought it was unique to LEAA; and now after yesterday, I find there are other agencies that have the same problem. LEAA is given a mission to enhance the capability of local and state agencies as well as to do things at the Federal level. The Impact program was specifically intended to do that, to create a planning and evaluation capacity at the local level and thereby to improve the performance of local criminal justice agencies. There is some very complicated mix of what can be done best through national evaluations or evaluations at a Federal level and what can best be done through improving the capacity of state and local agencies to do their own kind of work.

What lessons do you get from the Impact program on that question?

MS. CHELIMSKY:

It seems to me that the research gap I was talking about earlier is really what dictates the answer to that question because the issue

²⁷ Editor's Note: There may be some misunderstanding here since the Impact program was only slated to endure over two fiscal (or three calendar) years and did in fact last throughout its expected duration period and longer. The "termination" action to which the participant refers can only have been the announcement by LEAA in January of 1974 that the program would, in fact, be extended through June of 1975 as regards the crime analysis teams, while Impact projects and programs could continue to be funded until September of 1976.

of who does what evaluation, as between local and national efforts, is presently driven more by level of expertise than it is by the appropriateness of the organizational or governmental locus. To improve the interaction between national and state and local evaluations requires you first to improve capabilities at the local level. But how much effort is needed and what will be the payoff to that effort? Why do you need to improve their capabilities, in other words? In the criminal justice area, there is an assumption that improved research or analytical capabilities will result in reduced crime. We know we can't prove that this is so, presently, but most of us believe it. Impact cast little light, I think, on who should do what research but it did show that local capabilities could be improved. That, I guess, is why we were interested in the results of the COPIE-cycle--that it could be done, that it was feasible, that the cities did it and got a lot out of doing it. It's a policy decision whether the ability to do local evaluation is worth the cost of improving local research capabilities. I think it is, but the important question is whether you can get a result that is meaningful to you, not in procedural terms, but in relation to the substantive outcome you are trying to achieve.

END